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THURSDAY, DECEMBER 14, 1871

THE COPLEY MEDALIST OF 1871

DR. JULIUS ROBERT MAYER was educated for the medical profession. In the summer of 1840, as he himself informs us, he was at Java, and there observed that the venous blood of some of his patients had a singularly bright red colour. The observation riveted his attention; he reasoned upon it, and came to the conclusion that the brightness of the colour was due to the fact that a less amount of oxidation sufficed to keep up the temperature of the body in a hot climate than in a cold one. The darkness of the venous blood he regarded as the visible sign of the energy of the oxidation.

It would be trivial to remark that accidents such as this, appealing to minds prepared for them, have often led to great discoveries. Mayer's attention was thereby drawn to the whole question of animal heat. Lavoisier had ascribed this heat to the oxidation of the food. One great principle, says Mayer, of the physiological theory of combustion, is that under all circumstances the same amount of fuel yields by its perfect combustion the same amount of heat; that this law holds good for vital processes; and that hence the living body, notwithstanding all its enigmas and wonders, is incompetent to generate heat out of nothing.

But beyond the power of generating internal heat, the animal organism can also generate heat outside of itself. A blacksmith, for example, by hammering can heat a nail, and a savage by friction can warm wood to its point of ignition. Now unless we give up the physiological axiom that the living body cannot create heat out of nothing, "we are driven," says Mayer, "to the conclusion that it is the *total* heat generated within and *without* that is to be regarded as the true calorific effect of the matter oxidised in the body."

From this again he inferred that the heat generated externally must stand in a fixed relation to the work expended in its production. For, supposing the organic processes to remain the same; if it were possible, by the mere alteration of the apparatus, to generate different amounts of heat by the same amount of work, it would follow that the oxidation of the same amount of material would sometimes yield a less, sometimes a greater, quantity of heat. "Hence," says Mayer, "that a fixed relation subsists between heat and work, is a postulate of the physiological theory of combustion."

This is the simple and natural account given subsequently by Mayer himself of the course of thought started by his observation in Java. But the conviction once formed that an unalterable relation subsists between work and heat, it was inevitable that Mayer should seek to express it numerically. It was also inevitable that a mind like his, having raised itself to clearness on this important point, should push forward to consider the relationship of natural forces generally. At the beginning of 1842 his work had made considerable progress; but he had become physician to the town of Heilbronn, and the duties of his profession limited the time which he could devote to purely scientific inquiry. He thought it wise, therefore,

to secure himself against accident, and in the spring of 1842 wrote to Liebig, asking him to publish in his "Annalen" a brief preliminary notice of the work then accomplished. Liebig did so, and Dr. Mayer's first paper is contained in the May number of the "Annalen" for 1842.

Mayer had reached his conclusions by reflecting on the complex processes of the living body; but his first step in public was to state definitely the physical principles on which his physiological deductions were to rest. He begins, therefore, with the forces of inorganic nature. He finds in the universe two systems of causes which are not mutually convertible;—the different kinds of matter, and the different forms of force. The first quality of both he affirms to be *indestructibility*. A force cannot become nothing, nor can it arise from nothing. Forces are convertible, but not destructible. In the terminology of his time, he then gives clear expression to the ideas of potential and dynamic energy, illustrating his point by a weight resting upon the earth, suspended at a height above the earth, and actually falling to the earth. He next fixes his attention on cases where motion is apparently destroyed without producing other motion; on the shock of inelastic bodies, for example. Under what form does the vanished motion maintain itself? Experiment alone, says Mayer, can help us here. He warms water by stirring it; he refers to the force expended in overcoming friction. Motion in both cases disappears, but heat is generated, and the quantity generated is the equivalent of the motion destroyed. Our locomotives, he observes with extraordinary sagacity, may be compared to distilling apparatus. The heat beneath the boiler passes into the motion of the train, and it is again deposited as heat in the axles and wheels.

A numerical solution of the relation between heat and work was what Mayer aimed at, and towards the end of his first paper he makes the attempt. It was known that a definite amount of air, in rising one degree in temperature, can take up two different amounts of heat. If its volume be kept constant, it takes up one amount; if its pressure be kept constant, it takes up a different amount. These two amounts are called the specific heat under constant volume and under constant pressure. The ratio of the first to the second is as 1 : 1.421. No man, to my knowledge, prior to Dr. Mayer, penetrated the significance of these two numbers. He first saw that the excess 0.421 was not, as then universally supposed, heat actually lodged in the gas, but heat which had been actually consumed by the gas in expanding against pressure. The amount of work here performed was accurately known, the amount of heat consumed was also accurately known, and from these data Mayer determined the mechanical equivalent of heat. Even in this first paper he is able to direct attention to the enormous discrepancy between the theoretic power of the fuel consumed in steam-engines and their useful effect.

Though this first paper contains but the germ of his further labours, I think it may be safely assumed that, as regards the mechanical theory of heat, this obscure Heilbronn physician in the year 1842 was in advance of all the scientific men of the time.

Having, by the publication of this paper, secured him-

self against what he calls "Eventualitäten," he devoted every hour of his spare time to his studies, and in 1845 published a memoir which far transcends his first one in weight and fulness, and, indeed, marks an epoch in the history of science. The title of Mayer's first paper was, "Remarks on the Forces of Inorganic Nature." The title of his second great essay was, "Organic Motion in its Connection with Nutrition." In it he expands and illustrates the physical principles laid down in his first brief paper. He goes fully through the calculation of the mechanical equivalent of heat. He calculates the performances of steam-engines, and finds that 100 lbs. of coal in a good working engine produce only the same amount of heat as 95 lbs. in an unworking one; the 5 lbs. disappearing having been converted into work. He determines the useful effect of gunpowder, and finds 9 per cent. of the force of the consumed charcoal invested on the moving ball. He records observations on the heat generated in water when agitated by a pulping engine of a paper manufactory, and calculates the equivalent of that heat in horsepower. He compares chemical combination with mechanical combination—the union of atoms with the union of falling bodies with the earth. He calculates the velocity with which a body starting at an infinite distance would strike the earth's surface, and finds that the heat generated by its collision would raise an equal weight of water 17,356° C. in temperature. He then determines the thermal effect which would be produced by the earth itself falling into the sun. So that here, in 1845, we have the germ of that meteoric theory of the sun's heat which Mayer developed with such extraordinary ability three years afterwards. He also points to the almost exclusive efficacy of the sun's heat in producing mechanical motions upon the earth, winding up with the profound remark, that the heat developed by friction on the wheels of our wind and water-mills comes from the sun in the form of vibratory motion; while the heat produced by mills driven by tidal action is generated at the expense of the earth's axial rotation.

Having thus with firm step passed through the powers of inorganic nature, his next object is to bring his principles to bear upon the phenomena of vegetable and animal life. Wood and coal can burn; whence come their heat, and the work producible by that heat? From the immeasurable reservoir of the sun. Nature has proposed to herself the task of storing up the light which streams earthward from the sun, and of casting into a permanent form the most fugitive of all powers. To this end she has overspread the earth with organisms which, while living, take in the solar light, and by its consumption generate forces of another kind. These organisms are plants. The vegetable world indeed constitutes the instrument whereby the wave-motion of the sun is changed into the rigid form of chemical tension, and thus prepared for future use. With this prevision, as shall subsequently be shown, the existence of the human race itself is inseparably connected. It is to be observed that Mayer's utterances are far from being anticipated by vague statements regarding the "stimulus" of light, or regarding coal as "bottled sunlight." He first saw the full meaning of De Saussure's observation of the reducing power of the solar rays, and gave that observation its proper place in the doctrine of conservation. In the leaves of a tree, the carbon and oxygen of carbonic acid, and the hydrogen and oxygen of water, are forced asunder at

the expense of the sun, and the amount of power thus sacrificed is accurately restored by the combustion of the tree. The heat and work potential in our coal strata are so much strength withdrawn from the sun of former ages. Mayer lays the axe to the root of many notions regarding the vital force which were prevalent when he wrote. With the plain fact before us that plants cannot perform the work of reduction, or generate chemical tensions, in the absence of the solar rays, it is, he contends, incredible that these tensions should be caused by the mystic play of the vital force. Such an hypothesis would cut off all investigation; it would land us in a chaos of unbridled phantasy. "I count," he says, "therefore, upon assent when I state as an axiomatic truth that during vital processes the *conversion* only and never the *creation* of matter or force occurs."

Having cleared his way through the vegetable world, as he had previously done through inorganic nature, Mayer passes on to the other organic kingdom. The physical forces collected by plants become the property of animals. Animals consume vegetables, and cause them to reunite with the atmospheric oxygen. Animal heat is thus produced, and not only animal heat but animal motion. There is no indistinctness about Mayer here; he grasps his subject in all its details, and reduces to figures the concomitants of muscular action. A bowler who imparts to an 8-lb. ball a velocity of 30 feet consumes in the act $\frac{1}{10}$ of a grain of carbon. A man weighing 150 lbs., who lifts his own body to a height of 8 feet, consumes in the act 1 grain of carbon. In climbing a mountain 10,000 feet high, the consumption of the same man would be 2 oz. 4 drs. 50 grs. of carbon. Boussingault had determined experimentally the addition to be made to the food of horses when actively working, and Liebig had determined the addition to be made in the case of men. Employing the mechanical equivalent of heat, which he had previously calculated, Mayer proves the additional food to be amply sufficient to cover the increased oxidation.

But he does not content himself with showing in a general way that the human body burns according to definite laws, when it performs mechanical work. He seeks to determine the particular portion of the body consumed, and in doing so executes some noteworthy calculations. The muscles of a labourer 150 lbs. in weight, weigh 64 lbs.; when perfectly desiccated they fall to 15 lbs. Were the oxidation corresponding to that labourer's work exerted on the muscles alone, they would be utterly consumed in 80 days. The heart furnishes a still more striking example. Were the oxidation necessary to sustain the heart's action exerted upon its own tissue, it would be utterly consumed in 8 days. And if we confine our attention to the two ventricles, their action would be sufficient to consume the associated muscular tissue in $3\frac{1}{2}$ days. Here, in his own words, emphasised in his own way, is Mayer's pregnant conclusion from these calculations:—"The muscle is only the apparatus by means of which the conversion of the force is effected; *but it is not the substance consumed in the production of the mechanical effect.*" He calls the blood "the oil of the lamp of life;" it is the slow-burning fluid whose chemical force in the furnace of the capillaries is sacrificed to produce animal motion. This was Mayer's conclusion twenty-six years ago. It was in complete opposition to the scientific conclusions of his time; but eminent investigators have since amply verified it,

Thus, in baldest outline, I have sought to give some notion of the first half of this marvellous essay. The second half is so exclusively physiological that I do not wish to meddle with it. I will only add the illustration employed by Mayer to explain the action of the nerves upon the muscles. As an engineer, by the motion of his finger in opening a valve or loosing a detent, can liberate an amount of mechanical motion almost infinite compared with its exciting cause, so the nerves, acting upon the muscles, can unlock an amount of activity wholly out of proportion to the work done by the nerves themselves.

As regards these questions of weightiest import to the science of physiology, Dr. Mayer in 1845 was assuredly far in advance of all living men.

Mayer grasped the mechanical theory of heat with commanding power, illustrating it and applying it in the most diverse domains. He began, as we have seen, with physical principles; he determined the numerical relation between heat and work; he revealed the source of the energies of the vegetable world, and showed the relationship of the heat of our fires to solar heat. He followed the energies which were potential in the vegetable up to their local exhaustion in the animal. But in 1845 a new thought was forced upon him by his calculations. He then for the first time drew attention to the astounding amount of heat generated by gravity where the force has sufficient distance to act through. He proved, as I have before stated, the heat of collision of a body falling from an infinite distance to the earth, to be sufficient to raise the temperature of a quantity of water equal to the falling body in weight $17,356^{\circ}\text{C}$. He also found in 1845 that the gravitating force between the earth and sun was competent to generate an amount of heat equal to that obtainable from the combustion of 6,000 times the weight of the earth of solid coal. With the quickness of genius he saw that we had here a power sufficient to produce the enormous temperature of the sun, and also to account for the primal molten condition of our own planet. Mayer shows the utter inadequacy of chemical forces, as we know them, to produce or maintain the solar temperature. He shows that were the sun a lump of coal, it would be utterly consumed in 5,000 years. He shows the difficulties attending the assumption that the sun is a cooling body; for supposing it to possess the high specific heat of water, its temperature would fall $15,000^{\circ}$ in 5,000 years. He finally concludes that the light and heat of the sun are maintained by the constant impact of meteoric matter. I never ventured an opinion as to the accuracy of this theory; that is a question which may still have to be fought out. But I refer to it as an illustration of the force of genius with which Mayer followed the mechanical theory of heat through all its applications. Whether the meteoric theory be a matter of fact or not, with him abides the honour of proving to demonstration that the light and heat of suns and stars *may* be originated and maintained by the collisions of cold planetary matter.

It is the man who from the scantiest data could accomplish all this in six short years, and in the hours snatched from the duties of an arduous profession, that the Royal Society has this year crowned with its highest honour. Dr. Mayer had never previously received any mark of recognition from the society.

It was not in my power to be present at our late president's last address; but Sir Edward Sabine has done me the honour of sending me a printed copy of it. It contains the reasons assigned by him for the award of the Copley medal. Briefly, but appreciatingly, he expresses his opinion of the merits of Dr. Mayer, committing to Prof. Stokes the task of drawing up a fuller statement of the case. This statement is marked by an evident desire to act fairly towards Mayer, and at the same time to qualify the award so that no erroneous inferences may be drawn from it. It will be observed that Prof. Stokes confines himself to Mayer's first paper, the real value of which, however, is best appreciated in connection with Mayer's subsequent work, as the soundness of the root is best demonstrated by the vigour of the tree. Prof. Stokes writes thus:—

"In a paper published in 1842, Mayer showed that he clearly conceived the convertibility of falling force, or of the *vis viva*, which is its equivalent or representative in visible motion, into heat, which again can disappear as heat by reversion into work or *vis viva*, as the case may be. He pointed out the mechanical equivalent of heat as a fundamental datum, like the space through which a body falls in one second, to be obtained from experiment. He went further. When air is condensed by the application of pressure, heat, as is well known, is produced. Taking the heat so produced as the equivalent of the work done in compressing the air, Mayer obtained a numerical value of the mechanical equivalent of heat, which, when corrected by employing a more precise value of the specific heat of air than that accessible to Mayer, does not much differ from Joule's result. This was undoubtedly a bold idea, and the numerical value obtained by Mayer's method is, as we now know, very nearly correct." Prof. Stokes then qualifies the award in these words:—"Nevertheless it must be observed that an essential condition in a trustworthy determination is wanting in Mayer's method; *the portion of matter operated on does not go through a cycle of changes*. Mayer reasons as if the production of heat were the sole effect of the work done in compressing air. But the volume of the air is changed at the same time, and it is quite impossible to say *a priori* whether this change may not involve what is analogous to the statical compression of a spring, in which a portion or even a large portion of the work done in compression may have been expended. In that case the numerical result given by Mayer's method would have been erroneous, and *might* have been even widely erroneous. Hence the practical correctness of the equivalent obtained by Mayer's method must not lead us to shut our eyes to the merit of our own countryman Joule, in being the first to determine the mechanical equivalent of heat by methods which are unexceptionable, as fulfilling the essential condition that no ultimate change of state is produced in the matter operated upon."

The judgment of Prof. Stokes, regarding the possible error of Mayer's determination of the mechanical equivalent of heat, gives me occasion to cite another proof of the insight of this extraordinary man. His paper of 1845 contains the details of his calculation, which were omitted from his first brief paper. Mayer prefaces the calculation with these memorable words:—

"To prove this important proposition, we must fix our attention on the deportment of elastic fluids towards heat and mechanical effect.

"Gay Lussac has proved by experiment that when an elastic fluid streams from one receiver into a second exhausted one of equal size, the first vessel is cooled, and the second one heated, by exactly the same number of degrees. This experiment, which is distinguished for its simplicity, and which, to other observers, has always yielded the same result, shows that a given weight and volume of an elastic fluid may expand to double, quadruple, in short, to several times its volume without experiencing, on the whole, any change of temperature; or, in other words, that for the expansion of the gas of itself (*an und für sich*), no expenditure of heat is necessary. But it is equally proved that a gas which expands *under pressure* suffers a *diminution* of temperature.

"Let a cubic inch of air at 1° , and under the pressure of 30 inches of mercury, be warmed by the quantity of heat x to 274° C., its volume being kept constant; this air, on being permitted to stream into a second exhausted vessel of the same size, will retain the temperature of 274° , and a medium surrounding the vessel will suffer no change of temperature. In another experiment, let our cubic inch of air be kept, not at constant *volume*, but under the constant *pressure* of the 30-inch mercurial column, and heated to 274° . In this case a greater quantity of heat is required; let it be $x + y$.

"In comparing these two processes, we see that in both of them the air is heated from 0° to 274° , and at the same time permitted to expand from one volume to two volumes. In the first case the quantity of heat necessary was $= x$, in the second case $= x + y$. In the first case the mechanical effect was $= 0$, in the second case it was equal to 15 lbs. raised one inch in height."

He then proceeds with his calculation.

Here it will be seen that Mayer was quite awake to the importance of the considerations dwelt upon by Prof. Stokes—that he knowingly chose for his determination a substance which, *an und für sich*, in expanding, consumes no heat. Hence, when by its expansion *against pressure* heat is consumed, no part of that heat is lost in producing "a change of state in the matter operated upon." The heat consumed is, therefore, the pure equivalent of the work done.

With regard to Dr. Joule, I have, to my regret, vainly endeavoured to find a mislaid document written a year ago, in which I ventured to describe his labours,* and to express the esteem I entertain for them. Supposing him to have derived his inspiration from Mayer's papers, that they had even caused him to prosecute his experiments on the mechanical equivalent of heat, he would still have rendered immortal service to science, and more than merited the honours bestowed upon him last year. For, wanting his work, the mechanical theory, however strong the presumptions, and however concurrent the evidence in its favour, could not be regarded as completely demonstrated. But Joule was not stimulated by Mayer. His work is his own, being practically contemporaneous with that of Mayer. He not only demonstrated experimentally the mechanical theory of heat, but in its completer form he was an independent creator of that theory. And so impressed was the Council of the Royal Society last year with the magnitude of his

merits, that they actually added to the Rumford Medal already bestowed upon him, the final distinction of the Copley Medal. If England rated him as highly as I do, his reward would not be confined to mere scientific recognition.

As regards the latter, however, I do not think that the possibility suggested by Prof. Stokes represents any real danger. I do not imagine that the eyes of Science are in the least degree likely to be "shut to the merits of our own countryman." And I believe that the Royal Society, by stamping in two consecutive years these two men with the highest mark of its approval, will have strengthened that confidence in its impartiality which, throughout the whole scientific world, it has so long and so justly enjoyed.

JOHN TYNDALL

AIRY ON MAGNETISM

A Treatise on Magnetism. By G. B. Airy, Astronomer Royal. (Macmillan and Co.)

THIS is a book written upon the true scientific principle expressed by Newton when he said "Hypotheses non fingo." The elementary laws of magnetism are deduced by rigorous induction from particular cases and are then applied to explain phenomena. The book contains the substance of a series of lectures delivered by the Astronomer Royal at the University of Cambridge. One great element of excellence in the book is that the mathematics employed throughout are of a simple character, so that the first principles of magnetism are thus thrown open to one who has gone no great way in mathematical reading.

Formulæ having been obtained in the early sections for the action of one magnet on another, and the law of the inverse square having been established by a comparison of calculation with experiment, the great bulk of the volume is occupied in investigations which bear more directly on terrestrial magnetism and the magnetism of iron ships. The methods of determining the values of the magnetic elements at any place are carefully explained and illustrated, and the necessary formulæ deduced from the theory established in the preceding sections. We would especially recommend to the reader's attention the articles on the theory of the dipping needle. One chapter of extreme interest is devoted to "Theories of Terrestrial Magnetism," and the beautiful theory of Gauss is sketched out. We sincerely hope that that theory which was carried by Gauss to the fourth order of approximation will be before long carried to a higher order. Data now exist for this advance, as it requires accurate determinations of only eleven more elements.

The subject of the deviation of the compass in iron ships is one upon which the Astronomer Royal is peculiarly justified in speaking or writing. All the sections relating to the disturbance of compass needles are full of most important and suggestive matter. One section is devoted to the continuous registration of small changes in terrestrial magnetism, and the concluding section just touches on the subject of the relation between galvanic currents and magnetic forces, without entering into any calculations.

The book supplies a distinct want which has hitherto existed in the list of our mathematical text-books, and is a most valuable contribution to the diffusion of physico-mathematical science.

JAMES STUART

* Thanks to the friendly efforts of Dr. Sharpey, this document reached my hands just as the proof of this paper was being returned for press. With the permission of the Editor of NATURE I will publish the document, with some additional matter, next week.
J. T.

OUR BOOK SHELF

Rudimentary Treatise on Geology.—Part II. Historical Geology. By Ralph Tate, A.L.S., F.G.S., &c. With Illustrations and an Index. (London: Lockwood and Co.)

THIS little book is partly based on Portlock's "Rudiments of Geology," and "is set forth in the full belief that it will be found to be an epitome of the history of the British Stratified Rocks." The first three chapters are introductory, and contain the usual table of the British Sedimentary Strata, with some brief remarks thereon, which are followed by what the author calls a "Palæontological Summary." In this summary he takes a rapid view of the animal and vegetable kingdoms, and points out briefly under which classes and orders fossil organic remains may be ranged. The rest of the volume is entirely occupied with descriptions of the Formations and their subdivisions, and with lists of characteristic fossils. We have no doubt that the preparation of this book has cost its compiler considerable labour; and he certainly has managed to cram a good deal into the short space at his command. The information, indeed, is just too tightly packed; it forms very dry reading, and will be apt to frighten a beginner. If it was necessary that the volume should be no larger than it is, we think some of the palæontological details might have been omitted, and here and there the description of minor subdivisions of formations conveniently cut even shorter than they are, so as to obtain room for certain particulars about the history of the strata, which are either too meagrely noticed or are altogether ignored. The references to former volcanic action in Britain are quite inadequate. We find no mention of the fact that volcanoes were active in the South-West of England during the deposition of the Devonian Strata; nor is there any notice taken of the occurrence of volcanic rocks in the Old Red Sandstone of Ireland. A slight allusion is made to the igneous rocks of the Scottish Middle Old Red Sandstone, but the far more extensive volcanic products belonging to the Lower Old Red series are passed over altogether. The igneous rocks of the Pentland Hills are not, as the author states, of "Upper," but of Lower Old Red Sandstone age. Again the reader, looking over what is said about the igneous rocks of Carboniferous age, would never learn that volcanoes played so active a part in Scotland during the accumulation of the Lower Carboniferous and Carboniferous Limestone periods; nor that in Ireland also volcanoes here and there piled up ejectamenta upon the bed of the Carboniferous Limestone sea. Surely in a book purporting to be an epitome of the history of British stratified rocks, the volcanic phenomena that characterise so many successive epochs of the past ought to have had a somewhat fuller notice. There are various other points in connection with physical geology which are quite ignored. For instance we find no mention of Prof. Ramsay's theory of the Glacial origin of certain breccias and conglomerates of Silurian, Old Red Sandstone, and Permian age—a theory which, whether Mr. Tate agrees with the Professor or not, ought certainly to have had some reference made to it no matter how brief. We had marked a number of passages where the author's meaning is not very clear and will be apt to puzzle a learner. One of these will suffice. Speaking of the Glacial epoch, the author says:—"Our inquiry has now come to that point where, though we still see in the recent results of geological phenomena evidence of the formative processes of nature, yet we are kept at a distance from the present epoch; for although the shells are all of living species, they are generally arranged in positions and associated with detrital matters of such a description that their appearance indicates the action of forces prior to the present order of things." Occasionally we come across statements which are very far from being consistent "with the opinions generally

held by geologists." We read, for instance, that "the first trace of a land plant is at the very top of the Upper Silurian, and we may conclude that there were no terrestrial plants during the long Silurian epoch, a vast interval far exceeding in duration that of any other system."

Besides figures of characteristic fossils, the volume is illustrated with a number of diagrammatic sections. A copious index is appended. J. G.

Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College. No. IV. Deep-sea Corals. By L. F. de Pourtales, Assistant U.S. Coast Survey. 1871.

COUNT POURTALES had the good fortune to be one of that band of naturalists who, dredging for the first time in deep water between Key West and Havana, came to the conclusion that "animal life exists at great depths in as great an abundance as in shallow water." This opinion was published in his "Contributions to the Fauna of the Gulf Stream at great Depths" (Cambridge, U.S., 1867). Moreover as a zoophytologist he had the credit of obtaining the first true stony corals from great depths. Numerous corals were dredged up under his superintendence in 1868 and 1869 from off the sea floor of the so-called Straits of Florida in the course of the Gulf Stream, and they were carefully described by him in Nos. 6 and 7 of the last-mentioned work. Now the results of the Deep-sea Dredging so far as the Corals are concerned, appear in the handsome essay in quarto before us; the specific descriptions have been revised, new forms are described, and the illustrations in lithography testify to the excellence of American printing from stone. The interesting coral fauna in the deep sea of Florida has already to a certain extent been compared with that of the cold and warm area of the North Atlantic, in the Proceedings of the Royal Society, March 24, 1870; and the new species described by M. de Pourtales, together with the remarks upon the classification of the corals, will probably enhance the importance of the labours of those English naturalists who have undertaken the description of the results of our abyssal dredgings. The great horizontal range of some of the deep-sea corals is as remarkable as the vertical range of others; and M. de Pourtales, although strongly impressed with the importance of some structural characters in the distinction of specific differences which are not thought so valuable and important in England, leans to the belief in these ranges. The American deep-sea coral fauna is not so rich in species, and apparently in individuals, as that of the North Atlantic and Lusitanian Coasts, but there is one form which is found in the globigerina mud off Bahia Honda, Florida, in 324 fathoms, which will always be of interest to the naturalist who studies palæontology. *Haplophyllia paradoxo* Pourtales, possesses all the essential characters of the Rugosa, and is allied to the simple coral, *Calophyllum profundum* Germar—the Permian *Polycalcia profunda* of King, but it has been shown to be also allied to *Guyonia annulata* Duncan, a small rugose coral dredged off the Adventure Bank in the Mediterranean. Both *Haplophyllia* and *Guyonia* have a strong central axis or columella, the existence of which is of generic importance, and it is therefore necessary to ally these two modern representatives of the old Rugosa which dominated in the coral fauna of the Palæozoic age with the Cyathaxonidæ of the Carboniferous rocks. M. de Pourtales is so gentle a critic that if one wished to differ from him in print, the desire would fail. When the Zoological Society print, which they are about to do, the Essay on the deep-sea corals dredged from H.M.S. *Porcupine*, nothing will be more satisfactory than that an interchange of notes and specimens should take place, so that in a supplement the American and English authors may terminate their unimportant little differences in classification. The beauty and correctness of the illustrations are extreme, and they do the artist, and especially the printer, great credit. It

is to be hoped that some English lithographic printer will see the American triumph in this particular, and will forthwith mend his ways.
P. M. D.

LETTERS TO THE EDITOR

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

Alternation of Generations in Fungi

IN Mr. Cooke's article on this subject, it is stated that I have shown that there are at least four consecutive forms of reproductive cells in the bunt (*Tilletia caries*). I imagine that by a slip of the pen he must have substituted this for hop mildew; but, be this as it may, what I really did say at a time (1847) when the formation of secondary fruit was not ascertained in *Ustilago*, *Puccinia*, and allied parasites, was as follows, after describing the curious anastomosing threads which are produced on the germinating processes of the bunt spores:—"I was at first inclined to think that it had something to do with the reproduction of the bunt, and it is quite possible that in plants, as well as in the lower animals, there may be an alternation of generations. This is, however, merely thrown out as a hint which may be followed out by those who have fewer avocations than myself. Many anomalous appearances, amongst *Algæ* especially, seem to indicate something of the kind."* This growth can only be regarded as an intermediate state, which is probably necessary for the propagation of the parasite, and the same must be said of other cases in which the anomalous form does not produce organisms similar to itself. In such cases as the hop and vine mildew, the *Oidium* forms may be propagated almost indefinitely with only an occasional production of another form, and this, perhaps, may safely be regarded as an alternation of generations, while mere conidia-bearing forms can scarcely be so regarded. In such cases as that of the *Uredos*, which accompany or precede *Puccinia*, though both are fertile, we can scarcely recognise such an alternation; but if it is once established that a *Puccinia* produces an *Æcidium*, or an *Æcidium* a *Puccinia*, we should have a clear case. The usual argument about wheat being subject to mildew where there are no berberry plants, or *Røstelia* where there are no savines, does not seem to me to be good. It appears quite clear that wheat mildew may be produced, either from the germination of *U. rubigo vera*, or from its own secondary spores, and that almost indefinitely, where there is no berberry; but this does not show that the spores of *Puccinia*, when sown on the berberry leaf, may not produce the *Æcidium*, or the spores of the *Æcidium* the mildew. I quite agree with Mr. Cooke, that the observations of Oersted and De Bary are not absolutely conclusive, though I may be inclined to give them more weight than he does. The observations should certainly be repeated; but, if the results should be the same, I should certainly feel inclined to accede to their views, indisposed as I always am either to jump hastily to conclusions myself, or to accede at once to the crude observations of others.

M. J. BERKELEY

WHETHER Mr. Cooke has sufficiently appreciated the labours of De Bary and Oersted, in his article published in your columns of last week under the above title, I leave for others to determine. I wish now merely to call attention to one sentence in his article, as follows:—"It is manifest that no amount of care in cultivation, under bell glasses or other exclusion from foreign influences, is sufficient against a contingency which dates back to the seed of the nurse-plant." Does Mr. Cooke mean that the spores of the fungi themselves deposited in the seed of the nurse-plant are carried up, so to speak, in the process of growth, into the leaves, where they germinate; or that the liability to produce parasitic fungi is communicated from the seed to the mature plant by some process which combines the Pangenesis of Darwin with the spontaneous generation of Bastian? I see no other explanation of the sentence than one or other of these alternatives.

MYCELIUM

Leibnitz and the Calculus

PROF. TAIT need not wonder if an attack that is "totally unexpected" should seem "appalling sudden." In the absence of a statute of limitations restricting to two years and a half

* "Journal of Horticultural Society of London," vol. ii. p. 112.

the right to take up a gage, there can be no reason why an attack should not be made, save its personal bearings; and the circumstances of the challenge might be cited in bar of any exception taken on that ground. I thank the Professor for his explanations. I could not have guessed that under cover of his challenge to produce a metaphysician who was also a mathematician, lurked the assumptions, that every mathematician was a metaphysician, and that every metaphysician was either a mathematician or (in the old sense) a physician. Well, he has a perfect right, for his own private convenience or pleasure, to identify two names which he had from the first asserted to be eternally distinct. Accepting his classification, then, for the sake of argument—certainly not for fruitless controversy—to wit, that everyone is either a mathematician or a non-mathematician, and that every true metaphysician must be either mathematician or physician (Faraday did not hate the term "physicist" worse than I do) we are confronted with some surprising results. Leibnitz, the author of the *Monadologie* and the *Théodicée*, works that are known to contain the germs of the *Kritik der reinen Vernunft*, was a *spurious* metaphysician. Why, in the name of common sense? "Because," says Prof. Tait, "he was a non-mathematician; there is no medium, you know; he must have been either a non-mathematician or a mathematician, and a mathematician he was not." What! Leibnitz not a mathematician? "Not a bit of it," says Prof. Tait; "for he was, I fear, simply a thief as regards mathematics, and in physics he did not allow the truth of Newton's discoveries." I do not object to the Professor calling a spade a spade; but I assure him that this charge is made just twenty years too late. It is exactly that time since the last vestige of presumption against the fair fame of the great German was obliterated. If Prof. Tait does not understand me, or, understanding me, disputes the unqualified truth of my statement, I promise to be more explicit in a future letter. But I incline to think the question is not susceptible of proof until the Council of the Royal Society, who so grossly disgraced themselves in 1712, shall do the simple act of justice and reparation required of them, viz., publish the letters and papers relating to this controversy, which since that date have slumbered in the secret archives. I advise Prof. Tait to utilise the meantime by reconsidering some of his utterances on the *Principia*, lib. ii. lem. 2.

It appears, too, that Descartes, notwithstanding his physics, which are very sad, was a mathematician, and therefore a true metaphysician, and this, I suppose, despite the *spurious* metaphysics of his *Discours* and his *Méditations*. By the way, when Prof. Tait parenthetically and admirably corrects me for calling him *Cartes*, he surely overlooked the fact that *Cartes* is his English name, the name by which he was known to the readers of Dr. Samuel Clarke, &c., and is therefore preferable to the dog-latin alternative.

Such, then are some of the surprising results of adopting Prof. Tait's classification of mathematicians and metaphysicians. But he objects to my classification of the former, that the greatest mathematicians of our own day—among which Prof. Tait will allow me to count himself—would fall into my second class, since they are not inventors of a calculus, and yet they are not mere experts. Among the names he adduces are Cayley and Sylvester, the co-inventors of a new calculus, viz., that which has been so fertile in its application to Linear Transformations; I mean, of course, the Higher Algebra. Accordingly, both would, of course, fall into my first class; and I will add, that I should assuredly think that "something is rotten in the state of Denmark" if I found the true mathematical *πρωτης* had ever contented himself with the improvement and application of other men's productions.

C. M. INGLEHER

Highgate, Dec. 4

The Science and Art Department

I HAVE been expecting, but in vain, to see Mr. Uhlgren's reply to the request made to him a few weeks since, to produce the Department's letter of which he spoke, and in which it was stated that the rumoured reduction of the number of certificates awarded had actually taken place through the examination papers having been returned for revision. I quite agree with your correspondent who challenged its production, that such a document ought to be made widely known if it exists; whereas if Mr. Uhlgren's statement is founded on any misapprehension, it ought to be corrected without delay.

If such a statement were unfounded, such complaints as those Mr. U. made are, I think, more likely to damage the cause of

science teachers, who have already enough grievances to urge against the Department on the score of its administration, than to obtain any amelioration of their status.

I do not think many science teachers will endorse more than one other of Mr. Uhlgren's complaints; so that it is of the greatest importance that that one which affects them all should be proved in the fullest and most circumstantial manner.

Plymouth, Dec. 9

A LOCAL COMMITTEE-MAN

Lunar Calendars

I wish to call attention to the variations observable between the true period of new moon and the commencement of lunar months, as set forth in the following table:—

Period of New Moons A.D. 1872 H.M.	Jewish Calendar A.M. 5632-3	Mahomedan Calendar A.H. 1288-9
Jan. 10 2.58 P.M.	Shebat commences 11	Jan. 12 Dulkanadah
Feb. 9 1.52 A.M.	1st Adar " 10	Feb. 11 Dulhagee
Mar. 9 0.53 P.M.	2nd " " 11	Mar. 11 Mulharram
April 8 0.32 A.M.	Nisan " 9	April 10 Saphar
May 7 1.19 P.M.	Iyar " 9	May 9 Rabia (i.)
June 6 3.23 A.M.	Sivan " 7	June 8 " (ii.)
July 5 6.25 P.M.	Tammuz " 7	July 7 Gomada (i.)
Aug. 4 9.46 A.M.	Ab " 5	Aug. 6 " (ii.)
Sep. 3 0.54 A.M.	Ellul " 4	Sept. 4 Rajab
Oct. 2 3.31 P.M.	Tishri " 3	Oct. 4 Shabân
Nov. 1 5.28 A.M.	Heshvan " 2	Nov. 2 Ramadân
Dec. 30 6.35 P.M.	Kislev " 1	Dec. 2 Shawal
Dec. 30 6.36 A.M.	Tebeth " 31	Jan. 31 Dulkanadah

As many eminent and practical astronomers write to NATURE, I shall be much obliged if some one will add a fourth column to the above, fully explaining these differences. My object is to ascertain if a calendar, founded on lunations, is at all susceptible of universal use, so as to be correct to time in all places. The true new moon is invisible, the visible new moon is not the true new moon; is there a medial average?

November 23

MYOFS

New Zealand Forest Trees

LET me recommend those of your readers who take an interest in this subject, to trust for correct information thereabout to the works whose names are appended, and not to the statements of recent correspondents of NATURE, who commit errors so great as to refer *Manuka* to the genus or family *Diosma*!

- (1) Dr. Hooker's "Handbook of the New Zealand Flora," which contains at the end of vol. ii. an "Alphabetical List of Native and Vernacular Names" of New Zealand plants, including trees.
- (2) A similar Catalogue of Native and Vernacular Names, published, subsequently to Dr. Hooker's list, by Dr. Hector, Director of the Geological Survey of New Zealand.
- (3) "Report and Award of the Jurors" of the New Zealand Exhibition of 1865; which contains at page 474 an admirable table—showing the strength and other qualities of New Zealand woods, in connection with the names of the trees yielding the said timbers—carefully drawn up by the late Provincial Marine Engineer of Otago, J. M. Balfour, C.E.; and
- (4) The 3 vols. already published of the "Transactions and Proceedings of the New Zealand Institute."

W. LAUDER LINDSAY

Solar Halo

SEEING in your last number an account of a Solar Halo, it has occurred to me that the following description of a similar phenomenon, which I saw in Norway this autumn, may not be uninteresting to some of your readers.

The sun, at 4 o'clock P.M., was just setting behind a range of mountains in the Romsdalen, when a bright halo of light appeared round it, forming a clearly-defined circle, and at the crown of the circle there appeared two horns, as of the beginning of another circle inverted, the junction of the two circles being very luminous; the limbs of the inverted circle—if I may so call it—were rather straight than curved, and were not very long. A

second and outer circle, just twice the diameter of the inner one, shortly appeared, and this circle had all the colours of a rainbow most distinctly visible. These two bows were strongly defined for an hour at least, and during that time constant waves of light shot up and across the sky, not always from the centre, where the sun was, but often from some point within the inner circle to the south of its centre. At other times rays of light would shoot out at a tangent from the outer bow, sometimes on one side and sometimes on the other. Again, some would shoot from one circle to the other, forming a series of bars parallel with the horizon, and at last the rays seemed to concentrate, and, radiating from the centre of the inner circle, shot right through both circles across the sky over our heads, forming a series of gigantic ribs, which extended from west to east.

The day (it was September 23) had been perfect, with a bright sun, a cold, frosty atmosphere, and a blue, cloudless sky. Snow had fallen heavily about three days before, and was still lying everywhere; but on the day we saw this grand display not a cloud had been visible from morning till evening. After all was over, the clouds crept up, and we saw several brilliant shoots of the Northern Lights.

W. W. HARRIS

Manningham, Bradford, Dec. 6

Proof of Napier's Rules

SUCH a structure in cardboard as that described by Prof. A. S. Herschel in NATURE, No. 106, may be found very useful in facilitating the study of the proof of "Napier's Rules," but the ingenious learner might object that the demonstration was confined to one particular species of triangle—the isosceles right-angled with a perimeter equal to a quadrant; for Mr. Herschel's angles a and b are plainly equal, and together with c make up a right angle. The corresponding construction for any case would be as follow:—Take a circular piece of cardboard with centre D (referring to Mr. Herschel's diagram), and on the circumference, in the same direction, take any two arcs B₁, 12. Let a perpendicular from B or D₁ meet it in D, and a second from C or D₂ meet it in A, and be produced to reach the circumference in B'. Finally, a semicircle on A B' as diameter and another with centre A and radius A C will determine by their intersection the point C'. To a construction thus generalised all that Prof. Herschel adds would apply.

As a question of "Queen's English," it seems hard to connect the last clause in the first paragraph of Prof. Herschel's letter with what precedes. "Them" can only refer grammatically to "difficulties;" but surely Mr. Cooley did not propose to himself 'to render them as easily accessible as possible to the inquiring student in mathematics.'

J. J. W.

The Cause of Specific Variation

I HAVE only just read Mr. Mivart's "Genesis of Species," and was glad to find that his ideas, so ably expressed, are nearly, if not quite, identical with my own, which I laid before the Victoria Institute in a paper "On Certain Analogies between the Method of Deity in Nature and Revelation," May 10, 1869. On p. 259 of his "Genesis of Species" he has the following remarks:—"But are there any grounds for thinking that, in the Genesis of Species, an internal force or tendency intervenes, co-operating with and controlling the action of external conditions?" This question appears to me to exactly correspond with the sentiments of the following passage from the "Journal of the Transactions of the Victoria Institute," vol. iv., p. 265:—

"Rather than venture on any attempt to explain the Divine methods by ordinary terms, I would prefer adopting some general expressions to convey an imagined idea of the causes of existing things, and as less liable to the charge of anthropomorphism.

"I purpose, therefore, adopting the general word *force*, and recognising all issues in nature as the effect produced upon matter by the resultant of component forces. These forces are separable into physical, chemical, biological, &c.; and, in addition to all those which the chemist and the physicist can eliminate and claim as the objects of their special studies, there still remains a residuum of forces in those organisms endowed with life, and which produce those results which we say are designed, and which it is customary to regard as witnessing to a Divine Intelligence.

"In recognising these latter forces, I would call them evolute, but as being so far like others that their resultant with them produces relative effects only according as in their continual

attempt at equilibration they are more or less counteracted or assisted by other natural forces.

"As an illustration I would recognise every special issue of evolution, as, for example, some well-marked variety of animal (say pigeon) or plant (say rose) as the effect of the combination of the usually so-called natural forces in conjunction with the evolutive, as a temporary stable form, so long as environing conditions to which it was subjected remain the same. Hence appears the permanency of some species and races. Subject them, however, to altered conditions, and thus bring an unaccustomed set of forces to bear upon them, e.g., by domestication or cultivation; the forms once so stable soon 'break,' the equilibrium is overthrown, and variations once more ensue.

"After all, therefore, what I have here called evolutive forces in the organic world may prove to be only particular phases of those which conspire to constitute animal and vegetable life. And just as in the vital force itself it is usual to recognise two such phases, viz., the vegetative and reproductive, so the power of development or continual advance or alteration from an assumed type may ultimately appear as particular forms of life-force issuing in those results which we are accustomed to look upon as designed."

GEORGE HENSLOW

ON DEEP-SEA THERMOMETERS*

THE objects of this paper and of the experiments and observations recorded therein, are:—

1. The ascertainment of the effect of pressure on thermometers used for deep-sea purposes.

2. To obtain a scale whereby observations made by the thermometers now in use could be corrected for pressure.

3. To obtain a scale whereby observations made previously by other thermometers can be utilised.

In the early part of the year 1868 the attention of the Hydrographer of the Navy was directed to the unsatisfactory nature of the deep-sea Six's thermometers then in use.

The objections made to these thermometers were:—

1. Their fragility, the slightest jar or blow often breaking them.

2. The necessity of their being always kept in a vertical position.

3. The uncertainty of the register, the indices being generally capable of being shaken down.

4. Their large size, in connection with friction in passing through the water.

5. The substance they were mounted on, being generally wood, became so swollen by pressure of the water as often to render them incapable of being withdrawn from the case.

It was also considered that in all thermometric observations at great depths we had been "working in the dark," in that we had no idea of the effect pressure had on the instrument, and consequently on the recorded results; and it was reasonable to suppose that as the action of a thermometer was affected *in vacuo*, an opposite effect would be had by placing them under pressure, the more especially as in the one case the pressure of only one atmosphere, or 15 lb. to the square inch, was removed, while in the other the atmospheres would have to be reckoned by hundreds and the pressure by tons. On this point we were not without actual observation; for Mr. Glaisher, during the year 1844, in some experiments made on the temperature of the Thames near Greenwich with delicately constructed instruments, found that the indications of temperature were affected by pressure on the bulb of the thermometers, and that at a depth of only 25 feet, or about three-fourths of an atmosphere, the readings were increased by 2°; but no definite conclusion could be arrived at from these observations in respect to our deep-sea thermometers, beyond the fact that they were liable to be so affected.

*Abridged from a paper read before the Meteorological Society, April 19, 1871, by Capt. J. E. Davis, R.N.

It was therefore suggested to the Hydrographer—

1. That the author might be placed in personal communication with different makers in respect to the best construction for the purpose required; and

2. That a series of experiments should be made by placing some thermometers in a hydraulic press in conjunction with one in an hermetically sealed iron bottle (as a standard) and subjecting them to pressure, that they should be kept under pressure sufficient time to allow the thermometer within the bottle to take up the temperature without, and then the whole compared with the standard.

The first suggestion was immediately acceded to; and those makers from whom the Meteorological Department obtained instruments were applied to, and a list of desiderata submitted to each. Three makers responded, and six instruments were ordered from each.

These instruments were sent in (hereafter called the Hydrographic Office pattern), and Mr. Balfour Stewart, of the Observatory at Kew, was consulted as to the *modus operandi* of testing by pressure, and he approved of that already suggested.

A difficulty arose in respect to a hydraulic press—the use of some in London could not be obtained, and others were not adapted to the purpose, so that the testing was deferred, and some of the instruments were sent to H.M.S. *Gannet*, then deep-sea sounding on the edge of the Gulf-Stream, and afterwards some to H.M.S. *Lightning* for her dredging cruise.

On the return of these vessels the conflicting nature of the temperatures obtained from those supposed to exist (as derived from observations in other localities) rendered the necessity of ascertaining the nature and amount of error due to pressure the more imperative.

At this juncture Mr. Casella undertook to have a testing apparatus constructed at his own expense, capable of producing a pressure of three tons to the square inch.

At a meeting of the Committee of the Royal Society, held in the Hydrographer's Room in April, 1869, and at which the plan of operation for testing the thermometers was discussed, that by means of an iron bottle approved. The late Dr. Miller, V.P.R.S., proposed encasing the full bulb in an outer covering of glass containing air, in order to permit the lighter fluid (air) to be compressed without affecting the bulb within, and one such was directed to be made; but instead of the outer casing being filled with air it was nearly filled with alcohol, which being heated to reduce the quantity of air, the bulb was then hermetically sealed. Mr. Casella was also directed to make others that would facilitate the observations.

At the time these experiments were proposed, it was not known that a thermometer had been constructed, at the suggestion of Mr. Glaisher, by the late Admiral Fitzroy's directions, with the view of removing the difficulty of pressure; this was done by encasing the long bulb at the back of the instrument in glass, and nearly filling the space between the case and the bulb with mercury;* and one on this principle was then in the Instrument-room of the Meteorological Office; but although some had been used for deep-sea purposes, the further issue of them had been stopped on account of their fragility, and thus the means for obtaining accurate observations were virtually the same as before.

It was decided to test them at pressures equal to the following depths in the ocean, viz., 250, 500, 750, 1,000, 1,250, 1,500, 1,750, 2,000, 2,250, and 2,500 fathoms, the rule to be applied being 33 feet = one atmosphere = 15 lb. on the square inch. From this a table was constructed for use.

On the 4th of May the following thermometers were taken to Hatton Garden, viz.:—

Nos. 56 and 57 Casella . . .	Hydrographic Office pattern.
66 and 67 Elliott . . .	" " "
72 and 73 Pastorelli . . .	" " "

* See Meteorological Papers, No. I., 1863.

- No. 1 Casella . Specially made with an extra-thick cylinder bulb to *defy* compression.
- 3 " . Spherical bulb ; extra-thick glass. This thermometer was made, at the special request of one of Mr. Casella's workmen, in order to resist effect by pressure.
- 4 " . Short cylinder bulb ; extra-thick glass.
- 6 " . A glass cup fitting over bulb, designed by Mr. Siemens.

All the above were Six's thermometers with the bulbs unprotected.

- No. 2 Casella . Glass-encased bulb, as proposed by Dr. Miller, but with the case nearly filled with spirit.
- 5 " . Long cylinder bulb at the back, encased in glass, and nearly filled with spirit.

These instruments were first compared in air and then immersed in a tub of water, No. 57 being placed in an iron bottle. Set the indices and placed the thermometers in the cylinder of the press, and pumped on a pressure equal to 250 fathoms, and kept it on two hours.

It is useless to record the result of this first experiment ; or it may rather be stated that the results were *nil*, except ascertaining the weak points of the process adopted.

The Miller-pattern thermometer subsequently proved so near perfection it was decided to use that as a standard for the Hydrographic Office pattern.*

It was found necessary to reduce the number of thermometers, and also of the readings, to a minimum.

With the view of testing the efficiency of Dr. Miller's pattern (No. 2) it was placed in the cylinder with No. 57, and subjected to a pressure of 4,032 lbs. (about 1,480 fathoms) for a quarter of an hour, with the following result.

EXPERIMENT NO. 1 (pressure = 1,480 fathoms).
Dr. Miller reading.

Thermometer.	Minimum.		Maximum.		Diff. of Max.
	Before.	After.	Before.	After.	
2	47°5	47°5	47°5	48°0	0°5
57	47°5	47°5	47°5	55°0	7°5

This experiment at once proved the efficacy of the encased bulb ; and the experiment was repeated with more thermometers, with the same pressure and for the same period of time.

It was found by this experiment that while the mean difference of the encased bulbs was only 0°·95, that of the two made to defy compression was 7°·25, that with the cover 10°·5, the Hydrographic Office pattern the same as in No. 1, 7°·5, and a Phillip's Alpine thermometer 70°·3.

The "Phillip's" was an ordinary make, with a very small bulb ; and the great difference shown by it proved that the amount of compression is in proportion to the thickness of the glass ; but in immediate connection with the subject the experiment clearly demonstrated two facts, viz. :

1. That very nearly all the difference, or error, is due to pressure on the full bulb ; and
2. That by encasing the bulb we have nearly a perfect instrument.

Notwithstanding the satisfactory result obtained in enabling us to decide on a thermometer for future use, it was necessary, if possible, to establish a scale whereby temperatures already taken with instruments of the Hydrographic Office pattern might be corrected for pressure, and also to ascertain if all, or what part, of the difference shown under pressure in the Miller pattern was due to calorific effect produced by sudden compression of the water in the cylinder or by compression of the unprotected parts : preparation was accordingly made to continue the experiments.

It being necessary, as before stated, to reduce the number of the thermometers, and also the readings, to a minimum, the following were selected, viz. :—

- Nos. 2 and 5 Casella . Encased bulbs.
56 and 57 " . Hydrographic Office pattern.
73 Pastorelli " " "
67 Elliott " " "
9641 Casella . Alpine.

These were attached to a float (to avoid immersing the hand in the water) and placed in the cylinder filled with water, to remain all night ; the cistern, from which the water is pumped into the cylinder, was filled, and also a tub of water for replenishing placed by the side in order that the water in each might be, as nearly as possible, of the same temperature in the morning.

The thermometers were read in the order in which they are placed ; when all were read, the indices were set as quickly as possible, and the instruments at once lowered into the cylinder and the pressure applied.

May 5. The first series of experiments were made, Mr. Casella reading.

FIRST SERIES OF EXPERIMENTS. Errors at different pressures. (*Abridged from original.*)

Thermometer.	No. 1. 250 fms. 682 lbs.	No. 2. 500 fms. 1,363 lbs.	No. 3. 750 fms. 2,045 lbs.	No. 4. 1,000 fms. 2,728 lbs.	No. 5. 1,250 fms. 3,400 lbs.	No. 6. 1,500 fms. 4,089 lbs.	No. 7. 1,750 fms. 4,771 lbs.	No. 8. 2,000 fms. 5,452 lbs.
2	1°5	2°1 ?	1°0	1°2	1°2	1°6	1°4	1°6
5	1°3	1°6	0°2	0°6	0°4	0°8	0°8	1°6
56	1°1	2°7	3°8	4°3	5°5	7°0	8°0	9°5
57	1°0	2°7	3°8	4°9	5°6	7°4	8°2	9°7
73	1°9	2°6	4°2	6°0	6°8	8°2	9°7	10°2
67	3°9	7°9	Broken†	...	...	...	...	...
66	...	...	...	...	13°3	16°4	18°7	Broken‡
Phillip's Alpine . . .	...	...	...	...	...	...	...	71°0
Thomson §.	...	...	...	...	...	...	...	1°1

* I was not aware at that time of the existence of the enclosed Phillip's thermometer as designed by Sir William Thomson.

† The instrument was taken out safely, but while reading off the full bulb cracked right across.

‡ Broke at a pressure equal to 1,848 fathoms.

§ This insulated thermometer is a Phillip's encased in a glass cylinder containing a little spirit, designed by Sir William Thomson.

The thermometers were under pressure for an average time of 37 minutes in each experiment.

May 6.—The following experiment was made with the Hydrographic Office pattern (not used yesterday) for comparison. Mr. Casella reading.

Pressure = 2,000 fathoms = 5,452 lbs. Under pressure seventeen minutes.

Thermometer.	Error.
2	1'4
5	1'2
53	9'9
58	10'7
71	11'3
74	10'3
75	9'6
Thomson	1'0

SECOND SERIES OF EXPERIMENTS

June 21.—The thermometers were placed in the

cylinder, which was filled with water ; the supply-tub or cistern for pumping in from, and a tub of water standing near the press, were also filled and thus left all night.

June 22.—A dull morning, with no sun, and all conditions most favourable for observing.

Before commencing, obtained two tubs of water with 12° difference of temperature, and tested the thermometers as to time in taking up heat and contrariwise, and it was found that, by allowing the thermometers to remain under pressure eight minutes, the same results would be obtained as if they were allowed to remain half an hour or more, as in the first series of experiments.

The thermometers used were—

Standard . . . Casella . Dr. Miller's pattern.
No. 54 . . . " . Hydrographic Office pattern.
56 . . . " . " " "
76 . . . Pastorelli " " "
73 . . . " " " "
Thomson . . . Casella . Encased (Sir William Thomson's design).

SECOND SERIES OF EXPERIMENTS. Errors at different pressures. (Abridged from original.)

Thermo- meter.	No. 1. 250 fms.	No. 2. 500 fms.	No. 3. 750 fms.	No. 4. 1,000 fms.	No. 5. 1,250 fms.	No. 6. 1,500 fms.	No. 7. 1,750 fms.	No. 8. 2,000 fms.	No. 9. 2,250 fms.	No. 10. 2,500 fms.
Standard .	0'7	0'7	1'2	1'5	1'6	1'5	1'7	2'0	2'0	2'2
54	1'4	3'1	3'9	5'2	6'4	7'8	8'3	9'7	11'1	12'9
56	1'8	2'8	4'0	5'3	6'3	7'8	8'8	9'9	10'9	12'0
76	1'2	2'5	4'2	4'9	6'3	7'2	8'4	9'6	10'9	11'7
73	1'4	3'0	4'6	4'9	7'4	7'8	10'2	11'5	12'3	13'7
Thomson .	0'0	0'1	0'0	0'3	0'1	0'5	0'3	0'6	0'8	0'4

The thermometers were under pressure eight minutes in each experiment.

The mean difference for each 250 fathoms by each thermometer is as follows (abridged) :—

BY FIRST SERIES OF OBSERVATIONS

Thermometer.	Diff.
2	+ 0'20
5	+ 0'20
56	+ 1'19
57	+ 1'20
73	+ 1'27

BY SECOND SERIES OF OBSERVATIONS.

Thermometer.	Diff.
Standard	+ 0'22
54	+ 1'29
56	+ 1'20
76	+ 1'17
73	+ 1'37
Thomson	+ 0'05

EXPERIMENTS FOR CALORIFIC EFFECT.

The Phillip's encased maximum thermometers (Thomson's) being entirely protected from any effect by compression, it was decided to ascertain by their means the calorific effect produced by the sudden compression of the water in the cylinder ; but, as in the two series of experiments recorded, there was such a gradual increase in the temperature of the air and also in the water used for

supplying the cylinder, that for any delicate observation the conditions were not favourable ; the observations for calorific effect were therefore delayed until the weather got colder, when a more equable temperature could be ensured throughout the experiment.

In order to ascertain what time it would require for these instruments to take up temperature (as it was of importance they should not be kept under pressure longer than necessary) observations were made for the purpose, and it was found that five minutes would be sufficient time for the Thomson thermometers to take up the most minute portion of heat observable.

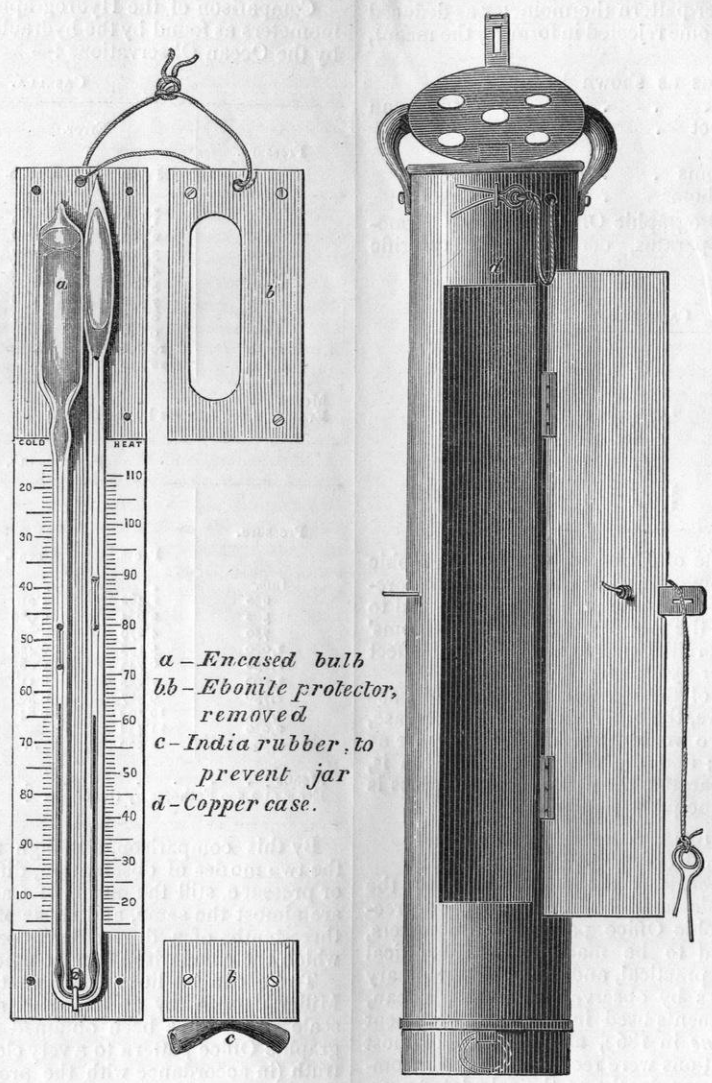
November 12.—The following observations were made day cloudy, all the conditions favourable.

No. 1. Pressure = 2,500 fathoms = 6,817lbs. Under ten minutes.

Thermometer.	Diff.	Remarks.
11,424	+ 0'1	Temperature in air . . 40'0
9,649	+ 0'4	" tub . . 41'6
9,645	+ 0'2	" cistern. 38'8
		" cylinder 38'0

EXPERIMENT No. 2 (same pressure). Under pressure twenty minutes.

Thermometer.	Diff.	Remarks.
11,424	+ 0'0	Temperature in air . . 43'6
9,649	+ 0'0	" tub . . 41'2
9,645	+ 0'2	" cistern. 41'2
		" cylinder 38'9



DEEP-SEA THERMOMETER USED BY THE HYDROGRAPHIC OFFICE

It will be observed that the water pumped into the cylinder was a little warmer than that in the cylinder ; but as the valve through which it passed into the cylinder is near the top, while the bulbs of the thermometers were at the bottom, the small difference it could have made in the upper water could not have affected them.

BY MR. CASELLA (previously observed).

Thermometer.	Pressure.	Diff.	Remarks.
	fms.		
	500	+ 0.2	
	1,000	0.0	
	1,500	0.0	
	2,000	+ 0.2	
	2,000	+ 0.6	
	2,000	+ 0.3	

The result of the foregoing Experiments (some rejected in forming the mean) :—

0.0178, calorific effect for each 250 fathoms' pressure.
0.18 " " 2,500 " "

It would seem almost unnecessary, for the purpose for which this paper is prepared, to record the above observations at all, so small is the result ; but as the amount of heat caused by compression is supposed by some to be much greater, it has been thought best to give it.

EXPERIMENTS TO DETERMINE THE AMOUNT OF HEAT PRODUCED BY FRICTION.

To ascertain if any error could arise from heat created by friction in a thermometer passing rapidly through the water, one of Casella's Hydrographic Office pattern was towed astern of one of the fast river-steamers (*Naiad*), keeping the thermometer well submerged by means of a

lead weight attached to the line before it; and with repeated trials at full speed not the slightest difference could be detected.

The error of the Miller-pattern thermometer as deduced from the observations (some rejected in forming the mean), *abridged* :—

Error per 250 fathoms as shown by hydraulic press 0°161 mean
Deduct for calorific effect 0°018

True error for 250 fathoms 0°143

True error for 2,500 fathoms 1°43

Mean Errors of Hydrographic Office pattern Thermometers, by testing-apparatus, corrected for calorific effect :—

Fathoms.	CASELLA.	PASTORELLI.
250	1°307	1°482
500	2°789	2°664
750	3°821	4°279
1,000	4°853	5°195
1,250	5°860	6°743
1,500	7°392	7°625
1,750	8°199	9°307
2,000	9°638	10°106
2,250	10°838	11°438
2,500	12°270	12°520

The Progressive Rate of Error of the Hydrographic Office pattern Thermometers, as deduced from the foregoing table, by testing-apparatus, is by Casella, equal to an increase of effect at the rate of 0°014 per 250 fathoms' pressure; and by Pastorelli, equal to a decrease of effect at the rate of 0°044 per 250 fathoms' pressure.

Thus, while one set of thermometers show an increase of effect under pressure, the other set denote a decrease, and the mean of the two would be so small a decrease as not to be appreciable; and the practical conclusion is, that, by the testing-apparatus, the elasticity of the glass is in exact proportion to the pressure applied.

OCEAN OBSERVATIONS BY STAFF-COMMANDER E. K. CALVER

Although from the result of the experiments with the testing apparatus, a scale could be formed for the correction of the Hydrographic Office pattern thermometers, that scale may be said to be made under theoretical conditions rather than practical, and as it was necessary to verify its correctness by observations in the ocean, a number of the instruments used in the press were sent on board the *Porcupine* in 1869, and a series of most carefully taken observations were recorded by Staff-Commander Calver at the same depths as the calculated pressure applied in the press.

It is unnecessary to give the details of these observations; it will suffice to give the progressive error derived from the mean of them, and corrected for the error of the standard.

Fathoms.	CASELLA.	PASTORELLI.
250	1°329	1°210
500	2°816	2°986
750	4°002	4°779
1,000	5°427	6°422
1,250	6°286	7°065
1,500	7°058	8°118
1,750	7°301	8°301
2,000	7°711	8°844

The progressive rate of error derived from the above is by Casella, equal to a decrease at the rate of 0°13 per 250 fathoms, and by Pastorelli, equal to a decrease of effect at the rate of 0°09 per 250 fathoms.

This result, contrary to that by the hydraulic press, proves that the elasticity is not regular or in ratio to the

pressure, but that after continuing regular up to a pressure of 1,000 fathoms, it decreases in a compound ratio to a pressure of 2,000 fathoms, when its elasticity nearly ceases.

Comparison of the Hydrographic Office pattern Thermometers as found by the hydraulic testing-apparatus and by the Ocean Observations :—

CASELLA.				
Pressure.	Error.		Per 250 fathoms.	
	Press.	Ocean.	Press.	Ocean.
fms.				
250	1°307	1°329	1°307	1°329
500	2°789	2°816	1°394	1°408
750	3°821	4°002	1°274	1°334
1,000	4°853	5°427	1°213	1°357
1,250	5°860	6°286	1°172	1°257
1,500	7°392	7°058	1°232	1°176
1,750	8°199	7°301	1°171	1°043
2,000	9°638	7°711	1°205	0°964
2,250	10°838	...	1°204	...
2,500	12°270	...	1°227	...
Means			1°240	1°233
Error at 2,500 fathoms by the means			12°4	12°3

PASTORELLI.				
Pressure.	Error.		Per 250 fathoms.	
	Press.	Ocean.	Press.	Ocean.
fms.				
250	1°482	1°210	1°482	1°210
500	2°664	2°986	1°332	1°493
750	4°279	4°779	1°426	1°593
1,000	5°195	6°422	1°299	1°606
1,250	6°743	7°065	1°349	1°413
1,500	7°625	8°118	1°271	1°353
1,750	9°307	8°303	1°329	1°186
2,000	10°106	8°844	1°263	1°105
2,250	11°438	...	1°271	...
2,500	12°520	...	1°252	...
Means			1°327	1°370
Error at 2,500 Fathoms by the means			13°3	13°7

By this comparison, although the errors, as found by the two modes of observation, differ at individual depths or pressure, still the means of Casella's per 250 fathoms are almost the same, and those of Pastorelli's differ only three-tenths of a degree in 2,000 fathoms, the extent to which the comparison can be made.

There can be little doubt that, without the aid of the Miller pattern, by an extended series of observations a scale could have been obtained to correct the Hydrographic Office pattern to a very close approximation of the truth (in accordance with the proposed first intention of the experiments); but the timely suggestion of Dr. Miller has quite set at rest any difference of opinion as to the instrument for future use.

OYSTERS IN IRELAND*

HIS Excellency the Lord Lieutenant of Ireland having had represented to him that the artificial propagation of oysters was imperfectly understood in Ireland, appointed in October 1868 Messrs. Blake, M.P., Francis, Hart, and Brady, commissioners to inquire into and report on the artificial cultivation and propagation of oysters.

The instructions to the Commission were to visit the principal places in France, England, and Ireland, where oyster cultivation is or can be carried on, to examine the best authorities on the subject, and to ascertain as far as possible the causes which have led to failures. It was also hinted that three weeks would suffice for Ireland, a

* Report of the Commission appointed to inquire into the Methods of Oyster Culture in the United Kingdom and France, with a View to the Introduction of improved Methods of Cultivation of Oysters into Ireland. (Presented to both Houses of Parliament by command of Her Majesty.) Dublin, 1870.

fortnight for England, and the same amount of time for France. The Commission proceeded in October 1868 to France to commence their fortnight's tour, and in June 1870 presented their report, which has now been laid before Parliament. The Report occupies about fifty pages; and 150 more are very usefully taken up with a series of appendices. Ten plates are also included in the volume.

The Report commences with a list of the places visited by the Commission, from which we notice the omission of Dub'lin Bay, although Howth and Malahide had each at one time a respectable name for oysters. It then proceeds to give the natural history of the oyster, which we pass over without further comment than that it is a pity the Commissioners did not consult some person tolerably skilled in malacology ere they printed it—to criticise it would be but to break a butterfly on a wheel. The various branches of oyster fisheries are well described, and an interesting epitome is given of Coste's labours. It would appear that the great bulk of the oysters bred at Arcachon are sent to Marennes and Tremblade, where the green tint, so much esteemed in France, is imparted to the beard of the oyster. Such a prejudice exists in England against this green tint, that the Essex oysters are largely exported to France. It should be recollected that oysters impregnated with copper have always a greenish tinge of body, while those with green beards do not owe their colour to copper but to their peculiar feeding. The reporters suggest that the Diatomaceæ are probably the cause, and give figures of some Diatoms, to which we would call the attention of Dr. Donkin, who is writing a monograph of this group; to say the least, they are very comical.

The diminution in oyster production which has taken place in England, though very considerable, is not so great as in France. The Hayling Island enclosure is described, and plans of the beds given. The various methods of oyster culture are described, and appropriate places for their cultivation are pointed out. In reference to this portion of the subject, we may refer to the elaborate report on the temperature of the surface of the sea on the coasts of Great Britain, Ireland, and France, by Prof. Hennessy, in which he deduces that:

"1. The temperature of the sea on the coast of Ireland varies within narrower limits than on the coast of Great Britain, or, in other words, it is more equable throughout the year and also during the summer season, when oyster breeding takes place.

"2. The temperature of the sea at noon on the Irish coast, especially on the south and west coasts during the months of June and July, is, upon the whole, higher than on the coast of Great Britain, and less than on the west coast of France.

"3. This temperature seems to be sufficient for the requirements of oyster breeding, and therefore, *a fortiori*, the temperature about two in the afternoon under the conditions above referred to.

"4. The highest temperature of the seas surrounding Ireland, and probably also of those surrounding Great Britain, is during the month of August, and the least during the month of February.

"5. Any advantages as to temperature possessed by the seas which wash the Irish coast are unquestionably due to the thermal influence of currents connected with the Gulf Stream."

Prof. Sullivan also appends an important Report on the Composition of the Soils of Oyster Grounds, and on the qualities which exert most influence on oyster cultivation, and comes to the conclusions:—

"1. That the influence of the soil upon the breeding and growth of oysters is complicated by: temperature, especially during the spawning season; sudden alternations of heat and cold, due to currents; alternation of depth of water, especially as regards whether the maximum of sun-heat and

light concords with low water during the spawning season; velocity of tide, angle of inclination of shore, &c.

"2. That the soil of oyster grounds may be made up of materials of any of the great classes of rocks, arenaceous, argillaceous, or calcareous, provided they contain—

"3. More or less of a fine flocculent highly hydrated silt, rich in organic matter, which indicates that Diatomaceæ, Rhizopoda, Infusoria, and other minute creatures abound.

"4. That the character and abundance of such small organisms in a locality seems to be the true test of a successful oyster ground.

"5. And lastly, that although oysters do undoubtedly assimilate copper from water where mine-water containing traces of that metal flows into the sea in the neighbourhood of the oyster beds, the copper is chiefly, if not exclusively, confined to the body of the oyster, and does not appear to reach the mantle or beard. That the so-called green oysters of Essex, Marennes, and other places, on the other hand, are green-bearded and contain no copper, nor can the most minute trace of copper be detected in the soil of the oyster grounds where such green-bearded oysters are produced."

The Report concludes with the following recommendations:—

"1. That all regulations with regard to the close time around the Irish coast should be strictly maintained.

"2. That the inspectors of Irish fisheries should have power, whenever they determine to reserve a bank or any portion thereof from public dredging for the purpose of recovery, to make such arrangements as may seem desirable for keeping the restricted part free from weeds and vermin.

"3. That there should be procurable at each coastguard station, at a small cost, general information as to oyster culture, and simple instructions as to the best modes of proceeding.

"4. That the inspectors be empowered to adopt such other means as they may deem necessary to afford information and instruction to those requiring it with respect to oyster culture.

"5. That having unsizable oysters in possession in places where it is prohibited by any bye-law to take oysters from any public beds under a certain size, shall be *prima facie* evidence that such oysters were taken in places so prohibited; such regulations not to apply to private oyster grounds.

"6. That facilities be afforded to the coast population to acquire the use of small portions of foreshore, or sea bottom, for oyster cultivation, and to obtain loans on satisfactory security for the preparation of same, and for the purchase of oysters, collectors, &c.

"7. That landed proprietors desirous of cultivating oysters on the shores adjoining their lands, be empowered to avail themselves of the provisions of the Irish Land Improvement Acts, for the purpose of oyster cultivation."

We would commend the perusal of this Report to those interested in this subject; of its importance there can be little doubt; and while we agree with the commissioners that no very extraordinary profits are to be made out of oyster culture, and that hence it is not a subject for extensive commercial speculation, yet we know of none more deserving of the attention of those interested in the general welfare of this country.

E. P. W.

ARTIFICIAL MILK

AMONG the many sorrowful records of the Siege of Paris, one of the most enduring, and not the least touching in its melancholy eloquence, is afforded by the

Comptes Rendus of the Academy of Science. The construction, the filling, the guiding, and general management of balloons, occupied so much of the attention of the Academy, that, if all other records of the Siege were lost, its date and effective duration might be pretty accurately determined by the sudden appearance, the continuance, and sudden cessation of these abundant papers on aërostation.

There is another series of papers of equal, if not greater significance, viz., those on the utilisation of strange materials for food, the economising of waste nutritive materials, and their substitutive uses.

The investigations on these subjects have led to more practical results than the papers on aërostation. This has been especially the case with the researches that are described in the papers of M. Boillott, M. Dubrunfant, and M. Charles Fua, on "Alimentary Fats."

"Alimentary fats" is a wide expression, including some rather unsavoury hydro-carbons and very curious refuse materials. The main object of these investigations was to determine how such substances may be "usefully employed in alimentation," or, in plain unsophisticated English, how to make butter from candle-ends, dirty dripping, colza oil, fish oils, the refuse of slaughter houses, the restored grease of the wool-dresser, &c. The general result has proved that the "frying process"—which was not altogether unknown to certain enterprising Englishmen before the investment of Paris—is triumphant over all its rivals; that by simply raising the fat to 140° or 150° Centigrade, and in the mean time cautiously sprinkling with water, the cellular tissue, the volatile oils, the rancidity, offensive odours, and all other non-sentimental impediments to "alimentation," are removeable.

This frying process has already effected something like a revolution in the industry of soap-boilers, some of whom have changed their trade to that of butter-fryers. We may thus explain the remarkable fact that, although the excessively dry summer of 1870 reduced the dairy produce of England to about half the average, and had nearly the same effect on our other sources of cow-butter supply, there was no material reduction in the supply or consumption of fresh butter for the London and Provincial markets during the following winter, the only notable disturbance which occurred being in the demand for kitchen-stuff and empty Dutch butter-tubs.

M. Dubrunfant is not content with superseding the cow in the mater of butter, but has subsequently made similar attempts upon milk. He proceeds in a strictly scientific manner, commencing with the following summary of the results of Boussingault's analysis of cow's milk:—

Nitrogenous material (caseine and albumen)	0.0337
Fatty material (butter)	0.0376
Sugar (of milk)	0.0567
Salts	0.0020
Water	0.8700

Quoting the observations of Payen and others which show that milk is alkaline, and owes its alkalinity to soda, he proceeds to refute the theory of churning which has been generally adopted by microscopists, viz., that the fat globules in milk are invested with a delicate membrane which is ruptured in the churn, and thereby permits the agglomeration of the fatty material into butter.

M. Dubrunfant contends that milk is simply an emulsion of neutral fatty matter in a slightly alkaline liquid, such as can be artificially imitated; and that the process of churning consists in hastening the lactic fermentation, thereby acidifying the serum of the milk, and at the same time agglomerating the fatty matter which the acidity sets free from its emulsion. He further controverts the cellular theory by showing that the fat globules of milk do not display any double refraction, as do all organised membranous tissues.

Having thus examined the theoretical constitution of milk, he proceeds to the practical method of imitating it,

and gives the following directions: Add to half a litre of water forty or fifty grammes of saccharine material (cane sugar, glucose, or sugar of milk), twenty or thirty grammes of dry albumen (made from white of egg), and one or two grammes of subcarbonate of soda. These are to be agitated with fifty or sixty grammes of olive oil or other comestible fatty matter until they form an emulsion. This may be done either with warm or cold water, but the temperature of 50° to 60° C. is recommended. The result is a pasty liquid, which, by further admixture with its own bulk of water, assumes the consistency and general appearance of milk.

Luxuriously-minded people who prefer rich cream to ordinary milk can obtain it by doubling the quantity of fatty matter, and substituting two or three grammes of gelatine for the dry albumen. The researches of Dumas and Fremy having reinstated gelatine among the nitrogenous alimentary materials, M. Dubrunfant prefers gelatine to albumen; it is cheaper, more easily obtained, and the slight viscosity which it gives to the liquid materially assists the formation and maintenance of the emulsion. He especially recommends this in the manufacture of "siege-milk," on account of the obviously numerous articles from which gelatine may be obtained.

The uses of artificial milk need not be limited to supplying the wants of the residents of besieged cities. As an ordinary element of the human breakfast table, it is not likely to supersede the product of the cow, but calves are suggested as being superior to vulgar human prejudices. In the ordinary course of rearing, these animals demand a large proportion of the milk of their mothers, and are commonly ill-fed or prematurely sacrificed on that account. By feeding them luxuriously on artificial milk (which may be still further cheapened by using colza oil, which has been rendered tasteless and alimentary by the frying process above described), the milk, butter, and cheese of the cow may be considerably economised, and the supply of veal improved both in quantity and quality, by keeping the calves a much longer time before they are killed.

I might make further suggestions in the direction of "dairy-fed pork," &c., but this is unnecessary, the commercial instinct is sufficiently strong to avail itself of all such cheapening applications of science. Those who are professionally engaged in detecting the adulterations of food will do well to study the physical peculiarities by which M. Dubrunfant's milk may be distinguished from that of the cow, both in the ordinary and condensed form. By substituting vegetable albumen for the white of egg or gelatine, the vegetarian may prepare for himself a milk that will satisfy his uttermost aspirations.

W. MATTIEU WILLIAMS

NOTES

THE following telegrams have been received from the Eclipse Expedition since our last:—"MANGLORE, Wednesday, Dec. 6.—We have landed here from the flagship; all well. The Government arrangements are admirable. The weather is promising. The parties are posted as arranged." From N. R. Pogson, at Avenashy, to the Astronomer Royal, Royal Observatory, Greenwich:—"Weather fine; telescopic and camera photographs successful; ditto polarisation; good sketches; many bright lines in spectrum.—Dec. 12." From Colonel Tennant, F.R.S., Dodabeta, Ootacamund, to W. Huggins, F.R.S., Dec. 12, 9.15 A.M.:—"Thin mist. Spectroscope satisfactory. Reversion of lines entirely confirmed. Six good photographs."

At the meeting of the Geological Society on the 6th inst., the President announced the bequest to the Society, on the part of the late Sir R. I. Murchison, of the sum of 1,000*l.*, to be invested in the name of the Society or its trustees, under the title of the "Murchison Geological Fund," and its proceeds to be annually devoted by the Council to the encouragement or assis-

tance of geological investigation. The donation of the proceeds of the fund was directed by the testator to be accompanied by a bronze copy of the Murchison Medal.

At the meeting of the Royal Geographical Society on Monday last, Sir Henry Rawlinson stated that the Council intended to address the Foreign Office, with a view of arranging, either directly from the Foreign Office, or through co-operation between the Foreign Office and the Society, some means of communicating with Dr. Livingstone, either by sending messengers into the interior of Africa, and offering a reward of 100 guineas to any African who will bring back a letter in Dr. Livingstone's handwriting to the sea-coast, or by organising a direct expedition, headed by some experienced and well-qualified European, who should himself penetrate to the point where Dr. Livingstone is supposed to be.

By a decree, dated April 18, 1866, of the Minister of Public Instruction in France, a prize of 50,000fr. (2,000*l.*) was offered for the most useful application of the Voltaic Pile, the period for competition to expire in April 1871. From a report of the minutes presented by the President of the Republic, it appears that candidates are few in number, and that in the opinion of the *savants* to whom the memoirs were submitted, none is of sufficient merit to have earned the prize. By a decree of the 29th of November, the competition is now extended for another period of five years, to terminate on November 29, 1876.

WE learn from the *Lancet* that the promoters of the scheme for commemorating the life and labours of John Goodsir, late Professor of Anatomy in the University of Edinburgh, have got only 700*l.* instead of 2,000*l.*, and have had to relinquish the idea of a fellowship, and adopt that of a triennial prize, to be open to all graduates of the University of not more than three years' standing, to be given for an essay or treatise containing the results of original investigations in anatomy, human and comparative, either normal or pathological, or in experimental physiology. The Acting Committee of the Association for the better Endowment of the University of Edinburgh have prepared the deed of endowment for the Syme Memorial. The capital sum amounts to 2,500*l.*; whereof 2,000*l.* were paid over to the Association by the Syme Memorial Committee, and 500*l.* was added by the Association.

THE authorities of the Museum of Comparative Zoology at Harvard College have placed in Prof. Allman's hands for determination the whole of the collection of hydroid zoophytes obtained by the United States Coast Survey during its late exploration of the Gulf Stream.

THE Council of the University of Edinburgh has decided to take into consideration on the 21st inst. the appeal against the decision of the Senate as to rescinding the regulations for the education of women in medicine.

THE Examiners in the Natural Science School at Oxford (W. Ogle, M.D., Corpus; J. A. Dale, Balliol; and R. H. M. Bosanquet, St. John's) on Saturday issued the subjoined class list:—Class I.—H. A. Black, Christ Church; W. T. Goolden, Magdalen; E. H. Jacob, Corpus; A. S. L. Macdonald, Merton; J. A. Ormerod, Jesus; A. G. Rücker, Brasenose; S. H. West, Christ Church. Class II.—E. H. Forty, Christ Church; J. Turner, Exeter; J. L. Twynam, St. Mary Hall. Class III.—*Nil.* Class IV.—*Nil.*

MR. W. A. BRAILEY, who was second in the Natural Sciences Tripos at Cambridge, has been elected a Fellow of Downing College in that University.

M. GEORGES DELAPORTE, engineer of M. Tessié de Motay's Oxy hydrogen Light Company, has been nominated a Chevalier

of the Legion of Honour, as an acknowledgment of the services rendered to the State during the Siege of Paris in the application of the Electric Light to strategic operations.

THE Lord President of the Council has nominated Mr. T. S. Aldis, formerly scholar of Trinity College, Cambridge (Second Wrangler in 1866), to be an Inspector of Schools.

The following are now announced as the probable arrangements for the Friday evening meetings at the Royal Institution before Easter 1872:—January 19, Mr. William R. Grove, F.R.S., on Continuity; January 26, the Archbishop of Westminster, on the Demon of Socrates; February 2, Prof. Odling, F.R.S., on the new metal Indium; February 9, Prof. Humphry, F.R.S., on Sleep; February 16, Dr. Gladstone, F.R.S., on the Crystallisation of Silver and other Metals; February 23, Mr. Henry Leslie, on the Social Influence of Music; March 1, Mr. C. W. Siemens, F.R.S., on Measuring Temperatures by Electricity; March 8, Mr. R. Liebreich, on the Effect of certain Faults of Vision on Painting, with especial reference to Turner and Mulready; March 15, Mr. John Evans, F.R.S., on the Alphabet and its Origin; March 22, Prof. Tyndall, F.R.S.

WE learn from *Les Mondes* that the lamentable disagreement between M. Daubrée, the director of the mineralogical department of the Museum of Natural History at Paris, and his assistant, M. Stanislas Meunier, is now happily terminated, and that the latter is again permitted to carry on his researches at the Museum.

THE Exhibition of the Photographic Society, held in its rooms in Conduit Street, closed on Saturday last. While among specimens of portraits the works of Grasshofer of Berlin, Rylander of Paris, and other Continental artists, challenged comparison with any of our home productions, there can be no question that in landscape photography, the exquisite workmanship of Bedford, Robinson, Cherrill, and some other English photographers, easily bore off the palm. There were some very fine specimens of Edwards's heliotype process, as well as of the autotype and other carbon-printing processes.

WE learn from the *American Naturalist* that the State Microscopical Society of Illinois has issued a prospectus of *The Lens*, a Quarterly Journal of Microscopy and the Allied Natural Sciences; with the Transactions of the State Microscopical Society of Illinois. It will be an octavo, each number containing at least forty-eight pages of reading matter. Terms, 2 dols. per annum in advance. The editor will be Mr. S. A. Briggs, 177, Calumet Avenue, Chicago. Though its appearance has been delayed by the fire, we learn that it will soon be issued.

At a recent meeting of the Asiatic Society of Bengal Mr. W. T. Blanford exhibited a collection of chipped quartzite implements found about forty miles west of Bhadrachalam, on the Godavari. The thirty-five specimens exhibited were all found within a space of about fifty yards square, and at least as many more were rejected on account of being badly made. The place where they were found was in dense jungle, the rock soft sandstone, and the implements, as was usually the case in Southern India, had evidently been chipped from pebbles. Several were formed of white vein quartz, an unusual circumstance. The forms of these implements were those of the kind most frequently found in French and English gravels, and they varied from about 3in. to 6in. in length. That the spot where they were found was a place of manufacture was probable, not only from the occurrence of ill-formed implements, but also from flakes, evidently chipped from the quartzite being abundant.

A VERY beautiful and extraordinary Aurora Borealis was witnessed at Montreal on November 21. The following account of

the phenomenon has been sent us by Dr. Smallwood of the Montreal Observatory:—A few minutes past 5 o'clock yesterday evening, the eastern horizon showed a bank of cumulo-stratus clouds, which reached to an altitude of 9°, behind which was discernible an auroral light, which increased in intensity as the darkness became more dense. At 5.30 a diffused light of a bright crimson colour occupied the whole of the eastern and north-eastern horizons. Rising behind this bank of clouds, streamers were frequently observed, reaching to the constellation Cassiopeia. The light was frequently so dense as to prevent even the stars δ and γ Ursæ Majoris being seen through it. While these appearances were present, a far more brilliant display was seen in the north-west, triangular in shape, its base hidden by the Mountain, but which appeared about 10° in breadth, and extended upwards, passing part of the constellations Hercules, Corona Borealis, and Draco, to the zenith. The bright crimson colour was very intense; its edges were occasionally softened by a band of narrow streamers of a palish green colour. Stars below the third magnitude were hidden from view, owing to the great density of this light. Small patches of cumulus clouds were seen passing across and in front of this display. The surpassing beauty of these appearances has rarely been equalled. At 6.15 P.M. the intensity of the brightness was much diminished, and at 7 only a soft auroral light was visible in the north and north-east. The declination magnet was very sensibly deflected from its normal state, showing a great easterly variation. The weather during the day was comparatively warm (having succeeded a slight fall of snow), with a rising barometer, which at 6 P.M. stood at 29.902 inches. Thermometer, 37°. Humidity, .806. Wind west; velocity three miles per hour.

PROF. PANCERI, of Naples, has been studying for some time past the phosphorescence of marine animals. He has examined *Noctiluca*, *Beroë*, *Pyrosoma*, *Pholas*, *Chatopterus*, and has lately published a paper on the phosphorescence of *Pennatula*. He finds in all cases that the phosphorescence is due to matter cast off by the animal—it is a property of dead separated matter, not of the living tissues. In all cases (excepting *Noctiluca*) he also finds that this matter is secreted by glands, possibly special for this purpose, but more probably the phosphorescence is a secondary property of the secretion. Further, the secretion contains epithelial cells in a state of fatty degeneration, and it is these fatty cells and the fat which they give rise to which are phosphorescent. Hence the phosphorescence of marine animals is brought under the same category as the phosphorescence of decaying fish and bones. It is due to the formation in decomposition of a phosphoric hydro-carbon, or possibly of phosphuretted hydrogen itself. In *Pennatula* Prof. Panceri has made phosphorescence the means of studying a more important physiological question—namely, the rate of transmission of an irritation. For when one extremity of a *Pennatula* is irritated, a stream of phosphorescent light runs along the whole length of the polyp-colony, indicating thus by its passage the rate of the transmission of the irritation. This admits of accurate measurement, and furnishes data for extending Helmholtz's and Donder's inquiries to animals so widely separated from their "Versuchs-thiere" as the *Calentaria*. It is also a proof of the thoroughness of Prof. Panceri's investigation that he has made use of the spectroscope for studying the light of phosphorescence.

ATTENTION has been called in *Harper's Weekly* to the injuries to the Florida submarine cable supposed to have been caused either by the bites of the sea-turtles, or from some kinds of fish; and we now learn that in China a similar difficulty has been experienced in consequence of the attacks of a minute crustacean. This is so small as scarcely to be perceptible to the naked eye, but can be readily defined under the microscope. Various breaks have been satisfactorily referred to the agency of these animals,

which had embedded themselves in the gutta percha. It has become necessary, therefore, to envelop the cables in certain localities with an external supplementary layer of metallic wire, in order to prevent injury in this manner.

WITH a commendable promptness, the first volume of the Annual Report of the United States Commissioner of Patents for 1870 has made its appearance, and inaugurates the new order in regard to this document. Instead of publishing the specifications of the patents with wood-cut illustrations, the present volume embodies—first, an alphabetical list of patentees during the year; second an alphabetical list of the patents extended during the year; next, an alphabetical list of inventions and of reissues. It will be remembered that at the present time the patents are printed in detail, accompanied by photo-lithographic drawings of working size, 150 copies being published, some of them to be distributed, and sets placed for free public inspection in the various State and Territorial capitals, and in the clerks' offices of the District Court of the various judicial districts throughout the United States. The issue of additional copies is also authorised in proportion to the demand, to be sold at a price not exceeding the contract price for such drawings. The total number of patents issued during the year 1870 amounted to 13,321, of which considerably the largest number were made out to citizens of New York, Pennsylvania, Massachusetts, Ohio, Illinois, Connecticut, Indiana, and Michigan, in the order mentioned.

THE *Mechanics' Magazine* states that amber is reported by the collectors as being sometimes found in a soft "unripe" state; Herr H. Spigatis was fortunate enough to receive a specimen from the Baltic, near Brusterort, East Prussia. Its interior consisted of an almost transparent yellow resin, surrounded by a thin opaque crust. When freshly broken the centre was soft and elastic, but on exposure to the air it soon became hard and brittle. Its analysis differed so much from that of amber, that though it evidently belongs to the same class of substances, it is not to be mistaken for it. Its percentage composition agrees with that of Benneheim asphalt, and with the fossil resins from the East Indies, examined by Duflos and Johnston.

ON Nov. 7 at 2.30 P.M. a slight earthquake was felt at Smyrna. It was simultaneous at Mytilene and Cheshmeh.

TWO smart shocks of earthquake were felt at Cavalla in Macedonia at 11 P.M. on Nov. 28.

ON Oct. 13 an earthquake was felt in the fort of La Libertad, at 11 P.M. It was also felt at La Union and Nicaragua.

ABOUT a year ago many English and foreign scientific journals, following the *Bulletino Romano*, announced that a large meteorite had fallen near the town of Murzuk, in December 1869. M. Rose has lately made a communication to the Berlin Academy, in which he states that the results of his inquiries made both at Tripoli and Murzuk have shown that no such fall was ever observed, much less that any such meteorite had been found.

It is stated in *Land and Water* that the whole of the pack of fox-hounds of the Durham County Hunt has been condemned to be destroyed in consequence of the prevalence among it of a form of hydrophobia defined as "dumb madness," which has run through the kennels, and has carried off twelve couple of hounds. As to the details of this "dumb madness," it will be interesting to hear more of the exact symptoms attending it. Old works upon canine diseases used to specify seven species of canine madness, "dumb madness" among them, the last and worst being "running madness," which was undoubtedly hydrophobia, though probably many other phases of so-called madness were simply distemper, which in primitive days was little understood as a specific disease.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, September. This number opens with numerous editorial notes, principally abstracts from other scientific journals; there is also the commencement of a description of the Stevens Institute of Technology in Hoboken. Amongst the notes we notice an account of Grubb's automatic spectroscope, and a description of the properties of Nitroglycerine as found by M. Champion. It is stated that when pure it may be heated up to 200° without explosion, but at 257° it deflagrates violently; and although it explodes with terrific force by a blow, the electric spark does not affect it. A number of original communications follow. Under the head of Civil and Mechanical Engineering, we find a paper containing some useful "formulae, rules, and examples for cases of earth-work under warped and plain surfaces," and another "On Descriptions of Wood-working Machinery." Under mechanics, physics, and chemistry, there is a paper "On Apparatus Illustrating Mechanical Principles;" the various pieces of apparatus are intended to show experimentally the truth of problems, such as the parallelogram of forces, the parallelepipedon of forces, and so on; a machine is also described to illustrate the action of the forces of gravity and projection in giving a projectile its parabolic trajectory. They are designed by J. Pemberton, and seem to be well adapted to the various purposes which have hitherto been neglected. The continuation of a lecture on the sun by Dr. Gould follows; he deals shortly with the prismatic analysis of light and with the solar spectrum, explaining the curves of thermal, luminous, and chemical intensity. Prof. Leeds contributes a valuable paper for the use of students "On the Measurement of the Angles of Crystals," and Mr. Coleman Sellers reviews Mr. Crookes's Experimental Investigation of a New Force; he boldly states that he believes Mr. Crookes has been deceived, giving several reasons why he is of this opinion. An editorial note is attached to this paper, stating that Mr. Sellers is very accomplished in the field of legerdemain, which would lend peculiar value to his view.

Journal of the Franklin Institute, October. The editorial notes contain several valuable abstracts, amongst which may be noticed one on Fluorescence, originally published by E. Lommel in the "Repert. der Physik." From his observations Lommel shows that Stokes's law "that the refrangibility of the exciting rays is always the upper limit of the refrangibility of the excited rays" does not always hold good, and also that the very common opinion that Fluorescence is an action by which refrangible rays are converted into less refrangible rays is not altogether true.—Prof. Thurston communicates a report "On a Steam Boiler Explosion," to which is added a clear statement of many of the causes of such explosions. Prof. Heines contributes the first of a series of papers on binocular vision; he deals shortly with the human eye and monocular vision, and then proceeds to some phenomena of binocular vision. The last paper was read before the American Association for the advancement of Science by Prof. Owen, "On Physiographic and Dynamical Geology involving the discussion of Terrestrial Magnetism," in which it is thought probable that the sun is the source of the modifications on the earth, giving the form and dimensions to the land, and that magnetism, either directly or by conversion into chemical force, has been the most powerful agent in causing various natural phenomena, such as the geysers, volcanoes, ocean currents, &c.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 7.—"On the Fossil Mammals of Australia. Part VI. Genus *Phascolomys*, Geoffr."—By Prof. Owen, F.R.S. In this paper the author premises a reference to former ones on the Osteology of existing *Marsupialia*, in the "Transactions of the Zoological Society," and to his "Catalogue of the Osteological Series in the Museum of the Royal College of Surgeons," in which are defined cranial characters serving to distinguish existing species of the genus *Phascolomys*, Geoffr.; and after showing, in subsequently received materials, the kind and extent of variety of such characters in the same species, he proceeds to apply the knowledge so gained to the determination of some fossil remains of species of Wombat, similar in size to the known existing kinds. The extinct *Phascolomys Mitchellii*, indicated by remains brought to England in 1835 by Sir Thomas Mitchell, C.B., the

discoverer of the bone-caves of Wellington Valley, Australia, is determined by specimens subsequently obtained by Prof. Alex. M. Thomson and Mr. Gerard Krefft, from the same caves. A second species, distinguished by characters of the nasal bones, is called after its discoverer *Phascolomys Krefftii*. Modifications of the lachrymal, maxillary, and palatal bones in the existing kinds of Wombat are also applied to the determination of the fossils: specimens from the fresh water deposits of Queensland are thus shown to belong to the species *Phascolomys Mitchellii*, originally founded on fossils from the breccia-caves of New South Wales. The author next proceeds to point out the characters of the mandible in existing Wombats, available in the determination of extinct species of *Phascolomys*. On this basis he defines specimens which he provisionally refers to his *Phascolomys Krefftii*. He then points out the mandibular characters of *Phascolomys Mitchellii*, and shows that the existing *Phascolomys latifrons* was represented by mandibular fossils from the breccia-caves of Wellington Valley. Proceeding next to the description of fossil mandibular remains of the genus *Phascolomys* from the fresh water deposits of Queensland, the author defines *Phascolomys Thomsoni*, *Phasc. platyrhinus*, and *Phasc. parvus*. The latter, seemingly extinct, species is markedly inferior in size to any of the known existing species. An account of the extinct kinds of Wombat, exceeding in size the existing species, will be the subject of a succeeding communication. The present is illustrated by subjects occupying seven plates and eight woodcuts, all the figures being from nature, and of the natural size.

"On Fluoride of Silver. Part III." By G. Gore, F.R.S.

"On the Solvent Power of Liquid Cyanogen." By G. Gore, F.R.S.

Zoological Society, December 5.—John Gould, F.R.S., V.P., in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the months of October and November 1871, and called particular attention to a young female specimen of the Cape Fur-Seal (*Otaria pusilla*), presented by Sir Henry Barkly, Governor of the Cape Colony, being the first example of this interesting animal received alive in Europe.—A letter was read from Dr. Burmeister, of Buenos Ayres, containing remarks on Messrs. Sclater and Salvin's "Synopsis of the Cracidae," published in the Society's "Proceedings" for 1870.—Dr. E. Hamilton exhibited and made some remarks on an adult skull of the newly-discovered Chinese Deer (*Hydropotes inermis*), and compared it with an immature skull of the same species exhibited by Mr. R. Swinhoe at a meeting of the Society, February 10, 1870. Dr. Hamilton also drew attention to the statement made by his correspondent respecting the wonderful fecundity of this animal, which tended to corroborate the facts stated by Mr. Swinhoe on that occasion.—Mr. Sclater exhibited and remarked on a skin of the Water Opossum (*Chironectes variegatus*), which had been sent to him by Mr. Robert B. White, from Medillin, United States of Columbia.—Prof. Newton exhibited and made remarks on the humerus of a Pelican (believed to be *Pelecanus crispus*), which had been found in the English fens.—A communication was read from Surgeon Francis Day, Inspector-General of Fisheries of British India, containing remarks on the freshwater Siluroids of India and Burmah, with observations on the range of the species, their classification, and general geographical distribution.—Mr. A. G. Butler read a paper on a small collection of Butterflies made at Loanda, the capital of the Portuguese Settlements of Angola. A second paper by Mr. Butler gave the description of a new genus of Lepidoptera, allied to *Apatura*, which was proposed to be called *Eulaceira*.—A paper by Mr. E. A. Smith was read, containing a list of species of Shells from the Slave Coast, West Africa, collected by the late Commander Knocker, R.N., the majority of which had been dredged at Whydah, on the Dahomey shore.—Prof. Newton communicated some notes by Herr Robert Collett, of Christiana, on the singular asymmetry of the skull in Tengmalm's Owl (*Strix tengmalmi*).—Mr. Sclater read the third and final portion of a series of notes on rare or little-known animals now or lately living in the Society's Gardens. Mr. Sclater gave an account of a collection of Birds from Oyapok, on the river of the same name which divides Cayenne from the northern frontier of Brazil, amongst which were two species believed to be undescribed, and proposed to be called *Ochthoeca murina* and *Heteropelma igniceps*. A third communication from Mr. Sclater contained remarks on the species of the genera *Myiozetetes* and *Conopias*, belonging to the family Tyrannidae.—Mr. E. W. H. Holdsworth read some notes on the Red-spotted Cat (*Felis rubiginosa*) of

Ceylon, and its varieties. — Mr. D. G. Elliot read a paper on various Felidae, rectifying the synonymy of several species, and giving a more perfect description of one recently obtained from North-West Siberia, which he proposed to call *Felis exultura*. Dr. Günther made a reply to some critical remarks in a paper by Surgeon Francis Day, read at a recent meeting of the Society.

Geologists' Association, December 1.—The Rev. Thomas Wiltshire, M.A., F.G.S., president, in the chair. — "On the Glacial Drifts of North London," by Mr. Henry Walker. These drifts were described under the classification and nomenclature given to the glacial deposits by Mr. Searles V. Wood, jun. They were traced from East End (Highgate) and Muswell Hill to Finchley, Colney Hatch Lane, and Whetstone. The profusion of chalk found in the glacial clay at these places bears out the designation of the main deposit in south-eastern England as the great Chalk Boulder Clay; but it is also found that the sands and gravels of the Middle Glacial, which Mr. Wood seems to restrict to a much lower horizon than Finchley, are also to be found at these localities. At Whetstone the Chalky Boulder Clay is found overlying twenty-five feet of gravel and sand, and in the apparently corresponding beds at Finchley and Hendon Lane, drift fossils and casts are occasionally found. Mr. Henry Hicks agreed with the conclusion that these sands and gravels are Mr. Wood's Middle Glacial. Mr. Caleb Evans thought that the heights to the north of London marked the southern termination of the glacial drift. Mr. Batt considered that the Drift had extended to the country south of the Thames. Several other gentlemen took part in a very animated discussion.—Collections of fossils and boulders from the Middlesex Drift were exhibited, and a quantity of peat obtained from the same source, was shown by Mr. J. T. B. Ives.

Society of Biblical Archaeology, December 5.—Prof. Donaldson, B.A., F.R.S., in the chair. A paper by the Chev. de Saucy, membre de l'Institut, "On the true sites of Capernaum, Chorazin, and Bethsaida (Julius)" was read by the secretary. In the chevalier's paper, which took the form of a letter (addressed to the Dean of Westminster), he stated that, having considered the whole tenor of the argument first advanced by him in the *Revue Archéologique* twenty years ago, he could come to no other conclusion than that the traditional town of Bethsaida and the identification of Kerâzeh as Chorazin and Tel Houm as Capernaum were unsupported by geographical evidence, and were contrary to the express statements of Josephus, who would be sufficiently exact in describing the town where he was wounded. At the same time the ruins of Kerâzeh were too extensive to be those of insignificant village like Chorazin; and those of supposititious Bethsaida were too few, and contained no indications of the Family Mausoleum of Herod Philip. The conclusion of the author was that Tel Houm was more probably the real site of Capernaum. A considerable amount of philological evidence illustrated these statements. On the close of the reading of this paper an interesting discussion ensued, in which the chairman and the following gentlemen took part:—Mr. W. R. A. Boyle, Dr. Cull, Mr. S. M. Drach, Mr. John Macgregor, and Captain Wilson.

Entomological Society, December 4.—A. R. Wallace, president, in the chair.—Mr. Shearwood exhibited an extraordinary variety of *Argynnis aglaia*, taken at Teignmouth. Mr. Bond exhibited varieties, or malformations, of various British *Lepidoptera*.—Mr. Janon exhibited a large collection of insects (chiefly *Coleoptera*) from the diamond fields of South Africa.—Mr. Higgins exhibited examples of *Tetracha crucigera* of MacLeay, from Australia.—Prof. Westwood made some remarks concerning *Papilio Thersander*, figured by Donovan, and arrived at the conclusion that this species (figured originally by Jones in his "Icones") was founded on the combination of a *Papilio* with *Charaxes Fabus*. A discussion ensued concerning the right of named figures of insects, by the older authors, to be regarded in questions of priority.—With reference to the question of the liability of large dragon-flies to the attacks of birds, Mr. Müller called attention to a statement by Natterer, to the effect that some species of *Falconide* habitually prey upon dragon-flies. Mr. Horne stated that during his residence in India he had never seen those insects attacked by birds of any description.—Major Parry communicated notes concerning *Lissapterus Howittianus*, and Mr. W. F. Kirby on the synonymy of various *Lepidoptera*.

Linnean Society, December 7.—Mr. G. Benthall, president, in the chair. "On the formation of British Pearls and their

possible improvement," by R. Garner. The author referred to the theory, now generally adopted, that the production of pearls in oysters and other mollusks is caused by the irritation produced by the attacks of the minute parasite known as *Distoma*, and believed that, by artificial means, this parasite might be greatly increased. British pearls are obtained mostly from species of *Unio*, *Anodon*, and *Mytilus*, but it is probable that all mollusks, whether bivalve or univalve, with a nacreous lining to the shell, might be made to produce pearls. An interesting discussion followed, in which Mr. Gwyn Jeffreys, Mr. Holdsworth, and Dr. Murie took part.—"On certain Coleopterous Larvæ," by Dr. Burmeister, of Buenos Ayres.—"On the Botany of the Speke and Grant expedition," by Lieutenant-Colonel Grant. Notwithstanding the difficulties of their journey, and that they had more than once to destroy or abandon their whole collection, Captain Speke and Captain Grant succeeded in bringing home between 700 and 800 species of plants, many of them entirely new, which have been described by Prof. Oliver, and will be published in the "Transactions" of the Society, with at least 100 plates.

Anthropological Institute, December 4.—Sir John Lubbock, Bart., president, in the chair.—Messrs. J. Cordy Burrows, J. Park Harrison, and P. C. Sutherland were elected members. Captain Richard F. Burton read his second paper on "Anthropological Collections from the Holy Land." The paper included a *catalogue raisonné* of articles presented to the museum of the institute, found by Mr. John S. Rattray at Sâhib El Zamân (Lord of the Age), the reputed tomb of Hezekiah. This "find" consisted of fragments of human skulls and long bones, old copper bracelets, brass bracelets, coins, bits of lachrymatories (the glass being highly iridescent), portions of Syrian majolica of the type of that usually made at Damascus by the Tartars, beads of various kinds, &c. The tomb was situated in a hollow on the Eastern slope of the Libanus, and proved to be an artificial cavern, with a shaft for ventilation. A full detailed description of this very interesting discovery was given. Another interesting discovery made by Captain Burton was at the upland village of Ma'alulah, distant three hours from the large convent Saidnâya, roughly speaking N.E. of Damascus, and occupying a position on the N.E. ranges of the Anti-Libanus. This find consisted of various fragments of skulls and lower jaws, which, together with the human remains from the tomb at Sâhib El Zamân, were described by Dr. Carter Blake. The third part of Captain Burton's paper was occupied by an account of a series of flint and stone implements and flakes, and articles of bronze and bone found near Bethlehem. In a detailed description of these articles Mr. John Evans, F.R.S., pointed out for special notice a basaltic hammer, which differed from the usual form of similar instruments discovered in Scandinavia, in Britain, and in North America, inasmuch as in the specimen the lateral depressions were absent.—Prof. Busk, F.R.S., read a communication from the Rev. Mr. Dale on flint implements from the Cape of Good Hope, which were exhibited on the table; and Mr. F. W. Rudler, F.G.S., exhibited a stone implement of unique form, also from the Cape. The President submitted for inspection some stone implements of rare beauty from Greece.

Quekett Microscopical Club, Nov. 24.—Prof. Lionel S. Beale in the chair. A paper was read by Mr. M. C. Cooke on "The Minute Structure of Tremelloid Uredines (*Podisoma*)," in which the structure of the Tremelloid masses, commonly found on juniper bushes, was detailed, together with the results of the observations of Tulasne, Oersted, and others on the germination and development of these fungi, with a critical examination of the species described under the genera *Gymnosporangium* and *Podisoma*. It was held by the author that no good foundation existed for the constitution of two genera, since the minute structure and development of both were identical. Some conversation ensued on the phenomena of alternation of generations which these and other fungi present, and especially in cases where some of the phases of existence were presumed to be passed on different hosts. Especial reference was made to the opinions entertained by Prof. Oersted that the *Podisomas* were found in one state parasitic on leaves of Pomaceous trees, as *Rostelias*, &c., in another stage inhabiting the branches of junipers, as *Podisoma*. The author of the paper did not consider that this supposed phenomenon was satisfactorily proved.

MANCHESTER

Manchester Literary and Philosophical Society, October 31.—E. W. Binney, F.R.S., president, in the chair.—

Mr. Wm. Boyd Dawkins, F.R.S., gave a short account of the discoveries in the Victoria Cave, made since the last account was published in the Transactions of the Society. The clay forming the bottom of the cave, and which hitherto had been barren, was now yielding broken fragments of bone, some of which had been gnawed by the cave-hyena. A lower jaw of this animal was found, which indicated the presence of the characteristic Pleistocene mammalia in a part of Yorkshire in which they had not been known to have existed up to the present time. There were, therefore, three distinct groups of remains in the cave, the Romano-Celtic on the surface, the Neolithic beneath, and lastly that which has been furnished by the clay which is glacial in character. And since two feet of talus had been accumulated above the Romano-Celtic layer during the last 1,200 years, it is very probable that the accumulation of debris of precisely the same character between the Romano-Celtic and Neolithic layers, six feet in thickness, was formed in about thrice the time, or 3,600 years. If this rough estimate be accepted, and it is probably true approximately, the Neolithic occupation of the cave must date back to between 4,000 and 5,000 years ago. There is no clue to the relative antiquity of the group of remains found in the clay; but it may safely be stated to be far greater than that of the Neolithic stratum. Throughout Europe the break between the Pleistocene age represented in the cave by the bones in the clay and the Prehistoric age—the Neolithic of the cave—is so great and so full of difficulty that it cannot be gauged by any method which has hitherto been invented. Mr. Boyd Dawkins also exhibited a remarkably perfect javelin head of bronze which had been dug up in a field near Settle.—“Species viewed Mathematically,” by Mr. T. S. Aldis. We have learnt that all energy is really one, whether seen in heat, constrained position, or motion. Many also believe that life is really one, whether seen in man or a toadstool. But for our part we have often felt a difficulty. Why, if all life be one, do we not see it passing through every variety of form instead of being restricted to certain well-defined types? The present paper is an attempt to explain this. Let us consider what Plato might have called the *αὐτοζών* or complete type of animal. It consists of a certain definite number of organs, composed of a certain definite number of parts. It will also have certain aliments, location, enemies, &c., which we may call its province, necessary for its life. Thus our type animal is capable of a flux passing through all possible forms and provinces in all possible combinations. I include amongst these, of course, many arrangements necessarily absurd. To each arrangement of organs and provinces thus imagined would correspond a certain vitality or power of living in the type. I mean not merely power of individual existence, but existence as a race. The vitality is therefore a function of a large number of variables, some independent, others connected by equations of condition. It is to us quite an unknown function, but not therefore indefinite. Therefore, as in any other function of variables, certain relations amongst the variables will give maxima values of the vitality. These maxima of vitality constitute species. Vitality is not mere physical might or agility or fecundity, but compounded of all. Now for a maximum, we know that any change in the variables lessens the function. We thus see how species are stable. In the constant variation, for no being seems capable of reproducing itself exactly, all individuals have less vitality as they depart from the special type which gives the maximum of vitality, and will be choked out by those which, being nearer to the type, possess more vitality. So hybrids, intermediate between two maxima, will possess less vitality than either, and will be choked out, though the main cause of failure is that the process is like that devised by Swift's Laputan philosopher, who sawed the Whigs' and Tories' heads in half, and changing them, left each brain to settle its politics in itself. So the poor mule, with a bundle of habits, half horse and half ass, in this intestine conflict, has little power to take care of itself. Of course all maxima may not have plants or animals representing them. If there be several maxima suited for nearly the same province, the maximum of greatest intensity will choke out the others. So, too, there are probably many maxima now unoccupied, as, for instance, the thistle represented a maximum of vegetable life in South America, but till man imported the thistle to fill it up, other maxima of less intensity held the ground. In some cases possibly several maxima are closely related, and differ little in their intensity, so that slightly differing species exist together, and may in their variation pass one into the other, as perhaps in brambles and some species of St. John's wort, &c. If then the province of a species, *i.e.*, the physical geography of

a country, alter, and its enemies and food with them, clearly the maximum will shift and the species change. But this is not the evolution of new species, though to a person who only notes geological evidence it appears so. For, just as in a storm the lightning shows the trees still, though really waving to and fro, so the different species in geology are probably but steps in a constant change. Such a change of course must be slow for life to follow it, for a species consists quite as much in a bundle of acquired and transmitted habits as in a certain formation of organs, and the change in habit will probably be far slower than the change in form. How then do new species arise? For we see that, if the species be a maximum of vitality, in a multitudinous progeny those nearest the type will choke out the others, and the species will be stable. Varieties will be connected with maxima of vitality in two ways. Firstly, slight differences in the province will slightly shift the maximum. Thus mountain sheep would be more agile than lowland sheep. Secondly, in such a way as this. Suppose this table a low mound, narrow though long. Then the height at any point will be a function of the distances from the north and east walls of the room. There will be one point of maximum height, but whilst a change north or south produces a great change in the altitude, one east or west will produce but little. So there will be variations in some characteristics which will produce little alteration in the whole vitality. Thus, amongst wild oxen probably no varieties without horns would exist, for they affect the vitality. Amongst protected races they do not, and so hornless varieties arise. Still these varieties are but varieties, and are not steps towards a new maximum which a gulf of lesser vitality still separates them from. Or let us consider the varieties that we try to make by select breeding. These are least of all likely to produce new species. We simply by main force depress vitality in removing individuals as far as we can from the normal type, and when the vitality is sufficiently depressed we can go no further. As for altering the province, the independent variables, so to speak, we know so little how to do it, and certainly could not do it gradually enough, that we have no chance in this way of effecting anything. How then can new species arise? Apparently in some such way as this, by what we may call the bifurcation of a maximum. If we drew a horizontal line along which the variation of the organs of an animal were expressed and the corresponding vitality were drawn by ordinates, we should get a curve we might call the vitality curve, whose maxima values would be species. As time elapses and the conditions of the earth, &c., alter, the constants, so to speak, of the curve alter, and we get our curve to vary and the maxima shift; and as the curve alters, one maximum may separate into two or more others, and thus in the lapse of time, one species may separate into two or more others. Roughly to illustrate it, suppose some species developed free from the influence of carnivora, and that, owing to various causes, size little affects its vitality, it may vary all through, from little and swift to big and heavy. Now, introducing carnivora, we can see how a bifurcation of our maximum would take place. The very light and swift would preserve themselves by their agility, the strong and heavy by their strength, whilst the intermediate would be killed out, and thus two distinct species would arise, which might in course of time by further variation separate still further apart. Doubtless, however, this bifurcation goes back to very remote times. Carnivores and herbivores probably separated not as mammals but as reptiles, or even long before, whilst ruminants and non-ruminants may have separated since they became mammals. Thus Australia seems to have possessed at one time only one marsupial, which has bifurcated into various marsupials, but not into any of another kind. The older the species grow, the deeper is the gulf between them, and, like a river, we have to ascend nearly to the source before we can make a passage from one bank to the other. To recapitulate—Maxima of vitality are species. Any alteration from the normal type produces less vitality, hence the normal type is stable. A slow change of physical geography, &c., slowly changes these maxima, and the species change with them, extinct species being generally glimpses of steps in this change. New species will generally arise from the bifurcation of maxima under circumstances over which man can exercise little control, and which, if he could, he would very likely alter so as either hardly to affect the maximum at all, or too rapidly for the species to shift with it. Selected breeding produces types of less vitality, and therefore will hardly produce new species. Thus the present stability of species is no argument against the doctrine of evolution.

GLASGOW

Geological Society, November 30.—Dr. Robert Brown, F.R.G.S., delivered a lecture on "Greenland: Its Physical Geology and Fossil Flora." After alluding to the interest which Greenland possessed, as presenting a picture of what the British Isles were supposed to have been during the glacial period, Dr. Brown gave a graphic sketch of the coast scenery of the country, which he compared to a succession of islands with water on the one side and ice on the other. He described the interior of Greenland as one vast sheet of ice of great thickness, pressing out on all sides to the sea, and occupying as separate glaciers the fiords which indent the coast. These glaciers in many instances push their way out to sea, where portions are broken off and drift away as icebergs; in other cases, the glacier dissolves near the head of the fiord, and great stores of muddy water escaping from it form a deposit of fine clay, which has sometimes silted up part of the fiord so effectually as even to turn the glacier aside into another channel. From what he had observed in Greenland, he was inclined to hold that the lower till, or boulder-clay, as it exists in the Forth and Clyde valley, was formed by such a sheet of massive land ice slowly moving over the country, while what he had described as resulting from the waste of the glaciers near the sea might account for some of the well-known beds of laminated clay associated with that deposit. He questioned whether icebergs really did much in the way of conveying rocks or *débris* to any distance. So far as he had observed they bore wonderfully little of such material in or upon them; and he thought that to call in their agency, as had sometimes been done, to account for the dispersion of plants, &c., was highly visionary. Dr. Brown then alluded to the rock-formations of Greenland, and to the plant remains of the Carboniferous and middle Tertiary periods which had been found in the country, showing that it once enjoyed a very different climate from that to which it is now subjected. The Carboniferous plants had only been recently discovered by Dr. Pfaff, and he trusted that gentleman, who was resident on the spot, would be enabled to make further researches.

PARIS

Academy of Sciences, December 4.—M. Chasles presented a number of theorems relating to the harmonic axes of geometrical curves, and M. C. Jordan a paper on Gauss's sums with several variables. — M. Tresca read a paper on the effects produced during the planing of metals; and M. H. Resal communicated some investigations on the calculation of the fly-wheels of steam-engines.—Letters were read from Father Secchi on a new method of measuring the heights of the solar protuberances, and on the temperature of the sun. Upon the latter M. Faye made some remarks.—M. Le Verrier presented a note on the shooting stars of the month of November, from observations made in France and Italy. Many meteors issued from the constellation Leo, but the point of radiation was slightly displaced. Five or six currents of meteors in different directions were observed. In August a displacement of the point of radiation was observed between the 9th and 11th.—An extract from a letter from M. J. F. J. Schmidt to M. Delaunay on the November meteors observed at Athens was also read.—M. C. Saint-Claire Deville communicated a note on the early cold weather of 1871, which appears to have prevailed over the whole of France.—M. F. de Biseau recorded the observation of aurora borealis in Belgium on the nights of the 9th and 10th November.—A note from M. de Magnac on the determination by means of chronometers of the differences of longitude of distant places was read.—M. Lecoq de Boisbaudran presented a note on the separation and quantitative determination of some metals by means of a voltaic current.—M. A. Béchamp communicated some observations on a recent note by M. Ritter on the formation of urea by albuminoid materials and permanganate of potash.—M. Wurtz presented a note by M. L. C. de Coppet on a new method of preparing supersaturated saline solutions, in which the author stated that solutions identical with those called supersaturated could be prepared by dissolving certain dehydrated salts (sulphate and carbonate of soda) in cold water.—M. Peligot presented a note by M. T. Schlessing, containing a comparison of the two conditions of a soil in part wooded and in part cleared and treated with lime.—M. Peligot also presented a note by M. A. Renard on the determination of ground-nut oil in olive oil. The process, which is rather complicated, consists in the saponification of the oil, and the separation from the soap of the arachidic acid which is characteristic of ground-nut oil.—M. Balard communicated a note by MM. Scheurer-Kestner and C. Meunier

on the composition and heat of combustion of lignites, containing the analyses and results of combustion of six lignites from various parts of France, and from Bohemia. The heat of combustion was always found to be inferior to that of the carbon and hydrogen contained in the lignites.—M. Elie de Beaumont exhibited a collection of minerals from Bolivia, Chili, and Peru sent by M. Domeyko.—M. S. Meunier presented a note on a new method of obtaining Widmannstätten's figures by attaching a polished plate of meteoric iron to the positive pole of a Bunsen's battery and a plate of silver to the opposite pole, and plunging both into a solution of bisulphate of potash.—M. Husson communicated an analysis of the milk of cows attacked by contagious typhus.—A note was read on the Garumnian type of the department of the Aude, by M. A. Leymerie, in which the author maintains the distinctness of this geological stage, and indicates some of the fossils which characterise it.

BOOKS RECEIVED

ENGLISH.—Marvels of Pond Life: H. J. Slack (Groombridge and Sons).—The Amateur's Flower Garden: Shirley Hibberd (Groombridge and Sons).—Flowers for Sundays: P. Spenser (Longmans).—The Laws of the Wind prevailing in Western Europe; No. 1, with Charts and Diagrams; W. C. Ley (E. Stanford).
FOREIGN.—(Through Williams and Norgate).—Die Axendrehung der Welt-körper: E. F. T. Moldenhauer.

DIARY

THURSDAY, DECEMBER 14.

ROYAL SOCIETY, at 8.30.—Contributions to the History of Orcin. No. II. Chlorine and Bromine Substitution Compounds of the Orcins; Note on Fuedsol: Dr. Stenhouse, F.R.S.—On some recent Discoveries in Solar Physics; and on a Law regulating the Duration of the Sunspot Period: W. De La Rue, F.R.S., B. Stewart, F.R.S., and B. Loewy.
MATHEMATICAL SOCIETY, at 8.—On the Celebrated Theorem that every Arithmetical Progression, if it contains more than one must contain an Infinite number of Prime Numbers: J. J. Sylvester, F.R.S.

FRIDAY, DECEMBER 15.

LONDON INSTITUTION, at 4.—Elementary Physiology, by Prof. Huxley, F.R.S. No. 7. (Extra Lecture.)

SUNDAY, DECEMBER 17.

SUNDAY LECTURE SOCIETY, at 4.—On the Physiology of Contagion and Infection: Dr. John S. Bristowe.

MONDAY, DECEMBER 18.

ANTHROPOLOGICAL INSTITUTE, at 8.—The Anthropology of Auguste Comte: Joseph Kaines.—On the Hereditary Transmission of Endowments: George Harris.

LONDON INSTITUTION, at 4. No. 8.

TUESDAY, DECEMBER 19.

STATISTICAL SOCIETY, at 7.45.—On the Comparative Health of Seamen and Soldiers: Dr. Balfour.

WEDNESDAY, DECEMBER 20.

GEOLOGICAL SOCIETY, at 8.—Further Remarks on the Relationship of the Limulidae to the Eurypteridae and to the Trilobita: Henry Woodward, F.G.S.—Further Notes on the Geology of the neighbourhood of Malaga: M. D. M. d'Orueta.

ROYAL SOCIETY OF LITERATURE.—On a capital Joke recorded by Suetonius: Dr. C. Mansfield Ingleby.—On a Collection of Roman Brick Stamps in the Ashmolean Museum at Oxford: Mr. Vaux.

SOCIETY OF ARTS, at 8.—On the Study of Economic Botany, and its Claims Educationally and Commercially Considered: James Collins.

THURSDAY, DECEMBER 21.

ROYAL SOCIETY, at 8.30.

LINNEAN SOCIETY, at 8.—On the Anatomy of the American King-Crab (*Limulus polyphemus*, Latr.): Prof. Owen, F.R.S.

CHEMICAL SOCIETY, at 8.

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