THURSDAY, MARCH 20, 1879

ROYAL AGRICULTURAL COLLEGE, CIRENCESTER

DURING the last few years the question of Agricultural Education has been very fully and fruitfully discussed. The experiment of an examination in the principles of agriculture, under the Science and Art Department, had an unexpected success; and showed that there was throughout the kingdom a demand for instruction in agricultural matters. At the present moment efforts are being made to satisfy this demand more completely by means of local organisation for developing and extending the facilities already offered by the Science and Art Department.

There is at Cirencester a college founded specially for the advancement of agricultural education. It has one—or more—Royal Charters; it has the power of granting diplomas; it is under Royal patronage, and has the advantage of being managed by numerous Earls and M.P.s. This institution ought to (and might) have been the centre of the movement to which allusion has been made; but, unfortunately, its own troubles seem to be enough to occupy the whole attention of the Committee of Management; and, for the second time in the history of the college, threaten to bring about its extinction. For the past few weeks the agricultural press has been teeming with letters and articles headed "Professor Church and the Royal Agricultural College." The facts, as to which there seems to be no dispute, are briefly these:—Prof. Church is about to be married. Other professors, his colleagues and juniors, had done the same, and non-residence in their cases was not found incompatible with the proper performance of their several duties; as a matter of fact each of Prof. Church's predecessors was non-resident. Yet the Principal intimated to Prof. Church that without residence he could "no longer discharge the duties of Professor of Chemistry in this college." It appeared that this decision on the part of the Principal was not authorised under the bye-laws: such a point could be determined only by the Committee of Management; and the case was referred to them. The result was, however, unaltered. While "fully sensible of the services rendered by Prof. Church during his sixteen years' residence in the College," the Committee "regret that they cannot accede to his recent proposal of non-residence." The consequence of this was two resignations. Prof. Lloyd Tanner regarding the decision "as showing that neither long and zealous performance of duty, nor special ability for work are duly recognised," has resigned the Chair of Mathematics and Physics; and Prof. Fream, "as the only protest it is in his power to make against the treatment his colleague has received," similarly vacates the Chair of Natural History.

Such are the circumstances under which the three senior resident professors at Cirencester College are leaving. Other matters have rendered the affair even more painful than it need have been, but we believe the simple, undisputed facts of the case are amply sufficient to enable our readers to form a just opinion of the mode of managing Cirencester College. Those who intend to become candidates for the vacant chair have had an opportunity of judging how one bye-law can be and is used against a man such as Prof. Church; we will only advise them to study the other bye-laws and guess how they may be used against younger and less known men. Having seen these bye-laws ourselves we are curious to know who will be induced to replace the vacancies just announced.

PROF. HUXLEY'S HUME

Hume. By Prof. Huxley. (London: Macmillan and Co., 1879.)

PROF. HUXLEY has given a clear and succinct account of the philosophy of Hume, in a style at once fresh and pointed. We should be thankful to him that, following the example of Locke and Hume himself, he discusses philosophical questions in genuine and idiomatic English, and consistently avoids the use of a lumbering phraseology, imported from abroad, amid which the thinking evaporates, for the most part, in pure verbalism. The volume before us is limited to a brief account of Hume's life and his philosophical opinions. It hardly touches what has been said on the other side in criticism or in correction of Hume's views. Here and there Prof. Huxley offers a criticism; but, though generally acute, it is seldom on anything but a point of detail. Indeed, the volume may be described as rather too much of a bare statement of Hume's principles and conclusions.

As Prof. Huxley may fairly be regarded as dogmatically accepting Hume's principles and boldly carrying them out to their results, while Hume may with probability be regarded as having only hypothetically held the principles, we might have expected a fuller vindication of them than is at all attempted in the volume. On all the metaphysical questions of greatest moment Prof. Huxley's position is a negative one; and if, as it seems, he accepts Hume's principles absolutely, it is one of complete negation.

In the opening chapter on the Philosophy (Chap. II.) Prof. Huxley has done good service in clearly stating the terms of the question. He very properly points out that the question regarding the limits of knowledge, or "What we can know," is not a primary but a secondary question. He is emphatic in showing that it implies the previous questions as to what we mean by knowledge, and how we come by the thing we call knowledge. And he very well points out that these latter questions are psychological, and that psychology, accordingly, is the only proper basis of assertions about knowledge, whether these refer to its nature, conditions, or limits. This clear and vigorous statement is not inopportune, for there is somewhat of a tendency at present, very inconsistently indeed, to ignore psychology. We have professions of "deducing" the conditions of "experience." It seems strange that it does not occur to the advocates of such a method that its basis is necessarily an accurate examination of what experience or consciousness in its fullest extent is; what, in a word, is the thing spoken of, whose conditions it is proposed to evolve. This implies a full and scientific psychology—the only safeguard against fantastic system-making, otherwise the so-called "deduction" becomes a method of *if* and *must*

—hypothesis and hypothetical inference; having no bearing on our experience.

What Hume really sought in philosophy was the ultimate element, out of which all valid knowledge might be shown to flow. This element was to be at once the source and the test of every conception of the human consciousness. This he supposed he found in the "simple impression" or "simple impression of sensation." The essence of Hume's method is to reduce all so-called knowledge of objects to this test; his constant demand is—show me the "impression" from which your alleged conception or idea is derived, and then, but then only, shall I admit the reality and validity of your knowledge. If our conception be meaningless, the object of it is unreal. It is easy to see how on such a method, whether adopted hypothetically or dogmatically, self-existence, self-identity, personality, and Deity must be given up.

But the question at once arises:—*What* precisely is this so-called "*impression of sensation*," or "*singular sensation?*" The psychological method has been admitted. And we must apply this method to find whether there is such a thing as an *impression per se*. It is at least a consciousness, or state of consciousness. If it be said that *impression* is not the full fact, but a mere abstract part of the complex fact which we call consciousness—this is a position which is quite as vindicable on Hume's psychological method as his statement of the fact is. We do not require to have recourse here to any "transcendental deduction," or to Prof. Huxley's "pure metaphysician." We only ask whether the psychological method is fairly applied to the fact. Here we do not think that Prof. Huxley has done any justice to those who say and seek to show that *impression per se* is a mere abstraction—possibly even a simple unintelligibility.

No doubt Prof. Huxley tells us that Hume omitted an entirely irresolvable element of consciousness, viz., relation, as of succession, co-existence, &c. But one does not see that Prof. Huxley apprehends the true force of his own admission. The *relation* of succession is still as much an abstraction as *impression* is, in fact, an unintelligibility, unless on the supposition of some one conscious being,—subsisting through varying times. An appeal to memory is of no use here. Memory itself is but a phrase for the act of one and the same conscious being subsisting and recognising impressions in successive times. The unity of the conscious being is the ground of memory; not memory the ground of it; as this unity is equally the ground of the possibility of a known relation of succession, or successive impressions. Prof. Huxley does not recognise this in its proper place; he even in the end gives in his adhesion to Hume's denial of a *self* or unity in consciousness at all. But by this he cuts away all ground of right to acknowledge relation in knowledge; all ground in fact to affirm or deny anything.

Hume at once naturally takes up the question as to the kinds of impressions conveyed, as he phrases it, through the senses. His answer to this question may be said to be that all we know through the senses is of the same kind, whatever be our natural belief to the contrary. Figure, bulk, motion, colours, tastes, smells, sounds, heat and cold—pains and pleasures, from application of objects to our bodies—are all simply impressions or conscious states—each class has but the same "interrupted and dependent being." They are "nothing but perceptions arising from the particular configurations, and motions of the parts of bodies." In that sentence lies the main inconsistency of Hume; and it is a key to the constant shifting of ground, which, with all deference to the admirers of the consistency and cogency of his reasoning, nullifies large portions at once of the "Treatise of Human Nature," and the "Inquiry Concerning Human Understanding." For if the senses can in no way give us more than a conscious impression, they are absolutely impotent to tell us of a body which is not itself merely a conscious impression. And to say, therefore, that bodily motions are the antecedents or causes of conscious impression is simply to say that conscious impression is the antecedent or cause of conscious impression. If Hume assumes that the senses do more than this, and distinctly inform us of objects called body and bodily motion, then he contradicts his own doctrine regarding the reach and sphere of the senses. And if he holds that body is the cause of impressions, he must admit a clear knowledge both of body and of what it can do.

But Hume is represented as stating and refuting with effect "the arguments commonly brought against the possibility of a causal connection between the modes of motion and the cerebral substance and states of consciousness" (p. 76). Hume's argument is as follows: Cause is simply constant conjunction; *à priori*, anything may produce anything; no reason is discoverable why any object may or may not be the cause of any other, however great or little the resemblance between them. Thought *may* therefore be the effect of motion; we may perceive a constant conjunction of motion and thought. Nay, it is certain we have this perception, "since the different dispositions of the body change the thoughts and sentiments." Hence "motion may be, and actually is, the cause of thought and perception."

In this so-called proof Hume evidently felt in a dim way the force of the objection, that, on his doctrine, thought and motion are really identical, that in fact he was only surreptitiously begging for motion, a character which his system denied it—the vulgar realistic view—in order to prove that thought as a distinct thing from motion was yet produced by it. Accordingly we find a clause, as is Hume's manner, quietly inserted to blunt this criticism by the way. "We find," he says incidentally, "by the comparing the ideas that thought and motion are different from each other." Possibly enough that is so; but the difference, whatever it may be, cannot, on Hume's doctrine at least, be allowed to extend beyond the common genus of conscious impressions; and it is, therefore, wholly irrelevant to his argument.

Prof. Huxley must know that all psychologists of note, and of the most different schools, from Hartley to Hamilton, have admitted the fact of "constant conjunction," of bodily organic impressions with conscious sensations and perceptions. But after all that Prof. Huxley has said, as to the place which this organic impression has in the production of the sensation, the questions remain whether it is the cause, or a concause, or merely a condition, on which a higher power comes into play. Prof. Huxley has surely read of the fact of mental absorption—that state of mind in which, when it is occupied by strong emotion, or by intense thought, all the organic impressions may take place, and yet no sensation follow. When a person is writing, the clock may strike in the room, the impressions on ear, nerve, and brain being complete, and yet the next moment he may have not the slightest memory of the sound; certainly, at least, not the consciousness or memory which in ordinary circumstances he would have had. These organic impressions have thus to meet as it were with something other than themselves—something we call consciousness or mind—ere even sensation becomes actual, or a mental fact. This truly reduces them to the place of a simple concause, and shows that there is another factor which they do not necessarily command, and which must concur in the realisation of the very lowest form of mental life. Then these physical antecedents relate to but the lower phenomena of mind. Even if it can be shown that imagination and intellect use portions of the brain, it must at least be admitted that they are there to use them. Can it be said that the apprehension of relations, or the act of generalisation, or volition, is properly spoken of as a conscious *impression?* Does Prof. Huxley imagine for a moment that any careful psychological analyst would place such operations on a level with the consequent of a series of organic movements

Again, what is the real meaning of the phrase that "the operations of the mind are functions of the brain, and the materials of consciousness are products of cerebral activity?" (p. 80). Prof. Huxley quite sees and admits that this is what is called "materialism," and indeed it is nothing else. One ought to thank him for his candour. But I should like very much to know the precise meaning of the statement so characterised. When analysed, it means this: that the nervous current generated by the brain out of food and blood is transmuted into mind; that as a certain molecular motion is transmuted into heat, so a certain nervous motion is transmuted into consciousness or mind. Now it seems to me, on the other hand, that not even sensation, to say nothing of intellect or the apprehension of relations of succession, coexistence, similarity, has been shown to be the transmutation of nervous force. We observe that physical forces are transmuted into each other; we can even quantitatively determine equivalents in this case. But the method fails us the moment we seek to show that or how a state of consciousness is a transmutation of the unconscious. For now we are no longer dealing with forces of the same kind—forces equally objects of consciousness itself—and known to be, to a certain extent, numerically determinable; we are dealing with the unconscious and the conscious; we are trying to bridge a gulf, on the further side of which we have no basis. We have no measure or rule for showing how the unconscious and the conscious are convertible, or that they have any conceivable relation whatever. Besides, even if we get sensation out of nervous force, what of the relations of difference, resemblance, succession, and coexistence among those sensations? Mr. Huxley calls these *impressions of impressions.* This is a very inaccurate expression. An impression of an impression must at least be picturable in the imagination. It is not so here. These relations are discerned by the intelligence; they suppose impressions; their material or nerve-antecedent is not observable, and they can in no way conceivably be referred to physical movements. Further, a physical or brain-force, though it give one definite sensation, or even a series, cannot provide for the pervading unity of self-consciousness. Physical forces, which are perpetually changing, successive and different, cannot be made convertible with the sense of unity which pervades all our consciousness. And further, the consciousness of a series of impressions, even of two impressions, the recognition of this fact or relation, its being in our consciousness at all, implies a standing unity of consciousness, a self or being, one and identical, which may be awakened into conscious life in or through those impressions, but which is in no way made by them—rather, is necessary to their being made or known.

But is Prof. Huxley's conclusion at all consistent with the law of physical energy? According to the law of the transformation of energy, the energy represented by motion or molecular change in matter passes into a consequent, which is also a movement or molecular change. The antecedent and the consequent states are still only forms of molecular change; and the amount or quantity of the antecedent is represented by the amount or quantity of the consequent. There is transformation of energy; but there is no change in the kind of the consequent. Now according to Prof. Huxley, a state of consciousness called sensation, or emotion, or idea, is as much the result of "the molecular changes which take place in that nervous matter which is the organ of consciousness, as the nerve-vibrations are the result of the impact of the light-waves on the retina." At the same time Prof. Huxley holds that the state of consciousness is distinct in kind or quality from the physical movement. It is psychical, or a form of *psychosis* as opposed to *neurosis.* And indeed he must admit a distinction in quality in the two cases. For the physical movement is possible—nay, is actually carried on apart from consciousness; whereas the sensation, the very lowest form of consciousness, is possible, is actual only in consciousness itself. There is all the difference between the fact which depends on observation by eye-sight and the feeling which is self-guaranteeing while it lasts, between the unconscious observed and the conscious felt. But be this as it may, he admits the distinction, as in fact impassable in thought. How is it then consistent to say that the state of consciousness is the effect of the physical movement? Either the law of physical energy is observed, and then we have only a physical movement as the determined result; or it is not, and then we have a state of consciousness, something distinct in quality from a physical movement; that is, we have as the result of the given physical force that which was not contained in the force as a simple quantum of physical energy.

But Mr. Huxley, following, as he thinks, Hume, tells us somewhat singularly that this materialistic doctrine of the origin of mind "contains nothing inconsistent with the purest idealism" (p. 80). In other words, what we call matter turns out in the end to be a purely hypothetical entity, assumed as a cause of certain states of consciousness. The very conception of such an entity is inconsistent with the basis here given; for if our sense-knowledge, indeed all our knowledge, be restricted to states of consciousness called feelings, we are precluded from forming an idea even of matter as an object transcending consciousness, or of anything but states of consciousness, their compounds, and relations among themselves. To speak of "matter" as a cause of our feelings is, on such a theory, meaningless; and such a cause as an inference is impossible. Matter and motion, then, are simply convertible with states of consciousness, in fact, with feelings. And when we are told that these phenomena precede and cause the states of consciousness we call sensations, emotions, thoughts, we say merely that one set of states of consciousness is antecedent and cause of certain others. We have, therefore, wholly given up the dualistic scheme and the aim with which we started, viz., that of explaining the feelings by material phenomena. We now really profess to explain the whole of our conscious states—or mind—by one set of its states or phenomena, viz., those we call matter and motion. But does Prof. Huxley not see the *petitio principii* involved in such an argument? When I am cognisant of the phenomena, matter and motion, have I not assumed consciousness and its states to account for consciousness and its states; or rather, which is worse, have I not assumed certain very elementary states of consciousness—to account for, in fact, to generate the whole contents of mind—in all their complexity and reach—intellect, emotion, desire, volition, and moral sense? This is cutting the knot coarsely with a hatchet. It is not even solving the problem as to how from rudimentary states of consciousness itself, mind can rise to its recognised fulness and complexity—rise, in a word, to that which we call matured consciousness.

J. VEITCH

SACHS'S VENEZUELA

Aus den Llanos. Schilderung einer naturwissenschaftlichen Reise nach Venezuela. Von Carl Sachs. (Leipzig: Veit, 1879.)

NO one who has a liking for natural history should omit to read Dr. Sachs' account of his adventures in the Llanos of Venezuela. German books of travel, though possessing a large amount of solid information, are often rather dry and heavy. But Dr. Sachs' volume is certainly an exception to the rule, and may, we think, be placed, as regards the interest of its narrative, nearly, if not quite, on a par with the well-known works of Bates and Wallace.

The late Dr. Carl Sachs, who was formerly assistant to the great physiologist of Berlin, Emil du Bois-Reymond, and lost his life in an unfortunate accident on the glaciers of Monte Cevedale in August, 1878, went out to Venezuela, not with the ordinary objects of the travelling naturalist, although no opportunity was lost of collecting specimens, but for the especial design of obtaining a better knowledge of that most wonderful of fishes commonly called the electric eel (*Gymnotus electricus*). No more appropriate use could certainly have been made of the "Humboldt-Fund," collected in order to preserve in memory that great naturalist, than the devotion of it to such a purpose. Humboldt's account of the electric eels and the mode of their capture, is among the best known portions of his travels. Nearly eighty years had passed without any naturalist having trodden in Humboldt's footsteps, or having attempted on the spot the further elucidation of the extraordinary properties of this fish, aided by the enormous development which the science of physiology had made since that period.

With this object, therefore, Dr. Sachs left Europe in October, 1876, determined to visit the home of the electric eels in the same streams that Humboldt had found them in the year 1800. To arrive at this destination is not in these days a matter of great difficulty. From Hamburg a swift ocean-steamer bore our naturalist to La Guayra, and a day's ride over the coast chain of the Andes brought him to Caracas, the capital of Venezuela. After a few days' spent in rest in this lovely city and in excursions in the neighbourhood, Dr. Sachs turned his face due southwards, and, accompanied by servants and baggage-mules, rode over the grassy plains, or Llanos, which cover the southern part of the republic. Ten days' travel brought him to the little village of El Rastro, situated on one of the small confluents of the Rio Sisnado, a branch of the Orinoco, the very spot where Humboldt had captured *Gymnoti* seventy-six years before.

Humboldt's account of the mode in which this operation was effected in his days is well known. The Indians "fished with horses." About thirty wild horses and mules from the Llanos were collected and driven into the river. The stamping of the beasts drove the eels out of their hiding-places in the mud into the middle of the stream, where they got under the bellies of the horses and attacked them with repeated discharges of their electric organs. The unhappy quadrupeds rushed out to the banks, but were driven back into the water by the shouts and sticks of the surrounding Indians, until many of them, exhausted by the repeated shocks of the *Gymnoti*, sank to rise no more. The eels thus lightened of their superabundant stock of electricity were easily captured by the Indians.

Such is Humboldt's well-known story. But strange to say the Venezuelans of the present day simply laughed when Dr. Sachs proposed to put a similar plan in operation, and said they had never heard of such a thing. Indeed Dr. Sachs after various inquiries on the subject, was at last driven to the conclusion that fishing for electric eels with horses, as described by his illustrious countryman, must have been quite an exceptional occurrence, and could never have been a recognised custom.

In fact, Dr. Sachs was altogether unsuccessful in inducing the people of El Rastro to procure him electric eels in any way, and, after some rather disheartening attempts, shifted his quarters to the neighbouring town of Calabozo, where he hoped to find better quarters and a more intelligent set of assistants. Here, also, although his offers for electric eels were raised to ten pesos (about 30*s.*) a head, the fishes did not "come in," and poor Dr. Sachs was almost beginning to despair, when he fortunately heard of a certain "Llanero"—General Guancho Rodriguez—the very man for the occasion. How under Don Guancho's generalship these redoubtable eels were at length captured and brought home to the doctor's laboratory at Calabozo, how the necessary experiments were conducted to the wonderment of the good Calabocenos, and how Christmas is passed in that city, is all well told in some entertaining chapters, which will be much appreciated by those who read Dr. Sachs's narrative. It musts suffice for us to say that during Dr. Sachs's stay at Calabozo, which lasted until March, 1877, the main objects of the expedition were fully attained, and a number of important researches carried out, the results of which, owing to Dr. Sachs's untimely decease, will, we fear, never be given to the public.

Leaving Calabozo, Dr. Sachs continued southwards to San Fernando de Apure, and thence down the Apure and Orinoco to Ciudad Bolivar, the capital of this part of Venezuela. From Ciudad Bolivar steam quickly carried him to Trinidad, and thence back to Europe. Of this section of his journey, as of the former part, Dr. Sachs's narrative is full of interest—nor will any one who reads it fail to regret that so promising a life should have come to such an early conclusion.

OUR BOOK SHELF

Education as a Science. By Alexander Bain, LL.D. (C. Kegan Paul and Co., 1879.)

THIS work, belonging as it does, to *The International Scientific Series,* naturally calls for some notice in our columns. We must confine our remarks, however, to the portions which deal with Mathematical, Physical, or Natural Science. The author, though Professor of Logic and English Literature, has already appeared before the public as one of the Editors of *Arnott's Physics,* and has laid down the law in a somewhat peremptory way about Elementary Geometry. We expect to find, therefore, accurate science, and above all, clear and definite composition, in his work.

The first of the following extracts supplied a hint which enables us to make the book review itself. We have taken the liberty of italicising a few words, in other respects we quote *verbatim.*

"Definite descriptions of definite failures, without note "or comment, are a power to punish. When there "are *aggravations, such as downright carelessness,* a "damaging commentary may be added; but in using "terms of reprobation, still more strict regard has to "be paid to discrimination and justice. The degrees of "badness are sometimes numerical . . . . this very "definiteness *literally stated* is more cutting than "epithets."

"The phrase 'cæteris paribus' (other things remaining "the same) is a mathematical coinage, for guarding "against the error of supposing that a course (*sic*) will "produce its effect under all circumstances indiscrimi-"nately."

"The advantages above set forth are such as Mathe-"matics is peculiarly fitted to give, and *without which* "*they* are scarcely ever attained at all. In so far as the "physical sciences unfold similar advantages the way is "paved for them by Mathematics. To this short sketch "of what Mathematics does, we should, for the sake of "clearness, append what it does not do, *and must be left* "*undone,* if we stop with it."

"The *earlier* parts of such subjects as Geometry and "Algebra need the *longest* iteration: the progress should "be at an *accelerating* rate. The higher Mathematics "should not be commenced with immature or incapable "minds."

"How to embody the actual problems in mathematical "language,—for example, the problems of motion in the "scheme of differential coefficients,—is a standing em-"barrassment, not to be met by any of the arts of "ordinary tuition."

"Try a child to lift a heavy weight first by the direct "pull, ..."

"Many trials must be allowed to *get a child into a new* "*shade of vowel,* as, for example, when Scotch (*sic*) "children have to learn the English sound of 'all.'"

"A high wrangler is a man professionally fitted for some "special post involving Mathematics; but, if he turns to "one of the other professions—Law, Medicine, the "Church, the Public Service, *he has incurred* an irreme-"diable waste of human strength."

Having attentively perused these extracts, the reader will probably be prepared to consider the following statement as more than plausible:—

"A purely psychological or metaphysical education "*might* be the worst case of any . . . ." P. G. T.

Life in Asiatic Turkey. A Journal of Travel in Cilicia, Isauria, and parts of Lycaonia and Cappadocia. By the Rev. E. J. Davis, M.A. Map and Illustrations. (London: Stanford, 1879.)

MR. DAVIS resides as Chaplain at Alexandria, and the present thick volume is the result of a tour in Northern Syria, in the summer of 1875. It is surprising that a region so full of interest should have been so little visited, and therefore Mr. Davis's account of what he saw is specially welcome. That there is much to interest in these parts is evident from all that Mr. Davis tells us, and his quiet and painstaking narrative will well reward a careful perusal. The pictures, coloured, from drawings by Mr. Davis, are unusually good, and add greatly to the interest of the volume, which is likely to take its place as a standard reference-work on the region with which it deals.

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.*]

Tempel's Comet

WE find in NATURE, vol. xix. p. 347, some detailed particulars about the return of the comet 1867 II. (Tempel) in the course of the present year. In this article Sandberg's calculations are also taken into account. The real value of his studies about this comet seems to be but little known, as in other instances as well, new calculations are based upon them. In the *Astron. Nach.* Sandberg has only given a very short account of the results of his researches, and it is therefore but natural that their real value must be more or less concealed. Sandberg has published the details of the first determination of the orbit in a special treatise (Specimen Inaugurale de Orbita Cometæ II., 1867, Zwollae, 1869), which seems to be not very much known, and it is not necessary to read much of this work to make sure of the negative value of the calculations.

I now take the liberty to give here some short notices, which are, however, quite proof enough to keep astronomers eventually from unnecessary calculations. Sandberg makes use of all observations, and forms the normal places by the deviations of the different observations from the provisional ephemeris. In doing this he rejects, "prompted by the example of others," (*sic*) all observations which deviate more than 1·5s. in R.A., and 20″ in D. from the *ephemeris,* and not from the average. If, for instance, as with a normal place the mean deviation comes to + 14″ in D., he rejects observations which deviate 6″ from this average, whereas he accepts others to their full value, the deviation of which from the average is nearly 30″ (− 14″ from the ephemeris).

According to this proceeding is the accuracy of his calculations. An observation of Pulkowa deviates, according to his calculation, from the normal difference in D. (− 2″·2) 21″ (18″·9). If he had taken the parallax with correct sign, the observation would deviate 4″ from the average. In the same manner the above-mentioned great deviation from the average is wrong, and does in reality only come to about 3″. These facts are sufficient. It would be easy to mention many others of similar nature, but I do not think it necessary to take up more room in your esteemed periodical.

W. VALENTINER

Mannheim Observatory, March 10

Experiment with a Vacuum Tube

A TUBE like a radiometer tube contained a concave metal disk within the bulb; this disk could be connected with the pole of an induction-coil, and about a quarter of an inch above it was a small wire which could be connected with the other pole. The bulb was exhausted to such a point that a 5-inch spark could not pass from the wire to the metal disk. The wire and disk were connected for several minutes with an induction-coil giving 4½ to 5-inch sparks; although no spark passed between them, the glass on the side of the bulb, which was *just in the focus of the metal concave disk, was melted*, and the pressure of the external air forced the melted glass inward, and a minute hole was formed, of course destroying the vacuum. The diameter of the hole was about that of the finest sewing-needle; it was in the centre of a depression in the side of the bulb about a tenth of an inch in diameter. H. ALFRED CUNNINGTON

Devizes, February 28

[Having succeeded in melting platinum by the heat of molecular impact (*Proc.* Roy. Soc., No. 191, p. 110, and NATURE, vol. xix. p. 137), it is not surprising that the heat is sufficient to melt glass when the focus falls on it. In a paper communicated to the Royal Society in November last, now being printed in the *Philosophical Transactions*, I mentioned that by drawing the focus on to the side of the glass tube by means of a magnet, the glass became heated to redness. In December I wrote to Prof. Stokes that I had melted up a piece of Gorman glass in the focus of the rays, and at the same time I sent a piece of the melted glass to my friend Mr. Sorby, of Sheffield, for microscopic examination, as the fusion *in vacuo* had produced an unusual appearance on the surface of the glass.—WILLIAM CROOKES.]

Tides in the Bay of Fundy

HAVING resided for some years in the neighbourhood of this bay, I am able to give a little information respecting its tides. The bay splits into two at its inner end. One of these branches leads through a narrow channel into the broad basin of Minas. The other, called Chegnecto Bay, is not interrupted by any such contraction, and is therefore more favourable for the formation of very high tides. This bay itself divides at its upper end into two, and one of these, called Chepody Bay, contracts very gradually for some thirty miles inland, forming the estuary of the Petitcodiac River. This is the place where the highest tides occur, and as far as I have been able to learn, their maximum height is 70 feet. A powerful "bore" is formed by the incoming waters. The captain of the steamer *Emperor*, which plied between St. John, N.B., and Windsor, N.S., informed me that the highest tide in any part of Minas Basin was about 55 feet. This would probably be at the head of Cobequid Bay, near Truro. Noel Bay, which is mentioned in Dr. Haughton's letter (NATURE, vol. xix. p. 432), is in Minas Basin, rather more than half way from its narrow mouth to the head of Cobequid Bay. If the range here at ordinary spring tides is 50·5 feet, any one looking at the map and knowing the effect of funnel shaped estuaries, would be prepared to learn that there is a range of from 60 to 70 feet in Chepody Bay and the estuary of the Peticodiac, at strong springs. J. D. EVERETT

Malone Road, Belfast, March 14

End-on Gas-Vacuum Tubes in Spectroscopy

WHILE nothing will give me greater pleasure and confidence in my own worked-out views than to learn, as you intimate in the editorial note (NATURE, vol. xix. p. 400), that so able a working scientist as Dr. Van Monckhoven had preceded me in pointing out the value of *end-on* gas-vacuum tubes, and had sent specimens similar to mine to several observers in England, allow me to inquire where I can find any published account in this country of his tubes, the parties to whom they were sent, and the work accomplished with them? And why, also, if the said tubes were found by those gentlemen as intensely superior for spectroscopic results as mine are proving themselves—they have not yet been described in any of the latest London books I have been able to look into on spectroscopy, natural philosophy, electricity, and instrument-makers' price lists, though the old, pale, imperfectly-lighted, transverse-vision tubes are referred to in all?

Your obliging answer to these questions will evidently be of interest to Dr. Van Monckhoven, as well as myself, while it will also have a far wider and more important bearing for many persons in Scotland. For they, conscientiously striving by all recognised public methods of study to keep up with progress in the south, and not having heard of *end-on* gas-vacuum tubes for the spectroscope before my recent paper on them, would very much like to have thereby and therein a practical demonstration of what a thing, and a good thing too, being, as you say of this, "already well known in England," really consists in; and to what extent, therefore, every member of the community here ought to have similarly known it on the 10th inst., and myself nearer to the same date in 1878, when M. Salleron made the first examples for me, on my then supposed new idea. PIAZZI SMYTH

Edinburgh, February 28

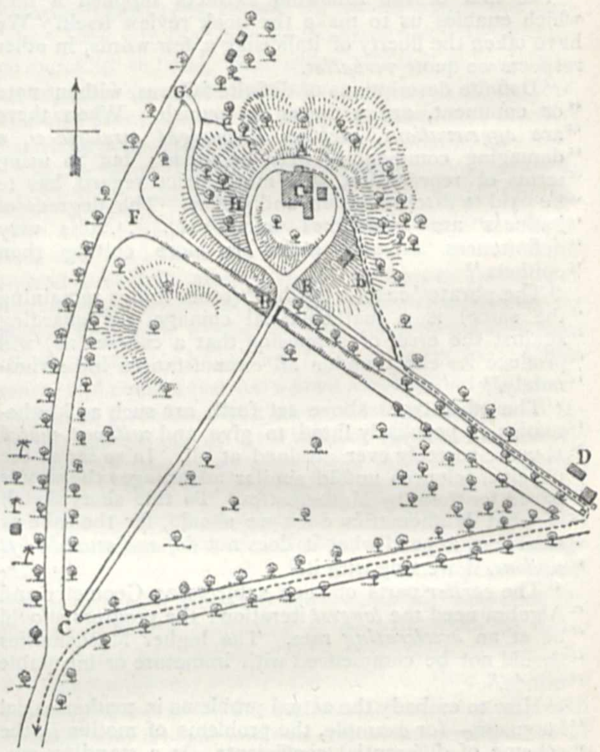
[Dr. Van Monckhoven writes that his new tubes were described to the Belgian Academy of Sciences in 1877, in a note, a copy of which he sends us. He states that he sent some of these tubes to Mr. Dallmeyer, who gave them to various English men of science. They give, he states, about 100 times more light than the ordinary spectrum tubes.—ED.]

Intellect in Brutes

DR. RAE has so fairly disposed of Mr. Henslow's examples of so-called "practical" and "abstract reasoning" that further comment is unnecessary. As, however, the subject of intellect in brutes is on the *tapis*, I will give an instance of sagacity in a dog that finally set at rest any doubts I ever entertained that the difference between human and animal intelligence is one of degree only.

If you have space for it, the accompanying plan will be of great value in describing the circumstances.

Mr. J. W. Cherry, of the Madras Forest Service, was owner of the dog in question, a bull terrier, called "Bully." We



lived in the bungalow (A), the compound of which was bounded south and west by public roads (D C) and (G F C) both leading to the cantonment of Mangalore in the direction C. There were three gates into the compound at (C) (D) and (G), the main approach to the Bungalow leading over a bridge (B), that spanned a branch public road (F D). The compound was filled with trees and shrubs, and bordered by dense lantana hedges, so that with the exception of a portion of the western road at F, neither of the cantonment roads were visible from the bridge, nor could the foot-paths (*a*) and (*b*) be seen thence.

Now Bully had a lady friend (canine) living in the cantonment, and at times she was so attractive that absences without leave on the part of the dog were frequent. After one of these excursions Bully had been brought back, and chained up for the night. Next morning, while his master and I were sitting at early breakfast, it was decided that he should be released, and to effectually stop further delinquency, a peon was sent down to the bridge with orders to intercept him if he started for the cantonment.

Bully was brought in and unchained; he had that unmistakable air of detected guilt deservedly punished, and spent some time in begging for scraps from the table in a most deprecating manner. Shortly, however, he strolled into the verandah, and then down the front steps on to the gravel walk. After wandering about aimlessly for a few minutes, he quietly started off down the approach (A H B). We followed, keeping out of his sight. At the turn of the road Bully met the unexpected apparition of the peon standing on the bridge. In a moment, though not a word was spoken by the man, the dog turned and came straight back to the room, whither we had in the meantime slipped back unobserved, and re-entered it wagging his tail violently and looking exceedingly sheepish. He now lay down and closed his eyes. The cocked ears showed that sleep was mere pretence, and he soon rose again, went out into the front garden, and hunted for buried bones, purely imaginary ones, I believe. His search gradually led him down the hill by the foot-path (*a*),—we keeping him in sight, as before—and he finally reached the road at the bottom. There all disguise was dropped, and he started off for the cantonment. As he neared the spot (F) the peon espied him, and shouted out his name. He turned at once, climbed the hill, and came into the bungalow, where the same farce of repentance was gone through.

Bully now seemed to have made up his mind that escape was impossible; he lay down on a mat in the verandah, and remained there for a long time. But for the persistent cock of the ears we should have imagined the animal really asleep. Mr. Cherry eventually went to his office-room, and I remained in the verandah reading the morning paper, and occasionally glancing at Bully. He lay very still, but once or twice I detected him opening his eyes and raising his head to look round him. Each time he caught my eye he wagged his tail vehemently for a moment or two, and then resorted to his sham sleep.

It may have been for half-an-hour, or thereabouts, that this state of things continued. I then became interested in an article in the paper, and when I next looked up Bully was gone. I called Mr. Cherry, and the house was searched. No Bully. The peon was sent for and interrogated; he had not seen the dog. As a last resource inquiry was made of the horsekeepers down at the stables (D). The reply was—"Yes, the dog had passed through the gate (D) some time before." Taking advantage of my occupation and the absence of his master, Bully had left the house, and taken his way to the cantonment by the only path by which he could have escaped unnoticed by the peon—that shown by the dotted line.

In this necessarily short account I have hardly done justice to Bully's diplomatic powers, but most of your readers will appreciate the intelligence that led the dog to successfully elude the watch set over him.

E. H. PRINGLE

A SMALL English terrier belonging to a friend has been taught to ring for the servant. To test if the dog knew *why* it rang the bell he was told to do so whilst the girl was in the room.

The little fellow looked up in the most intelligent manner at the person giving the order (his master or mistress, I forget which), then at the servant, and refused to obey, although the order was repeated more than once.

The servant left the room, and a few minutes afterwards the dog rang the bell immediately on being told to do so.

Royal Institution, March 14 JOHN RAE

OBSERVING the remarks of Mr. G. Henslow (NATURE, vol. xix. p. 433) in reference to "abstract reasoning" as not to be observed in the lower animals, it has occurred to me that the following facts may have a useful bearing on this subject:—My sister, who lives just opposite to my own house, possesses a cat (now about thirteen years old) whose intelligence is very remarkable. He has the habit of making use of the knocker of a side door, which is just within his reach as he stands on his hind legs, whenever he desires admission. A single knock is tried in the first instance, but if this is not answered promptly it is followed by what is known as a "postman's knock;" if this is not successful, trial is then made of a scientific "rat-tat" that would not disgrace a west-end footman. I should say that "Minnie" holds the knocker in his paw as we should hold it in our fingers, and not by simply tipping it up. How far this practice involves "abstract reasoning" I will not say, but something like an approach to it is suggested, for he was never taught to knock at the door, and adopted the habit some three years ago, evidently to gain admittance, very often to the annoyance of my sister's family, who have occasionally been disturbed in this way at unseemly hours. I should be sorry in thus referring to the sagacity of poor pussy (who is now also somewhat feeble) to reflect upon him by noticing some other of his peculiarities, one of which is his fondness for a little brandy and water and other alcoholic stimulants; but I think what I have referred to may be interesting to Mr. Henslow or some other of your correspondents, and it is within my own knowledge and observation. G. M.

March 15

MR. HENSLOW asks for "cases of purely abstract mental reflection in animals," and in reply I mention a case in Somersetshire of a kitten about half grown, at a house where I was stopping, having mental reflection of some sort.

I was sitting in one of the rooms, the first evening there, and hearing a loud knock at the front door, was told not to heed it, as it was only this kitten asking admittance. Not believing it, I watched for myself, and very soon saw this kitten jump on to the door, hang on by one leg, and put the other fore-paw right through the knocker and rap twice.

The knocker was an ordinary-shaped one fixed in the centre of the door, half way up; the top part of the door was glazed. I saw this performance dozens of times afterwards, and often used to put the kitten outside to see it done. It was never known to knock when any one stood in the garden, but if one went in-doors and shut it outside, in a few minutes came the usual knock.

A sister kitten to this one was never known to knock, but sat on the doorstep and entered when the door was opened, and in nine cases out of ten the knocks were successful.

This kitten was never taught in any way; it would knock at both front and back doors. I should like to know if Mr. Henslow considers this practical or abstract reflection; the result was *practical*. MAURICE BELSHAM

Simla Cottage, Barnes

THE explanations by Mr. Nicols (NATURE, vol. xix. p. 433) fail to convince me that the rats cut the pipes to get at the water. I have seen the edge of joists cut or gnawed about eight inches above the ground, where the rat would have to stand on its hind legs to do it—What was that for? Again, why does our cat scratch the legs of the kitchen table? It seems to me that rats are often like children, they must be doing something to work off the energy within them, and fill up the time, and they often do things without any definite reason. Lastly, if the water is at high pressure especially do they stop to drink the water at all? It also runs in my mind that the rats cut the lead pipes where there was plenty of clean water without doing so.

Glasgow, March 17 W. P. BUCHAN

I BEG to thank Mr. Nicols for his courtesy in supplying the missing links of evidence in the rat cases (NATURE, vol. xix. p. 433), cases which may, I think, be applied with reference to Mr. Henslow's difficulty concerning "abstract mental reflection;" for it seems to me now that the most probable supposition is that the rat-community had learned through experience (likely got accidentally in cutting pipes which obstructed their operations) that such-like pipes at times contain water, and by exercise of reason came to the conclusion that it was worth while to make the exploration in the instances given.

I give the following as told me by my wife—now dead—who personally witnessed the transaction on various occasions:—At her sister's house in Kent a donkey which, when not employed by the children, grazed in a field with some cows, was in the regular habit of acting as follows:—At the usual hour for the cows to come home to be milked the donkey lifted the latch of the field gate, opened and held back the gate (which would otherwise have swung close again) till all the cows passed out, then allowed the gate to shut, and went home with the cows. Of course no one taught the donkey to do this; but the quadruped gave the biped a practical lesson, from which I am not aware that they drew the abstract verbally formulated conclusion that reason may be exercised without rhetoric.

March 14 HENRY MUIRHEAD

I BELIEVE that instances of rats gnawing through water-pipes are frequent. Two have come to my knowledge during the past fortnight. The one instance occurred at the house of a gentleman near West Hartlepool; in the other case a large hole, $3\frac{1}{4}$ inches long, and varying from $\frac{13}{16}$ths of an inch to $1\frac{1}{8}$th inch in breadth, was gnawed in the fresh-water pipe of the screw-steamer *Mary Coverdale*. A portion of this pipe, containing the hole, was cut off, and is preserved by me; it is a stout leaden pipe, a quarter of an inch thick, and with a diameter of $2\frac{3}{8}$ inches. It is very doubtful whether there was any flaw before the hole was begun.

R. MORTON MIDDLETON

West Hartlepool

Distribution of the Black Rat

PERHAPS some of the readers of NATURE may be able to throw some light on the present geographical range of the Black Rat (*Mus rattus*, L.). In the early part of 1877 some individuals of this species came on board the steamship *Lady Frances* either at Bombay or at Rangoon, but, as the captain believes, at the latter port. The animals multiplied on board the vessel, and in August last I had the pleasure of receiving from the ship a living specimen, which was at once forwarded to the Zoological Gardens in Regent's Park, where, I believe, it may still be seen. In a "Catalogue of the Mammals of the Sahara," by my friend, Canon Tristram, F.R.S. (*vide* "The Great Sahara," p. 385), the author states that the "Far el Kla," as the black rat is called by the Arabs, "still maintains its position" in the Algerian Sahara. And I was yesterday presented by Mr. F. Donald Thompson, of Seaton-Carew, with a skin of *Mus rattus* from New Zealand. This example, like those from Burmah, was brought over by a vessel (the *Trevelyan*) which loaded grain at Lyttelton, in the province of Canterbury, New Zealand, where the rats embarked. In August, 1878, Dr. Sclater, F.R.S., was good enough to inform me that "*Mus rattus* has rather an extensive range over Europe and Western Asia," and added, "I fear it would not be possible to state it very exactly." But it is evident that the range of the species is much wider, as it is known to occur in North Africa, British India, and New Zealand; and it is also said, by Prof. Bell and Mr. Macgillivray, to have been carried to America and the South Sea Islands by ships. I should be glad to have further evidence as to its occurrence in Burmah, and it would be also desirable to know if it is found in the Malay Archipelago, China, Japan, or Australia. Dr. Peters, of the Zoological Museum at Berlin, assured me, in June last, that the species was extremely rare, if not actually extinct, in Germany, and showed me the only specimen in the fine collection under his care—an old and faded skin from Hanover. The animal lingers in one old building at Stockton-on-Tees, and there is clearly a possibility of its being reintroduced in many seaport towns through the agency of ships.

West Hartlepool, March 11 R. MORTON MIDDLETON

The United States Fisheries

IN your review of the report of the United States Commission of Fish and Fisheries, you say you are of opinion there is almost no difference between *Salmo salar* and *Salmo quinnat*. My friend Prof. Baird sent me his report some time since, and also forwarded several thousand eggs of *Salmo quinnat* for experiment in the hatching tanks of the Southport Aquarium. The eggs hatched out remarkably well, a very small percentage only being lost, and have proved much more hardy and tenacious of life than any *Salmo salar* I ever had to do with, and very much easier to feed. *Salmo salar* have never done well except when fed on the minute red worms found on the mud in the beds of some rivers and streams (our supply was obtained from the Thames). *Salmo quinnat*, however, live well, and grow faster on the roe of fish (refuse from the fish market), such as whiting, than *S. salar* will on anything. From what I have seen of them I quite agree with Prof. Baird in his admiration of this member of the salmon family, and I share his surprise that it has attracted so little attention among English fish-culturists. It would certainly be a most valuable addition to our food-fishes, stronger, and apparently of more rapid growth than our native species. On the continent, and in New Zealand and other countries, it is most greedily sought after, and each season for several years past an agent has carried from America to France, Germany, and other countries, large consignments of the ova. In England, so far, it appears to have been quite neglected.

Hill Fold, Bolton, March 15 CHAS. L. JACKSON

Plovers in the Sandwich Islands

I CAN vouch for the truth of the visit of golden plovers to the Sandwich Islands mentioned by Prof. A. Newton in NATURE, vol. xix. p. 433. They are very numerous during the winter from November until March. I do not know the scientific name, but I have shot a great many on Oahu and Hawaii.

If it will help Mr. Newton in the solution of the very interesting question he raises I may mention that M. Baillière, Consul-General for France at Honolulu, is in the habit of sending specimens of birds to (I think) the Jardin des Plantes, Paris, where doubtless a specimen might be found.

S. LONG

Hertford, March 15

Unscientific Art

IN the *Graphic* for December 28 there appeared a sketch of a man taking a reading on a marine barometer, on board the *Sarmatian*, during the voyage of the Marquis of Lorne to Canada. To see the scale better by the light of his lantern, the observer is represented as sloping the barometer at an angle of about 30° from the vertical.

JOHN W. BUCK

New Kingswood, Bath

ON THE POSSIBILITY OF EXPLAINING THE CONTINUANCE OF LIFE IN THE UNIVERSE CONSISTENT WITH THE TENDENCY TO TEMPERATURE-EQUILIBRIUM

THE idea of the ultimate final cessation of all physical change and life in the universe[1] has been contemplated by many physicists with some dissatisfaction, and with the desire if possible to find some explanation or physical means by which so apparently purposeless an end is averted, and of avoiding the necessity for assuming in past time a violation of physical principles at present recognised to exist.[2] Several attempts have been made to surmount the difficulty,[3] but apparently with no generally satisfactory result. Having given much time to physical problems having a relation more or less to this question, and having always kept the question itself in view, I should like to submit the following conclusion to the readers of NATURE as an attempt to solve the difficulty, though what I have to bring forward is probably not entirely new, as considerations partially tending towards the same final result have already been published by Mr. James Croll, *Phil. Mag.*, May, 1868, "On Geological Time;"[4] and Mr. Johnstone Stoney, "On the Physical Constitution of the Sun and Stars," *Proc.* of the Royal Society, 1868-69. The groundwork of what I have to suggest may be described in a few words.[5]

Taking a general view of the universe, we may consider it as so much matter, which contains a certain quantity of energy. Let us suppose for illustration the energy of

[1] Thomson, "On the Universal Tendency in Nature to the Dissipation of Mechanical Energy," *Phil. Mag.*, October, 1852; Clausius, Ninth Memoir, *Pogg. Ann.*, July, 1865; see also Tait, "Recent Advances in Physical Science," second edition, p. 22.

[2] The allied idea of the whole universe tending to agglomerate into one mass under the action of gravity, the notion of instability thus involved, all this has something incongruous and unnatural about it that appears to be scarcely in harmony with the orderly working of physical phenomena, and would seem to point to the necessity for some additional explanation.

[3] Grove, "Corr. of Physical Forces," p. 67; Rankine, "On the Reconcentration of the Mechanical Energy of the Universe," *Phil. Mag.*, November, 1852, &c., &c.

[4] Also *Quarterly Journal of Science*, July, 1877.

[5] The same problem was considered by the writer in special reference to Le Sage's theory of gravitation in the *Quarterly Journal of Science* for July last, but my present object is to deal with the question entirely independently of any special theories, and solely on the basis of generally accepted facts, or facts which if not known would be in harmony with or deducible from those which are known.

this matter to be raised to such a degree that the whole forms a gas, consisting of separate or dissociated molecules, filling space uniformly. This would evidently be the result of applying sufficient heat, just as for example a gas consisting of compound molecules breaks up into its elementary molecules when sufficient heat is applied, the molecules being unable to aggregate into groups on account of the expansive action of the high temperature. When the temperature of the gas is lowered the molecules (as is known) commence again to aggregate into groups, *i.e.*, to cluster about common centres in chemical union. So in the case of the universe, in the imaginary instance of an (adequately) extremely high temperature, we should have the entire universe consisting of separate molecules or forming a very rarefied gas, the molecules being unable to aggregate into distinct groups under the action of gravity, owing to the enormous velocities of the molecules. The molecules would simply rebound from each other in straight lines, according to the principles of the kinetic theory of gases.[1] Let us suppose, now, the excessive temperature to fall, or in other words the total energy to diminish. Then the molecules would commence to cluster into groups (forming masses) under the action of gravity, the mean size of such aggregated groups of molecules becoming greater as the temperature is less, and the number of such groups diminishing in the same proportion. At first, by a slight fall of temperature we should have a large number of small groups (or clusters) of molecules; by a further fall of temperature a further clustering of molecules under the action of gravity would occur, *i.e.*, the size of the separate masses would increase and their number diminish. The case is, in broad principle, exactly parallel to that of a compound gas when subjected to extreme variations of temperature, indeed as far as the purely mechanical considerations are concerned, it is only a question of scale.[2] We know that when a compound gas whose molecules possess a high complexity has been heated up to the temperature of dissociation, and the temperature is gradually lowered, then at first only a clustering of elementary molecules takes place; but as the temperature is further lowered, these compound molecules may cluster together to form compound molecules of a secondary order or higher degree of complexity (*i.e.*, molecular clusters of a larger mass). Thus the mean mass of the clusters of molecules in the gas increases as the temperature is lowered, and the number of such clusters (or centres of aggregation) diminishes correspondingly.

We will therefore suppose that the universe has attained a final state analogous to this, *i.e.*, such that the mean mass of a cluster of molecules (a stellar mass) and the number of such clusters (stellar masses) is such as exactly to represent that which must exist by the actual mean temperature of the universe. But it may be said, as far as we are able to appreciate and judge of the universe, it certainly appears as if the entire universe were losing its heat in the ether of space, and that this final state of things (equilibrium) were not yet attained. But it may be urged, in reply, we are judging of the entire universe from the point of view of a single stellar sun to which we belong. It is as if we were to judge of the temperature equilibrium of a gas from the point of view of a single molecule (or of a few others surrounding it), in which case it is certain we should be unable to form an idea of the state of temperature equilibrium of the gas. It is known to be a demonstrated consequence of the kinetic theory that the utmost diversity exists among the velocities of the molecules of a gas, or the temperature from molecule to molecule. In order to have a true idea of the state of temperature of the gas, we must investigate the conditions of a region containing some thousands of millions of molecules (any appreciable region or space actually containing this number). So in order to have an adequate idea of the state of temperature equilibrium of the universe, we should require the mean temperature (state of energy) of a region containing some thousands of millions of stellar masses, not the narrow view we have from one of these, and the velocities of the few we have measured—not to speak of the countless dark suns that may exist in space, and about whose velocities we know nothing. Mr. Croll has pointed out[1] how it is probable that such dark suns may possess exceptionally high velocities, as the bright (visible) suns would naturally have lost in the collisions which developed their heat part of their normal velocity of translation, the translatory motion having been partly lost by conversion into heat. In the parallel case of a gas, it is a known fact that even if the mean temperature of the gas be low (less than normal temperature), some molecules in certain parts must acquire in the accidents of collision enormous velocities, and are thrown into very forcible vibration at the encounters, such that they would become luminous if we were able to visualise single molecules. In other words, if *all* the molecules of the gas possessed the velocities of these single molecules (relatively few in number), the entire gas would appear like a flame. So in like manner, though single stars in the universe may be luminous, it (by analogy) by no means follows that this at all approximately represents the mean condition of the entire universe. This luminous state might be quite exceptional, and the mean temperature of the universe might be exceedingly low for aught we may know. We may happen to be in a part where the mean temperature of the component matter is exceptionally high, as, of course, from the fact of our being in existence, we must be in a part which is suited to the conditions of life. What is there, then, to oppose the inference that the mean temperature of the universe may be such that each stellar mass (or detached portion of matter, glowing or not) on an average receives as much heat from others as it emits itself, in analogy to the molecules of a gas in equilibrium of temperature; and this does not prevent single stellar masses (in analogy single molecules of the gas) from acquiring exceedingly high temperatures, indeed, they would naturally acquire this from the encounters in certain instances, according to the accepted principles of the kinetic theory.

As regards the state of aggregation of the matter of the universe as dependent on the energy, it would clearly in the same way be misleading if we were to attempt to judge of the mean state of aggregation from the point of view of the few masses in our immediate neighbourhood (or the narrow range of the universe overlooked by us). Thus, to recur to the smaller scale illustration of a gas. In the case of the molecules of a compound gas in a state of temperature-equilibrium, it is known that some of these compound molecules (representing a cluster of molecules aggregated about a common centre) must acquire now and then, in the accidents of collision, velocities corresponding to dissociation temperature. The compound molecule is thus broken up into its components at the collision, these components clustering together again in some other part of the gas, the *mean* state of aggregation remaining unchanged. Thus it would evidently be misleading to judge of the state of aggregation of the molecules of a compound gas from the point of view of an inappreciable region, containing a few hundred thousand molecules, which might in the accidents of collision have become exceptionally heated. In order to judge of the state of aggregation of the gas, we must investigate that of an appreciable region, containing some thousands of millions of molecules. So in the case of the universe, it

[1] The deflection from a straight line owing to the feeble action of gravity in the case of single molecules, would evidently be inappreciable.

[2] There is, of course, this detail of difference, viz., that while the aggregation of molecules about a centre in chemical action is limited, the aggregation in the case of gravity is unlimited. We merely apply in principle the same general considerations to molecules aggregated into clusters (lumps) under chemical action, as to molecules aggregated into lumps under gravific action (stellar masses).

[1] *Quarterly Journal of Science*, July, 1877.

would obviously be fallacious if we were to form an estimate of the general state of aggregation from that of the few masses we can judge of in our immediate vicinity; but we should require to know the condition of a region of an extent that we have no chance of overlooking, and under the principles of the kinetic theory, the local variations of the states of aggregation (themselves depending on local variations in the velocities of the masses) would fluctuate within wide limits. In order to have an idea of the actual (mean) state of aggregation, a being would be required that could (on comparative scale) sweep over the universe with the same facility as we sweep through or examine regions in a gas representing a multiple countless millions of times that of the mean distance of the detached portions of matter composing the gas.[1]

We are led to apply the principles of the kinetic theory to the case of the universe not so much as a speculation, but rather as a necessary deduction following from the known principle that detached masses moving freely in space (as the stellar masses are observed to do) and at such distances apart that gravity between the several masses is incompetent to deflect the path of the masses appreciably, must move in straight lines, and have their motions regulated under the mutual encounters in accordance with the principles of the kinetic theory. Only in the relatively near approach of the masses to one another does gravity come sensibly into play and deflect the path, causing under certain conditions rotation about a common centre (double stars), or, perhaps, by almost direct impact, nebulæ with but feeble rotation, &c.[2] To carry the analogy again to the smaller scale-case of a gas, it is there known that the molecules are in some cases feebly impelled towards each other at a near approach, the path of the molecule being thus deflected at its termination, whereby the conditions are given for causing a temporary rotation of the pair of molecules about a common centre, in an analogous way. The relatively vast distances of the stellar masses, compared with their dimensions, would involve, as a rule, an extremely long mean path before the encounters, corresponding to a proportionally long epoch of time adapted to the conditions of life. The apparent extreme simplicity of the means to the end by the application of the kinetic theory to the case would at least seem not to be out of harmony with its truth.

Thus the final conclusion to which these considerations lead would be that the universe has attained its final state of temperature equilibrium (if we set no fixed limit to its past existence), in the sense that if we were able to measure the temperature (or contained energy), of a sufficient number of masses through a sufficiently extensive region, we should find that in every such *equal* region throughout the universe the temperature (or contained energy) would be the same; just as (on a smaller scale) in the case of a gas, if we could measure the temperature of some thousands of millions of molecules in a given region, we should find that though the temperature differed to a practically unlimited extent from molecule to molecule, yet the temperature of every such equal region was the same.

[1] Just as in the case of a compound gas, the *uniformity* of temperature, of states of aggregation, &c., does not apply to the individual unit lumps of matter (molecules) forming the gas [which may be in vastly different states from one to the other]—but to unit *volumes* containing vast numbers of such units; so in the case of the universe, the uniformity of temperature, state of aggregation, &c., would not apply to the unit lumps of matter (stellar masses) but to unit volumes. In fact the universe may be regarded as a larger scale gas, with the difference that the central force producing the aggregated lumps of matter that move as wholes is not chemical action but gravific action. If we imagine (merely for illustration) a being on relative scale situated on a single compound molecule of a gas in a state of normal temperature equilibrium; this minute being would observe vast differences of temperature and of states of aggregation around him (some molecules in scattered parts glowing in a state of dissociation, &c.), and he would form a perfectly wrong judgment of the state of the gas from such a narrow point of view. So the observer connected with one unit lump of matter of the universe (stellar mass) can form no idea of the state of the rest from his narrow point of view.

[2] The occasional flashing out of stars, as if due to some sudden convulsion that might be referred to collision as a suitable cause, is a notorious fact in astronomy; though, from the extremely limited view of the universe that we possess, it would be unreasonable to expect such phenomena to be of frequent occurrence.

So in an analogous way as regards the *state of aggregation* of the matter of the universe, since this depends on the temperature, it would follow, assuming an indefinite past time, that the mean state of aggregation of the matter, like the mean temperature (mean energy), is the same throughout, *i.e.*, the average size of the separate masses, or the number in unit of volume (taking sufficiently large units of volume for comparison) would be equal throughout, though indefinite fluctuations of dimensions would occur from one mass to another, in analogy with the fluctuations of velocity from one mass to another.

It would further follow from the known principle that molecules of different densities (molecular weights) tend forcibly to become uniformly diffused, that by an indefinite past duration of the universe all the matter must be uniformly diffused if (as in the case of uniform velocity and uniform state of aggregation) regions of sufficient extent could be taken for relative comparison. This again resembles in principle the smaller scale case of a gaseous mixture, where it is known that the small detached portions of matter (molecules) are uniformly mixed, only when appreciable regions containing vast multitudes of molecules are examined, but that there is room for considerable local fluctuations of mixture (such as if only a few hundred thousand molecules were examined).

Thus it appears that the kinetic theory, applied to the universe, would have the peculiar characteristics of allowing almost indefinite local fluctuations of temperature, of states of aggregation, and of composition, of the matter forming the universe within regions very extensive, absolutely speaking, but infinitesimal, relatively speaking (*i.e.*, in comparison with the boundless universe), these regions being amply extensive enough to allow an amount of activity and variability of energy adapted to the conditions of life; while at the same time the principles of the theory, from their very nature, involve perpetually recurring and yet indefinitely variable changes within certain localised limits, the constitution of the vast whole (looked at broadly) remaining uniform throughout.

S. TOLVER PRESTON

FRITZ MÜLLER ON A FROG HAVING EGGS ON ITS BACK—ON THE ABORTION OF THE HAIRS ON THE LEGS OF CERTAIN CADDIS-FLIES, &c.

SEVERAL of the facts given in the following letter from Fritz Müller, especially those in the third paragraph, appear to me very interesting. Many persons have felt much perplexed about the steps or means by which structures rendered useless under changed conditions of life, at first become reduced, and finally quite disappear. A more striking case of such disappearance has never been published than that here given by Fritz Müller. Several years ago some valuable letters on this subject by Mr. Romanes (together with one by me) were inserted in the columns of NATURE. Since then various facts have often led me to speculate on the existence of some inherent tendency in every part of every organism to be gradually reduced and to disappear, unless in some manner prevented. But beyond this vague speculation I could never clearly see my way. As far, therefore, as I can judge, the explanation suggested by Fritz Müller well deserves the careful consideration of all those who are interested on such points, and may prove of widely extended application. Hardly anyone who has considered such cases as those of the stripes which occasionally appear on the legs and even bodies of horses and apes—or of the development of certain muscles in man which are not proper to him, but are common in the Quadrumana—or again, of some peloric flowers—will doubt that characters lost for an almost endless number of generations, may suddenly reappear. In the case of natural species we are so much accustomed to apply the term reversion or atavism to the reappearance of a lost part that we are liable to forget that its disappearance may be equally due to this same cause.

As every modification, whether or not due to reversion, may be considered as a case of variation, the important law or conclusion arrived at by the mathematician Delbœuf, may be here applied; and I will quote Mr. Murphy's condensed statement ("Habit and Intelligence," 1879, p. 241) with respect to it: "If in any species a number of individuals, bearing a ratio not infinitely small to the entire number of births, are in every generation born with any particular variation which is neither beneficial nor injurious to its possessors, and if the effect of the variation is not counteracted by reversion, the proportion of the new variety to the original form will constantly increase until it approaches indefinitely near to equality." Now in the case advanced by Fritz Müller the cause of the variation is supposed to be atavism to a very remote progenitor, and this may have wholly prevailed over any tendency to atavism to more recent progenitors; and of such prevalence analogous instances could be given. CHARLES DARWIN

Blumenau, St. Catharina, Brazil,
January 21, 1879

MY DEAR SIR,

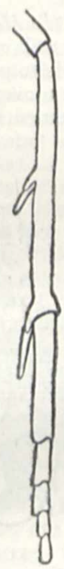
If I remember well, I have already told you of the curious fauna which is to be met with between the leaves of our Bromeliæ. Lately I found, in a large Bromelia, a little frog (*Hylodes ?*), bearing its eggs on the back. The eggs were very large, so that nine of them covered the whole back from the shoulders to the hind end, as you will see on the photograph accompanying this letter, Fig. 1 (the little animal was so restless that only after many fruitless trials a tolerable photograph could be obtained). The tadpoles, on emerging from the eggs, were already provided with hind-legs; and one of them lived with me about a fortnight, when the fore-legs also had made their appearance. During this time I saw no external branchiæ, nor did I find any opening which might lead to internal branchiæ.

FIG. 1.

There is here another locality in which a peculiar fauna lives, viz., the rocks of waterfalls, which are of very frequent occurrence in almost all our mountain rivulets. On these rocks, along which the water is slowly trickling down, or which are continually wetted by the spray of the waterfall, there live various beetles not to be met with anywhere else, larvæ of diptera and caddis-flies, and a tadpole remarkable for its unusually long tail.

The pupæ of caddis-flies living on the rocks of waterfalls (I examined three species belonging to the *Hydropsychidæ*, *Hydroptilidæ*, and *Sericostomatidæ* [*Helicopsyche*]), as well as those living in the Bromeliæ (a species belonging to the *Leptoceridæ*), are distinguished by a very interesting feature. In other caddis-flies the feet of the second pair of legs (and in some species those of the first pair also) are fringed in the pupæ with long hairs, which serve the pupa, after leaving its case, to swim to the surface of the water for its final transformation. Now neither on the surface of bare or moss-covered rocks, nor in the narrow space between the leaves of Bromeliæ, the pupæ have any necessity, nor would even be able, to swim, and in the four species living on such localities which I examined, and which belong to as many different families, the feet of the pupæ are quite hairless, or nearly so, while in allied species of the same families or even genera (*Helicopsyche*) the fringes of the legs, used for swimming, are well developed.

This abortion of the useless fringes in the caddis-flies inhabiting the Bromeliæ and waterfalls appears to me to be of considerable interest, because it cannot be considered, as in many other cases, as a direct consequence of disuse; for at the time when the pupæ leave their cases and when the fringes of their feet are proving either useful or useless, these fringes as well as the whole skin of the pupa, ready to be shed, have no connection whatever with the body of the insect; it is therefore impossible that the circumstance of the fringes being used or not for swimming, should have any influence on their being developed or not developed in the descendants of these insects. As far as I can see, the fringes, though useless, would do no harm to the species, in which they have disappeared, and the material saved by their not being developed appears to be quite insignificant, so that natural selection can hardly have come into play in this case. The fringes might disappear casually in some individuals; but, without selection, this casual variation would have no chance to prevail. There must be some constant cause leading to this rapid abortion of the fringes on the feet of the pupæ in all those species in which they have become useless, and I think this may be atavism. For caddis-flies, no doubt, are descended from ancestors which did not live in the water, and the pupæ of which had no fringes on their feet. Thus there may even now exist in all caddis-flies an ancestral tendency to the production of hairless feet in the pupæ, which tendency in the common species is victoriously counteracted by natural selection, for any pupa, unable to swim, would be mercilessly drowned. But as soon as swimming is not required and the fringes consequently become useless, this ancestral tendency, not counterbalanced by natural selection, will prevail, and lead to the abortion of the fringes.



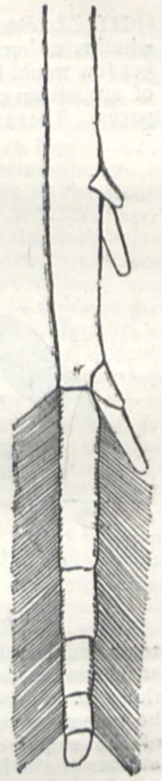


FIG. 2. FIG. 3.

FIG. 2.—Tibia and tarsus of the two pairs of legs of the pupa of a species of Leptoceridæ, inhabiting Bromeliæ. FIG. 3.—The same of a nearly allied species inhabiting rivulets.

I do not remember having seen, in any list of cleistogamic plants, the Podostemaceæ. These curious little aquatic plants, which Lindley placed near the Piperaceæ, Kunth between the Juncagineæ and Alismaceæ, and which Sachs considers as being of quite dubious affinity, cover densely the stones in the rapids of our rivers; on the branches which come above the surface of the water, there are pedunculated, open, fertile flowers; but there are numerous sessile flower-buds also on the branches.

which probably remain submerged for ever; I have not yet ascertained whether these submerged flowers are fertile; if they are so, they can hardly fail to be cleistogamic. FRITZ MÜLLER

A STUDY IN LOCOMOTION[1]

II.

III. *The Paces of the Horse.*—Every one can recognise whether a horse is walking, trotting, or galloping, and yet few would be able to point out the rhythm and order of succession of the movements of the limbs in different paces. These movements, in fact, succeed each other

FIG. 8.—Registering apparatus for horse's paces.

too rapidly for the eye to follow them, and their rhythmic succession is more readily perceived by the ear than by the eye. It is indeed ordinarily by the ear that we become aware of a horse's pace. When at each return of the step (*revolution du pas*) we hear two distinct strokes of the hoofs, we call it an amble, or a trot; three

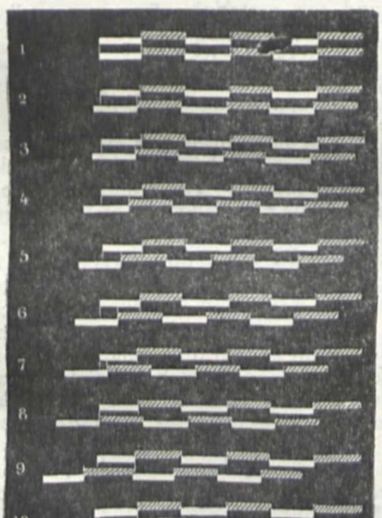


FIG. 9.—Synoptic table of the different paces of the horse, after the classic authors: 1, amble; 5, foot-pace; 8, trot, &c.

strokes unequally separated denote a gallop; lastly, four strokes indicate a foot pace. But these paces may be more or less irregular, variable, or crippled; besides that, when an animal passes in a very short space of time from one pace to another, how shall we decide upon the manner in which the transition is effected? To clear up these points great efforts have been made by horse-

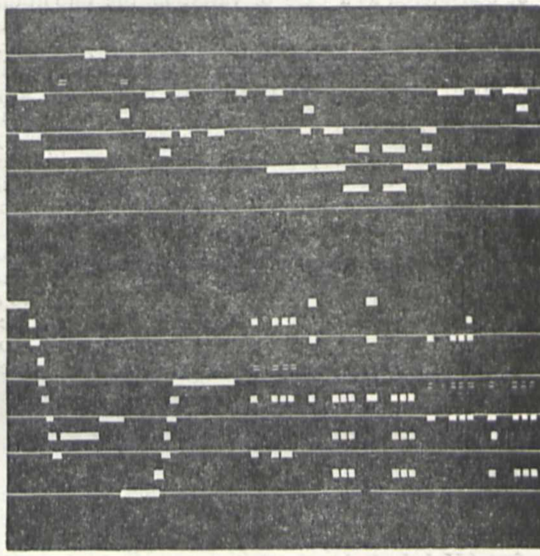
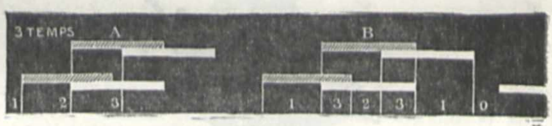


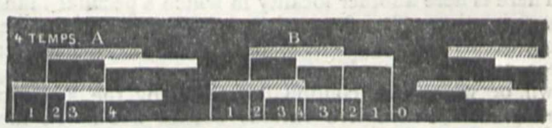
FIG. 10.—Notations of two airs, A and B, executed upon the keyboard of a harmonium.

trainers and veterinary surgeons, to whom the questions involved are of considerable importance.

Now, as I have just said, the ear judges better than the eye as to the rhythm of successive movements, but in order to demonstrate the production of these rhythmic strokes in twos, and threes, and fours, it is essential to know to which foot each separate sound is to be attributed. Ingenious experimenters have attached to the four feet of the horse bells of different tones, but in perfect harmony with each other. Varied melodies or harmonies are thus produced, according to the succession or synchronism of the strokes. But such an arrangement would certainly not give the length of time each foot remained upon the ground, therefore the question of the paces of the horse has not been entirely resolved even by this method. Turn to any special treatises on the subject, and you will see that beyond the amble, the downright



Gallop of 3 steps. A, indications of the three steps; B, indications of the number of feet which form the support of the body at each instant of the gallop of 3 steps.



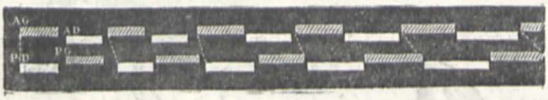
Gallop of 4 steps.

FIG. 11.—Notations of the gallop of 3 and 4 steps.

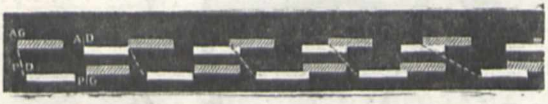
trot, and the three-step gallop, there is, perhaps, not a single pace respecting which contradictory theories are not held. In face of the difficulties of this problem, you will doubtless foresee what will be my conclusion; it will be necessary to have recourse to the graphic method which will solve the question in the simplest manner possible.

[1] "Moteurs animés; Expériences de Physiologie graphique." Lecture by Prof. Marey at the Paris meeting of the French Association, August 29, 1878.

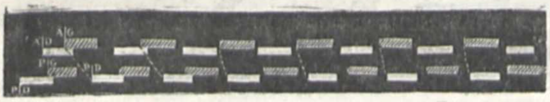
Let us take up the question at the point to which it has been brought by the experimenters to whom I have just alluded. The succession of the movements of the horse's legs, since it is rhythmical, and since also we produce



Transition from the trot to the foot-pace.



Transition from the trot to the gallop of 3 steps.



Transition from the gallop of 3 steps to the trot.

Fig. 12.—Transitions between different paces.

from each foot a different tone, forms a sort of music. Now this music is very simple, because it is only composed of four tones. The following arrangement will

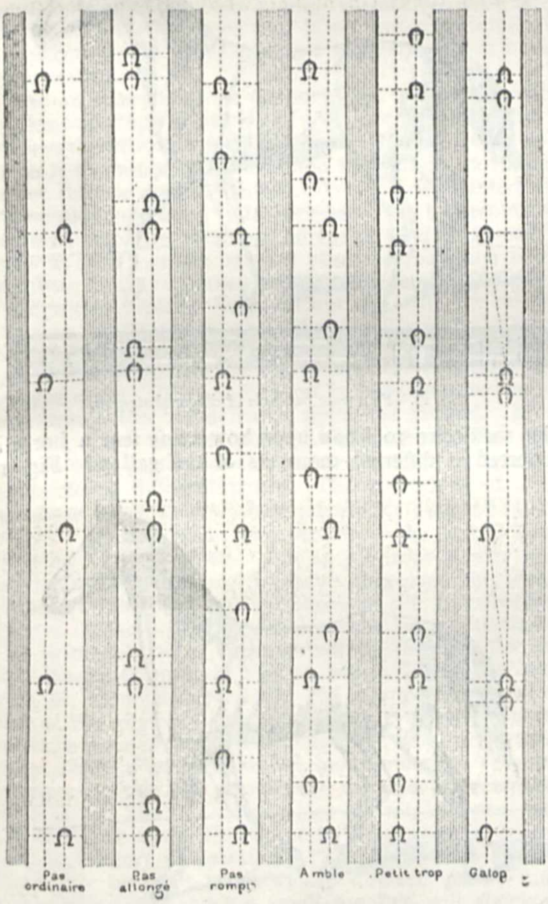


Fig. 13.—Diagram of the tracks of horses in different paces.

permit us to obtain the notation of this music, written by the horse himself. Under each of the horse's shoes we place a bag (*ampoulle*) full of air, which, by means of a tube is connected with another similar small bag, which, by its changes in bulk, acts upon a pencil. When the horse plants its foot upon the ground, it raises a pencil, which remains raised as long as the foot is kept down. Four pencils are placed in position, connected with the four under the horse's feet, and these pencils placed in a right line parallel with the axis of the cylinder, spontaneously trace the succession and duration of each setting down of a foot.

Fig. 8 shows the arrangement of the apparatus. The four limbs of the horse bear four india-rubber tubes, which converge to the hand of the jockey, and so to the inscribing apparatus on the cylinder which he holds.

The results thus obtained by the different paces are shown side by side to the number of ten in Fig. 9. Each foot, as in musical notation, has its characteristic sign, by the height of which the symbol of its stay on the ground is traced. We will agree that the fore feet shall mark upon the lines above, the rear feet upon the lines below. Now the horse can, with his four feet, execute the most rapid movements, and yet nothing will be wanting from the inscription. And in the first place, in order to give you confidence in the employment of this method, let us show you how it solves a problem still more difficult. When a clever pianist passes his fingers over the keyboard, who could describe the movements that he executes, say what note has been touched first, and for how long, then what notes followed, together or separately, with their rhythms and tones? But let us write down these movements, and when they are fixed upon paper we can analyse them with ease.

In the apparatus which I use, the keys act by pressure upon the bags of air connected by tubes with other bags, which cause pencils to move. See with what ease these little pencils reproduce all the movements of the fingers passing over the mute keyboard; you will presently see the instrument in operation. I present to you, first of all, what it has just written (Fig. 10). Observe these notes drawn up in gamuts and arpeggios, these varied accords, these changes of tone where sharps and flats reveal themselves by strokes drawn longitudinally. This instrument, which is now in action for the first time, has been constructed by our colleague, M. Tatin, whose skill has already excited the admiration of all who know him.

And now that you no longer doubt, I trust, the faithfulness of the method, let us analyse the tracings on the diagram I have just now shown you (Fig. 9). In order to understand it fully, let us borrow the ingenious idea of Dugès, and compare the horse or any other quadruped to two bipeds walking one behind the other. If the two walkers execute the same actions at the same time, that is to say, if they both simultaneously raise and set down the right foot and then the left, it is the *amble* (No. 1) which is produced. The ear only detects two sounds at each combined step, because two feet touch the ground together. This is an example of *lateral bipedal* movement. If the rearmost walker has half finished the resting of one of his feet when the foremost walker plants the same foot on the ground, it represents the *foot-pace*. Here the four footsteps are separate, and the ear distinguishes four equidistant sounds; the order of succession would be (commencing to count by the right foot), *foremost right*, *rearmost left*, *foremost left*, *rearmost right*. Let us imagine that the rearmost walker makes movements absolutely the reverse of those of the walker in front, that is to say, that one of the right feet strikes the ground when the other right foot rises from it, we shall then have the trot. Two feet will be always associated and will give but one sound, and these feet will exemplify *diagonal bipedal* motion.

Such, then, is our knowledge of the rhythm of paces, or at least the points upon which all are agreed. But if we desire to gather from divers authors the definition of more complicated paces, intermediary to those we have just described, we shall only find, as I have before stated, a number of contradictions between writers holding different views. In the most rapid pace, the gallop, that of three steps,

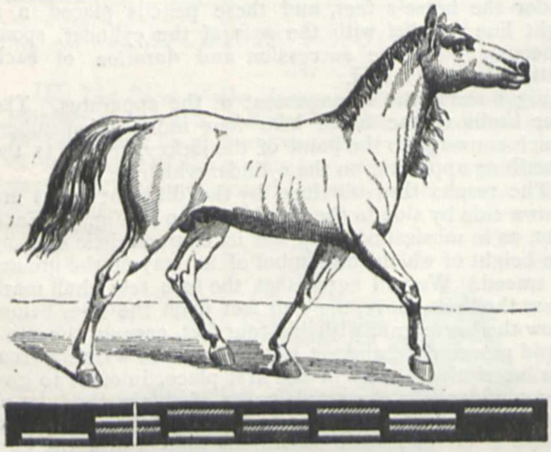


Fig. 14.—Trotting horse represented at the moment of diagonal support.

for example (Fig. 11), the first step is with the hind-foot, upon which the horse falls back after it has raised itself

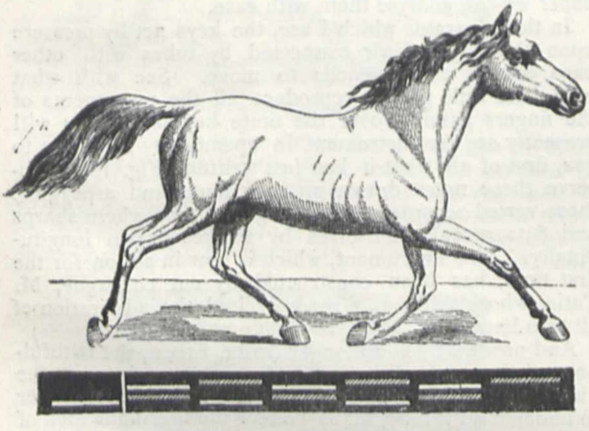


Fig. 15.—Trotting horse at the moment of suspension.

from the ground; then the other hind-foot and fore-foot which is associated with it diagonally fall together, and

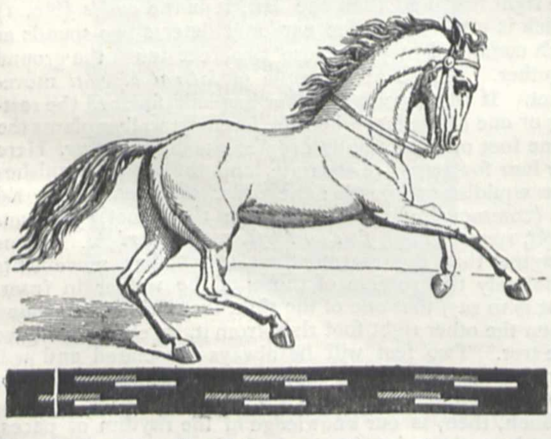


Fig. 16.—Horse at a gallop of three steps. Moment of the first step.

form the second step. Lastly, one hears the other fore-foot fall, and this is the third step.

The gallop of three steps may be distinguished in two forms; the right-hand gallop, in which the right-hand

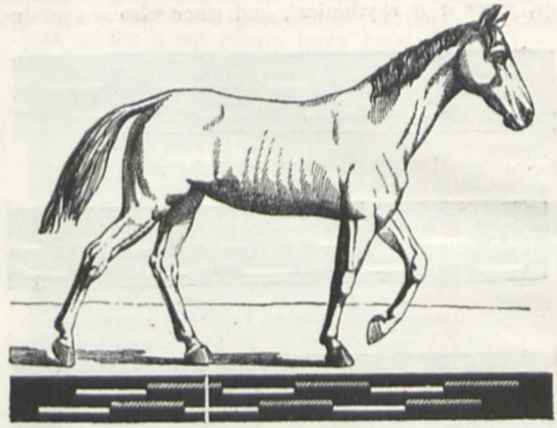


Fig. 17.—Foot pace, with effort of traction.

hind foot reaches the ground last; the left-hand gallop, in which the left fore-foot is the last to reach the ground.

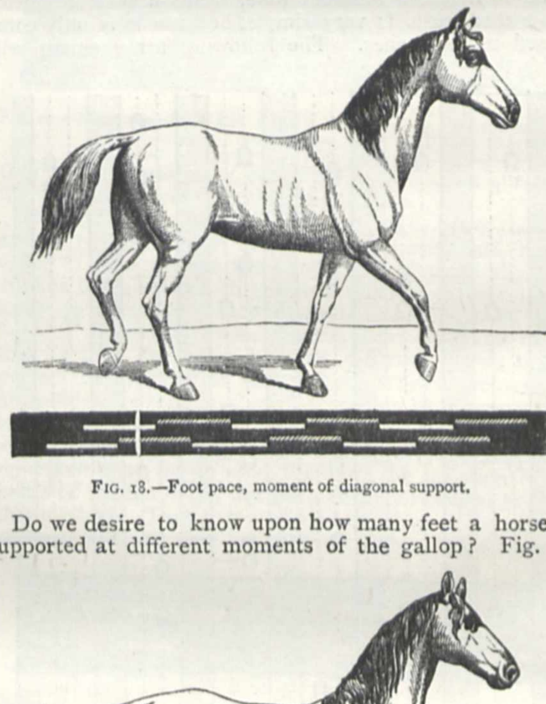


Fig. 18.—Foot pace, moment of diagonal support.

Do we desire to know upon how many feet a horse is supported at different moments of the gallop? Fig. 11

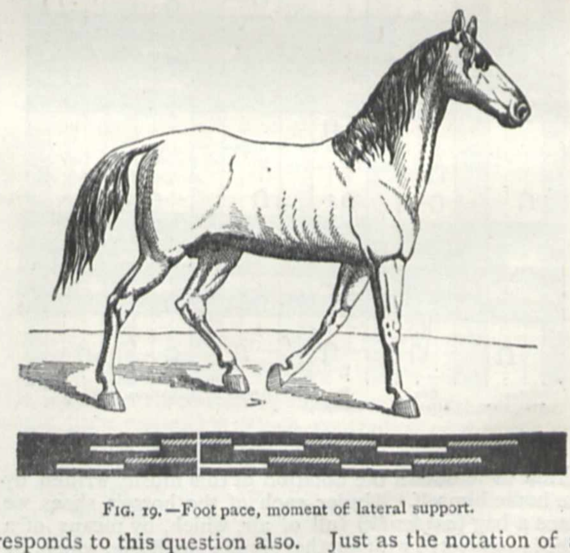


Fig. 19.—Foot pace, moment of lateral support.

responds to this question also. Just as the notation of a piece of music shows how many fingers rest at once upon the notes on the keyboard, in the same way Fig. 11 shows that the horse, at the moment when he falls back upon the earth, is supported only by one foot; then, when the two diagonal feet strike the ground together in their turn, the horse has at this moment a triple support. Without the notation of paces we had certainly failed to distinguish this series of supports.

The gallop of the racecourse used generally to be considered as a pace of two steps, in which the horse struck the ground alternately with the two fore-feet and the two hind-feet. This gallop shows itself in the notation as a pace of four steps; the tracing dissociates the two fore-feet and the two hind-feet, although they follow each other at a very short interval.

The transition from one pace to another, impossible to determine by direct observation, is clearly inscribed in Fig. 12.

IV. *Artistic Representation of the Horse and other Animals.*—The artistic representation of animals requires a special and varied acquaintance with their peculiarities. Nothing can replace the patient study by which the painter or the sculptor acquires an anatomical knowledge of the limbs of animals and the aspects which they assume in different positions. But if the painter or the sculptor wishes to animate his work, if he wishes to show the horse putting forth its efforts in powerful traction, or to represent it urged rapidly forward in the race, it is necessary to have an exact acquaintance with different paces.

That which is true of the horse is equally so of other animals; but all present among themselves such great analogies in this respect, that if we are acquainted with the paces of the horse, we can represent those of any other animal.

The summary analysis we have just now made of the rhythms of steps in each pace is not yet sufficient to express the attitudes which represent them; we have as yet only examined in connection with these movements one of the two essential ideas. We are acquainted with the relations of time; it is necessary also to become acquainted with the relations of space, that is to say, to know at each moment in what place to find each of the members raised or planted upon the ground. Obliged to abridge this already long discourse, I will not tell you how one determines graphically the phases of the movement of a foot that is raised, but I will show you summarily how we determine the place where each foot is brought down. This indication is furnished by the imprint which the horse leaves upon the ground. M. de Curnieu, Capt. Raabe, and M. Lenoble du Teil have studied with particular care these imprints or tracks of the horse at different paces. The smooth sand of the sea-shore presents a surface admirably adapted for this study. Persons who have acquired the habit, easily decipher such imprints. But in order to render them easily read by every one, we have conceived the idea of giving a different form to the shoes of the fore-feet from those of the hind-feet by furnishing the latter with clamps. The principal paces represented by their tracks have been thrown together in Fig. 13, which I have borrowed from M. Lenoble du Teil. In combining with the idea of the rhythms, that of the place where each foot would be planted, the errors of attitude which disfigure so many *chefs d'œuvres*, would be avoided. You will, perhaps, say that few persons are capable of recognising faults of this kind. On this point one might repeat what Baron Dupin said with respect to perspective in its relation to the art of painting. "In proportion as exact knowledge becomes more widely diffused, many faults which to-day only shock a small number, will shock the general public, and artists will no longer be able to perpetrate them with impunity."

Those artists who at the present time make such praiseworthy efforts for the correct representation of the horse, would find great assistance from making use of the notation of paces. Let us see in what way. Take, for example, the notation of *the trots*. We all grant, in the first place, the possibility of dividing each step into a series of successive instants, ten or twenty, for example. At each of these instants the horse will have a different attitude, but throughout the time the limbs diagonal to each other will be executing the same motions at the same time.

Let us take one of these instants at random and mark it by a vertical line (Fig. 14). The notation shows us that at this instant the right hind-foot and the left fore-foot are planted upon the ground, but that the right fore-foot and the left hind-foot are still raised, and are about to be set down. That is exactly as represented in the figure.

Upon another notation (Fig. 15) we have chosen another instant, that in which the horse is suspended in the air, and when the hind-feet have already quitted the ground, whilst the fore-feet do not yet touch it.

Let us pass on to the pace of the gallop (Fig. 16). The moment chosen is that in which the horse falling back upon the hind-foot has just made his first step. Two limbs in diagonal relationship are about to strike the ground at the same time, namely, the right hind-foot and the left fore-foot, represented as already being lowered towards the earth. As to the right fore-foot, that will strike the ground last, accordingly it is shown as still the furthest removed from the earth.

With regard to the foot pace, which is the most difficult to explain, three instants have been selected on the notation: 1. That where the hind-foot is about to be raised, and where the fore-foot is in the middle of its elevation (Fig. 17). At this moment there are three feet at rest, which only takes place with horses when making an effort of traction. 2. The moment in which the two diagonal feet are raised, the one having just quitted the ground and the other being about to be placed upon it (Fig. 18). 3. The moment when the animal, supported by two feet on the same side, is raising one of its fore-feet and is about to set down one of the hind-feet on the same side (Fig. 19).

These pictures have no other pretensions than to be correct as regards the position of the members; it would be the artist's duty to add elegance of form. But is it not something to have a simple and sure means of representing a horse in any pace and in any phase of the steps in that pace? The employment of the graphic notation would give to the artist the double advantage of representing the paces with truthfulness and of varying them to an extent almost illimitable. Now, imperfection in art is not displayed solely by errors that may be committed, for too often an artist who is thoroughly acquainted with a correct attitude repeats it with regrettable monotony.

(To be continued.)

GEOLOGY OF NATAL AND ZULULAND

SOME years have now passed since Mr. Griesbach gave to the Geological Society his paper and map illustrating the geology of Natal and the borders of Zululand. Passing events now give to his investigations the greatest interest, not simply due to the possible light that may be thrown on unsolved, or partly known, problems by the sojourn in that area of the contingent that has lately left our shores, but from the fact that the safety and success of our forces in great measure depends on the surface contour and physical character of the country to be traversed, which are necessarily directly dependent on its geological structure.

Few can have studied a geological map, without noticing the close connection between long lines of escarpment and belts of level plains with particular rocks, or noticing the marked uniformity in direction of strike of geological formations over large portions of the earth's surface; rock series after rock series plunging beneath its neighbour, and disappearing from view, only to be succeeded by higher and higher strata above them. No better example of this could well be found than the map of Natal of Mr. Griesbach; the rocks all run parallel to the sea-coast, striking into the Zulu territory, and dipping steadily the one after the other into high ground, forming the watershed between the Indian and Atlantic oceans. The eastern or seaward belt about fifty miles in width, consists of ancient mica-schists, resting on granite and gneiss seen at the bottom of the deeper valleys, the whole surmounted by the "Table Mountain Sandstone," of carboniferous age, forming extensive plateaux, lying perfectly flat on a horizontal surface of clay-slate, and broken by lines of fault, into a series of steps, plateaux rising above plateaux, with precipitous sides to a height of 2,300 feet between the sea and Pietermaritzburg, where the country again descends to 2,080. These tablelands are covered with extremely poor soil, supporting a dense grass vegetation, on which feed numerous herds of cattle; not a shrub occurs to enliven the endless uniformity of the scene, broken only by the ravines formed by the rivers cutting down through the sandstones to the granite and old rocks beneath, often forming precipitous cliffs several thousand feet high, the vertical drop, from the Krantzkop Mountain to the River Tugela being nearly 3,800 feet. The top of this mountain is composed of melaphyre; these melaphyre greenstones contain copper ores and strike south-westwards to the Ingeli range in Kaffirland.

At Pietermaritzburg, the next belt of country commences, the town being built on the basement beds of the Karoo formation, belonging to the *Dicynodon* beds of South Africa, of triassic age, the name being given from the "karoos," or immense plains of the interior, forming the largest part of South Africa, including the elevated tract of "Kalahari, the Free States, and the Transvaal, as well as the country to the north, as far as Limpopo." They are present in the Zambezi, and rise to a height of 12,000 feet in Mont-aux-Sources, in the Drakenberg Mountains. The base of the Karoo series rests unconformably on the carboniferous table mountain sandstone, and consists of large angular blocks of transported granite, greenstone, and gneiss, in a matrix of clay and grit. They occupy a large area, and pass under plant-bearing shales. These boulder beds have been ascribed a glacial origin by Dr. Sutherland, Surveyor-general of Natal. Mr. Griesbach points out the overlying plant-beds correspond to the plant-beds of Southern India, associated with *Dicynodon* remains, and also resting on a boulder bed (Talchir group).

The great Karoo plains, Dr. Grey is inclined to regard as the bed of an inland sea; salts of soda predominate largely in the salines of the soil, and assist in forming the "background" of this region (sandy soil, with salt, carbonate of soda, and some salts of magnesia and alumina). Its surface forms the sweet-grass country of the Dutch "Zout-Veldt," yielding the valuable Karoo plant (*Adenchæra parviflora*). In this tract the climate is most salubrious, and the higher the country ascends the more fruitful is the ground. The yellow wood flourishes, wheat and European fruits flourish, and the cold of the winter, though not so severe as that of northern Europe, braces the European settler, and agrees with his constitution.

Fringing the Natal shore, there is a narrow belt of the Karoo formation, resting unconformably on the table mountain sandstones and older rocks, so that the latter form an exceedingly low and flat-topped anticlinal arch, throwing off the Karoo beds on either side. Landward these rise to the Drakenberg, seaward they have for the most part been denuded away, though their presence in Southern India points to the former extension over what is now the Indian Ocean of a series of lakes fringed by lands covered with plant growths, extending over Southern India, and parts of South Africa. The investigations of Mr. Blanford in Southern India support the views of Prof. Huxley and Mr. Sclater as to the existence of an extending submerged mesozoic continent, "Lemuria," which was shadowed forth in Mr. Darwin's researches on coral reefs. Mr. Blanford comments strongly on the great relation between the plants of the Indian and Australian (New South Wales) coalfields, many of the species being identical, the two localities being no less than 5,550 miles apart. In India these plant-beds rest on the supposed glacial (Permian?) beds of the Talchir group, the included scratched blocks being often forty-two feet in circumference.

The Karoo boulder bed is described by Dr. Sutherland as containing well-scratched blocks, inclosed in a material which has since been metamorphosed, and resting on scratched old silurian sandstones. The characters of the various members of the Karoo series is well capitulated by Prof. Rupert Jones in Mr. Ralph Tate's paper on South African jurassic marine mollusca, the sequence being: Stormberg beds (Huxley); Beaufort Beds; Koonap beds, and Ecca beds. The Beaufort beds most closely correspond to the Dicynodon beds of India, the boulder beds in both countries, according to Mr. Blanford, being pre-triassic, and he carries back his Indo-oceanic continent to Permian times, and extending up to a late jurassic epoch—South Africa, India, and Australia being connected at the early part of the period, Africa and India, up to the end of the miocene.

In 1824, some caves called *Izinhluzabalungu* (white men's houses) were discovered by Mr. Fynn to be fossiliferous; the name, given by the natives, was due to shipwrecked sailors having taken up their abode in them. In 1851 Capt. Garden had his attention called to these fossils, especially some gigantic *Inocerami*, two feet by one foot, by his servant, named Thomas Souton, a Private in the 45th Regiment, after whom one of the fossils obtained was named by Mr. Baily, who examined them at the request of the late Prof. Forbes. The deposit occupies a small tract on the south end of the colony, and, as Mr. Baily pointed out, may be correlated with the lower cretaceous of Southern India, one species *Pecten quinquecostatus* being common to the English greensand. The investigations of Mr. Griesbach have largely added to the number of the species, and supported Mr. Baily's conclusions, twenty-two of the species occurring in India, thirteen being peculiar. Another patch of cretaceous rocks occurs at St. Lucia Bay, in Zululand, resting unconformably on the Karoo strata.

At the close of the jurassic period, the Indo-oceanic continent was submerged beneath a shallow cretaceous sea, surrounded by coasts, covered with vegetation, extending from India to Natal. At the close of this epoch elevation commenced, and is probably still going on, as raised beaches, coral reefs, and oyster banks may be seen twelve feet above the sea. Through this action the Port of Durban must inevitably be silted up, which will be the fate of most of the ports on this coast, except the large port of Delagoa Bay, which is naturally clean swept by the north and south Mozambique current, which has gradually hollowed out the Bay.

CHAS. E. DE RANCE

OUR ASTRONOMICAL COLUMN

PRIZES OF THE PARIS ACADEMY.—At the annual public sitting of the Academy of Sciences at Paris, last week, the medal on the foundation of Lalande was awarded to M. Stanislas Meunier for his researches on the constitution of meteorites, which, in the opinion of the Commission appointed for the consideration of claims, have led to results that occasion surprise, but at the same time appear justified by M. Meunier's investigations. Astronomers had followed with interest the labours of M. Daubrée, who has contributed so much to establish a connection, little expected, between these bodies falling from the heavens, and the lower strata of our globe, and this circumstance has caused an increased amount of attention to the researches of his pupil and follower, M. Meunier, who finds by his recent work that the analogy alluded to is not confined alone to mineralogical constitution, but that it is extended to the relation which these cosmical materials, disseminated in space, present when compared amongst themselves, as is done for the constituent rocks of our globe. The Commission considered that M. Meunier had reason to conclude, from his experiences, that all these masses once belonged to a considerable globe, like the earth, of true geological epochs, and that later it was decomposed into separate fragments, under the action of causes difficult to define exactly, but which we have more than once seen in operation in the heaven itself. Such a conclusion, it is remarked, adds greatly to the interest attaching to these "minute stars:" the astronomer, once occupied only with their motions and their probable distribution in space, finds himself confronted with a sidereal geology, as he already was under the necessity of having regard to celestial physics, celestial chemistry, and celestial mineralogy. The medal is awarded with the view to encourage M. Meunier to follow up his studies, so interesting in regard to the constitution of the solar system.

The Valz prize was adjudged to Dr. Julius Schmidt, for his great chart of the moon, and the immense labour which its production has involved during a period of thirty-four years. The report of the commission for this prize contains a brief *résumé* of earlier work in this direction, concluding with a remark, the truth of which will be sufficiently obvious, that Dr. Schmidt's work, "aujourd'hui déjà si précieux, servira dans l'avenir de base à de nombreuses investigations, et nous pensons que le temps ne fera qu'en accroître la valeur."

The Damoiseau prize, first proposed in 1869 for a revision of the theory of Jupiter's satellites, discussion of the observations, and redetermination of the constants involved, with the formation of tables of the satellites, has been renewed without effect in 1872, 1876, and 1877, and is further remitted to 1879. The value of this prize is 5,000 francs.

FAYE'S COMET.—Dr. Axel Möller, continuing his elaborate investigations on the motion of Faye's comet, which he has conducted with so much success during the last twenty years, has communicated to the Stockholm Academy elements and an ephemeris for the next appearance, which it now appears will not take place under such favourable circumstances for observation as has been stated elsewhere. From November, 1874, to April, 1876, the distance of the comet from Jupiter was less than twice the mean distance of the earth from the sun, and in June and July, 1875, was not more than 1·5; the effect of this has been to retard the next perihelion passage by more than thirty-eight days, or to delay it till January 22, 1881, under which conditions the theoretical intensity of light can at no time be half as great as at the date of discovery by M. Faye in 1843. At the last return only four observations appear to have been secured, owing to the comet's excessive faintness, three by M. Stephan, at Marseilles, on September 3, November 28 and 30, and one by Dr. C. H. F. Peters, at Clinton, U.S., on December 23; so admirably had the calculations of the perturbations during the preceding revolution been effected by Dr. Axel Möller, that M. Stephan's first observation gave the comet's position only *four seconds of arc* from the predicted place. The chief disturber of the motion of this comet is, of course, the planet Jupiter, but Dr. Möller takes into account also the effect of the attraction of Venus, the earth, Mars, Saturn, and Uranus. The amount of perturbation during the actual revolution is greater than in any other since the comet's discovery. The next perihelion passage takes place 1881, January 22·665, G.M.T., the comet at this epoch moving in an ellipse with a period 56·526 days longer than at the previous perihelion passage in July, 1873. Dr. Axel Möller's ephemeris extends from 1880, July 1, to 1881, January 1; the comet will be nearest to the earth on October 3, distance = 1·09, and situate at this time some ten degrees south of α Pegasi.

BIOLOGICAL NOTES

FOSSILS OF THE AMAZONIAN DEVONIAN.—MR. R. Rathbun, late of the Geological Survey of Brazil, has published a list and description of the Brachiopods of the three Amazonian-Devonian localities, showing that of the twenty-one species recorded from the Mæcurú, thirteen were also found on the Caruá, including all the commoner species of the former. There is not so close a relationship between the Ereré fauna and the Mæcurú. Several of the commonest Mæcurú species do not occur at Ereré, and *vice versâ*. At Ereré there are five species of Lingula, four of Chonetes, four of Spirifera; at Mæcurú there are no species of Lingula, four of Chonetes, and six of Spirifera. Several of the Amazonian shells are identical with those of the North American Devonian; three in the Mæcurú, and Caruá, viz., *Spirifera duodenaria, Amphigenia elongata*, and *Strophodonta perplana*. Two forms of these are only known in the Corniferous limestone and Schoharie grit of North America. The Ereré beds are more closely related by their fossils to the Hamilton group than to any other North American group. In Pará, on the whole, there is the same general succession of species as in the Corniferous and Hamilton groups of North America, and a similar intermingling of forms. The lamellibranchs are not published yet, but it appears probable that many species are identical with New York State forms. Among the Trilobites are species of Homalonotus, Phacops, and other genera. (*Proc.* Boston Society of Nat. Hist., 1878.)

AUSTRALIAN FOSSIL CORALS.—The subject of Australian fossil corals has occupied much attention among palæontologists of late years. The investigations of the forms found in the deep sea has brought the tertiary forms into prominent notice. Following in the line of the researches of Prof. Duncan, the Rev. J. E. T. Woods has recently published (*Journal* and *Proceedings* of the Royal Society of New South Wales, vol. xi., 1878) a paper on some Australian tertiary corals, in which he describes some new species from Muddy Creek, near Hamilton, in Western Victoria. Some of the species are very interesting, and the author concludes his paper by asserting:—1. That there is no species of the genus Caryophyllia living in the Australian seas, or to be found fossil in its rocks. 2. That there are three well-marked and peculiar forms of Deltocyathus. 3. That of the two species known of Sphenotrochus in Australia, one is still living (*S. variolaris*, n.s.) at a depth of seventy fathoms. 4. That there are two fossil analogues of the living *Conocyathus sulcatus*, which itself is supposed to be identical with a European miocene form. 5. That there is a fossil form in the miocene rocks of Australia, of the cretaceous genus Smilotrochus. The Rev. W. Woods mentions that he is preparing a monograph of the recent species of Australian corals.

HERRING CULTURE.—Dr. H. A. Meyer has published an interesting contribution to the natural history of this important fish, as part 1 of a series of short papers to be issued by the Commission for the Scientific Investigation of the German Sea (Berlin, 1878). In this he supplements his previous researches into the influence of the temperature on the development of the spring herrings' eggs. It may be remembered that in the large report published by the Commission it was found that the escape of the herring from the egg, in the case of the autumn herring, could be very considerably delayed by keeping the eggs in very cold water; and now experiments made with the same object in view prove that in this respect there is very little, if any, difference in the behaviour of the autumn and spring spawnings. As in the previous experiments eggs artificially fecundated were those operated with, and while some of the eggs were exposed to the salt water at its ordinary temperature at Kiel, others were placed in a wooden refrigerator, into which the same sea-water, but cooled down to the desired degree, was admitted. A most necessary precaution was keeping the eggs from being heaped together, as they then almost invariably became mouldy. Another series of experiments was made to test whether the eggs exposed to the very salt waters of the North Sea would ripen quicker or slower than those exposed to the less salt water of the Baltic, but the time of the development, the temperature of the waters being the same, was found to be very slightly, indeed hardly perceptibly different. A third series of experiments were of a very interesting nature, supplementing those already made, as to the rearing of the herring from artificially fecundated eggs. So far as is known, no one has yet succeeded in rearing the young herring, and even Dr. Meyer's repeated attempts broke down, owing to the impossibility of stopping the formation of the hyphæ of some fungus, and also in some measure to the difficulty of obtaining suitable food. Very soon after the yolk was altogether consumed they would die, so that most of the experiments on their growth were made on specimens freshly caught from time to time. Once he succeeded, in the spring of 1878, in rearing a few until they attained the size of 72 mm. However, as the result of these experiments, a great deal of insight has been obtained into the food—at first of almost microscopical dimensions—which the young herring consumes, and as to the enormous voracity of the little fish.

MADAGASCAR FORMS IN AFRICA.—At a recent meeting of the Society of Naturalists of Berlin Herr Eichler exhibited specimens of a new species of *Ouvirandra* lately discovered by Herr Hildebrandt in Eastern Africa. The remarkable form of water-plant known as the Lattice-leaf Plant (*Ouvirandra fenestralis*) with two other species of the same genus have been hitherto regarded as amongst the wonders of the peculiar flora of Madagascar, so that the discovery of a member of the same group in continental Africa is a fact of much importance in botanical distribution. The new *Ouvirandra*, although agreeing with the Madagascar species in all essential points of structure, does not present the singular holes in the leaves that distinguish the *Ouvirandra fenestralis*, but one of the other Madagascar species is likewise abnormal in this respect.

THE "DIGGER" MOLLUSC AND ITS PARASITES.—The little digger, *Donax fossor*, represents a countless mass of life off Cape May, New Jersey, large areas looking like barley grains lying on a malting floor when the tide retires. It gets uncovered by the breaking surf and instantly reburies itself with its powerful foot when the waves retire. The siphons are long and active, looking like so many wriggling worms. Although the prey of shore birds and fishes, and beset with parasites, they lie so thick as even to interfere with one another in burying themselves. The liver of these bivalves is always found beset by flukes, from half a dozen to several dozen, and a bell-shaped trichodina crowds the branchial cavity.

ACTION OF THE HEART OF THE CRAYFISH.—M. Felix Plateau, of Ghent, has succeeded in applying the graphic method to the study of the heart's action in the crayfish. A curve is obtained, of which the ascending portions correspond to diastole, and the descending to systole, contrary to what obtains in the vertebrate heart. It is strikingly like the trace of the contraction of a muscle; a rapid, almost sudden ascent, with a short flat summit, then a gradual descent, at first quicker, then slower. This, however, does not represent the whole truth; it is possible, also, to demonstrate a wave affecting the muscular wall of the heart, and travelling from behind forwards, thus demonstrating that this condensed heart is a true dorsal vessel. On the stimulus of the entrance of renovated blood, it is only the hinder half or two-thirds of the heart that contracts immediately. This forces blood into the forward half, which contracts only when the posterior division is again dilating. When the temperature is increased, as a general rule the diastolic phase is abbreviated, the number of pulsations rising at the same time. M. Plateau has also succeeded in making experiments on the action of the cardiac nerve of Lemoine, an unpaired branch of the stomatogastric ganglion. It is proved that excitation of this nerve quickens the pulsations of the heart, and augments their energy, while section of it slows the heart. Excitation of the thoracic ganglia always retards the heart, the converse of the cardiac nerve. Acetic acid applied to the heart substance arouses its contractions even when they have ceased, and maintains them for several hours. The action of a number of other substances is equally noteworthy, and M. Plateau's full communications to the Académie Royale of Belgium will be awaited with interest by physiologists.

GEOGRAPHICAL NOTES

THE various geographical journals to hand contain several papers of importance. In the January *Bulletin* of the Paris Society M. Maunier gives a full and intelligent sketch of geographical work during 1878, while Dr. Harmand gives a brief statement of the results of his recent journeys in Anam. The Paris Society seems to have followed the example set by the London Society, and has introduced a new feature, "Nouvelles," containing notes of geographical work beyond the limits of its own papers. The *Zeitschrift* of the Berlin Society contains two instructive papers, on the Andamans, by Ad. de Roepstarff, and an account of a journey in south-west Persia, by Dr. A. H. Schindler. In the two numbers of the *Verhandlungen* of the same society, the last for 1878 and first for 1879, the papers of most interest are those on the Mining Industry of Russia, by C. Skalkovsky; on the latest researches on the Aurora Borealis, by Herr Förster; and on the people of East Africa, by Herr Hartmann. In the *Mittheilungen* of the Vienna Society, No. 2 of this year, Herr Franz Heger gives some hints as to a solution of various geological questions,—glaciation, climate, coal-deposits, &c.—apparently seeking to account for many of the great geological problems by a change in the earth's axis. The March number of Petermann's *Mittheilungen* contains several papers of interest. From the journal of a Bremen merchant a narrative is given of a journey up the Jenissei, from its mouth to Jenisseisk, in the summer of 1878; and M. N. Latkin gives a detailed account of our knowledge of the Lena and its basin. Exact news of Nordenskjöld's position is given from the San Francisco whaling captain, who was the first to hear of him, and a statement as to the course to be followed by the steamer *Nordenskjöld*, now building at Malmö, and which will start in May, first to succour the Swedish expedition, and then to proceed to the mouth of the Lena. If it cannot return through Behring's Strait, the staff will spend the winter in collecting all possible data in various departments of science. Nos. 3 and 4 of the *Bulletin* of the American Geographical Society contain, the former a paper by Rear-Admiral Ammer, on the Inter-oceanic ship canal across the American Isthmus, and the latter an interesting sketch of the life and work of Mercator, by Mr. E. F. Hall.

NEWS of two African expeditions are to hand, in one case telling of misfortune, and in the other of success. The Belgian expedition, unfortunate from the beginning, has met with another disaster in the death, from dysentery, of M. Wautier, at a place called Kekongen (? Ukonongo). On the other hand, Major Pinto, the leader of the Portuguese African expedition, telegraphs to Lisbon from Pretoria, that he has virtually crossed Africa from the west coast, after struggling with hunger, thirst, beasts, natives, floods, drought. His route must have to some extent almost coincided with that of Livingstone, and he tells us he has saved all his papers, twenty geographical charts, many topographical maps, several vols. of notes, drawings, meteorological data, a diary of the exploration of the Zambesi's seventy-two cataracts and rapids. He says he has discovered the secret of the Cubango, by which he seems to mean the river which, under various names, was for a time taken by some to be the upper course of the Congo. He lost many followers, and his expedition seems in a small way to have been modelled on that of Stanley's.

THE *Times* Roman correspondent writes that Menotti Garibaldi and Achille Fazzari intend, if England does not object, to sail in summer or autumn with 3,000 Italians for the south coast of New Guinea, to establish a colony there, and found a new city under the name of Italia. The arrangements, it is said, are almost completed, the 30,000,000 francs required ready, and that applications to join the party are more than can be granted. Part of the equipment will be a telegraph cable, to place the colony at once in communication with North Australia. Men of all ranks and callings (except lawyers) are included in the party, and among them several men of science. The proposed colonists express the greatest good will towards England, and it seems to us the trial would be worth making. The Italians make better colonists than the French, and Italians have done so much for the exploration of New Guinea that it seems only fair that they should be allowed to reap some benefit from the labours of such men as D'Albertis and Beccari.

AT the last meeting of the Société Commerciale de Géographie at Paris Dr. Raffray gave some particulars respecting his recent explorations in New Guinea, and called attention to the fact that that country offered a vast field for discovery and study to the traveller, especially from an ornithological and entomological point of view. A report on the subject of a railway across the Desert of Sahara was afterwards read, being the result of the labours of a committee, of which M. Gazeau de Vautibault is president. M. Deloncle also made a communication respecting the Volta region in West Africa, which has been explored by M. Bonnat, and he announced that two Lyons merchants had already determined to establish business houses there.

MGR. LAVIGERIE, Archbishop of Algiers, has forwarded to *Les Missions Catholiques* the commencement of the journal of the Algerian missionaries, recording the incidents of their march towards the Nyanzas and Lake Tanganyika. This portion of their journal stops at Mukuduku in Ugogo on August 20, and the first instalment of this is now published. It had been intended to accompany it by a map of Equatorial Africa, sent home by Père Charmetant some time back, but it has been thought better to delay the publication, in order that the itinerary of the missionaries and the additional geographical information contained in their journal may be included in it.

IT is stated in an Italian newspaper that the Duke of Genoa will go on an exploring expedition, and will sail from Venice in the *Vittore Pisani* at the end of this month. The programme of the route is to be Port Said, Suez, Aden, Ceylon, and Singapore, where a longer sojourn will be made. Afterwards the traveller will proceed to the Chinese and Japanese coasts; in 1880 he will visit Australia and direct his special attention to the exploration of New Guinea. On the return journey the *Pisani* will cruise in the Persian Gulf. Capt. Sebastian and Count Antonela have started on an exploring tour through Africa.

A POSTCARD was received at Berlin on February 15 from Dr. Gerhard Rohlffs, dated January 27 and posted at Sokna, some 250 miles south of Tripolis, at the foot of the Black Mountains, stating that he was in perfect health. The postcard bears the stamp of Dr. Rohlffs's desert post, and a prettily-drawn postage-stamp with African palm-leaves.

TO accompany the map of Zululand, noticed last week, Mr. Stanford has published a few useful notes on the physical features and population of the country.

THE *Jeannette* is fitting up in San Francisco harbour, and will leave for polar exploration in the month of June. Mr. Bennett, who is now in Europe, has been making inquiry at Paris as to the best means of constructing and inflating balloons in the Arctic regions. It is thus likely that aërial navigation will play a part in this new effort to solve the mystery of the north.

EDISON'S TELEPHONE

OUR readers may remember a few months ago we stated, in an article on the Carbon Telephone (NATURE, vol. xix. p. 56), that Mr. Edison had devised an entirely new form of receiver, for use with his telephone, which delivered the voice as loudly as if the words were spoken at the distant end. This receiver has now arrived in England in charge of Mr. Edison's nephew, and to judge from its performances last Friday, it is likely to accomplish all that Edison has stated concerning it.

The principle of this new receiver is that of the *electro-motograph*, and to those of our readers who may not be acquainted with this instrument the following extract from a recently published lecture, on Edison's inventions, by Prof. Barrett will explain what the electro-motograph is.[1]

"Mere ingenuity in contriving machines does not add to the sum of human knowledge, and if Mr. Edison were merely a clever inventor and nothing more, I should feel less interest in the man. It is, however, a noticeable feature of Mr. Edison's inventions that they, in general, contain some new principle, some original observation in experimental science, which entitles him to the rank of a discoverer. Such is the character of the next invention we must consider, the so-called *electro-motograph*. This is an entirely new method of receiving telegraphic messages, discovered by Edison in 1874. As every one is aware, the ordinary system of telegraphy depends upon the production of magnetism by means of an electric current, the current either attracting and releasing a movable piece of iron, or deflecting a magnetic needle to the right or to the left. By the to-and-fro movements of the iron or the needle the conventional signals are produced which are employed in telegraphy. Now Mr. Edison made the curious and important discovery that messages could be received by the well-known Morse recorder without the use of any magnet. This, to a telegraphist, would be like attempting to perform the play of "Hamlet," while omitting the part of *Hamlet* himself. In fact, all that is necessary in this simple telegraphic instrument is a band of moistened paper drawn beneath a metal style. The accident of holding his finger against the style of a Morse instrument led Mr. Edison to notice that when an electric current passed from the paper to the point resting upon it the friction of the moving paper was lessened. Hence, if the paper were drawn with a uniform force it would slip more easily beneath the point the moment the current passed. The slipping of the

[1] "Science Lectures for the People," No. 5, Tenth Series. (Manchester, Heywood.)

paper is converted into a to-and-fro motion of a lever to which the point is attached, and which is made to actuate a bell, or "sounder," and give rise to audible signals in the usual way. It is necessary to moisten the paper with a solution of certain chemicals. Potash was at first used, but a solution of sulphate of soda or of common salt and pyrogallic acid is found to be best.[1]

"The advantage of this instrument, which Mr. Edison calls an *electro-motograph*, is said to be its extreme sensitiveness, it having been worked over a circuit of two hundred miles with only two cells, so that with weak currents, unable to affect ordinary instruments, the electro-motograph can receive messages. More than this, the speed of its working is greater than with the ordinary instruments. Using it as a relay, that is, an instrument for translating weak currents into strong ones, no less than 1,200 words per minute have been transmitted by its means, or five times as fast as it is possible for any person to read the message which comes through the instrument. So prompt and delicate is the motion of this machine that Edison has applied it to the purposes of the receiving instrument for the Reiss telephone, a musical telephone that was made many years ago. The slipping of the paper causes a slight sound. If, then, we sing a certain note into the Reiss transmitting instrument, which vibrates in unison with that note, we obtain the same number of electric currents produced per second as we had of sonorous vibrations in the moving diaphragm. Thus, if we sounded the middle C we should get 264 vibrations, and there would be 264 electric currents, and 264 slips of the paper, thus producing a note of the same pitch in a distant room. The cause of the curious slipping has not been fully ascertained. It may possibly be due to that peculiar repulsive effect to which Mr. Crookes has lately drawn attention, and which produces the dark region around the negative electrode during the continuance of an electric discharge in a vacuum tube, or it may simply be due to electrolytic action."

It is, then, this principle which Edison has made use of in his new receiver, which is of the simplest construction. A diaphragm, preferably of mica, some four inches in diameter, held in a suitable framework, has attached to its centre a spring, or "pawl," the free end of which rests on a little cylinder of chalk, capable of rotation by the hand or other means. The chalk cylinder replaces the paper in the electro-motograph, and is necessarily impregnated with sulphate of soda, or other suitable solution. As the cylinder is rotated, the friction of the spring on the chalk causes the diaphragm to be pulled in or pushed outwards, according to the direction of the rotation. So far the operation is purely mechanical; as soon, however, as the current passes, either owing to electrolytic action or the friction, it is lessened, and the diaphragm tends to spring back to its normal position; on the cessation of the current the friction is restored, to be lessened on the recurrence of another electric wave. Thus, a series of tremors are given to the diaphragm corresponding to the swiftly changing character of the electric waves, and these again faithfully express the motion of the diaphragm at the transmitting end. It will thus be evident that the incoming current has simply to do the work of liberating the already strained diaphragm. As everyone knows, in Bell's telephone the voice has to do the work of creating the current at the transmitting end, and the feeble magneto-electric currents thus generated throw into motion the diaphragm at the receiving end. In Edison's telephone this is not so. The voice at the transmitting end has simply to vary the *electric* resistance in the path of a current generated by an ordinary voltaic battery; stronger currents can thus be sent along the line, and these arriving at the receiving end, have merely to vary a *mechanical* resistance, and not to do the work of overcoming the inertia of the diaphragm. It is probable the rotating chalk cylinder acts on the diaphragm with its attached spring like a resined bow on a violin string; vibrations are set up, the extent, rate, and manner of which are modified by the varying friction due to the telephonic currents. Whether these new receivers will retain their present efficacy when in constant use remains to be seen. We should be inclined to think the soft surface of the chalk will eventually wear with the friction, and that a more permanent arrangement will have to be devised. No invention, however, reaches perfection at once, and the present receivers, excellent as is their performance, were, we understand, hastily made in a few days, in compliance with the urgent request of Mr. Edison's courteous representative in London, Col. Gouraud.

The instrument has the appearance of a small box attached to the wall, and from which there projects a single funnel. Sounds of singing, speaking, whistling, sent from the other end, quite a mile off, were heard in every part of a moderately sized room. Telephonic connections, now so common in America, have been established by Col. Gouraud between various business houses in the city; and we believe that shortly this method of communication must become quite common.

[1] Practical difficulties have, we believe, been found in the working of the motograph, so that it has not come into telegraphic use.

NOTES

Dr. Miclucho Maclay, the eminent Russian Naturalist and New Guinea explorer, has been trying to rouse the Linnean Society of New South Wales and the scientific public of Sydney to the necessity of founding a zoological station, similar to that at Naples. He tells of the great inconvenience he himself has suffered during his residence at Sydney from the want of such a station, even though the Hon. Mr. Macleay placed his museum at his disposal. But Dr. Maclay's scheme embraces much more than a station at Sydney. He has written to the German Eastern Asiatic Society at Japan and to Mr. August Godeffroy at Samoa, urging that similar stations be founded at these places, and he has reason to believe that his proposals will not be without result at both places. Thus should zoological stations be instituted at Sydney, in New Zealand (as Dr. Maclay also proposes), in Japan, and at Samoa, we might hope in a very few years to have a fairly complete knowledge of the fauna of the Pacific. Dr. Maclay's proposal deserves the heartiest encouragement, and we trust that ere long it will be fully carried out. We hope the people of Sydney, at any rate, will take Dr. Maclay's appeal to heart; he tells them, moreover, that he will judge of the intensity of the scientific life of Australia by the interval which elapses between the reading of his paper on the subject and the actual foundation of the station. He shows what valuable results have followed the foundation of the Naples station, and gives a few hints as to how such a station at Sydney should be organised. We shall be curious to see what will be the result of Dr. Maclay's fervent appeal.

We are requested to state that on and after April 1 any person may obtain by telegraph from the Meteorological Office the latest information as to the weather in any district of the United Kingdom by payment of a fee of 1*s*. in addition to 2*s*. the cost of the message to the Meteorological Office and the reply. The telegram containing the inquiry must not exceed twenty words in length, and must be addressed, "Meteorological Office, London." The Meteorological Office does not undertake to give any information which is not substantially included in the latest notice posted at its own doors, nor does it give forecasts of the weather on the Atlantic coasts of the British Isles; although it is ready to furnish any information it possesses as to the actual state of the weather on those coasts. The Meteorological Office is open for such inquiries between the hours of

11 A.M. and 8 P.M. on week days, and between 6 P.M. and 8 P.M. on Sundays.

THE Emperor of Germany has confirmed the election of Sir G. B. Airy as a foreign member of the Berlin Academy.

RUSSIAN astronomers seem determined to outstrip their *confrères* in other countries in the matter of telescopes; we are informed that funds have been subscribed for the construction, for Pulkowa Observatory, of a refractor of thirty-two inches aperture.

WE have received a circular from the Research Committee of the Institution of Mechanical Engineers, drawing attention to three subjects which they have selected for first investigation, and asking for any information, bearing on all or any of them, which any one interested may be willing to communicate: *e.g.*, records of unpublished experiments, references to authorities on the question, copies of books or papers in which it is treated, &c. The Committee would be glad to receive such information in full detail, and at the earliest convenient date; and it will be suitably acknowledged in their report. The subjects are:—Subject A. The hardening, tempering, and annealing of steel. Subject B. The best form of riveted joints to resist strain, in iron or steel, or in combination. Subject C. Friction at high velocities, specially with reference to friction of bearings and pivots, frictions of brakes, &c. The address of the Institution is 10, Victoria Chambers, Victoria Street, Westminster.

IN the Monthly Weather Report of the U.S. Signal Service for January, 1879, are many points of great interest to meteorologists. The particular report before us belongs to the division of "Telegrams and Reports for the Benefit of Commerce and Agriculture," and contains a multitude of well-arranged data received up to February 14. The charts accompanying the Report are of special interest. One of them shows the tracks of ocean-storms from November 23, 1878, to January 16, 1879, and exhibits the paths of seven storms. No. 6 of these is shown to have commenced in California on January 6, to have come down to the Gulf of Mexico by the 8th, to have gone north-east through the United States to Newfoundland, between the 8th and 11th, across the Atlantic between Scotland and Iceland, the storm expending itself off the coast of Norway on the 15th, one week after starting from California.

EXPERIMENTS in electric signalling and reconnoitring have been made at Mont Valérien on a large scale. Details are wanting, as the French Government think it prudent to keep secret almost all experiments relating to military matters.

WE have received an interesting sketch, with portrait, by Prof. Ehlers, of Göttingen, of the late Wilhelm Engelmann, so long the head of the well-known Leipzig scientific publishing house, and who did so much for the advancement of scientific knowledge in Germany.

The Science Index is the title of "A Monthly Guide to the Contents of the Scientific Periodicals," the first number of which we have just received, though dated January. This delay is apologised for, on account of the difficulty of getting together a first number. The aim of the journal is highly to be commended, and if carried out on a thoroughly well-considered plan, ought to prove of great service. We are not disposed to criticise this first number too severely, though we think there is considerable room for improvement. It is by no means exclusively devoted to science, including as it does Art, Architecture, Strikes, Baking and Confectionery, Bells, Commerce, and other miscellaneous topics. Indeed, on the principle which has been partially followed, we do not see where the line is to be drawn short of an index to everything. We hardly think the plan has been well considered, and we think that many of the subjects indexed might be omitted with advantage if it is meant to be really a Science Index. Judging from the number of misprints, this number seems to have been hastily got out. In geography alone we meet with such horrors as "Afkanistan," "Leybian Desert," "Oxies" for Oxus, &c. References to the Swedish Arctic Expedition occur under different headings, as if the compiler did not know that the items referred to the same thing. Still, the index is a step in the right direction, and we hope the editor will take competent council, and introduce such improvements as will make his *Science Index* what it might and ought to be.

MAJOR MAJENDIE, as the result of a series of experiments with dynamite, has come to the conclusions that frozen dynamite is considerably less sensitive to explosion by a blow than unfrozen dynamite; that cartridges of dynamite having small quantities of exuded nitro-glycerine within them are decidedly more sensitive to explosion by a blow than cartridges in which there is no such exudation; that frozen dynamite is much more susceptible to explosion by simple ignition than unfrozen dynamite; that frozen dynamite is much less sensitive to explosion by the impact of a bullet than unfrozen dynamite; that the danger attending the mere breaking in two of a frozen dynamite cartridge does not seem to be of the formidable character indicated by the Austrian regulations; and that frozen nitro-glycerine is not susceptible of detonation by detonators of the same strength as those with which the detonation of unfrozen nitro-glycerine may be readily and certainly effected.

THE *Bradfordian* is the title of a magazine "written and supported by the two Grammar Schools" of Bradford. It has a varied programme, in which, we are pleased to see, science finds a place.

THE *Times* Geneva correspondent writes that M. A. Borel, of Chaux-de-Fonds, has just had the good fortune to find in the Lake of Neuchatel, between Bazuge and Chatelard, a prehistoric canoe, probably the finest specimen of the sort that has yet come to light in Switzerland. Hollowed out of a single piece of oak, the vessel is 8 metres long, 90 centimetres wide, and 65 centimetres high. It is well finished, and in a perfect state of preservation. The stern carries a spur, and the prow is curved in the form of a hook, probably for the purpose of attaching it by a rope to a landing-place. The canoe is sufficiently large to carry twelve persons. There is no appearance of rowlocks, but the supports on which the thwarts formerly rested are still plainly to be seen. M. Borel proposes to present this interesting "find" to the Museum of Chaux-de-Fonds.

A PETROLEUM spring, one boring of which has yielded 2,000 kilos in twenty-four hours, has been discovered at Pohar, in Austrian Poland.

ACCORDING to the report of H.M.'s Consul for Hiogo and Osaka, the Japanese claim that petroleum has been known in Japan for over 1,200 years, and it would certainly be curious if the numerous springs which exist in certain localities should have escaped notice in their immediate neighbourhood. It is doubtful, however, whether it was ever utilised, and certainly no attempt was made to refine it before the arrival of foreigners. The first efforts in that direction were made near Niigata in 1875, but the petroleum then refined failed to stand a higher test than 75° F. Accordingly Prof. Lyman, who had previously performed a similar service for India, was sent for from America to conduct a professional survey of the region. His report, however, was unfavourable, chiefly on the ground of an insufficient supply. This opinion the Japanese are now about to test, for which purpose they have established a refinery near Hiogo. Its supplies of crude oil are to be drawn from the province of Potomi, distant about 100 miles to the north, the transport being conducted by sea.

AT the evening meeting on Monday, the 31st inst., at 8.30, at the Royal United Service Institution, D'Arlincourt's telegraph will be exhibited and worked, illustrative of a means of communicating orders in the field. The paper of the evening is on "Orders in the Field and the Means of Communicating them," and will be read by Major Webber, R.E.

A PAMPHLET has just been issued at Quebec, compiled under the auspices of the Boards of Trade of that town and Montreal, dealing with the subject of telegraphy with the coasts and islands of the Gulf and Lower River St. Lawrence, and the coasts of the maritime provinces, considered from the point of view of its relation to the shipping, to the fisheries, and to the signal service. The *brochure* is accompanied by a large-scale coast telegraph chart of the region named, delineated under the direction of the Hon. P. Fortin, on which are shown existing and projected telegraph lines and the ordinary tracks of vessels.

AT the last meeting of the Eastbourne Natural History Society an interesting paper was read by Dr. Murdie on sea-water, its adaptations to various purposes, and its management in reference to marine zoology.

MR. TEGETMEIER has published a facsimile reprint of Moore's "Columbarium" from the original edition of 1735. The work, the source from which all subsequent works have taken their rise, ought to interest both fanciers and naturalists; by its aid, as Mr. Tegetmeier says, the latter may trace the alterations produced in varieties of the same species, continued for 150 generations. Moore was a well-known doctor of the City of London, and was so eminent in his way as to merit a bantering satire from Pope.

PROF. EDWARD MORSE, of Tokio, announces that he has discovered undoubted evidences of the practice of cannibalism among the early inhabitants of Japan. These evidences he describes in a paper read at the Biological Society of the Tokio Dai Gaku.

THE *Colonies and India* has an interesting note on the subject of Vanilla, which appears to be mainly supplied by Mauritius, Brazil, and Mexico, but could probably be grown in many of our colonies. The parasitical plant which yields this aromatic bean will climb up any tree that gives sufficient shade; it attains a height of about a foot, and thrives for thirty or forty years, producing some fifty pods each year after the second. The beans take eight or nine months to mature, and are gathered between October and December; they are oiled occasionally to prevent excessive shrinking, and dried in the sun; when warm they are wrapped in woollen cloths to absorb the evaporation, and during the process attain their black silvery hue. Vanilla is the most costly, in proportion to weight, of all vegetable productions, and only a few hundredweights reach England annually.

MASSON, of Paris, has published in a collective form, under the title of "Revues scientifiques publiées par le journal *La République Française*," a large number of papers on subjects of scientific interest published at intervals in that paper, under the direction of Prof. Paul Bert.

THE comparative effects of pressure and hammering in changing the volume of soft masses, has been investigated by Herr Kick (*Ding. Pol. Jo.*). A carefully cast lead cylinder 100·3 mm. high, 70·2 mm. diameter, and 387·85 ccm. at 15°, was compressed in a Gollner's machine to 69 mm. and 50 mm. height respectively. The volume was found to be hardly altered at all (it became 387·814 ccm. in the second case). A lead cylinder 59·7 mm. high and 50 mm. diameter was now beat down by means of a steam hammer to 16·7 mm. height. The volume was reduced from 117·56 to 117·33 ccm. Thus, to condense metals, it is necessary to resort to beating, or to use extraordinary pressure on inclosed material.

SOME interesting experiments with regard to tension of carbonic acid in blood have recently been made by Herr Gaule in the physiological laboratory of Prof. Ludwig (Du Bois Reymond's *Archiv*). He notes the following differences between blood and serum in this connection:—1. The proportion of carbonic acid in serum is higher than that in blood. 2. The tension of carbonic acid in serum is less than that in blood. 3. If in serum the quantity of free carbonic acid diminish, there is decomposed only the quantity of bicarbonate of soda which corresponds to this diminution, but in blood more. 4. On addition of simple carbonate of soda, the tension of free carbonic acid diminishes in the serum, but in blood it does not. 5. In blood there exists a substance which is capable of dissolving the entire combination between carbonic acid and soda, but in serum not. This substance, which attracts the carbonate of soda, is very probably hæmoglobin, which then further decomposes the combined salt into soda and carbonic acid, and thus maintains with the bicarbonate salt and the free carbonic acid a series of complicated exchanges, which probably render possible and promote the giving out of carbonic acid and the removal of it from the tissues and the lymph.

CARL'S *Repertorium für Experimental Physik* (xv. Band, 2 Heft) contains an illustrated account of the new meteorological magnetic observatory for St. Petersburg at Pawlowsk. In the same number we note a useful simplification of the spectroscope, by Herr Hüfner.

THE measuring of sea-depths with the lead leaves much to be desired in point of accuracy. Dr. Rühlmann is of opinion (*Annalen der Physik*) that reliable results will be had, only when it is practicable to measure the weight of the water column. This weight, and therewith the height of the column, might be easily ascertained from pressure on a manometer. And the point is, to combine with the pressure-measuring instrument an arrangement whereby one may read off what the apparatus indicates at that point, the depth of which below the surface is to be determined. To construct such a manometer need not, he thinks, greatly puzzle the mechanician. The various forms of aneroid barometer are a good direction. Suppose, *e.g.*, a manometer made after this fashion, and available for very high pressure, and let there be adapted to it an electro-magnetic arrangement, wherewith, when a current is sent through it from the ship, the index is pressed so forcibly against the scale, that a mark is produced on this. In this way, closing the circuit at different times, it would be possible to determine the pressure in different parts of the sea. If, at the same time, there were sunk a junction of a thermo-element, the other junction of which was kept at constant temperature, and if the current of this thermo-element were conducted through a sensitive marine galvanometer on the ship, one could ascertain the temperature of the water at the points for which measurements of pressure were obtained. Very accurate results could only be expected, when not only the pressure at the sea-bottom, but also the temperature of the water at as many points of less depth as possible, were known. Dr. Rühlmann is not aware if such a method has been put into practice, but he gives an exposition of the theory of it.

THE additions to the Zoological Society's Gardens during the past week include an Indian Fruit Bat (*Pteropus medius*) from India, presented by Capt. F. P. Millett; a Mule Deer (*Cervus macrotis*) from Ottawa, Illinois, U.S.A., presented by Judge Caton; two Gaimard's Rat Kangaroos (*Hypsiprymnus gaimardi*) from Australia, presented by Mr. Ernest E. Harrold; a Spotted Ichneumon (*Herpestes auropunctatus*) from India, presented by Miss H. Boteler; a Brent Goose (*Bernicla brenta*), European, presented by Mr. H. A. Dombrain; a Black-faced Spider Monkey (*Ateles ater*), a Black-handed Spider Monkey (*Ateles melanochir*) from South America, received in exchange; a Black necked Stilt Plover (*Himantopus nigricollis*) from South America, purchased; three Common Badgers (*Meles taxus*) born in the Gardens.

THE PARIS ACADEMY PRIZES

LAST week the Paris Academy held its annual public meeting, when the prizes for 1878 were awarded. According to old custom, M. Fizeau, the president of last year, was in the chair. He remarked on the unprecedentedly large number of prizes that were not awarded, either because there was no competition or because there were no competitors of sufficient merit. On this account several of the most important prizes have not been awarded this year, and it seems to be the common opinion that some of the problems proposed are much too difficult. M. Dumas read an *éloge* on M. Balard, the discoverer of bromine, and M. Bertrand did the same for Leverrier. M. Bertrand made no allusion to the part taken by Leverrier in the public affairs of his time, and made but slight allusion to his organisation of the Meteorological Service, and that almost as if it were not a thing quite worthy of encomium. M. Bertrand's address does not seem to have given universal satisfaction, and several of the audience on leaving the hall were heard to say: "Quant à l'éloge de Leverrier il est encore à faire." The following are the principal prizes awarded at the meeting:—The Extraordinary Prize of 6,000 francs for the greatest prgress in naval construction, to M. Perroy and Lieut. Baills; the Poncelet Prize in Mechanics to M. Maurice Lévy; the Montyon Prize of 1,000 francs, in Mechanics, to Mr. George H. Corliss, for his well-known engines; the Plumey Prize to Capt. Vallessie, for his differential counter to regulate the progress of steamships. In Astronomy the Lalande Prize was awarded to M. Stanislas Meunier, for his researches on meteorites; the Valz Prize to Dr. Julius Schmidt, for his lunar charts. In Physics the Bordin Prize was awarded to M. Reynard for his researches in connection with Ampère's law. In Chemistry the Jecker Prize was awarded to M. Reboul, specially for his memoir on the isomers in the propylene series. In Botany the Barbier Prize was given to M. Ch. Tauret, and encouragements of 500 francs each to M. Cauvet and M. E. Heckel; the Desmazières Prize to Dr. Bornet; the Shore Prize to Prof. Ardissone for his "Floridee Italiche;" in Anatomy and Zoology the Serres] Prize was awarded to Prof. Alexander Agassiz, for his various embryological and other investigations; and the Montyon Prize in Physiology to M. Charles Rechet, for his researches on gastric juice. The Tremont Prize was given to M. Marcel Deprez for his application of electricity to the solution of various problems in mechanics; the Gegner Prize to M. Gaugain; the Delalande Guérineau Prize to M. Savorgnan de Brazza, for his exploration of the Ogové River; and the Prize founded by Mdme. de Laplace to be awarded to the pupil who leaves the Polytechnic School with the highest honours, to M. de Béchevel.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

M. FERRY, the French Minister of Public Instruction, has presented a project for the reorganisation of the Superior Council of Universities. According to the proposals of the minister, which are sure to be adopted by the Assembly, the bishops and other religious members are to be excluded, and the Council exclusively composed of persons belonging to the teaching profession. Moreover, it is proposed that all degrees be henceforth granted by the State, and only to those who have taken the curriculum of a recognised university.

THE examiners for the Burdett-Coutts' Scholarship (Oxford) have awarded it to Mr. Algernon Philips Thomas, B.A., Scholar of Balliol College; and they consider Mr. Henry Nicholas Bidley, B.A., of Exeter College, worthy of honourable mention.

SCIENTIFIC SERIALS

Bulletin de l'Académie Royale de Belgique, No. 12, 1878.—In this number is a paper by M. van Beneden, giving a historical sketch of whale-fishing and of the first Arctic expeditions.—A lecture by M. Houzeau, the president, has for its subject certain enigmatical phenomena of astronomy.—M. van Rysselberghe describes a parabolic regulator, rigorously isochronous, and the velocity of which can be varied at will. Regarding it, M. Folie reports that it has too many articulations and movable rings for common use, and it hardly realises ideal perfection for physical and astronomical apparatus.—M. Malaise announces the discovery of a mineral species new for Belgium, viz., arsenopyrite or mispickel, and M. Monier describes a hydrophane opal and hydrated transparent silica, obtained by action of oxalic acid on alkaline silicates.—There are also several mathematical papers and reports on prize competitions (subjects chiefly botanical).

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xii. fasc. ii.—We note the following papers in this number:—Considerations on a letter of Tyndall's regarding heterogeny, by Prof. Giovanni.—On the causes of asphyxia and the agglutination of the blood corpuscles in diphtheria, by S. Trevison.—The Sanitary Office of the German Empire, by Dr. Zucchi.—Studies on milk (continued), by Drs. Pirotta and Riboni.—On cortical psycho-sensory centres, by Professors Luciani and Tamburini.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, March 13.—C. W. Merrifield, F.R.S., president, in the chair.—Mr. J. D. H. Dickson was admitted, Mr. R. Hargreaves and Prof. W. E. Story were elected, and Mr. Donald McAlister was proposed for election into the Society.—Prof. Cayley, F.R.S., spoke briefly but in high praise of the late Prof. Clifford's work as a mathematician, instancing more particularly his papers "On the Canonical Form and Dissection of a Riemann's Surface," "On Mr. Spottiswoode's Contact-Problems," and "The Classification of Loci."—The chairman, the Rev. A. Freeman, and Dr. Hirst, F.R.S., added a few remarks on the loss the Society and the mathematical world generally had sustained, and expressed the hope that steps would be taken to secure the publication, if desirable, of any mathematical papers Prof. Clifford might have left.—Dr. Hirst made a statement respecting the "De Morgan Memorial" Medal to be presented to the Society to be awarded in such manner as the council shall hereafter determine; it appeared that the bust and die for the medal had been executed by Mr. Woolner, and that after all claims had been met there would still be a small sum required to make up the requisite total for the purpose contemplated. The late Prof. De Morgan was the first president of the Society and always took a warm interest in its advancement. It was resolved that a subscription list should be opened in order that old pupils and members of the Society might have an opportunity of aiding in the above design. Subscriptions for this special purpose may be sent to Mr. Tucker (Hon. Sec., University College School, W.C.), or to Mr. Alfred Wills, Q.C., 12, King's Bench Walk, E.C., the Hon. Sec. to the general fund. Copies of the medal were exhibited (Profile with dates of birth and death, on the reverse, Pascal's hexagram, surrounded by the "Zodiac of Syllogisms," and the title President of the London Mathematical Society).—The following communications were made:—On differential equations, total and partial, and on a new soluble class of the first and an exceptional case of the second, by Sir J. Cockle, F.R.S.—Discussion of two double series arising from the number of terms in determinants of certain forms, by Mr. J. D. H. Dickson.—Two geometrical notes relating to surfaces of the second order, by Prof. H. J. S. Smith, F.R.S.

Physical Society, March 8.—Prof. W. G. Adams in the chair.—Dr. Hurst and Mr. Jacob were elected Members.—Prof. Ayrton brought forward a new theory of terrestrial magnetism originated by himself and Prof. Perry of the Imperial Engineering College, Japan. It is well known that metal cages act as screens against induction in the case of static electricity or electricity at rest, and hence Clerk Maxwell, at the British Association meeting for 1876, suggested that no earth connection was necessary for lightning conductors, since a cage would be sufficient. But dynamic electricity is different from static in this respect, and Professors Ayrton and Perry found that even a thick block of copper will not screen a coil of wire from the induction of a *current* flowing in a neighbouring one. Some experiments of Dr. Muirhead, not yet published, would seem to favour the view that a current is a series of intermittent changes of potential, and that the inductive effect was due to a difference in the epochs of the currents in the two coils. It was found by Helmholtz that a quantity of static electricity in mechanical motion performs work. Conversely Mr. Crookes finds that the stream of molecules from a – pole *in vacuo* is electrified, and may be deflected by a magnet. It is upon that fact that Professors Ayrton and Perry have based their theory, which is easily explained by supposing the earth to be an isolated sphere with a static charge residing on its surface. Then, since each electrified particle at the surface will be moving relatively to a point in the interior, it follows that the interior must be magnetic. The theory is independent of the substance of the interior; but in order to simplify the working the authors treated the case of a solid iron ball, and curiously enough arrived at the result expressed by Biot's law for the distribution of magnetism on the surface of the earth—

$$I^2 = M\sqrt{1 + 3\cos^2\theta},$$

and similarly they found that if the earth were electrified to the potential of 10^8 volts, relatively to interplanetary space, its magnetisation would be as it is. If the earth were alone in the universe, then, by this theory, it would have its own magnetic state by virtue of its electric charge and axial rotation. If other bodies in the universe, however, had their magnetic states too, these would influence the earth's, and hence we should have terrestrial tides and storms of magnetic force, such as are known to exist, as, for instance, when changes take place in the sun's atmosphere by approach of planets or other causes. Lastly, the iron in the interior of the earth may give it a certain amount of coercitive force, but the theory does not rest on this.—Dr. J. Hopkinson then read an account of some experiments made with the quadrant electrometer, which showed that Clerk Maxwell's formula for the sensibility of the electrometer—

$$(A - B)\left(C - \frac{A + B}{2}\right),$$

where A and B are the potentials of the two pairs of quadrants, and C the potential of the needle, only holds good when C (the charge of the jar or needle) is less than 200 Daniell elements. Above that a different law appears to hold. Dr. Hopkinson also remarked that any degree of low sensibility down to zero could be got from the electrometer by connecting a condenser to each pair of quadrants and adjusting their capacities.—Mr. F. D. Brown described his apparatus for maintaining constant temperatures and pressures. A constant temperature can be obtained if the pressure can be kept constant. The vessel in which the constant pressure is desired communicates with an air-pump by a pipe in which a movable tap or valve is placed. By opening or closing this tap the pressure is regulated. This is effected by an electric clutch arrangement. A mercury anemometer sends a positive or negative current from a battery through the clutch according as the pressure is too high or low, and this current actuates the clutch to close or open the valve. The clutch consists of an axle driven by a turbine to get power to work the valve, and the current, by means of electromagnetism, connects the tap to the axle, which then opens or closes it as the case may be. In this way a pressure varying no more than one-fifth millimetre each way can be obtained.

Linnean Society, March 6.—William Carruthers, F.R.S., vice-president, in the chair.—Mr. Thos. Christie exhibited and made remarks on a series of specimens illustrating the little-known and remarkable Australian Pituri plant; also the *os sepia* of a rare Australian cuttle-fish, obtained by Dr. Bancroft.—Mr. R. Irwin Lynch showed a growing example from Kew and dried leaves of *Xanthosoma appendiculatum*, bearing pouch-like excrescences from the midrib of the leaves.—The Vice-President announced from the chair an alteration in the Bye-laws, Chap. XIII., proposed by the Council.—A letter was read from a correspondent referring to the increased production of beet-root sugar by careful artificial selection of the beet. The saccharine produce of sugar-cane, on the contrary, remains stationary, if not retrograde, and its continual multiplication from stolons some regard as giving rise to various diseases. Crossing and selection are now suggested as worthy of a trial in the interest of commercial results.—A short paper on Entozoic Florideæ growing within living Bryozoa and Sponges, by Dr. P. F. Reinsch, was read, and Mr. A. W. Waters exhibited in connection therewith, under the microscope, specimens of Polyzoa containing parasitic algæ.—In a note on the fruiting of *Wistaria sinensis* in Europe, by Mr. W. T. Thiselton Dyer, the author avers from his own and others' observations that plants trained on a garden wall at Glyon, east end of the Lake of Geneva, yield abundance of brown tomentose pods annually. Near the town of Geneva, however, fruiting is of rarer occurrence, but again more frequent at Lyons and the Rhone valley. Fruiting, he suggests, may be a question of temperature and not of nutrition, dependent on presence or absence of support to the stem and branches. From the above and other data, Mr. Dyer fails to see the evidence of the antagonism of the vegetative and reproductive forces, as asserted to be the governing law, according to Mr. Thos. Meehan's experiments, and lately quoted by the Rev. G. Henslow. If such barrenness were the case with its scandent habit, then *Wistaria sinensis* would probably already be extinct.—The Secretary read a paper by Mr. Edward J. Miers, on the classification of the Maioid crustacea or Oxyrhyncha. The Maioid crabs have been placed by nearly all carcinologists at the head of the Brachyura, from the high degree evinced in their sensory organs and nervous system, and the group, moreover, is interesting on account of the variety of types. Though closely related to the Oxystomata, the Oxyrhyncha differ from them in their triangular buccal cavity and position of afferent branchial channel; but Mesorhœa approximates on the part of the Parthenopidæ to the Oxystomatous type. From the Cancroid crabs (Cyclometopa) typical Maiidæ are distinguished by longitudinal antennules and position of basal antennule joint, the Parthenopidæ occupying an intermediate place between the rest of the Oxyrhyncha and certain Cancroidea. The author reviews the various classifications, and then gives a new synoptical arrangement founded on certain anatomical configurations, &c., of their buccal cavity, situation of afferent and efferent canals, antennules, genital appendages, &c. He divides the group into 4 families, 12 sub-families, 106 genera, and 14 sub-genera, giving short diagnoses of each.—Prof. J. Reay Greene, Dr. P. H. Stokoe, Mr. R. Johnston (of Tasmania), Mr. B. S. Williams, and Prof. J. Wood Mason, were balloted for and elected Fellows of the Society.

Entomological Society, March 5.—J. W. Dunning, vice-president, in the chair.—The chairman referred to the great loss sustained by the Society in the death of Mr. F. Smith, of the British Museum.—Mr. C. Brogniart, of Paris, was elected a Foreign Member and Mr. J. T. Harris, of Burton-on-Trent, a Subscriber to the Society.—Sir Sydney Saunders exhibited a series of bees belonging to the genus *Halticus*, from Greece, containing several remarkable new forms. The following papers were communicated:—On some new species of British hymenoptera, by Mr. Peter Cameron, and descriptions of some new species of coleoptera from New Zealand, by Dr. Sharp.

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