THURSDAY, OCTOBER 18, 1877

HUXLEY'S "ANATOMY OF INVERTEBRATED ANIMALS"

A Manual of the Anatomy of Invertebrated Animals. By Thomas H. Huxley, LL.D., F.R.S. (London: Churchill, 1877.)

IN the year 1871 Prof. Huxley published a Manual of the Anatomy of Vertebrated Animals as the first part of a treatise on Comparative Anatomy. By the publication during the present autumn of the work now about to be noticed, he has fulfilled his undertaking to produce a treatise for students on this extensive and complex branch of anatomical inquiry. As might be expected from the author's well-won reputation, not only as a philosophical thinker and scientific observer, but as extensively read in the literature of his subject, the work is one which, in proportion to its size, furnishes the student with the most compact account of the present aspect of the science of comparative anatomy in the English language.

In writing a work on comparative anatomy, two modes of arranging and classifying the multitude of facts that are to be considered may be pursued by an author. In the one, which may be called the anatomico-physiological method, he may take as the basis of his arrangement the several organs found in the animal body, and may trace out the various modifications exhibited by each organ in different animals. This plan has been pursued in the well-known systematic treatises of Cuvier, Meckel, and Milne-Edwards. In the other, or zoological method, animals are grouped in "natural orders" according to a taxonomic system, and the anatomical characters of the animals belonging to each of these orders are described and compared with each other. Siebold, Stannius, Owen, and Gegenbaur adopted this mode of arrangement, and a somewhat similar plan has been carried out by the author of the work before us.

Although the anatomico-physiological method is not without its advantages to a particular class of readers, yet the zoological basis of arrangement is, we think, more generally useful, as it enables the reader to acquire by the perusal of a single chapter, a knowledge of the anatomy of a given natural order of animals, instead of having to pick out from a number of different chapters the various facts bearing on their structure.

In carrying out this plan Prof. Huxley has selected for special and more elaborate description a characteristic specimen of each group or order, whilst the other members of the same group or order are either only incidentally alluded to, or merely the chief features of difference between them and the selected specimen are pointed out. For a student's book this is unquestionably the most suitable plan, as it gives him the means of perusing, within the compass of a moderate-sized volume, a carefully drawn-up account of well-marked examples of the various orders, without confusing him with a multitude of subordinate facts which it would be difficult to keep in mind, whilst it facilitates practical study by supplying a somewhat full description of individual forms. In addition to the description of adult forms much information is also communicated on the development of the animals described, and the facts of embryology are made to throw much valuable light on the details of structure.

To the general reader the most interesting sections of the book are the introduction and the first and last chapters. The introduction commences by pointing out that the distinctive properties of living matter are due to its chemical composition, to its universal disintegration and waste by oxidation, and its concomitant reintegration by the intus-susception of new matter, and to its tendency to undergo cyclical changes. A number of interesting examples are then given of the dependence of all the activities of living matter upon moisture, and upon heat within a limited range of temperature. The arguments which have been advanced in favour of the origin of living from non-living matter, or abiogenesis, as it is now termed, are then considered. The conclusion is come to that there is no ground for assuming, as is done by the supporters of the hypothesis of abiogenesis, that all living matter is killed at some given temperature—between 104° and 208° F., so that the evidence adduced in its favour, from the experiments where organic infusions have been subjected to this high temperature, is logically insufficient to furnish proof of its occurrence. Prof. Huxley abides, therefore, by the opinion expressed in his well-known Liverpool Address to the British Association on this subject. There is no necessary connection as has sometimes been assumed between the theory of evolution and a belief in the occurrence of abiogenesis as a mode of origin of living things at the present day. Life must at one time have been breathed into non-living matter, but there is nothing to show that existing organisms, or those occurring in any recorded epoch of geological time, have had any other origin than from pre-existing forms of life.

It has been assumed by many writers that the developmental changes, which an organism passes through during its embryonic existence, furnish a key to decipher the full pedigree of the organism and proclaim its family history. So keenly has this branch of biological speculation or phylogeny, as it is termed, been followed out by some naturalists, that they have based their systems of classification on the supposed ancestral history of animals. We are glad to find that Prof. Huxley interposes some wise words of caution on this matter. The reconstruction of the pedigree of a group from the developmental history of its existing members is, he says, fraught with difficulties. And again, of the numerous phylogenic hypotheses which have of late come into existence, few have any other significance than as suggesting new lines of investigation; in the absence of any adequate palæontological history of the *Invertebrata*, any attempt to construct their phylogeny must be mere speculation. It is to be hoped that this protest against the fanciful hypotheses of some phylogenists, a protest which might have been unheeded if it had emanated from an opponent of the doctrine of evolution, may, seeing that it comes from him, who by the lucidity, vigour, and logical power of his writings, has, next to the illustrious Darwin, done more to gain credence for the doctrine, than any other writer, check the tendency to hasty speculation in this direction, in which some naturalists have of late indulged.

Had space permitted we should have liked to have given a sketch of the groups or natural orders in which Prof. Huxley arranges the *Invertebrata*, the classification of which is surrounded with so many difficulties, that scarcely any two writers on classification adopt the same taxonomic system. Prof. Huxley does not look upon the arrangement he has adopted as more than temporary, as our knowledge of the anatomy and development of the Invertebrata is increasing with such prodigious rapidity that the views of taxonomists in regard to the proper manner of expressing that knowledge by classification are undergoing, and for some time to come are likely to undergo, incessant modifications.

We heartily commend this book to all students of Comparative Anatomy.

EVERETT'S "TEXT-BOOK OF PHYSICS"

Elementary Text-Book of Physics. By J. D. Everett, M.A., D.C.L., F.R.S.E., Professor of Natural Philosophy in the Queen's College, Belfast. (Glasgow: Blackie, 1877.)

IN the preface to this book the author says: "It is primarily intended as a text-book for elementary classes of Physics. It aims at presenting, in brief space, those portions of theoretical physics which are most essential as a foundation for subsequent advances, while at the same time most fitted for exercising the learner in logical and consecutive thought. It does not give minute directions for manipulation, but, avoiding details as much as possible, presents a connected outline of the main points of theory. . . . The aim must be not so much to teach them [the bulk of the boys in our public schools] many facts, as to teach them rightly to connect a few great facts together. . . . The book is not intended to supersede oral instruction, but rather to create a demand for amplification and illustration such as the teacher will supply."

Judged from this point of view the text-book must receive almost unqualified praise. The different divisions of the book treat respectively of the subjects, dynamics (in its modern acceptation), hydrostatics, heat, light, sound, and electricity including magnetism, and in each division the author explains the leading facts in clear, concise, and accurate language. What mathematics is introduced is of the simplest possible kind, and need not prevent the veriest tyro in geometry and algebra, provided he is possessed of ordinary intelligence, from reading and understanding the book from beginning to end. The definitions are, as a rule, very exact, and the explanation of the units, as might have been expected from the author of the pamphlet, the "centimetre-gramme-second system of units" is singularly precise. Add to this that the diagrams are numerous, and, which is of rare occurrence in an English text-book, of unusual excellence, and that each division is followed by a collection of examples (except the last, which apparently has not been thought worthy of the honour) at once good and easy, and enough has been said to show that the text-book is one with many merits.

It has its demerits too, and if we dwell longer on them it is only in the hope that a truly excellent manual may be rendered still more excellent in a second edition. Why does the author make not the slightest mention of Newton's laws of motion, although all the statements made in them are asserted, but in such a casual off-hand sort of way that the student wonders what is the evidence for such important statements? For example, in Art. 9 it is said "If a body with a movement of translation (unaccompanied by rotation) is acted on either by no forces or by balancing forces, it continues to move with uniform velocity in a straight course." This assertion is introduced by no explanation, neither is it followed by any remark or illustration. The same thing may be said, and even more forcibly, of the treatment which the second law receives. This fault, of making assertions without any explanation or shadow of proof, is rather too apparent throughout the book, as a few instances will show. The examples are taken from pages that are all near together. At p. 147, line 4, we read, "the last image consists of two coincident images, as has already been shown [for 'shown' read 'asserted'] to be the case when the angle is a right angle." Again, at p. 150, line 3, it is said, "The angles of incidence and refraction increase together and the deviation increases with them." At p. 153, after a description of Airy's simple and beautiful apparatus for illustrating refraction, it is added "A B C will be the path of a ray, and a stud at C will appear in the same line with studs at A and B." At p. 158, line 9 from bottom, we find—"If the eye is moved with uniform velocity from one side of the normal to the other (in one straight line), the image moves with a velocity continually diminishing till the normal is reached, becoming zero at the normal, and then again gradually increasing. This is a general property of geometrical images, whether formed by refraction or reflection. . . . ." At p. 161, line 1:—". . . . The rays reflected from the outer portions of the mirror will fall sensibly short of the middle point of O C. If the point of incidence be supposed to travel with uniform velocity along the arc M O from M to N, the intersection of the reflected ray with O C will move towards F with velocity gradually diminishing to zero." Once more, on p. 163, last line, and p. 164, first three lines, we read:—" . . . . It can be shown that if the angular aperture be small all the reflected rays will meet sensibly in one point, P." It can also be shown that—

$$\frac{1}{OP} + \frac{1}{OP'} = \frac{1}{OF}.$$

Although it is not to be expected that any other seventeen pages will supply as many examples as these, still such faults do occur throughout the book. We do not object to statements heralded by the words "It can be shown," as the student is at once put on his guard, and virtually referred to other sources for the proof, if he wishes it; but most frequently he is left either to take the statement on trust, or, in the words of the preface, "to exercise his mind in logical and consecutive thought." Only we think that in the latter case the text-book can hardly be called one "for elementary classes."

From inaccuracies the book is wonderfully free, but there is surely one, and not a small one, about harmonics at the bottom of p. 241. The author asserts that the origin of harmonics depends on two very different causes. Sometimes it is found in the different modes of free vibration of the body which emits the sound (and this is especially the case with the sounds of stringed instruments), but in other cases (including the tones of the siren, and the human voice) it depends on a very different cause, namely, the mathematical law that every series of precisely similar vibrations is either simple, or compounded of one set of simple vibrations, giving the fundamental tone, and other sets of simple vibrations giving the harmonics." Now the second cause—Fourier's law—is surely the only one for the existence of harmonics, and in every vibration, whether of a string or any other body, the ear analyses the motion of the air in accordance with the law, that is, hears harmonics. Is it possible that the origin of the misconception here lies in the old notion that when a string vibrates in any regular manner there are secondary waves riding on the primary ones, another set on these secondary, and so on? as—

> "Great fleas have little fleas
> Upon their backs to bite 'em,
> The little fleas have lesser fleas,
> And so ad infinitum."

By the way, why are the notes of the human voice, which are produced by the vibrations of the vocal chords, distinguished from the notes of stringed instruments?

It is not to be expected in an elementary manual that every subject, even of importance, should be noticed, but we should have thought that even in the briefest treatise on heat, some notice would have been taken of the "theory of exchanges," and yet we find no mention whatever made of it. Also a little more space than two pages might have been devoted to the electric telegraph, especially, first, as room has already been found for the description of the venerable three kinds of lever, and the antiquated three systems of pulleys (which are rarely seen except in text-books of physics); and, secondly, inasmuch as three times the space is taken up in the description of the air-pump and its modifications.

The good qualities of the book are so conspicuous, and its faults either so slight or so easily corrected by the teacher, that we have no hesitation in warmly recommending it as a good text-book for junior classes.

T. H. C.

OUR BOOK SHELF

Popular British Fungi; containing Descriptions and Histories of the Principal Fungi, both Edible and Poisonous, of our Country. Illustrated. By James Britten, F.L.S. (London: the *Bazaar* Office.)

THIS admirable little book forms an agreeable and popular introduction to a much neglected group of plants. Written in a pleasant easy style, it yet conveys a great deal of sound information. Mr. Britten having drawn on his imagination merely for the setting of his facts, not for the facts themselves. The different illustrations convey a tolerably accurate idea of the plants represented. The edible fungi are carefully described, and most useful hints and directions given as to the modes of cooking and preparing for table. The poisonous forms also receive a considerable share of attention, and the characters are carefully given, but even with all sorts of descriptions we cannot but think that there is always danger from such genera as Lactarius and Russula. Besides treating of the usual edible and poisonous fungi, Mr. Britten gives a chapter on Dry Rot, another on Luminous Fungi, and another on the Sphaeriacei. Throughout the whole book we constantly meet with quaint quotations from old authors. The book, then, is not only a very pleasant and readable one, but conveys a great deal of sound information on the subject therein treated.

Zeitschrift für das chemische Grossgewerbe. Herausgegeben von Jul. Post. II. Jahrgang. Heft I. (Berlin: Robt. Oppenheim.)

WE have already had occasion to express our high opinion of the value of Dr. Post's contributions to chemical technology. The present work is to the chemical manufacturer what the well-known "Jahresbericht" of Liebig and Kopp is to the scientific chemist. It attempts to give the technologist a systematic account of the latest advances in the several departments of manufacturing chemistry and the allied arts. As the various contributions are from the pens of men who, in the majority of cases, have made the matters upon which they write the objects of special attention and study, we can confidently recommend the work to the notice of our chemical manufacturers.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications. The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall Statistics

SEVERAL correspondents in the *Times* and elsewhere have lately complained regarding the class of information furnished by the Government of India with reference to the famine. One of them pointed out that while the Indian official *Gazette* is filled with tables of food-prices, and other statistics, there seems to be no attempt to deduce the real significance of those figures, nor are any data of comparison offered by which the public might make deductions for themselves. I have just come across a very glaring instance of this. The Indian rainfall having been discussed a good deal this summer in your columns, I heard with some rejoicing that a long list of returns had been given in the Indian *Gazette* for stations in Madras and Bombay. The returns purport to bear upon the variation of rainfall in tropical India with reference to the cycle of sun-spots. As an old resident in Madras and Hyderabad, I think it would be difficult to produce a series of figures more irrelevant or more misleading with regard to the matter in hand. So far as I understand your articles, it s alleged that of the six famines in Southern India since 1810, five were caused by great droughts at the periods of minimum sun-spot frequency. It also seems to be alleged by you that the rainfall at Madras itself follows a cycle curiously coincident with the eleven-year cycle of sun-spots. These are two propositions distinct in themselves, and either of them is well worthy of investigation by the Meteorological Reporter to the Government of India.

The figures signed by that officer in the *Gazette* yield no information on the subject. He begins by completely mis-stating the case. Instead of testing the one statement as to whether drought and famine in Southern India have been coincident with periods of minimum sun-spots, or the other statement as to whether the rainfall at Madras itself forms a cycle coincident with the sun-spot cycle, he assumes that the question at issue is whether the rainfall at all the stations throughout Southern India shows a common periodicity coincident with the cycle of sun-spots.

Now, sir, it surely displays a gross ignorance with regard to the geographical facts of Southern India, to suppose that a periodicity, or any other feature of the rainfall of a place on the coast, like Madras, can be reproduced at the stations which the paper seems to take haphazard at the inner recesses beyond the Ghauts. I shall take, for example, only three stations in which I have resided, and whose monsoons I have seen. Madras derives about two-thirds, or twenty-nine inches, of its total rainfall from the north-eastern monsoon between October and December. The periodicity of its total annual rainfall is chiefly due to this monsoon. The Calcutta meteorological reporter compares this station with Secunderabad, where the north-eastern monsoon yields only three inches, and with Bangalore, where it yields only six or seven. I may add that he also compares it with Bellary, which derives only three inches from the north-east monsoon. It must be evident that any cyclic periodicity in the Madras rainfall which is chiefly derived from the north-eastern monsoon, cannot possibly be common to such stations as Bangalore, Bellary, and Secunderabad, which are scarcely reached by the north-east monsoon, and which derive an altogether insignificant rainfall from it.

I have only quoted three instances, but the paper is full of similar absurdities. The Government of India should not be surprised that we Madrassees are jealous of its interference and distrustful with regard to its ability to interfere for good, so long as it puts its official *imprimatur* on papers such as this. I do not suppose that Mr. Blanford, the meteorological reporter, is personally responsible for it. The explanation no doubt consists in the fact that the paper has been drawn up by some ignorant native clerk, and signed *pro forma* by his official superior. Any subaltern in a marching regiment who has crossed the Ghauts knows that the north-east monsoon cannot possibly exert a common influence upon the coast where it breaks in its first fury, and the walled-in plateau of Hyderabad or Bellary, where it scarcely reaches at all.

OLD MADRASSEE

Potential Energy

WHEREVER the fault may lie, your correspondent, "P. M.," has quite misapprehended John O'Toole, whose amanuensis I am, in every point on which he, "P. M.," touches, but one, viz., John's wish to place the potential energy in the force instead of in the body to be moved.

X.

Dublin, October 12

IN the capacity of poor P. I have suffered much at the hands of the doctors, and am glad to find in your correspondent "X." so competent an exponent of many of the difficulties with which the subject of energy, as generally taught, is beset. But while in the main I agree with the criticisms of "X." there are two or three points with reference to which I may perhaps venture an opinion.

It seems to be admitted (NATURE, vol. xvi. p. 459) by your correspondent that in the case of a moving body "the kinetic energy is undeniably in the body." This appears to me to lie at the root of the chief difficulty, for, as far as I can see, we have no more right to assert that a cannon shot possesses kinetic E. when it leaves the muzzle of the gun than to say that a clock weight possesses potential E. when it has been wound up; for it may happen that the shot is at rest relative (shall we say) to the centre of gravity of the physical universe. At any rate we are well assured that of itself it can do no work. Suppose the shot to find a home in the side of a ship; in entering the ship it does work, but the amount done depends upon the original motion of the ship, being greater if the ship were moving to meet the shot than if it were at rest; it also depends on the mass of the ship, for if the shot sensibly change the motion of the ship its own motion will be less altered by the impact. When a shot strikes a target, which we assume to be rigidly connected with the earth, the mass of the latter is so great that we may consider the target as fixed, and thus we have only to contemplate the mass and velocity of the shot. Simple problems of this nature were of course more inviting than those in which the mass and velocity of both the colliding bodies had to be taken into account; and thus it came to pass that the kinetic E. was attributed to the shot alone instead of being considered an attribute of the system consisting of the earth and shot together and due to the relative velocity of the two. Adopting this latter mode of expression, we may, if we please, suppose the shot to be at rest and the earth moving relative to it with the velocity of (say) 1,400 feet per second, and the energy of the system will be unaltered by our convention; but where should we be if in this case we supposed the kinetic E. of a rigid moving body to be an attribute of it alone?

Again, when we say that the kinetic E. of m units of mass moving with v units of velocity is $\frac{1}{2} m v^2$, this velocity must be measured with reference to some point or other which we for the time consider fixed. In order to obtain $\frac{1}{2} m v^2$ units of work from the body, we must bring it to rest relative to our point of reference, but in so doing we must take care that no motion is imparted to the point of reference itself, for if this be the case the body will come to rest relative to it without losing v units of velocity, and therefore without d ing $\frac{1}{2} m v^2$ units of work. If our system consist only of two bodies, and work is to be done by bringing them finally to relative rest, this condition will be fulfilled only when the body which we choose to consider at rest is indefinitely great compared with the other, and only then may we represent the kinetic energy of the system by $\frac{1}{2} m v^2$, m being the mass of the smaller body and v its velocity relative to the other.

I think we shall avoid all difficulty if we define the kinetic energy of a system as *the energy which the system possesses in virtue of the relative motions of its parts*; we shall then never hear of the kinetic E. of a shot or other rigid body, except as an abbreviation for the kinetic E. of the system consisting of the earth and the shot, &c.

A precisely similar line of argument may be followed with reference to potential E. or the E. of position. The potential E. of a rigid body, whatever its position may be, is an absurdity. The very notion of position implies relation to other bodies, as we have no fixed points in space, and thus it is necessary, if only for this reason, that in speaking of potential E., at least two portions of matter which are capable of changing their position relative to one another should be taken into account. In the case of a raised weight, the system consisting of the earth and the weight possesses energy in virtue of the separation of its parts, and the system can be made to do an amount of work equivalent to this energy by keeping the earth fixed and letting the weight fall, or by keeping the weight fixed and letting the earth move up to it, or by letting each move to the other, as in nature. In any case the work done will be the same, though the time required may be very different; but according to the modes of expression complained of by your correspondent, I suppose that in the first case the potential E. belongs to the weight, in the second to the earth, and in the third it is divided between the two. Should we not avoid all difficulty by defining the potential energy of a system as *the energy which the system possesses in virtue of its configuration?* We should then never speak of the potential E. of a single rigid body, such as a raised weight, except as an abbreviation for the potential E. of the system consisting of the body and the earth. A strained elastic body, such as a bent bow, of course possesses potential E., for in this case the particles of the body have been moved relative to one another from their position of rest, and thus the configuration of the system has been changed.

Of course I agree with your correspondent that the potential energy of a system is just as truly *energy* as is its kinetic energy, and this brings me to the last point I proposed touching upon, viz., the term "energy of tension." Perhaps *energy of stress* might avoid some objections, since tension and its antithesis, pressure, have very special meanings; but surely in adopting such a phrase we can hardly say that the designation implies an "essential characteristic," while the term E. of configuration refers only to a "condition." In the case of two attracting bodies the potential E. is greatest when the attraction is least on account of the increased distance, and it depends not upon the actual attraction between the bodies in their existing configuration, but upon the attractions which are called into play in all the configurations assumed by the system as the bodies approach each other, and which therefore belong *in potentiality only*, to the system in its initial condition. If we define the potential of a point in the neighbourhood of a system of bodies as the amount by which the energy of the system would be increased by the introduction of the unit mass at this point, then so long as the mass is absent, the energy due to it can be only *potential*, but when the mass is placed there the energy is *actual*. In strictness, then, we ought not to apply the term potential to the energy thus introduced into the system. We, however, require some mark to distinguish this energy from kinetic energy, and the word potential serves to remind us of the condition of affairs before the mass was introduced. Again, we may have a stress as great as we please acting between the parts of a system without any consequent potential energy, so that the space "condition" seems to be at least as important as the stress "characteristic." On the whole I think the phrase *potential energy* preferable to energy of tension, stress, or configuration, although it is applied to energy which is as truly actual, and belonging to the material system, as is that of a shaft-impelled-against-an-ironclad.

Cambridge

W. G.

WITH reference to the question concerning the bricks, in NATURE, vol. xvi. p. 477, it is obvious that if a man lifts a brick down from a wall and places it on the ground, the *vis viva* of the brick at the bottom is very much less than it would be if it fell from the wall under the influence of gravity. In lifting it down the man does work *against* gravity, and therefore the energy of position of the brick on top of the wall finds its equivalent (1) in the *vis viva* of the brick at the bottom, and (2) the mechanical value of the heat of oxidation of the man's muscles working against gravity. This last is of course the difference between the *vis viva* under gravity alone, and the actual *vis viva*. Hence the man wastes tissue *more*, digests *more* food, and radiates more heat than if he were at rest.

I beg to add a word upon the letter of "X." concerning the term potential energy. Used as the term is to denote energy of position, it cannot be considered "felicitous" or logically exact. Energy of position is *potential vis viva* (or kinetic energy); and *vis viva* (against a force) is *potential* energy of position. In the expression potential energy, we are led to inquire *which* energy. As it stands it properly implies the idea of possible *vis viva*, as if "energy" was used only to signify *vis viva* or kinetic energy. It may be noticed that Prof. Helmholtz uses *vis viva* for "energy" (the adjectives actual and latent being understood) in two places; viz., in his tract on "Conservation of Force," p. 128 of translation, "The *vis viva* of a single particle Δm, &c.," and in "Popular Lectures," p. 196, trans., "The *vis viva* of motion of revolution round the sun, &c.," where the algebraic expression given shows that the *whole vis viva*, actual and latent, or, as it is now called, the *whole* energy, kinetic and potential, is meant. While then the German philosopher uses *vis viva* for work-power in general, the English writer in the terms potential and actual energy employs adjectives which logically require that energy should signify *only* work-power of motion. Out of this maze "X" suggests a way by proposing to use the term "energy of tension" for potential energy, with a reminiscence of Helmholtz's "Sum of the tensions;" but if we keep the expression kinetic energy, we require a corresponding *adjective* to distinguish the other form of energy, and what more expressive, more exact, more "felicitous" word could we find than "statical," the word originally employed by Sir W. Thomson? We have here a most appropriate word, supported by a great name, and I venture to suggest that "statical energy" should come into general use.

Arnesby Vicarage, Rugby W. P. O.

Dealers in Zoological Specimens and Models

HAVING had a considerable amount of trouble in ascertaining the addresses of the various dealers in zoological specimens in this country and abroad, I think it likely that I may be doing service to others who like myself are charged with the formation of an educational museum of zoology and comparative anatomy, if I give in the columns of NATURE a complete list of such dealers as I have found useful. They are as follows:—

1. For spirit specimens and dried parts of fish, molluscs, insects, corals, &c.—Cutter, Bloomsbury Street, London.
2. For skeletons, &c., Ed. Jerard, jun., College Place, Camden Town, London.
3. For Ceratodus, insects, &c.—Higgins, 22, Bloomsbury Street, London.
4. For molluscs, and various marine forms—R. Damon, Weymouth.
5. For American fish and amphibians—Prof. Henry Ward, Rochester, New York.
6. For skeletons of fish, &c., &c.—Erber, 7, Sigmundgasse, Vienna.
7. For skeletons and exotic specimens generally—Gustav Schneider, 67, Grenzackestrasse, Basel.
8. For exotic specimens generally—Museum Godeffroy, Hamburg.
9. For Mediterranean fish, molluscs, &c.—Gal, frères, 1, Maritime, Nice.
10. For glass models of invertebrates—Blaschka, 9, Schiesgasse, Dresden.
11. For wax models of anatomy of parasitic worms and of vertebrate anatomy and embryology—Weisker, 13, Thalstrasse, Leipzig.
12. For wax models of vertebrate and invertebrate embryology—Dr. Ziegler, Freiburg, Baden.
13. For live starfishes, Myaarenaria, Cyclopterus, and other forms—J. Thompson, 11, York Place, Southend, Essex.
14. For anemones, and channel marine fauna—R. T. Smith, 25, St. Alban's Street, Weymouth.

I trust that some of your correspondents will enlarge this list, and that such as it is it may be of use.

E. RAY LANKESTER

Ornithology of Costa Rica

IN NATURE, vol. xvi. p. 446, I see that you announce my return to this country. I take the liberty of rectifying two errors in the announcement:—1st. I was five months collecting in Costa Rica (not four months), from the end of December to the end of May. 2nd. I have brought home 250 species (not 200), and it may interest your readers to know that among these 250 species, besides the female of *Carpodectes nitidus*, are also some other very rare birds many of which—one or other of the sexes—are new to science. I add a list of some of them in case you may feel disposed to give it in your journal.

Odontophorus guttatus (Gould), *Dendrortyx leucophrys* (Gould), *Geotrygon costaricensis* (Lawr.), *Tonurus hoffmanni* (Cab.), *Tetragonops frantzii* (Sclat.), *Turdus nigrescens* (Cab), *Turdus obsoletus* (Lawr.), *Catharus frantzii* (Cab.), *Catharus gracilirostris* (Salv.), *Dendraeca vieilloti* (Cass.), *Setophaga torquata* (Baird), *Phainoptila melanoxantha* (Salv.), male and female (just described by Mr. Salvin, from a single specimen, sex unknown, sent by Mr. Rogers), *Chlorophonia callophrys* (Cab.), *Pezopetes capitalis* (Cab.), *Pyrgisoma cabanisi* (Sclat. and Salv.), *Pyrgisoma leucotis* (Cab.), *Eugenes spectabilis* (Lawr.), both sexes, *Oreopyra hemileuca* (Salv.), *Oreopyra cinereicauda* (Lawr.), *Selasphorus flammula* (Salv.), both sexes, *Panterpe insignis* (Cab.), ditto, and several new species belonging to the families *Fringillidæ*, *Trochilidæ*, and *Tyranidæ*, of which I am preparing a description, as well as a general list of all the species collected by me (with notes on many of them), for publication in the *Proceedings* of the Zoological Society.

A. BOUCARD

55, Great Russell Street, W.C.

On the Supposed Influence of Light on Combustion

BEFORE Dr. Ingleby referred to my experiments as "inconclusive," his reference should at least have been accurate. He says that I "actually used a dark cubbard into which there was no free influx of atmospheric air." So far from this being the case, the "dark closet," as I call it in my paper, was the photometer-room of Price's Candle Company, an enclosure $12\frac{1}{2}$ feet long, $3\frac{1}{2}$ feet wide, and $6\frac{1}{2}$ feet high, with arrangements for constant ventilation both at the bottom and at the top. So far from candles "naturally burning there with inferior combustion," as Dr. Ingleby supposes, it is in constant use for testing the burning of candles, and any deficiency in the supply of air would be shown quickly by the production of smoke, and yet after being so used for many hours there is not a trace of smoke in the air.

Dr. Ingleby's assumption that the candles burnt with inferior combustion in the closet is in direct opposition to the statement made in my paper. In the first and fourth trials there is a greater consumption in the light than in the dark; and in the second and third trials the consumption is greater in the dark than in the light; but in any case the difference is so small, amounting only to from two to seven grains per hour, that it may fairly be referred to slight differences in temperature, in currents of air, and in the composition and make of the candles—the *cæteris paribus* which Dr. Ingleby, with unnecessary emphasis, says I "left *entirely* out of the experiment."

The method adopted by me has the advantage of measuring the results by actual weighing, and I attach no importance to any opinion that is not founded on a similar basis. I cannot follow Dr. Ingleby's theory. What does "insidious eclipsing the waning glimmer of expiring embers," mean? I can understand that sea-coal—a caking coal—may form hard cakes, below which the fire burns out unless the cake is broken, an action which does not occur with non-caking coals such as a great part of the Staffordshire and Lancashire coals, but I cannot see why if the "last faint gleam is invisible" in consequence of a brighter light, therefore "the fire goes out as a matter of course." That the sun puts out the fire by rarefying the air necessary for combustion I take to be pure fiction.

In my experiments differences of temperature were slight. If there was any difference one would expect the temperature to be higher in the closet than in the open room, but in the fourth trial the temperature in the sunshine was the higher. If the candle material melts down more rapidly in consequence of increased temperature, then up to a certain point, at which guttering begins, the rate of consumption of the candle material will increase. But in these experiments there was no guttering and no smoking of the wick. C. TOMLINSON

Highgate, October 13

Selective Discrimination of Insects

THE remarks of Sir John Lubbock in a late lecture on the relation of insects and flowers leads to the inference that in his opinion the brilliancy of colour rather than the odour is the attraction. My observations lead me to suppose that it is not the colour, but the particular odour of each variety or species of flower which induces the visit. With great interest, not unmixed with curiosity, I have observed (my attention was at first casually excited) that bees particularly, and also butterflies, visit a distinct variety and for the time confine their attention to it, settling on and sucking the honey of that variety only; *e.g.*, a bee settling on a scarlet geranium will not go from it to another species or variety, but gives its attention to the particular variety only, irrespective of colour, whether scarlet, pink, or white, never going from a scarlet geranium to another scarlet flower, even if in contact. Whatever the species of flower, it is the same—pelargoniums, petunias, heliotropes, lilies, &c. The visit is from pelargonium to pelargonium, not from pelargonium to geranium (both cranes bills), and from lily to lily, irrespective of colour. I never remarked a bee go from a lily to an amaryllis, or the reverse. The object of this distinctive selection appears to be fertilisation. The indiscriminate admixture of the pollens of distinct varieties would probably frustrate the ends of nature and lead to monstrosities or barrenness. What would be the effect of the admixture on its own stores is a distinct question. So far as the insect is concerned, doubtless the fact has relation to its own economy. Whatever be the reason, there appears to be the harmonious adjustment of two facts under the relations of one law. If the colour, and not the odour, was the attraction, the visits would be indiscriminately made to all flowers of a brilliant hue. The observation of the lecturer as to flies being attracted by stinking plants or carrion seems to prove the fact suggested. Flies settle indiscriminately on all putrefactions, and will go immediately from a flower to offal or from offal to a flower. With bees and butterflies there is certainly a discriminative selection guided by odour; I have also remarked that some flowers are rarely, if ever, visited by bees.

I have never in the books I have read met with this observation, and when so acute and distinguished an observer as Sir John Lubbock passes over the circumstance, I presume either the fact has not been observed, or, if observed, has been considered to be inconsequential. The observation may be worth nothing, but in these days of *minute science*, when every infinitesimal variation is noticed and invested with importance, there may be a significance in the fact which escapes me, but which, with others, may have its value. So far as I know, the occurrence is invariable; being so, the inference is that odour, and not colour, is the attraction. I have called the attention of others to the occurrence, who have, watching the results, always come to the same conclusion as myself. S. B.

OUR ASTRONOMICAL COLUMN

THE NEBULA, MESSIER 8 (G.C. 4361).—Dr. Tempel draws attention to the different appearance presented by this large nebula at the present time from that depicted in Sir J. Herschel's drawing made at the Cape of Good Hope in 1836-37, which he considers can only be explained on the assumption of a shifting of the whole nebula with respect to the stars by reference to which it was delineated at the Cape, or by great changes in the nebula itself. The case will be worthy of attention, because it appears Sir J. Herschel's drawing was made with much care, as he says "every attention has been paid to exactness." The whole area occupied by the nebula, so far as he could trace its convolutions, is stated to be about one-fifth of a square degree. The relative positions of the stars in and near it, to the number of 186, were ascertained by differential observations with 9 Sagittarii; "from these measures skeleton charts were then constructed, and being divided into convenient triangles, the nebula was worked in upon them." A drawing made under these circumstances might certainly be expected to represent its actual features, and it appears to be given with confidence by Sir J. Herschel. Dr. Tempel, observing with the large Amici-telescope at Arcetri, near Florence, finds that the reference stars entered in the Cape drawing are still as they then were, with some insignificant variations of position or brightness; but the difference of the details of the nebula as projected on these stars, from those shown by Sir J. Herschel, are so marked as to leave, in Dr. Tempel's opinion, no other explanation than is suggested above. Prof. Schiaparelli, to whom Dr. Tempel had forwarded his own delineation of the nebula for comparison with that made at the Cape, remarks, after twice examining it:—"Je dirai tout-de-suite, qu'il m'aurait été impossible de reconnaître la nébuleuse avec le seul dessin de J. Herschel." The nebula is figured on Plate I. in the Cape observations; the description will be found at p. 14. Probably Mr. Ellery, who, as was stated last week, is still occupied with new drawings of Sir J. Herschel's figured nebulæ, may be able to express an authoritative opinion with respect to the supposed changes in this object.

THE BINARY STAR α CENTAURI.—It appears from the supplementary number of the *Monthly Notices* of the Royal Astronomical Society that this star has not been neglected during the critical portion of its orbit at the observatory at Sydney. Mr. H. C. Russell, the director, publishes measures taken in each of the years 1870-77, excepting 1875, when he was in Europe, and expresses his intention to observe it accurately during the next few months, that the true time of the periastre, &c., may be determined. The later measures indicate the necessity of a correction for bias, the observer getting sensibly differing angles of position according as the telescope was east or west of the pier—in which, by the way, he is not singular; the amount of the necessary correction was to be investigated. It is very satisfactory to find that a sufficient number of measures of this grand binary system, for obtaining pretty good elements of its motion, are likely to be put upon record at the present periastre. The next we know will not occur until the middle of the ensuing century.

JUPITER'S SATELLITES.—On October 8, M. Yvon Villarceau laid before the Paris Academy of Sciences a memoir, by M. Glasenapp, on the satellites of Jupiter which appears to have been forwarded in competition for the *Prix Damoiseau*, and which had been found amongst the papers of M. Leverrier, one of the commission to whom the adjudication of the prize had been referred. It is known that M. Glasenapp has been occupied for some time past at Pulkowa upon investigations connected with these bodies.

THE PRESENT COMETS.—The comet discovered by M. Coggia on September 14, though faint, is still well situated for observation in the morning sky. The following elements calculated by Herr E. Hartwig, from observations on September 14, 18, and October 6 have been received from Prof. Winnecke:—

Perihelion passage September 10·7566 M.T. at Berlin.

Longitude of perihelion	108°	10′	57″	M.Eq. 1877·0.
,, ,, ascending node ...	251	3	52	
Inclination	77	51	6	
Log. perihelion distance	0·197506			

Motion—retrograde.

Hence the following positions for Berlin midnight:—

	R.A. h. m.	N.P.D. ° ′	Distance from the earth.	Distance from the sun.
Oct. 19	7 39·8	54 3	1·234	1·662
,, 23	7 28·1	56 16	1·166	1·680
,, 27	7 14·9	58 48	1·102	1·699
,, 31	7 0·0	61 42	1·044	1·720
Nov. 4	6 43·5	65 3	0·994	1·743
,, 8	6 25·6	68 49	0·953	1·766

Of the comet discovered by Dr. Tempel at the Observatory of Arcetri, near Florence, on October 2, the following elements by Dr. Schur are also from Prof. Winnecke :—

Perihelion passage June 27·970 M.T. at Berlin.

Longitude of perihelion	... ...	83° 30′·0
,, ,, ascending node	... ...	184 17·8
Inclination	... ...	64 54·2
Log. perihelion distance	... ...	0·00994

Motion—retrograde.

On June 28 the comet was in R.A. 5h. 51m., N.P.D. 34°·4, distant from the earth 1·71; on August 1 in R.A. 4h. 47m., N.P.D. 38°·8, distance 1·35; and on September 3 in R.A. 2h. 36m., N.P.D. 55°·4, distance 0·79, so that an earlier discovery might have been expected.

The places subjoined are from these elements for 12h. G.M.T. :—

	R.A. h. m.	N.P.D. ° ′	Distance from the Earth.	Distance from the Sun.
Oct. 18	23 5·5	112 54	1·241	2·036
,, 20	23 2·0	113 48		
,, 22	22 58·9	114 37	1·350	2·084
,, 24	22 56·0	115 20		
,, 26	22 53·5	115 58	1·462	2·132
,, 28	22 51·3	116 31		
,, 30	22 49·4	117 2	1·577	2·180
Nov. 1	22 47·7	117 29		
,, 3	22 46·3	117 54	1·695	2·227

BIOLOGICAL NOTES

Boring Power of Magilus.—We have received from Mr. Charlesworth a preliminary note giving briefly a result of his study of the genus Magilus, the remarkable testaceous gasteropod that is found immersed in the large hemispherical corals of the genus Meandrina. The current belief, as set forth by Sowerby, Owen, Woodward, and other authorities in molluscan biology who have treated of this coral-inhabiting mollusc, is that Magilus in its young state effects a lodgment in a crevice of a Meandrina, and that as the coral enlarges, the Magilus extends the margins of the mouth of its shell in the form of a cylindrical corrugated tube, the growth of this tube and of the coral proceeding together *pari passu*, and consequently that there is no penetration of the coral by the Magilus at all. Mr. Charlesworth, however, finds that Magilus not only drives through solid masses of coral in any direction with apparently the same facility that the bivalve Teredo tunnels masses of wood, but he finds that it even surpasses Teredo in its power of suddenly reflecting its shell and returning to the point from which it commenced its advance; and this bending back of the shell upon itself is not accomplished in such natural cavities as frequently prevail in large corals of the Meandrina genus, but in the solid mass of the coral.

Great Vitality of Ants.—Several interesting observations have been made by the Rev. H. C. McCook on the endurance of extremes of heat and cold by ants. This year a formicary of *F. pennsylvanica* was cut from an oak bough and exposed out of doors to the rigour of a mountain winter, and survived. A number were dropped separately upon ice, and were found alive after forty-eight hours, each in a little depression. *F. rufa* was found active in its formicary at 34° F., sluggish at 30°. The extreme of heat seemed also to be endured by *F. pennsylvanica;* they did not suffer at all from the heat of stones walling in a camp fire, having been driven into this position out of a burning stump. A community of agricultural ants (*M. molefaciens*) lived in a mound upon which some smiths in Texas made their fires for heating waggon tires. Numbers of ants were seen at work by Dr. Lincecum, cleaning out the entrance to their city, before the entire extinction of the fire just used for heating tires. They had learnt all about the fire, and knew how to work in and around the dying embers without injury. A quantity of mason ants (variety of *F. rufa*) observed by Mr. McCook were accidentally flooded under five inches of water, and they appeared to be quite dead, and floated about in this condition for many hours. But subsequently most of them recovered full activity. In Texas Mr. Lincecum found that the agricultural ants are seen in great numbers in wells, forming a sort of floating mass as large as an orange, clinging together. In this condition they get drawn up in the bucket, and though they may have been in the water a day or two, they are all found alive. Yet individuals cannot survive under water more than six minutes; and life in these balls can only be preserved by the mass revolving, either by the continued struggles of the individual insects, or by an instinctive and orderly movement of the outer tier of ants (*Proc* Acad. Nat. Sci. Philadelphia, 1877, p. 134).

The Striped Mullet.—This fish, so abundant off the coast of North Carolina, seems to suffer from several serious drawbacks, which would appear to threaten its extinction. It moves through the water so slowly that a man may easily walk as fast. The young fry suffer from a disease which gradually destroys the sight, and great numbers perish; they are also much infested with parasitic worms. To counterbalance these destructive agencies, the female has an enormously distended roe.

The Mediterranean Flora.—From personal observations in Italy and Greece, with the aid of literature bearing on the subject, M. Fuchs comes to the conclusion that the so-called Mediterranean flora, so far as represented by evergreen woody plants, and plants of the sage, thyme, lavender, and rosemary order therewith always associated, occurs, at least in France, Italy, Greece, Southern Russia, and Northern Asia Minor, exclusively on calcareous formations, while soils with little or no lime (granite, gneiss, flysch, sandy and muddy alluvia of rivers) in the whole of that region, and south to Sicily and Morea, bear exclusively deciduous foliaceous trees, and in general, a vegetation hardly differing from the ordinary central European flora. We are not, however (M. Fuchs says), to conceive the phenomenon as if the former class of plants required the lime as nutriment; the correct view rather is, that the southern evergreen flora is better able to press northwards on the drier and warmer calcareous formation, than on the damper and colder clayey soil. And he finds support of this view in the fact that, in the Azores, Madeira, and the Canary Islands, with a truly subtropical climate, an evergreen shrub vegetation closely agreeing with the Mediterranean flora flourishes on various soils indifferently, even on basaltic and trachytic rocks. The same appears to be the case in Algiers.

FOX TALBOT

HAD the photographic art never been invented, Mr. W. H. Fox Talbot, whose death we recently recorded, would have a claim to take a good rank as a scientific investigator. In the popular estimation his work in connection with photography is what alone gives him a claim to remembrance; but we are sure there are many of our readers who must be familiar with writings by him in various departments of science. He was indeed in many respects a wonderful man, and a glance at the Royal Society Catalogue will show that he has left behind him a great amount of varied work. In mathematics, in physics, in chemistry, in astronomy, in botany, in archæology, in literature, Fox Talbot at various periods of his life did substantial work, and in addition filled faithfully and liberally the responsible position of an English country gentleman on his estate of Lacock Abbey, Wiltshire.

Fox Talbot was the eldest son of Mr. William Davenport Talbot, his mother being a daughter of the Earl of Ilchester. He was born in February, 1800, and received his early education at Harrow. Thence he went to Trinity College, Cambridge, where he gained the Porson Prize in 1820, was Chancellor's Gold Medallist, and graduated in 1821 as Twelfth Wrangler. Just after the passing of the first Reform Bill he sat for two years in Parliament as member for Chippenham, when he retired from public life, and devoted himself almost entirely to work in various departments of science and literature. In the Royal Society's Catalogue alone is a list of about fifty papers by him in various domains of science, and ranging from the year 1822 the year after his graduation, down to 1872. The first paper on the list is a mathematical one contributed to Gergonne's *Ann. Math.* (1822), "On the Properties of a certain Curve drawn from the Equilateral Hyperbola." In 1822-23 he contributed six mathematical papers to the same journal, one of them being "On a Curve the Arcs of which represent Legendre's Elliptic Functions of the first kind." He was the author of at least eight other mathematical papers contributed to the Royal Society, the *Phil. Trans.*, and the *Transactions* of the Royal Society of Edinburgh. Some of these papers are very remarkable, as those on Definite Integrals, and show Fox Talbot to have been a mathematician of no small power.

He seems to have commenced his researches on light at an early period. There is, for example, in the *Edinburgh Journal of Science*, for 1826, a paper describing "Some Experiments on Coloured Flames;" and in the *Quarterly Journal of Science*, for 1827, one "On Monochromatic Light." Other papers in the same direction appear in the *Phil. Mag.*, for 1833, "On a Method of Obtaining Homogeneous Light of Great Intensity," "Experiments on Light," 1834, "On the Nature of Light," 1835 In 1861 he published in the *Chemical News* papers on "Early Researches on the Spectra of Artificial Light from Different Sources," and "Some Experiments on Coloured Flames;" and so late as 1872, we find in the *Proceedings* of the Royal Society of Edinburgh, "Notes on Some Anomalous Spectra," "On the Early History of Spectrum Analysis," and "On a New Mode of Observing Certain Spectra."

In chemistry, as might be expected, his researches were many, being mainly connected, however, with photography. One of his earliest chemical papers will be found in the *Phil. Mag.* ii. 1833: "Remarks on Chemical Changes of Colour." We find other papers contributed mainly to the *Phil. Mag.* on Nitre, Iodide of Silver, Iodide of Mercury, &c.

In January, 1839, Daguerre published his account of his process. On the 31st of the same month Fox Talbot gave an account of his own process to the Royal Society, in a paper entitled "Some Account of the Art of Photogenic Drawing, or the process by which Natural Objects may be made to delineate themselves without the aid of the artist's pencil" (*Roy. Soc. Proc.* 1839; *Phil. Mag.* xiv. 1839); and at the meeting of the British Association that year he read a paper on the subject. From that time onwards he continued to write papers in connection with his invention, though for several years before his death he seems to have lost his interest in the subject, and turned his versatile intellect to other lines of inquiry.

The original photogenic drawing is nothing more nor less than the silver printing process of the present day, which has received little or no modifications since it passed out of his hands, unless it be the application of albumen to the paper and the fixing with sodium hyposulphite. Early in 1840 a new process due to Talbot created a sensation in scientific circles, the results being a marked advance on everything that up to that time had been produced. This was no other than the Calotype or "beautiful picture" process, a patent for which he took out dated 1841. The main features of this process may be described as the production of a photographic picture on sensitised silver-iodide, held *in situ* in the pores of paper, and its development by means of gallic acid. The credit of the discovery of this method of development has often been ascribed to Fox Talbot; but we believe that to the Rev. B. J. Reade it is really due, but was so modified by Fox Talbot as to render it manageable in the hands of the operator. The next patent that Fox Talbot took out was registered under the title of "Improvements in Calotype," in which, amongst other things, he included fixing the photographic image on the paper by means of sodium hyposulphite, a solvent for the haloid salts of silver which Sir John Herschel had used in February, 1840.

The third patent taken out by Talbot, in conjunction with Malone, was for the use of unglazed porcelain in lieu of glass, on which to support the photographic image, using an albumen process. In this patent also we have a protection granted for an invention which has several times since been rediscovered, viz., the use of a transparent and flexible support in lieu of glass capable of being adapted to a curved surface, by which means a panoramic view might be taken in the camera by the gradual rotation of the lens round its optical centre. This flexible support was paper rendered transparent and non-absorbent of the liquid albumen applied to its surface. The last novelty included consisted of an application of photography to the production of an image on steel plates, doubtless with a view of helping the engraver.

The fourth patent was for a process (described in the *Athenæum*, December 6, 1851) by which instantaneous pictures could be taken, and was so sensitive that an experiment undertaken at the Royal Institution to prove its value is worthy of redescription. Printed matter was fixed on a wheel which was caused to revolve at a rapid rate, and being illuminated by the spark from a battery of Leyden jars, a facsimile of it was produced in the camera, "every letter being perfectly distinct." We doubt if at the present day any greater degree of instantaneity could be secured even by the most rapid collodion processes extant. The success of the process was due to the extreme sensitiveness of silver iodide when prepared by double decomposition of the iron salt, and also to the great facility with which silver nitrate could be reduced by ferrous sulphate. The debt he owed to Dr. Woods, of Parsonstown, and to Robert Hunt, who respectively discovered these facts, Talbot duly acknowledged in his communication to the *Athenæum*.

The last patented invention in photography with which Fox Talbot's name is connected was that of photographic engraving. This process is based on the discovery by Poitivin, of the possibility, by exposure to light, of forming an image in gelatine when impregnated with bichromate of potassium. The steel-plate on which the etching was to be engraved was covered with a dried layer of thin chromated gelatine, and after exposure in the camera the plate was placed in cold water to remove part of the gelatine and as much of the bichromate as possible. It was then covered with the etching fluid which penetrated in a greater or less degree through the gelatine film and the "biting-in" thus effected enabled the plate when inked up and printed in the usual manner to give an impression on paper of the object photographed. This method was most successful in the reproduction of line engravings, and when half tones had to be produced he adopted other artifices to which we need not here refer.

It has been stated that Fox Talbot did not protect his processes, but the above list of patents at once contradicts the assertion. Not only did he—as we think quite justifiably—do so, but he strictly claimed his rights, even going so far as to bring an unsuccessful action for infringement, claiming to include in his Calotype patent—which was essentially a paper process—the collodion process of Le Gray and Archer. Mr. P. Le Neve Foster writes to us that Fox Talbot was so tenacious of his rights that the formation of the Photographic Society was for a time prevented. "I had," Mr. Foster writes, "more than one conversation with him at that time on the subject, and he only yielded, and in favour of amateurs, after much solicitation on the part of the late Lord Rosse and Sir Charles Eastlake, who thereupon became the first president of the Photographic Society."

The accompanying extract from the correspondence which appeared in the *Times* of August 13, 1852, between the inventor of the Calotype process and the presidents of the Royal Society and Royal Academy, shows the spirit in which the two latter approached the subject of the patent rights, and the generous tone in which the former responded:—

"The art of photography on paper," Lord Rosse and Sir Charles Eastlake write, "of which you are the inventor, has arrived at such a degree of perfection that it must soon become of national importance; and we are anxious that, as the art itself originated in England, it should also receive its further perfection and development in this country. At present, however, although England continues to take the lead in some branches of the art, yet in others the French are unquestionably making more rapid progress than we are. It is very desirable that we should not be left behind by the nations of the Continent in the improvement and development of a purely British invention; and, as you are the possessor of a patent right in this invention, which will continue for some years, and which may, perhaps, be renewed, we beg to call your attention to the subject, and to inquire whether it may not be possible for you, by making some alteration in the exercise of your patent rights, to obviate most of the difficulties which now appear to hinder the progress of art in England. Many of the finest applications of the invention will probably require the cooperation of men of science and skilful artists. But it is evident that the more freely they can use the resources of the art, the more probable it is their efforts will be attended with eminent success. As we feel no doubt that some such judicious alteration would give great satisfaction, and be the means of rapidly improving this beautiful art, we beg to make this friendly communication to you in the full confidence that you will receive it in the same spirit—the improvement of art and science being our common object."

This letter is dated "London, July," and Fox Talbot replied as follows, under date "Lacock Abbey, July 30":—

". . . I am as desirous as any one of the lovers of science and art, whose wishes you have kindly undertaken to represent, that our country should continue to take the lead in this newly-discovered branch of the fine arts; and, after much consideration, I think that the best thing I can do, and the most likely to stimulate to further improvements in photography, will be to invite the emulation and competition of our artists and amateurs by relaxing the patent right which I possess in this invention. I therefore beg to reply to your kind letter by offering the patent (with the exception of a single point hereafter mentioned) as a free present to the public, together with my other patents, for improvements in the same art. . . . The exception to which I refer, and which I am desirous of keeping in the hands of my own licensees, is the application of the invention to photograph taking for sale to the public. This is a branch of the art which must necessarily be in comparatively few hands. . . . With this exception, then, I present my invention to the country, and trust that it may realise our hopes of its future utility."

In the *Phil. Mag.* iii. 1833 will be found a very curious paper, which might interest Sir Wm. Thomson (who, however, has probably read it), "On the Velocity of Electricity; a proposed method of ascertaining the greatest depth of the ocean." Crystallography and optics came in for a considerable share of Talbot's attention. In 1836, in the *Comptes Rendus*, we find him describing researches on borax crystals, and besides various papers on the subject mentioned produced in 1836, he gave the Bakerian lecture of that year, the subject being "Facts relating to the Optical Phenomena of Crystals." In 1842 he read a paper at the British Association "On the Improvement of the Telescope," and another in 1847 "On a New Principle of Crystallisation." He describes in the Astronomical Society's *Memoirs* (xxi.) a total eclipse of the sun, July 28, 1851, observed at Marienburg, Prussia, and in the British Association Report for 1871 will be found a paper by him "On a New Method of estimating the Distances of some of the Fixed Stars."

The subject of heat also had its attractions for his many-sided mind, and in 1836 he contributed to the *Phil. Mag.* papers on the Repulsive Power of Heat and on Radiant Heat. Even botany received a share of his attention, for we find in the *Transactions* of the Edinburgh Botanical Society for 1868 a "Note on *Vellozia elegans* from the Cape of Good Hope."

But the half is not told, and it would take up more space than we can spare, even were it quite appropriate in these pages, to refer to his numerous contributions in literature and archæology to the Royal Society of Literature (of which he was vice-president), the Society of Biblical Archæology, and by other methods. Orientalists will call to mind that Talbot was one of the first who, with Sir Henry Rawlinson and Dr. Hincks, deciphered the cuneiform inscriptions brought from Nineveh. He was the author of several books of much interest and learning, and in his "Pencil of Nature," a fine quarto published in 1844, and probably the first work illustrated by photographs, he describes the origin and progress of the conception which culminated in his invention.

THE PHOTOGRAPHIC EXHIBITION

THE Photographic Exhibition which is now open at 5A, Pall Mall East, is well worthy of a visit by all lovers of the art-science, exemplifying as it does the progress that has been made in dry-plate processes. The perfecting of these processes must have a marked effect on the future of photography, as when they are capable of being employed under all circumstances, the heavy paraphernalia attendant on the wet process may be consigned to the lumber-room, and the worker in the field or laboratory need only be dependent on his box of sensitive plates and his camera. We cannot enumerate all the processes, examples of which are exhibited. We may mention, however, that the simple bromide of silver emulsion either held on the plate embedded in collodion or gelatine appears to bear away the palm for excellence, unless it be the process with which Mr. England has produced his splendid collection of Swiss views, in which (though no information is given in the catalogue regarding it), we think we can trace the delicacy due to albumen in the sensitive film, combined probably in some way or another with bromide of silver. Another feature of the exhibition are the enlargements which are shown by various exhibitors, amongst whom we may name, as being specially worthy of mention, the Woodbury Company, the Royal Engineers, and the Autotype Company. The enlargements taken by Mr. E. Viles with the microscope are also worthy of more than a passing remark. They are all beautifully executed, but perhaps the picture of the proboscis of the common blow-fly should be specially singled out, being almost perfectly enlarged to 200 diameters. We believe that a comparatively low-power objective was employed, and that from the small negative obtained by it an enlargement in Monckhoven's solar camera was produced. These pictures are hung too high to be well seen, and Mr. Viles perhaps might be persuaded to show them at some of this season's scientific *soirées*. As regards the application of photography to scientific purposes there are no other examples to be found in the exhibition, a matter which we deeply regret, seeing the large use that is made of the art-science in nearly every investigation of the present day. As regards the artistic element present, it is not in our province to dwell upon it. In many examples of portraiture it would have been well had that abomination — retouching of the negative—been avoided. As showing what a grand pencil is sunlight to the artist, we may mention the exhibits of Robinson, Blanchard, Mrs. H. Roscoe, and Slingsby, in all of which are to be found true artistic feeling and perfect manipulation. The works of Payne-Jennings, Bowness, the Royal Engineers, Stephen Thompson, and England may be classed amongst the best of the landscape work.

Amongst technical work we have examples of a capital photo-relief process by Warnerke, by which an artist's own drawing can be faithfully reproduced as a block for surface printing. The mechanical printing processes from gelatine are also admirably represented by the Autotype Company, as is that known as Woodbury-type.

This notice would be incomplete without calling attention to the photographs taken during the recent Arctic expedition under Sir G. Nares, which have been exhibited by the Admiralty, and also to those taken by Mr. Grant, who accompanied Sir Allan Young in the *Pandora*. Both sets of photographs are very good when the difficulties under which they were taken are considered.

THE NORWEGIAN DEEP-SEA EXPEDITION[1]

THE *Vöringin* left Tromsö on July 14, lay the following day, which was a Sunday, in Kjosen, by Lyngen, and we recommenced our work on the 16th, off Fuglö (71° N. lat.). From this point a cross-section was made to lat $71\frac{1}{2}°$, long. 14° E., the bottom reaching nowhere more than 900 fathoms. On the 18th we steered southwards, and took up another cross-section parallel to the above, and about twelve geographical miles distant. This was finished on the 20th, and we sailed to Tromsö, where we arrived at midnight. In the last cross-section

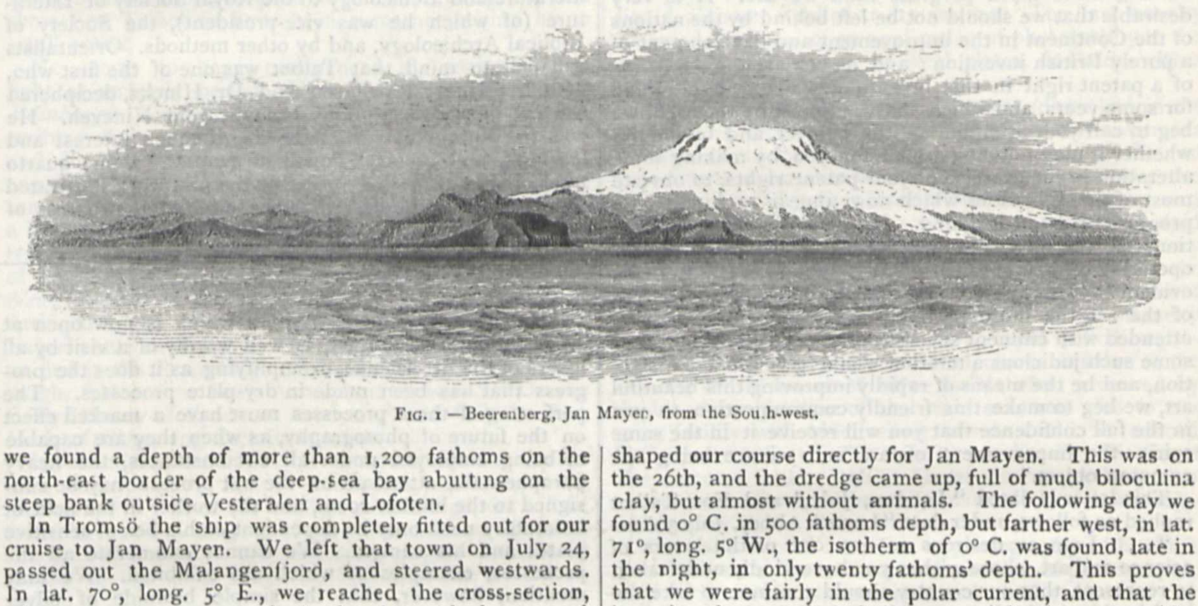


FIG. 1.—Beerenberg, Jan Mayen, from the South-west.

we found a depth of more than 1,200 fathoms on the north-east border of the deep-sea bay abutting on the steep bank outside Vesteralen and Lofoten.

In Tromsö the ship was completely fitted out for our cruise to Jan Mayen. We left that town on July 24, passed out the Malangenfjord, and steered westwards. In lat. 70°, long. 5° E., we reached the cross-section, whose eastern part we had already worked out, and shaped our course directly for Jan Mayen. This was on the 26th, and the dredge came up, full of mud, biloculina clay, but almost without animals. The following day we found 0° C. in 500 fathoms depth, but farther west, in lat. 71°, long. 5° W., the isotherm of 0° C. was found, late in the night, in only twenty fathoms' depth. This proves that we were fairly in the polar current, and that the boundary between it and the warm Atlantic current (the

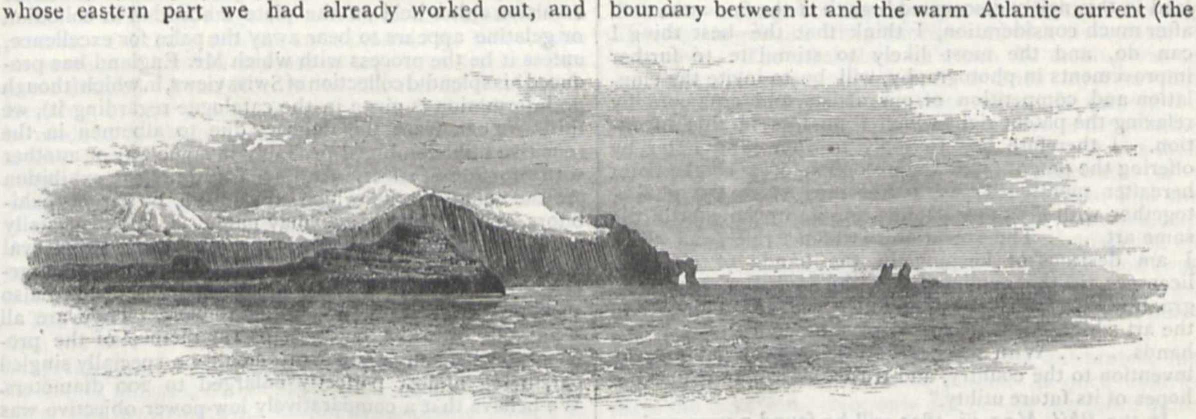


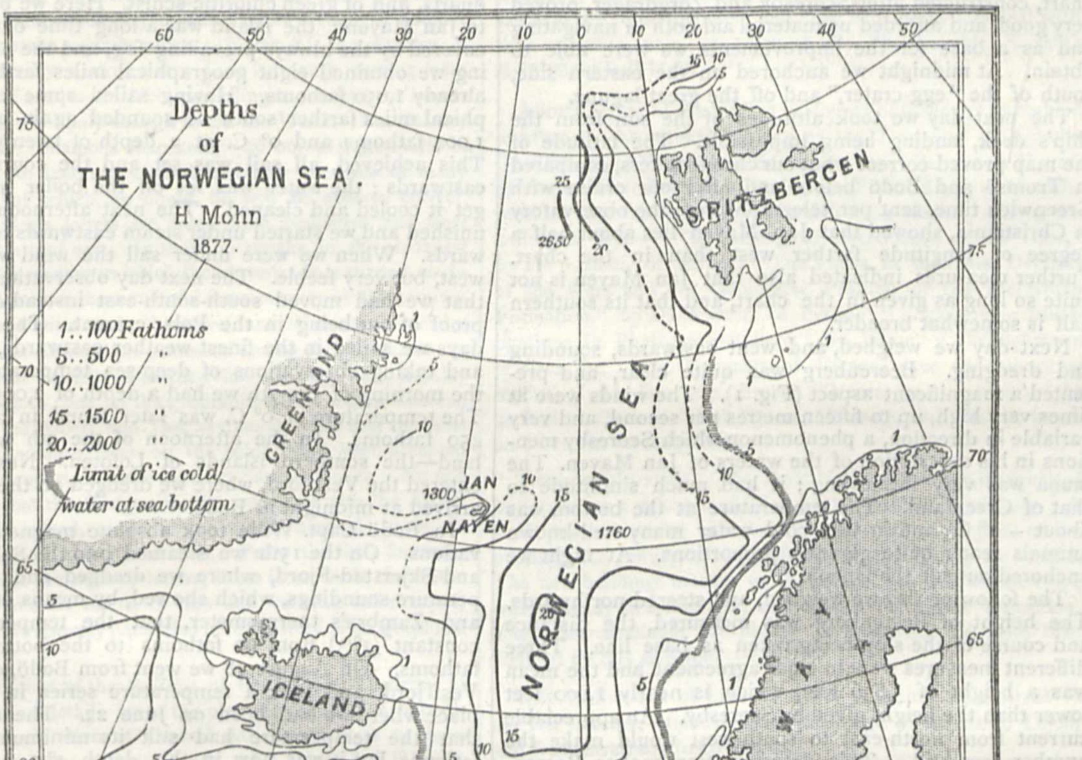
FIG. 2.—Jan Mayen—South-west Cape, and the Seven Rocks.

so-called Gulf Stream) is a very steep surface, like that of the "cold wall" on the American coast. The temperature of the surface of the sea was here 4°·6 C. At night the fog came on, and the next day we steered cautiously westwards, sounding at short intervals; but the depths went on increasing to more than 1,000 fathoms, before a less depth was sounded. At last, just when we were sitting at the dinner-table, we heard the mate cry out, "I can see the glacier ahead." The ship was immediately stopped, and a sounding gave 140 fathoms. The fog began to ascend a little, and we were soon able to dis-

[1] See NATURE, vol. xvi. p. 271.

tinguish a huge glacier, hanging on the steep rock, and bathing its foot in the sea. In order to ascertain the distance from the shore we fired a gun, and the observation of the echo gave the distance a little less than one nautical mile. By-and-by the fog lifted higher, and we could see the two terminating points of the eastern side of the base of the great Beerenberg. In the afternoon we started anew in order to round the island and find shelter on the west side, the wind and sea coming from the north-east. But as soon as we were on a northerly course, abreast of the north point of Jan Mayen, the fog again grew so thick that we could only see a couple of ships' lengths ahead. We steered then first northwards, then westwards, southwards, and south-east, the temperature of the sea surface being taken every quarter-of-an-hour to keep us informed of the proximity of the ice. The surface temperature went down to 2°·3 once, but kept generally at 3°·5, or more. At last when we were approaching the west coast and had commenced sounding to find a suitable anchorage, the fog lifted so that we could see the shore and allowed us to choose our berth. At midnight we dropped the anchor in the northernmost of the three large bays of the west side, about half-a-mile from the nearest shore.

The next day was wonderfully calm, but the fog covered the higher parts of Jan Mayen. We went on



shore, the sea being so calm that we could step on shore without any inconvenience. The shore consisted of volcanic sand, quite black, and was, higher up, covered by driftwood thickly strewn on a level surface. To the left was a steep cliff wonderfully rich in colour, the abode of thousands of sea-birds, whose inner slope, consisting of ash and scoria, showed it to be a part of a former crater. The scientific party spread in different directions and made the best of the time in surveying, collecting specimens of plants and rocks, and drawing; one polar fox was killed. The plants found belong to very few species, the vivid green we had seen from the ship being only a cover of moss. The flowers had just come out, and all the lower part of the island, up to about 2,500 feet, was generally free from snow, the snow lying only in patches in the lower regions where a larger mass was gathered in ravines. The rocks were all volcanic, and the peaks seemed all to be built of loose stones thrown out from craters, while solid lava and tufa were found in the lower parts. In the afternoon I went on shore again, happily, because this was the last time we were able to do so, the sea being on the following days too high, and Jan Mayen does not present any bay giving shelter for a boat. I directed my course northwards, found a lagoon filled with fresh water, and shut off from the sea by a low wall some 300 feet in breadth.

The wall was covered by driftwood. On the east side of the island there is a similar lagoon, but much longer and with brackish water. On my return to the boat I shot a fox which came suddenly upon me and showed the greatest curiosity at seeing a human being.

The next day the wind and sea had risen, and landing was impossible. We weighed, therefore, and sailed again round the north part of the island. The clouds allowed us to see the lower part of the land, and for a while Beerenberg, the huge volcano of Jan Mayen, showed its snow-white cone to our enchanted eyes. The captain, assisted by his officers and myself, surveyed the coast as far as circumstances permitted, and got abundant material for an improved map of Jan Mayen. The Admiralty chart, constructed after Scoresby and Zorgdrager, proved very good, and afforded us material aid both in navigating and as a base for the improvements we were able to obtain. At midnight we anchored on the eastern side, south of the "egg-crater," and off the great lagune.

The next day we took altitudes of the sun from the ship's deck, landing being impossible. The latitude of the map proved correct; but our chronometers, compared in Tromsö and Bodö before and after our cruise with Greenwich time, sent per telegraph from the observatory in Christiania, showed that Jan Mayen lies about half a degree of longitude farther west than in the chart. Further measures indicated also that Jan Mayen is not quite so long as given in the chart, and that its southern half is somewhat broader.

Next day we weighed, and went eastwards, sounding and dredging. Beerenberg was quite clear, and presented a magnificent aspect (Fig. 1). The winds were at times very high, up to fifteen metres per second, and very variable in direction, a phenomenon which Scoresby mentions in his description of the waters of Jan Mayen. The fauna was very interesting; it had much similitude to that of Greenland. The temperature at the bottom was about $-1°$ C., and in this cold water many well-known animals reach quite gigantic proportions. At night we anchored outside the lagoon.

The following day we weighed, and steered northwards. The height of Beerenberg was measured, the distance and course of the ship being taken as base line. Three different measures were in close agreement, and the mean was a height of 5,836 feet, which is nearly 1,000 feet lower than the height given by Scoresby. An appreciable current from north-east to south-west would make the number less still. This determination makes Beerenberg lower than the highest point in Iceland, viz., Oeroefa-Jökull, which is more than 6,000 feet high. We went to the north of the island, and found 1,000 fathoms in a distance of only one geographical mile from the north point, which indicates that the foot of Beerenberg continues its slope of 10° so far beneath the surface of the sea. Thence we steered westwards and took soundings, which were continued the whole night. The wind was north-west, and the temperature of the air fell to $+0°\cdot2$ C.

A depth of 1,032 fathoms having been found seven geographical miles north-west of Jan Mayen, and a serial sounding having shown 0° C. in a depth of only ten fathoms—surface water being as low as $+2°$ C., we steered again towards the west coast of Jan Mayen. On the morning of August 8 we were near our first anchorage, but the swell was too heavy to try to land. We then went south-westwards along the shore and studied the country as well as circumstances permitted, the fog sometimes hiding it from our view. We were nevertheless able to get a series of sketches, and our rate told us that the island, as mentioned above, must be shorter than it is shown on the map. At noon we passed Cape Southwest and got a fine view of it (Fig. 2). The point north of Cape Southwest showed two extremely regularly built volcanic cones, the outer one close at the sea, the inner one quite small, and both of a reddish tint. On the higher land between Cape Southwest and this point there is also a similar larger cone. The Cape itself is perforated by a tunnel at the level of the sea.

I got the distinct impression that Jan Mayen is from end to end of comparatively recent volcanic origin. Its aspect reminded me of parts of Iceland, which are of more recent volcanic origin, *e.g.* the peninsula of Reykjanes. It had nothing in common with the doleritic formation of the Färö Islands. In the afternoon a sounding was taken and the dredge lowered some five geographical miles south-west of Jan Mayen. The depth was only 263 fathoms. The dredge brought up lots of stones and a rich fauna. The stones were mainly volcanic, but among them I found a piece of granite, one of quartz, and of green chloritic schist. Here we bade adieu to Jan Mayen; the island was a long time out of sight, covered by the always prevailing fog, and the next morning we obtained eight geographical miles farther south, already 1,050 fathoms. Having sailed some ten geographical miles farther south we sounded again and found 1,004 fathoms and 0° C. at a depth of twenty fathoms. This achieved, all sail was set and the course shaped eastwards; the water was let off the boiler in order to get it cooled and cleaned. The next afternoon this was finished and we started under steam eastwards and homewards. When we were under sail the wind was north-west, but very feeble. The next day observations showed that we had moved south-south-east instead of east—a proof of our being in the Polar current. The following days we sailed in the finest weather eastwards, sounding and taking observations of deep-sea temperatures. On the morning of the 7th we had a depth of 2,005 fathoms. The temperature of 0° C. was later found in a depth of 450 fathoms. In the afternoon of the 9th we sighted land—the southern islands of Lofoten. Next day we entered the Vestfjord, where we dredged on the 11th, and arrived at midnight at Bodö.

In Bodö Capt. Wille took absolute magnetical observations. On the 15th we steamed into the Salton-Fjord and Skjerstad-Fjord, where we dredged and took temperature soundings, which showed, by means of Negretti and Zambra's thermometer, that the temperature was constant ($3°\cdot3$) from 90 fathoms to the bottom in 270 fathoms. On August 18 we went from Bodö, out in the Vestfjord, and took a temperature series in the same place where we had been on June 22. The result was that the temperature had still its minimum in sixty fathoms, but it was now in this depth $4°\cdot7$, and at the bottom, in 140 fathoms, $5°\cdot8$. The whole mass of the lower water had consequently had its temperature raised as much as 1° C. The cause of this singular distribution of temperature so late in summer is inexplicable to me. This was our last operation this year. The course was shaped southwards, and on August 23 we arrived at Bergen, where the ship was paid off and the members of the scientific staff returned to their homes.

The expedition has this year been favoured with the best weather. Nothing has prevented the work from being carried on day and night, and the results obtained are therefore very extensive compared with last year.

The accompanying small chart shows in broad features the results of the soundings, combined with those of the Swedish expeditions to Spitzbergen and those of the *Bulldog*, *Porcupine*, and *Valorous* expeditions. The shaded line shows the boundary line at the sea-bottom between the ice-cold water of the Polar Sea and the warmer water of the Atlantic, as far as our observations hitherto have determined it.

Next year the expedition will work up the region between North Cape, Jan Mayen, and the north of Spitzbergen, and possibly make a trip eastwards in the direction of Novaya Semlya, in order to determine the site of the isothermal line of 0° C. at the sea-bottom, which may be regarded as the limiting line for the wanderings of the masses of cod, the object of the great winter and spring fisheries of Northern Norway. For this expedition the Norwegian Storthing has already voted the necessary sum of money. H. MOHN

NOTES

THE communications from Mr. Stanley in the *Telegraph* of Thursday and Monday last, though containing few positive additions to our knowledge, are full of interest; the episode on the arrival of the starved and wretched party at Ni Sanda is quite thrilling. Notwithstanding the number of cataracts and rapids on the Lualaba—Congo, Stanley maintains it is well fitted to become a great commercial highway—2,000 miles of uninterrupted water communication, opening up an extent of country embracing 600,000 square miles. North of the equator it receives a tributary 2,000 yards wide at its mouth, coming from a little north of east, and which, according to our present imperfect knowledge, is likely enough to be the Welle. Mr. Stanley speaks of the "infamous inaccuracy" of our present charts of West Africa, an inaccuracy which cost him the lives of many of his men, but which, no doubt, he will be able to correct. Three of Stanley's letters are dated from Nyangwe, and were written about a year ago. In them he speaks in the strongest language of the manner in which the slave-trade is carried on in that region, describes the wonderful forest scenery of the country between Tanganyika and Nyangwe, and gives some tender reminiscences of Livingstone preserved among the people, among whom the great traveller sojourned for so long. Mr. Stanley also endeavours to clear up the geography of the region between the Victoria, the Albert, and Tanganyika, showing that the most erroneous and confused ideas on the subject had been accepted mainly on the reports of natives to Sir Samuel Baker. No one now believes that the Tanganyika is connected with the Albert Nyanza, and, indeed, as Stanley suspects himself, he is, in refuting this notion, slaying the slain. From the little foretaste given us in these preliminary letters, there is no doubt that there is a rich feast in store for us of new and valuable information, and of adventure scarcely paralleled in the history of geographical exploration.

THE last number of the *Bulletin* of the Belgian Academy of Sciences contains details as to the plans of the Belgian expedition for the exploration of Central Africa, which is to leave Europe in the course of this month. Dr. Maes, of Hasselt, will accompany the expedition as surgeon and naturalist. The first Belgian station in Central Africa will be placed under the arrangement of Capt. Crespel, with whom Lieut. Cambier and Dr. Maes will be joined. The travellers will start for Zanzibar, and thence reach Lake Tanganyika, where it will be definitely settled whether a station be founded on the shores of the lake, or, a simple depôt being left there, the station be fixed at Nyangwe, or elsewhere in Manyuema. The Tanganyika, or Manyuema, or Unyamwesi will become a basis for further scientific exploration; and agriculture will be carried on on the spot for the purpose of enabling the expedition to exist on its own resources.

WE would draw the attention of those of our readers who are interested in the matter to the announcement in our advertising columns with reference to the next distribution of the Government grant of 4,000*l*. Applications should be forwarded to the secretaries of the Royal Society before December 31.

VOL. VII. of the Royal Society's Catalogue of Scientific papers will be out in a few days.

THE *Gardener's Chronicle* hears that Signor Beccari is likely to succeed Prof. Parlatore as Director of the Herbarium and Botanic Garden at Florence, if arrangements can be made for some other Professor to undertake the duties of lecturing.

THE death is announced, on September 30, at the age of sixty-five, of Major-General Eardley-Wilmot, F.R.S., formerly chairman of the Council of the Society of Arts. At one time he was Director of Gun Factories at Woolwich, served on many Government committees on military matters, and was frequently consulted on scientific and educational subjects connected with the army.

THE Lords of the Admiralty have ordered that sets of the photographs taken during the Arctic Expedition of 1875-76 shall be presented to the British Museum, the South Kensington Museum, the United Service Institution, the Royal Artillery Institution at Woolwich, the Royal Engineer Institution at Chatham, and other Government or official institutions. Fifty sets only are to be prepared, and they will all be identical with the collection now on view at the Photographic Society's Exhibition, Pall Mall.

ABOUT eighty of the leading geologists of Germany assembled together in the annual meeting of the Deutsche geologische Gesellschaft, at Vienna, on September 27. Baron von Hauer, of Vienna, Herr Beyrich, of Berlin, and Prof. Gümbl, of Munich, presided over the three sessions which took place. Among the addresses were—"The Geological Constitution of the Harz," by Dr. Lossen, of Berlin; "The Fauna in the Older Deposits of the Harz and the Geological Position of the Hercynian Formation," by Dr. Kayser, of Berlin; "Phylogenetic Investigations in Phyto-paleontology," by Baron v. Ettinghausen of Graz, &c. Prof. Neumayr, of Vienna, gave an interesting report of his late trip through Greece, and exhibited the geological chart of North Greece, Thessaly, and Chalcis, based on his recent investigations.

THE administration of the Paris National Library inaugurated last Saturday a valuable addition to its internal machinery. A small pneumatic tube has been constructed to all parts of the building for conveying notes from readers asking for books. The new buildings erected on the site of the old lecture-room will be ready in a fortnight, and opened for public inspection. The space available for library purposes will be more than doubled by this addition.

THE earthquake of Monday week, to which we referred in our last number, extended from the Lago di Garda to Dijon, and from Strasburg to Grénoble.

THE French Society of Hygiene has just held its first monthly meeting at the Hôtel de la Société d'Encouragement, under the presidency of M. Chevalier, the eminent hygienist. M. Pietra Santa, the secretary, announced that the number of registered members of the new institution, modelled on the English pattern, amounted to more than 300. A letter from the Sanitary Institute announced that the Société d'Hygiène had taken a diploma of honour at the Leamington Exhibition.

THE Manchester Scientific Students' Association commenced its winter session yesterday, when a paper was read by Mr. Thomas Harrison, F.C.S., on "The Unity of the Senses," with experiments. Other papers to be read are by Mr. J. Plant, F.G.S., on "Silica;" Mr. William Gee, on "Telephones;" Mr. M. Stirrup, F.G.S., Notes on Auvergne—Puy-de-Dôme—Extinct Volcanoes; Mr. E. P. Quin, on "Vertebrate and Invertebrate Animals;" Mr. Robt. E. Holding, on a visit to the Zoological Society's Gardens, London; giving a description of some remarkable Animals and Birds—illustrated by diagrams from life; Mr. Geo. C. Yates, F.S.A., on "A Ramble amongst the Dolmens of the Morbihan."

THE annual *Conversazione* of the Whitehaven Scientific Association took place at the Town Hall of that town on October 9, when the president, Mr. R. Russell, C.E., F.G.S., delivered the annual address before a large gathering. A practical exhibition of the telephone as well as an extensive display of late scientific inventions and objects illustrative of the natural history of the district, rendered the entertainment pleasant and profitable. The programme of the session for the next six months offers an attractive list of lectures, including a series of six from the president on Geology; six from Mr. A. Kitchin, F.G.S., on Light and Spectrum Analysis, and single lectures by fourteen other gentlemen. Among the titles we notice The Chalk, its Origin, Characteristics, and Scenery; How an Animal is Built Up: Flowers, their Shapes, Perfumes, and Colours; &c., &c. The Association is in a flourishing financial condition, owning a house of its own, and is not only popular but succeeds in infusing a healthful love for science into the district about. A library, a museum, and frequent field-days in the picturesque and geologically interesting neighbourhood, evidence the activity of the Society.

THE European Bureau of Longitude held its annual conference at Stuttgart, September 27, General Ibannez, of Spain, presiding. Representatives were in attendance from Austria, Bavaria, Belgium, France, Hesse, Italy, Norway, Prussia, Saxony, Switzerland, Spain, and Würtemberg. Gen. Baeyer, of Prussia, was elected president for the coming year.

AT the meeting of the German Anthropological Society at Constance on September 24, Prof. von Virchow described the results obtained by him in his researches on the colour of eyes, hair, and skin of German school-children, and to which we have already referred. He examined no less than 2,114,153 children. In the whole of North Germany the fair, blue-eyed type with light skin is prevalent. In Mid-Germany the darker individuals become more numerous and reach their maximum frequency at the south-west and south-east corners. The passages from one type to another in a geographical sense are perfectly gradual. Upper Bavaria and Alsace are the extremes, between wichh the fairer type reaches southwards like a wedge.

AT the same meeting Dr. Gross, of Neuveville, on the Lake of Biel (Switzerland), exhibited a number of objects dating from the lake dwellings of the earlier stone period, amongst which some hatchets made of nephrite, a mineral now only found in China, were of special interest. Prof. Desor expressed his opinion that these relics were originally brought from Asia by the lake inhabitants; he believed it quite possible that they may have carried their valuables with them, and this hypothesis would explain the rarity of nephrite hatchets. A keen discussion was raised with regard to the discoveries in the Thayingen cave. It will be remembered that Herren Merk and Messikommer had found several bones from the prehistoric reindeer upon which drawings of animals were carved, besides a rough piece of sculpture representing the head of a musk buffalo. These objects are now in the Rosgarten collection, and many naturalists had believed them to be mere imitations. The result of the discussion, proved them to be perfectly genuine. This, however, is not the case with other pieces sent to France and England, and said to have been found at the same place.

A SPECIAL division of the Paris International Exhibition will be devoted to electricity, so that all the systems of electric lighting may be tested comparatively. The electric light continues to create the greatest interest in Paris. The experiments which we mentioned some time ago have been conducted during forty consecutive days at the Lyons railway station. A force of about 40 horse-power is sufficient to keep going twenty-eight electric lamps, each of which gives a light equal to eighty gas lamps, and works with regularity for ten and a-half hours. The effect is splendid, the whole of the station, except the waiting-room, being lighted *à giorno*. The question of economy, however, is not yet settled. It is not known whether the company will agree to pay a somewhat higher price in order to multiply the power of its illumination. These experiments have been tried on Lontain's system, a modification of Wilde's and Siemens' principle. M. Lontain has contrived to send the current generated by an ordinary Wilde's machine into an electro-magnetic engine called a distributor. The central part being strongly magnetised by the current from a Wilde's machine, a number of electro-magnets are influenced by its rapid rotation, and in each of these an induction-current is generated. These induction-currents are powerful enough to feed three electric lamps, and as there are two series of twelve magnets a single machine could, theoretically, feed seventy-two lamps. Actually, however, it feeds only twenty-eight. Lontain uses a new regulator, which works very well by the dilatation of a small silver wire. By its dilatation this part of the apparatus works a lever system, and brings the carbon electrodes into contact. The French Northern Railway has purchased a number of Gramme magneto-electric machines. They intend to use them at their goods terminus and stores.

AMONG works of scientific interest announced for publication during the coming season we note the following:—Messrs. Macmillan and Co. are about to publish a new work by Prof. Clifford, F.R.S., "Elements of Dynamics; an Introduction to the Study of Motion and Rest in Solid and Fluid Bodies." This book is intended for engineers and students of physical science who are unable or unwilling to devote much time to mathematics. Its method consists in making use of the simpler ideas of motion to teach so much of mathematical processes as is required for understanding the more advanced parts. Also, by the same publishers, "An Elementary Treatise on Spherical Harmonies, and Subjects connected with them," by the Rev. N. M. Ferrers, M.A., F.R.S. Messrs. Longmans have just published of the London Science Class-Books Series, "Astronomy," by Dr. Ball, and "Thermodynamics," by Dr. Wormell. Other volumes to follow are "Algebra," by Prof. Henrici; "Botany," by Prof. McNab; "Biology," by Prof. McKendrick; and "Zoology," by Prof. McAlister. Messrs. Chapman and Hall promise two new volumes of "The Library of Contemporary Science"—"Biology," by Dr. Charles Letourneau and "Anthropology," by Dr. Topinard. Messrs. Trübner and Co. announce: "The Epoch of the Mammoth and the Apparition of Man upon the Earth," by James Southall; "The Parthian Coinage," by Percy Gardner, M.A., and "The Ancient Coins and Measures of Ceylon," by T. W. Rhys Davids, being Parts 5 and 6 of "The International Numismata Orientalia;" "The Birds of Cornwall," by Edward Hearle Rodd; "The Barents Relics," by C. L. W. Gardner; "Chemistry in the Brewing Room," by C. H. Piesse; "Origin and Migrations of the Polynesian Race," by Abraham Fornander, Circuit Judge of the Island of Maui; "Dr. Beke's Discoveries of Sinai," by Mrs. Beke; "A Statistical Account of Bengal," by Dr. Hunter. Mr. Stanford promises: "Africa," edited by Keith Johnston, being the first volume of "Stanford's Compendium of Geography and Travel," a work founded on Hellwald's "Die Erde und ihre Völker;" other volumes to follow "Africa" will be "Europe," by Prof. A. C. Ramsay, "North America," by Dr. F. V. Hayden, and "South America," by Mr. H. W. Bates; "Fifteen Thousand Miles on the Amazon and its Tributaries," by C. Barrington Brown and William Lidstone; "The Physical Geography and Geology of Ireland," by Edward Hull; an English edition of M. De Fonvielle's "Aventures Aériennes." Messrs. Kegan Paul and Co., successors to Messrs. H. S. King and Co., promise "Hygiene and the Laws of Health," by Prof. Corfield; and of the International Series, "Studies in Spectrum Analysis," by J. Norman Lockyer, F.R.S.; "The Physical Geography of the Sea," by Dr. W. B. Carpenter, F.R.S., "The First Principles of the Exact Sciences," by Prof. Clifford, F.R.S., and "The Brain as an Organ of Mind," by Dr. Charlton Bastian, F.R.S. Messrs. Blackie will publish a new edition of Thompson's "Gardener's Assistant, Practical and Scientific," revised and xtended by Thomas Moore, F.L.S., Curator of the Chelsea, Botanic Gardens, &c., assisted by several eminent practical gardeners; also "Upper Egypt, its People and its Products," a descriptive account of the manners, customs, superstitions, and occupations of the people of the Nile Valley, the Desert, and the Red Sea Coast, with sketches of the natural history and eology, by C. B. Klunzinger, M.D., formerly Egyptian Sanitary-Physician at Koseir on the Red Sea. Mr. Maclehose, of Glasgow, announces: "Outlines of Physiology," by Prof. McKendrick; Messrs. Collins: "Building Construction," by R. Scott Brown; "Machine Construction," by E. Tomkins; and "Mineralogy," by J. H. Collins, in their Advanced School Series.

In a paper in the *Journal de Physique*, on the spectrum of the electric spark, by M. Cazin, the author concludes that the electric spark in a gas contains incandescent gas particles, which give a bright line spectrum, and solid and liquid particles which produce the continuous spectrum, the former coming from the gaseous medium and the electrodes, the others from the electrodes and the sides near the spark. If the pressure increases, the solid or liquid particles become more abundant, and their continuous spectrum predominates; at last this makes it impossible to distinguish the bright gas lines, or, in other words, the latter, while the pressure increases, seems to dilate, and eventually flow together into one continuous spectrum. By making photographs of the spectra M. Cazin found his views confirmed. Of the nitrogen spectrum at ordinary pressure he photographed sixty-two lines, using nine cells in the battery giving the spark.

Herr J. Stefan has lately communicated the results of some interesting researches to the Vienna Academy of Sciences, relating to the heat-conducting power of several substances. The conducting power of copper being taken as unity, he found that of iron to be 0·17, ice 0·0057, glass 0·0016, water 0·0015, hydrogen 0·00039, hard india-rubber 0·00026, and air 0·000055.

In a recent communication to the Vienna Academy M. Ciamician discusses the spectra of chemical elements and their compounds. He finds, in agreement with Lockyer, that the compound spectra, as well as those of the first order of the elements, consist exclusively of bands; and further, that band-spectra belong to molecules and molecular groups, line-spectra to free atoms. From a comparison of the spectra of thirty-one elements he draws these conclusions: 1. The spectral lines of chemically-allied elements correspond to each other either individually or group-wise, so that each natural group of elements has its own spectrum, which, in the individual members of the group, is different only in that the homologous lines are displaced towards the one or the other end of the spectrum, *i.e.*, increase or decrease in wave-length, and that certain lines or line-groups disappear. 2. The increase or decrease of wave-lengths of homologous lines in chemically-allied elements depends on the intensity of their chemical *vis viva*, a greater wave-length corresponding to a greater chemical *vis viva* of the particular element.

Although for years there has been no scarcity in France through drought, still the want of irrigation is much felt almost every summer in the departments of the Mediterranean region. The French Government is about to take measures which might serve as a hint to the Indian Government. A project is being considered for taking advantage of the waters of the Rhone to irrigate systematically that large and already fruitful country. It is impossible to foresee what wonderful changes may result from such a scheme, which it is contemplated to bring into speedy execution.

The Annual Report of the Queensland Philosophical Society, 1877, just received, is a satisfactory one. It contains the address of the president, Sir James Cockle, on some of the aspects of the evolution theory.

The second volume, for 1877, of Dr. Emilio Huelin's "Cronicon cientifico popular," has just been published at Madrid. In a recent number we gave a short notice of the first volume. The second volume is in every respect equal to the first.

The additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus*) from India, presented by Mr. T. Golding and Miss Ward; a Layard's Flying Squirrel (*Sciuropterus layardi*) from Ceylon, presented by Sir Charles Peter Layard; a Brown Coati (*Nasua nasica*) from South America, presented by Dr. G. P. Best; a River Jack Viper (*Vipera rhinoceros*) from West Africa, presented by Mr. I. J. Kendall; two Red Kangaroos (*Macropus rufus*) from Australia, four Chinese Turtle Doves (*Turtur chinensis*) from Java, deposited; a large-billed Crow (*Corvus culminatus*) from India, purchased; a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

THE LIMITS OF NATURAL KNOWLEDGE[1]

The subject of my address was excellently treated at the Leipzig meeting in 1872, by Prof. Du Bois Reymond. If I take up the same matter again, I do so because I would consider it from a somewhat different and more universal point of view.

I shall also depart from the form and language in which the subject has hitherto been frequently treated. In its generality the theme easily induces the speaker to make excursions into the philosophical domain and to adopt the corresponding manner of expressing himself. I shall use words only of the simplest and clearest description, and I shall not suppose my hearers possessed of anything but a knowledge of the most elementary phenomena in the various domains of nature. In general matters expression is all the simpler and the more intelligible the closer our ideas approach clearness, and, at the same time, truth. I think it advisable, before entering upon the subject itself, to mention shortly the different ways in which the question of the limits of natural knowledge is generally conceived and answered by naturalists.

Amongst the so-called practical scientific men (*Praktiker*) the view is widely spread that a certain and lasting knowledge and understanding of natural phenomena is, on the whole, impossible. They know that hitherto their systems and opinions have not been permanent, and think that scientific theories generally are only attempts to approach the inaccessible reality, attempts which change their tenor and expression with the views of the time. This is evidently not a view based on principles, but only despair caused by failure, the necessary consequence of wrong method and of scientific incapacity.

The practical scientific man relies upon his experience, as he says. This, however, is gained in the following manner:—Each natural phenomenon is accompanied by different and often numerous causes and other circumstances. It is the task of the investigator to find out what are the effects of each one of these causes and circumstances; and this task cannot, in most cases, be accomplished by mere observation. The practical man then selects some cause or circumstance which happens to appear conspicuous to him, and in this he finds the fundamental cause of the phenomenon. This he calls his experience. We therefore understand how these practical men may hold different opinions upon the same phenomenon, why their views bear the stamp of the scientific epoch, and why in course of time they change. We also understand why the theories based on so-called experience are most fertile in those domains where phenomena are most complicated, as in organic morphology, in physiology, and pathology.

[1] Address delivered at the Munich meeting of the German Association, by Prof. C. von Nägeli, of Munich. (The author, in a note to the German original, remarks that this lecture had to replace another in the programme, which had been promised by Prof. Tschermak, of Vienna. At the eleventh hour Prof. Tschermak announced his inability to attend the Munich meeting, and the author was requested by the secretaries to fill the gap thus occasioned. The address therefore, the author states, bears the stamp of its hasty origin, as it was written during a journey in the Alps, when there was neither sufficient leisure nor opportunity for careful and elaborate work.)

The problem of a natural phenomenon is an algebraic equation with many unknown factors. The practical man looks at the equation and tries to solve it, substituting for one or the other unknown factor a generally large and decisive value; the proof of correctness he does not attempt. It is easy to see that in this manner the solution—and with it the true understanding—will certainly not be attained in all eternity.

The solution of an equation with many unknown factors is only possible if just as many equations can be obtained as there are unknown factors, and if the same unknown factors are contained in all. As this is generally impossible with natural phenomena we try to get equations in which there is only *one* unknown factor. This is done by *scientific* experiment (not by the so-called experiment of these practical men) in which all unknown factors are removed save one, and by which the value and effect of this one can be securely determined.

For a long time physics has adopted this way of scientific experiment. Physiology has only recently recognised it in a more general manner as the only correct one. It is true that by this tedious and time-devouring but yet exclusively safe and progressive method we do not erect large edifices of systems which are only fated to fall to pieces again shortly, but we gain simple facts, perhaps insignificant by themselves, but which retain their value for ever and enable us to find new facts. Thus the stock of recognised facts increases slowly but securely. A snail which takes the straight road for its goal progresses, while a grasshopper, with its bounds in all directions, remains always on the same spot. Thus scientific investigation proves to the empirics by facts, that by the exact method certain and permanent knowledge of natural phenomena may be gained.

Many methodical investigators who by the exact method augment the stock of permanent facts, when asked for the limits of natural knowledge, and thinking a solution based upon principles inadmissible, simply reply, "Belief always begins where knowledge ceases." In saying so the course of their thoughts runs thus: Humanity faces the totality of nature. Its insight constantly masters new domains by dint of meditation and investigation. Thus, for example, in the present time we have progresssed much further in the knowledge of nature than was the case during the middle ages and antiquity, and European civilisation is far ahead of that of the rest of humanity. With progressing mental work the empire of knowledge always increases in extent, and the domain where we must be satisfied with belief decreases as constantly.

This conception has an undeniable value in a certain regard. It gives us a measure of the height which scientific natural knowledge had generally attained in every century, and at the same time a special measure for the different human races and nations, for the different classes in a nation, and finally for every single individual. Considerations of this nature have as much scientific interest to the historian and anthropologist, as practical interest to the theologist, the politician, and a number of others.

The phrase that belief begins where knowledge ceases is an actual solution of the question for certain ends. But with this our interest is not satisfied. We turn to the theoretical part of the problem with special sympathy. We wish to know whether the limit where human knowledge must stop can be determined at all or not—if yes, how far our understanding may penetrate into nature, how much humanity may scientifically understand of nature, if during an immeasurable period, let us say at once during eternity, it is occupied with natural investigations, assisted by all imaginable means—what are the boundaries, therefore, which the scientific understanding of nature can never and under no conditions overstep? what is the fundamental limit between the empire of knowledge and that of belief?

This question deserves all the more to be seriously investigated since it is well known that from two opposed sides the absolute power of the human mind over nature is claimed with complete certainty—with decreasing energy by the natural philosophers, with increasing energy by materialists. The former think they can construct formal nature out of herself, and natural knowledge for them only consists in finding the concrete natural phenomena for the constructed abstract ideas, where, of course, they can in no point be freed from the self-deception that they construct the ideas according to conceptions by the senses instead of out of themselves. The latter admit only force and matter in time and space; and that man, who is built up of matter and force, shall master nature, which is built up of the same factors, seems to them a reasonable idea. Both, natural philosophers as well as materialists, raise man to a flattering height, with regard to his own consciousness and pride; they declare him lord of the world, not the real lord who makes the world, it is true, but yet the imaginary lord, who understands the work of the real lord. Can we lay claim with good reason this eminent position?

Many have often tried to answer this question from different points of view; perhaps one of the best replies was given by my predecessor in this assembly, Prof. Du Bois Reymond, in his much-talked-of and often misunderstood address, "On the Limits of Natural Knowledge." I shall only consider this latter reply, which, in an intellectual manner and in rich, poetical language, adorns and covers the gems of thought with the most beautiful flowers of speech. It would have been useful, and would have shown the right way to many a one who cannot so easily get at the kernel through the shell, if result and proof had been comprised in a few short phrases.

The speaker, like the conqueror of a world in the olden times on a day of rest, wishes to point out clearly the true limits of the immeasurable empire which world-conquering natural science has subjected to its understanding, and arrives at the following three conclusions:—1. Natural knowledge, or understanding, is the reduction of a natural phenomenon to the mechanics of simple and indivisible atoms. 2. There are no atoms of this description, and therefore there is no real understanding. 3. Even if we could understand the world through the mechanics of atoms, we could nevertheless not understand sensation and consciousness through it.

General understanding would no doubt have been facilitated considerably if these results had not been introduced as the limits of natural knowledge, but as the impossibility and futility of natural knowledge. Because, since the speaker does not go beyond this negation, investigating natural science cannot define the limits of a domain which she does not even possess—and if she is even deprived for ever of all insight into material phenomena, it can hardly matter to her, as a deposed potentate, whether or not she might claim the spiritual domain, in case of a supposed accession to power.

We may perfectly agree with Du Bois Reymond's thoughts, and yet be convinced that they are not complete and all-comprising enough to define natural knowledge in all directions, that in their incompleteness they lead to false deductions which contradict our natural scientific conscience, and that it is desirable to treat this question not only on the negative side, but to examine whether the human mind is not capable of natural knowledge, of what nature this knowledge is, and what is its extent.

The solution of the question: In what way and how far may I know and understand nature? is evidently determined by three different things, viz., by the answers to three questions:—(1) The condition and capacity of the Ego; (2) the condition and accessibility of nature; and (3) the demands which we make of knowledge. Subject, object, and copula therefore participate in the solution. A separation of this kind may perhaps be thought superfluous, perhaps even inadmissible, because it may be said that the understanding of the object by the subject is an indivisible process. And yet it is correct, because consideration gives prominence now to the one and now to the other factor, and it is also useful, because it requires exhaustive treatment. The difficulties which are in the way of knowledge with regard to the subject or object, are even most conspicuous if we entirely remove one of the two factors by supposing that it offers no difficulties at all. With regard to the capacity of the Ego to understand the phenomena of nature, the undoubted fact is decisive, that our power of thinking, in whatever condition it may be, only gives us nature as we perceive her by our five senses. If we could not see nor hear anything, nor smell, taste, nor touch anything, we would not know at all that there is anything besides ourselves, nor indeed that we are in bodily existence ourselves.

The condition with regard to the correctness of our conceptions therefore always exists—that our external and internal senses report correctly. Our knowledge is only correct in so far as observation by the senses and internal perception (*die innere Vermittelung*) are correct. But an infinitely great probability exists that both, after all, lead us to *objective* truth, because the errors committed by the single individual or by all, are finally always recognised and proved as such, and because natural science, the further it progresses, knows how to remove more and more all apparent contradictions, and how to make all observations agree amongst themselves.

If we remain satisfied in this direction, the question arises, to what extent and in what fulness the senses acquaint us with natural phenomena. With regard to the extent we need only point out the boundaries in order to make them perfectly clear to everybody. In time only the present and in space only that which belongs to our own circumstances is accessible to us. We cannot directly perceive anything of what happened in the past, and of what will be in the future, and nothing that is too distant in space, or that is of too large or too small dimensions.

With regard to the completeness of sensual perceptions there is another boundary which is generally not thought of, and upon which I must enter a little more in detail. Scientific analysis shows the following:—In the totality of force-endowed matter, which we call world, each particle of matter by all its inherent forces is in relation with all others; it is influenced by all, and in its turn acts upon all, of course according to distances. A conglomeration of particles of course behaves like a single particle; the effect which it causes and receives is the total of the effects of all single particles. The crystal, the plant, the animal, man are acted upon by the presence of all material particles, of each single one by itself and of each conglomeration of particles, and this with reference to all forces which are inherent in them, and consequently with reference to all movements which they perform. But these effects in the infinite majority of cases are so insignificant that they may be neglected as quite imperceptible.

The *theoretical* possibility therefore exists that the human organism may obtain bodily perceptions of all phenomena in nature. But how is this matter in *reality?* What impressions are so powerful that they become perceptible to us, and which of them are lost, being too insignificant? Amongst the beings known to us, man and the higher animals have the advantage, that certain parts have developed themselves into organs of sensation, which are extremely sensitive for certain natural phenomena. These organs of sensation, in the course of numerous and successive species and of innumerable generations within each single species, have been developed from the smallest beginnings to high degrees of perfection.

The ingenious idea of Darwin that in organic nature only such arrangements attained full development which were useful to the individual bearer, is so simple, so reasonable, and agrees so well with all experience, that physiologists, who alone are competent to decide here, agree with him perfectly, and are greatly astonished, that a Columbus should not long ago have placed this physiological egg upon its point.

The degree of perfection which each organ of sensation has attained in development therefore corresponds exactly to the requirements, and there is not one in which the human organism is not far surpassed by some animal species, if to the latter the extraordinary fineness of some particular sensual perception became a condition of its existence. But according to this both the human and animal organisms have only developed organs of sensation for such external influences as bear upon their existence in a favourable or unfavourable sense.

We are endowed, for instance, with great sensitiveness for temperature; it is necessary for our existence, otherwise we might perish through cold or heat without knowing it. We are very sensitive towards light; it acquaints us in the best and quickest manner with all objects which surround us and which may be useful or dangerous to us. On the other hand we are not organised to perceive the electricity which surrounds us. While we perceive the increase or decrease of heat and light, we do not know whether the air in which we breathe contains free electricity or not, whether this electricity is positive or negative. If we touch a telegraph wire we cannot feel whether its particles are electrically at rest or in motion.

It was of no use that the sense for electricity should be developed particularly in man and the higher animals, because it is immaterial for the species whether every year some individuals were killed by lightning or not. If this danger were daily to threaten all individuals, the sense for electricity which the lowest animals possess in its first beginnings in the same degree as they possess those for light and heat, would necessarily have developed itself further. We would then perceive by a special organ of sensation the vicinity of a substance in electric tension and be able to escape the stroke of lightning. We would perceive small changes in the electric state and weak electric currents in our vicinity, and also be able to peer into the secrets of the telegraph wire. The want of such an organ might easily have been the cause of our total ignorance of electricity. We can very well imagine the atmosphere of the earth without lightning and thunder. These great electric discharges have helped us to the knowledge of electricity. If accidentally they had not happened, if, moreover, some quite accidental experiences which revealed an attractive and repulsive force generated by friction had not been made, we very probably would have had no idea of electricity, no idea of that force which doubtless plays the greatest part in organic and inorganic nature, which materially affects chemical affinity, which in all molecular motions in organised beings acts perhaps more decisively than any other force, and of which with regard to still mysterious physiological and chemical phenomena we expect the most important explanations.

Our senses are indeed only organised for the requirements of our bodily existence but not to satisfy our intellectual cravings,—to acquaint us with all phenomena of nature and explain them as well. If at the same time they perform this function it is only incidentally. We therefore cannot rely upon our sensual perceptions acquainting us with *all* phenomena of nature. Just as in the case of electric phenomena, which occur in every material particle, we have, as it were, learnt something only accidentally, it is easily possible, indeed very probable, that there are still other natural forces, other forms of molecular motion, of which we obtain no sensual impressions, because they never unite to any remarkable outcome, and therefore remain hidden to us.

Our power of perceiving nature directly by our senses is therefore very confined in two aspects. On the one hand we are probably deficient of the power of sensation for whole domains of natural life, and on the other, as far as we really have this power it is confined in time and space to an insignificantly small part of the whole.

It is true that our natural knowledge is not confined to what we perceive with our senses. By conclusions we may also obtain knowledge of what our senses do not reach. The farthest planet of our solar system, Neptune, was known by calculation with regard to its position, its size and weight, before astronomers had discovered it with the telescope. We know, although we cannot see it with the best microscopes, that water consists of infinitesimal particles or molecules which are in motion, and if it is sugar-water or salt-water, we know perfectly the proportionate weight and the proportionate number of the water, sugar, and salt particles of which it is composed.

By conclusions from facts which were recognised by the senses, we arrive at facts equally certain which can no longer be perceived by the senses. We might therefore, perhaps, indulge in the sanguine hope that starting from the small domain which is opened to us by our senses, little by little the entire domain of nature will be conquered by reason. But this hope can never be fulfilled. As the effect of a natural force decreases with its distance, the possibility of knowledge also decreases as the distance in space and time increases. Of the condition, the composition and the history of a fixed star of the least magnitude, of the organic life upon its dark satellites, of the material and spiritual movements in these organisms we shall never know anything. In the same way the possibility decreases of discovering a still unknown natural force, a still unknown form of motion of the smallest material particles, the less this force or motion possesses the peculiarity of accumulating and causing some collective effect. We may consider ourselves fortunate if ever we obtain only a notion of such a force.

The confined capacity of the Ego therefore allows us only an extremely fragmentary knowledge of the universe.

We now pass from the consideration of the subject to that of the object, *i.e.*, the condition and accessibility of nature. The boundaries, which nature herself opposes to our knowledge, are most evident if we adopt the hypothesis that man, on his side, has the most perfect capacity for natural knowledge. This would be the case if the obstacles of time and space did not exist for him, if he could judge of every phenomenon in the past as well as he can of everyone in the present; if the most distant object did not present more difficulty to him than one in his immediate vicinity, and if he could as easily survey the largest systems of fixed stars and the smallest atoms, as he can a body of his own size; if finally he were provided with senses so perfect that all phenomena of nature, all forces and all forms of motion could be perceived directly by him.

A human race, provided with these perfections, might perhaps be enabled to try the solution of Laplace's problem. Laplace says: "A mind, which for a given moment knew all forces which are active in nature, and the respective positions of the beings of which she consists, if it were comprehensive enough to analyse these data—would unite in the same formula the motion of the largest heavenly body and of the lightest atom. Nothing would be uncertain for it, and the future as well as the past would be present to its gaze. The human mind, in the perfection which it has been enabled to give to astronomy, offers a weak reflection of a mind of this description."

But even a mind as universal as that supposed by Laplace would not be able to solve the problem given. Because the other supposition, of which Laplace does not speak, but from which he starts unconsciously, is the finiteness of the world in all directions, and this is not given. The difficulty which nature opposes to human knowledge is her *endlessness*, endlessness of space and of time, and of everything which depends on this as a necessary consequence.

In space nature is not only infinitely large; she is endless. The ray of light travels through some 190,000 miles in one second; to travel through the whole known universe of fixed stars it would require some twenty million years according to a probable estimate. Let us place ourselves in thought at the end of this immeasurable space, upon the farthest fixed star known to us, then we would not look out into empty space there, but we would see a new starry firmament. We would again believe that we were in the middle of the universe, in the same way as now the earth appears to us as the centre of the universe. And thus we may in thought continue endlessly the flight from the farthest fixed star to the farthest fixed star, and the actual starry heavens we now see, compared to the universe, are after all still infinitely smaller than the smallest atom compared to the starry heavens.

What applies to space applies equally to grouping in space, to the composition, organisation, and individualisation of matter, which is the object of descriptive and morphological natural science. Everything we know consists of parts, and is in itself part of a bigger whole. The organism is composed of organs, these of cells, and the cells of smaller elementary particles. If we analyse further we soon get to chemical molecules and the atoms of chemical elements. The latter certainly still resist further sub-division at present, but we must nevertheless look upon them as compound bodies on account of their properties. Thus in thought we may continue sub-division further and endlessly. In reality no physical atoms in the strict sense of the word can exist, no little particles which would really be indivisible. All size, indeed, is only relative; the smallest body in existence which we know, the particle of the light-and-heat ether may be of any size we choose for our conception, even infinitely large, if only we imagine ourselves to be sufficiently small by the side of it. Just in the same way as *indivisibility* never ceases, we must suppose, by analogy of what we find confirmed in the whole domain of our experience, that the *composition* also of individual particles separated from one another, continues endlessly downwards. In like manner we are forced to suppose an endless composition upwards in always larger, individual groups. The heavenly bodies are the molecules which unite in groups of lower and higher orders, and the whole of our system of fixed stars is only a molecular group in an infinitely larger whole, which we must again suppose to be a unit (*einheitlicher*) organism, and only a particle of a still larger whole.

As space is endless in all directions, so time is endless on two sides; it has never begun and will never cease. The Bible says: "In the beginning God created heaven and earth," and geologists say: "In the beginning the world was a gaseous mass, from which heavenly bodies formed by condensation." But this beginning is only a relative one, the beginning of a finiteness, and the time which has passed since this beginning is only as a moment compared to the eternity before.

From the union of time and space an empire of phenomena results, which forms the contents of descriptive natural sciences as well as of the other part of the investigation of nature, viz., the physical and physiological sciences. Matter, which fills space, is not at rest but in motion, and as the material particles act upon one another with different (attractive and repulsive) forces, each body which moves causes the others to move as well, or rather it changes their motions. It gives off a part of its motion and of its potential energy to others, and these again to others, and so on. This is the chain of cause and effect, also an endless one, as in our conception it neither could begin with a first cause nor can finish with a last effect.

Nature is everywhere uninvestigable where she becomes endless or eternal. We cannot, therefore, conceive her *as a whole*, because a process of conceiving which has neither beginning nor end, does not lead to conception. And this is the reason why Laplace's problem is futile from the beginning. Of course we are permitted to make any supposition we like, even one which for some reason or other is impossible, but not one which is unthinkable. But a formula is unthinkable for which we have not even got the component factors, and which if these factors were given, would never come to an end. The knowledge of *all* forces, which is required for Laplace's formula, supposes that the bodies are subdivided down to their last force-endowed particles, and this is impossible on account of divisibility being endless. The elements therefore are wanting, from which we might compose the formula, viz., the simple natural forces; we cannot even begin with the setting of the formula—and even if we could begin with it we could never come to an end with it on account of the endlessness of the universe in space. Du Bois Reymond has already mentioned the former endlessness as an insuperable limit; even if we could overstep it, the other would still prove equally insuperable.

If indeed the formula of Laplace comprised only the universe known to our senses, or even one infinitely larger (but not one really endless), and if we could introduce into this formula the forces of the known chemical elements and of the supposed ether particles, or even of much smaller material particles, then it might perhaps suffice for long periods of time backwards and forwards from the present, particularly for the middle of the system and for the greater phenomena. But on the one hand disturbances from the circumference would at once necessarily take place, and these would at last render the formula useless for the middle also; on the other hand, disturbances would begin on each single point as well, and as they would increase constantly, they would at last lead to perceptible inaccuracies, because the supposed "atoms," are not real unities, and because the resulting force, with which each single "atom," as a body composed of separate particles, influences the totality, does not remain a constant one, but with its varying surroundings assumes at every moment an equally varying value. Anyhow, a formula of this kind would give us, as astronomical calculation really does, a solution, correct within certain limits, a practical solution, but not a fundamental one.

The investigator of nature must remember distinctly, that all his investigations are confined by limits in all directions, that on all sides uninvestigable eternity bids him categorically to stop. The fact that this has not always been clearly recognised, that particularly the Infinitely Large and the Infinitely Small have been mixed up with Endlessness and Nothing, has led to several erroneous conceptions. Amongst them are the theories of physical atoms in the one direction, and those of beginning and end of the universe in the other. I will only speak of the latter.

It is supposed that the matter constituting the heavenly bodies was in the beginning distributed in a gaseous state; and in this Du Bois Reymond only finds one difficulty: if this matter, as the theory demands, had been at rest and distributed equally, he cannot find out whence motion and unequal distribution have come.

Condensation of matter has now gone on for an infinite time, *i.e.*, since that supposed beginning, and the results are first nebulæ, then burning-liquid drops, which cool down to dark bodies. In the present we are upon one of these condensed, and no longer incandescent world-drops. According to the natural laws known to us, the still incandescent and the already dark heavenly bodies must continue to give off their store of heat to universal space. By and by they must fall upon one another, and even if then a local rise in temperature again takes place, this after all only serves to accelerate the process of cooling on the whole. At last all heavenly bodies will unite in a dark, solid, icy mass, upon which there is no longer any motion nor life.

This is the result of a correct physical consideration. It shows us the desolate end of a present full of change and motion and glowing with life and colour. But in reality this result is only the consequence of our confined human insight; it would only be a logical necessity if we knew *everything*, and therefore were allowed to use our knowledge for deductions with regard to the beginning and end. But as we see only an infinitesimal part of the universe, and possess only a fragmentary knowledge of the forces and forms of motion in this infinitesimal part, our deductions backwards and forwards may perhaps for certain general conditions be without perceptible error for billions of years, but yet, with the lapse of greater periods of time, they must become more uncertain, and eventually totally erroneous. We may illustrate this particularly well with regard to the past.

What we are most certain of, with regard to the past, is the incandescent state in which our earth was at one period, and from this we draw the conclusion by analogy that the other planets of our system were incandescent bodies as well, just as the sun is still to-day. If we go backwards from these suns we get, by further conclusions, to accumulated masses of clouds, the embryos of the later suns, then to cloud-belts, and eventually to the gaseous mass distributed tolerably uniformly, and this is the original state beyond which, with our present insight, we cannot get.

All this proves distinctly that just as upon the earth an eternal change takes place, the heavens likewise are constantly changing. Each change consists of a sum of motions, and supposes a former change or sum of motions, from which it resulted with mechanical necessity, and further on a chain of changes from all eternity. Thus the gaseous state of our solar system must have been preceded by a continuous endless series of changes, and if our scientific insight does not lead us to this, does not even justify us in this supposition, it thus proves only its own inadequacy.

We must, on the contrary, conclude from the eternity of changes in the universe that the whole process of development of our solar system or of the whole starry heaven, from the original gaseous mass, through the ball-shaped nebulæ, fiery and dark globes, to the cold, solid, and dense mass, is only one of the numberless successive periods, and that analogous periods and occurrences have preceded and will follow endlessly. It is true that we perfectly understand, according to our present physical knowledge, how a mass of gas in a state of progressing condensation produces heat, and how the hot condensed mass again gives off this heat until its temperature and that of its surroundings, in our case that of universal space, have become equal. But we do not understand how the solid mass can again become gaseous, and how the necessary heat, distributed in universal space, can again be collected.

There is a gap in our knowledge at this point; and we may fill it by various suppositions. In the present state of almost complete ignorance among physicists and chemists of the properties of chemical elements and of ether, it is possible that, with sufficient condensation of matter and approach of its particles, forces become active of which we have no idea at present, and which may perhaps bring about an explosive dispersion of the solid mass into a gaseous state. It is also possible that the quantity of heat in the endless universe (not in our starry heaven) is distributed unequally, and that there are domains in it which are of a much higher, and others which are of a much lower, temperature than our starry heaven; that in the endless space of the universe heat currents exist, similar to the air currents in our atmosphere, and that we have perhaps for some billions of years been in one of these currents of lower temperature, in which the process of solidification continues on a large scale, just as on a small scale it occurs on the earth's surface during north winds, and that some hot current which sooner or later may pass through our starry heaven may again bring about a gaseous distribution of matter.

This example shows that we may use our experiences of the finite only for deductions within the finite. As soon as man wishes to overstep this domain, which is opened to him by his senses and which is accessible to his knowledge, and wants to form some conception of the whole, he falls into absurdities. Either he leaves unconsidered what he has gained by experience and meditation, and then he loses himself in arbitrary and empty fancies; or he proceeds logically from the laws fo the finite and then he finally arrives at perfectly ridiculous consequences.

The example mentioned before may again serve to illustrate this. The world known to us changes. If we follow these changes according to the laws of causality, backward into the past and forward into the future, and place ourselves upon the before-mentioned physical stand-point of the nebular theory, and adopt what is known to us there as a measure, then we find stages both in the past and the future which more and more approach perfect rest, without ever reaching it altogether. But if we assume a further point of view, and suppose that heavenly bodies and systems of heavenly bodies arise and perish without end in the universe, then we find two possibilities: either, according to the materialistic conception, the successive stages are of the same value; or, according to the philosophical conception, they continually change their relative value, becoming more perfect every time, in which case the universe would in the eternal past more and more approach absolute imperfection (therefore rest), and in the eternal future absolute perfection (therefore again rest). All three conceptions are equally irrational. The first (physical) and the third (philosophical) let the world awaken from dead rest and return to it. The second (materialistic) condemns it to eternal rest, because a change which always repeats itself, means for an eternity nothing else but rest.

With space we do not fare better than with time. We naturally wish to imagine the universe as of finite extent in space and thus make it accessible to our conception. But as the space filled with matter can but everywhere be limited by more space filled with matter, we arrive at the absurd deduction that the world in its circumference is bordered by itself. But if we allow infinity to universal space, and according to our ideas of space it must be infinite, then heavenly bodies follow upon heavenly bodies without end, in different sizes, different compositions, and different stages of development. Now as size, composition, and stages of development move within finite limits, the combinations which are possible constitute of course, to our ideas, an infinitely great, but yet not an endless number. If this number is exhausted the same combinations must repeat themselves. We cannot deny this, even with the conviction that centillions upon centillions of heavenly bodies or systems of heavenly bodies would not suffice to complete the number of possible combinations. Because centillions compared to endlessness are less than a drop of water compared to the ocean. We therefore arrive at the mathematically correct, but to our reason most absurd, deduction that our earth, just as it is now, must occur several times, indeed an infinite number of times in the universe, and that also the jubilee festival, which we celebrate to-day, is celebrated just in the same way upon many other earths.

The logical consequences of this kind may be multiplied. The examples suffice to show, that our finite reason is only accessible to finite conceptions, and that, when it wishes to raise itself to conceptions of the eternal in however logical a manner, its wings become paralysed, and, like a second Icarus, before the sunny heights are reached, it falls back into the depths of finite and obscure ideas.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Physical Science Postmastership at Merton College, has been awarded to Mr. E. T. Milner, of Manchester Grammar School.

CAMBRIDGE.—Mr. J. N. Langley, B.A., of St. John's College, has been elected a Fellow of Trinity College. Mr. Langley was bracketed second in the first-class of Natural Science Tripos 1874.

EDINBURGH.—Mr. Thomas Annandale, who was assistant to the late Prof. Syme, has been appointed to the chair of Clinical Surgery in Edinburgh University, vacant by the removal of Mr. Lister to King's College, London.

UPSALA.—The *Scotsman* of Thursday last, contains a very full and interesting account of the recent Upsala celebration, evidently by one of the Edinburgh delegates. The writer, speaking of a visit he paid to one of the largest schools of Upsala, built for about 500 pupils, says:—"Here, as elsewhere in Sweden, the expense of education is wholly borne by the State. The pupils pay no fees. The building is spacious and airy, and the class-rooms and playgrounds furnished, almost to luxuriousness, with the requisites for the development of healthy minds in healthy bodies. The arrangements for the securing of the required heating and ventilation of the rooms during the long severe winters of Sweden are particularly good. Nearly every class-room is seated for about thirty pupils. Each pupil has his own little desk before him, and a chair with a back fitting comfortably to his body, and adjustable as to height so as to suit each pupil. This seat he retains during the session, so that there is no taking of places in the classes. There are several carefully-selected libraries for the pupils in the school—a marked feature of which is the number of books in English, French, and German; and there is the best proof everywhere that these volumes, which are mostly classic authors in these languages, do not lie idly on the shelves, in the number of Swedes one meets with who can converse tolerably well in one or all of these languages. But what struck us as deserving of the very warmest commendation, are the well-appointed and well-kept museums of apparatus illustrative of the simplest and most fundamental facts of natural philosophy and chemistry; well-dried mounted specimens of the common plants of the district; stuffed and otherwise prepared specimens of the Swedish fauna; large models of typical plants and animals, showing the details of their structure; and skeletons and plaster casts by which the fundamental facts of anatomy and human physiology can be successfully taught. Thus, with the aid of these admirable elementary museums and appliances, which Mr. Forster might well envy, the broad principles of physical, chemical, and biological science are taught to all whose education goes no further than the public schools; and as regards the others, such instruction in the elements of science forms an admirable introduction to the University course."

SOCIETIES AND ACADEMIES

LONDON

Royal Microscopical Society, October 3.—Mr. H. C. Sorby, president, in the chair.—The president read a paper on an improved method for distinguishing the axes of double refracting substances which consisted of a wedge-shaped piece of quartz cut parallel to the positive axis of the crystal, and made to slide into the eye-piece of the microscope. When this passed across the field of view in polarised light every gradation of tint was successively produced by the varying thickness of the quartz, and by viewing crystals through this it was very easy at once to determine the position of their axes by noting the effect upon the series of coloured bands produced by the quartz scale.—A paper by Mr. F. H. Wenham on the aperture of object glasses was read by the secretary. The purport of Mr. Wenham's paper was further explained, and illustrations of the method proposed were drawn on the black-board by Mr. J. E. Ingpen.—Mr. Slack described some curious observations made as to the habit and power of offensive attack by the genus diglena upon anguillula and other species.

PHILADELPHIA

Academy of Natural Sciences, May 1.—On the Cambari (crayfishes) of Northern Indiana, by W. F. Bundy (*Proc.*, 1877, p. 171).—Synopsis of the fishes of Lake Nicaragua, by Drs. Gill and Bransford (pp. 175-191).—On lavendulite from Chili, by E. Goldsmith.

May 15.—Prof. Leidy, on gregarines.

May 22.—Prof. Leidy, on flukes infesting molluscs.—H. C. Yarrow, notes on the natural history of Fort Macon, N.C. (pp. 203-218).—On the brain of *Chimæra monstrosa*, by Dr. Wilder (pp. 219-250).

June 12.—Prof. Leidy, remarks on parasitic infusoria.

June 26.—Prof. Leidy, the birth of a rhizopod (*Euglypha*).

PARIS

Academy of Sciences, October 8.—M. Peligot in the chair.—On an incident mentioned at the congress of Stuttgart, by M. Faye. This relates to recent geodetic operations in the north-east of Spain, directed by Gen. Ibanez.—Apparatus for measuring the heat of vaporisation of liquids, by M. Berthelot. He aims at greater simplicity, while transmitting the vapour dry from generator to calorimeter. A phial with hermetically sealed neck is traversed by a wide vertical tube open at its inclosed top, passing down through the phial to a serpentine in a calorimeter, and (in its way) through a metallic disc, a circular lamp, another metallic plate, a sheet of paste-board, and a wooden plate (the last three forming the cover of the calorimeter). He finds on an average 636·2 as the total heat furnished by water between 100° and zero (Regnault 636·6).—On the determination of the heat of fusion, by M. Berthelot. The two phenomena of fusion and solidification in a body like hydrate of chloral are not reciprocal when one directly follows the other, and the heat absorbed in one case is not equal to that liberated in the other. To measure the calorific work in fusion the body should be brought to a certain final state, proved identical by thermal measurements, *e.g.*, dissolving hydrate of chloral at a given temperature and in a constant quantity of water, and comparing specimens recently fused, and others kept several months or years. Then a known weight of the substance is raised to different temperatures, sometimes above sometimes below the point of fusion, then immersed and dissolved suddenly in the water of the calorimeter. He finds the heat of fusion to be 33·2 cal. for 1 gramme.—On the variations of the heat liberated by union of water and sulphuric acid at different temperatures, by M. Berthelot.—On the relation which should exist between the diameter of magnetic cores of electro-magnets and their length, by M. Du Moncel. For equal resistances of circuit the diameters should be proportional to the electromotive force for equal electromotive forces, in inverse ratio of the resistance of the circuit, including the battery resistance; for equal diameters proportional to the square roots of the resistances of the circuits; for given electromotive force and with electro-magnets in their conditions of maximum the electro-motive forces of the batteries should be proportional to the square roots of the resistances of the circuits.—Programme of the expedition of next year (July, 1878) to the glacial sea of Siberia, by M. Nordenskjöld.—Observations of the planet 175 Palisa, and of the new comet of Tempel, with the garden equatorial, by MM. Paul and Prosper Henry.—On a general method of transformation of integrals depending on square roots; application to a fundamental problem of geodesy, by M. Callandreau.—On the spectrum of the new metal davyum, by M. Kern. He indicates the principal lines.—Pyrogenous decomposition of chlorhydrate, bromhydrate, and iodhydrate of trimethylamine; new characteristic of methylamines, by M. Vincent. The new characteristic is the production of chloride, bromide, and iodide of methyl from such decomposition.—On iodide of starch, by M. Bondonneau.—Synthesis of benzoic acid and of benzophenone, by MM. Friedel, Crafts, and Ador.—Experiments on the tape-like development of human cysticercus, by M. Redou. Man may, like swine, become completely infested by cysticerci. M. Redou caused some cysts from a human body to be ingested with tepid milk into young pigs and dogs; he also swallowed some himself. It appeared that only man presents a favourable medium; the pigs and dogs gave no trace of the tape-worms; but the author after about three months discovered worms in his stools. This throws light on the nature of the development of the human cysticercus, and presents a striking exception to the law of parasitism with alternating generations.—Description of the meteoric stones of Rochester, Warrenton, and Cynthiana which fell respectively on December 21, 1876, and January 3 and 23, 1877, with some remarks on previous falls of meteorites in the same region, by Mr. Smith.—M. Bouvel called attention to an arrangement for compressing oxygen and hydrogen to considerable pressures. The wires from a battery are conducted into a thick metallic block containing a strong glass voltameter with one chamber double the other; under the chambers are the terminal electrode immersed in acidulated water, the bottom of the reservoirs being closed by a strong screw. The reservoir communicates also with another cylinder in which a screw can be made to press on the liquid. Two narrow passages rise from the gas chamber and are closed by screws.

CONTENTS

ERRATUM.—In NATURE, vol. xvi. p. 330, the reference to the *Astron. Nach.* should be to Nos. 1,663 and 1,733.