THURSDAY, APRIL 22, 1880

THE ST. GOTHARD TUNNEL

THE news that the two advance borings had met in the middle of the mountain traversed by the St. Gothard tunnel resounded like a joyful echo in every civilised country. It announced the success of the greatest work hitherto attempted by man; and, on the completion of so important an event, the scientific public must with good reason insist on having some details of the gigantic labours which have excited the attention of all intelligent men, and which, though well nigh finished, still demand much skill and energy.

The St. Gothard Tunnel is intended to form part of the railway connecting the North Sea with the Mediterranean; the ports of Belgium, Holland, and Germany with Genoa; the Rhine basin with that of the Po, by passing through the chain of the Alps at its most central point. If this point has been chosen in preference to any other, it is because the line through St. Gothard is the most direct. A proof that the passage is well selected is that, if one compares all the roads crossing the Swiss Alps, he will find that the St. Gothard one is more frequented by travellers either walking or riding than all the others together.

If, therefore, the Mont Cenis railway preceded that of St. Gothard, it is because there were political reasons for its accomplishment; it is because science had not yet discovered the means of boring, in a relatively short time, long tunnels without shafts. But the experiments made in Mont Cenis being conclusive, the governments, cities, companies, and private persons who were interested all set to work, and the great international line subsidised by Italy, Germany, and most of the Swiss cantons was commenced in real earnest.

Before the Company was formed, a most thorough inspection was made on the spot, and the engineers concluded that the great inevitable tunnel could have its openings only near the small village, Goeschenen (Canton Uri), on the north, and near Airolo (Canton Tessin) on the south. Goeschenen is 1,109 metres above sea-level, and 672 above the Lake of the Four Cantons. Airolo is 1,145 above the sea. Considerable works, therefore, are necessary to reach so great elevations by means of a railway. But to lower the level of the tunnel by only a few metres while awkwardly increasing the length, and to ascend higher to lessen that length, only alleviated the difficulty to a trifling extent, especially on the south slope, where Airolo is almost the only possible point of entrance.

Both openings being thus decided upon, the first thing to do was to lay down the plan of the tunnel and ascertain its direction and level. Thanks to the excellent topographical map possessed by Switzerland, it was evident, even without examination made on the spot, that the tunnel might be straight, and that its length would be about 15,000 metres. Afterwards it was thought preferable to complete it at the southern end by a slight curve, to give more facilities for building the Airolo station, but the tunnel throughout was opened in a straight line, and it was not till later that a curved bifurcation of 125 metres was made near the mouth.

In St. Gothard there were not the same topographical advantages as in Mont Cenis. In the operations made to trace the tunnel under the latter, there was at the highest part of the ground a starting-point from which could be seen, if not the two openings, at least points in their neighbourhood placed on the continuation of the very axis of the tunnel. They were able accordingly to build at a high elevation an observatory supplied with a field-glass rotating in a vertical plane passing through the middle of the tunnel. From that observatory two other observatories were determined in the same vertical plane towards the mouths of the tunnel, and the field-glasses of the two new observatories very easily supplied, when necessary, the direction of the tunnel axis.

On St. Gothard it was entirely different. The mountain presents several ridges over the tunnel. At no point can the places near both openings be seen at once, and some of the summits are so steep and high that it is impossible to take observations, and any *direct* tracing of the line over the mountain is impracticable. It was therefore necessary to make an *indirect* tracing, that is, to connect the extremities of the line of direction by means of a chain of triangles, calculate the relative position of the two openings, and thence deduce the angle formed by the tunnel axis either with the sides of the triangles of which Goeschenen and Airolo are the vertices, or with the meridian. This operation was facilitated by the great triangulation of the map of Switzerland, five vertical points of which are near the tunnel and could be utilised, but only as checks, since the entire work was done afresh, and it was even necessary to reconstruct the points of observation, many having disappeared, and nearly all of them being on peaks placed above the perpetual snow-level. M. Gelpke, the engineer who had charge of the triangulation, measured a base line of 1,450 metres in the Urseren Valley, and then measured all the angles not only of the eleven triangles connecting the two mouths, but also of several triangles used for verification. It was necessary to work with extreme care, as a mistake in direction of only five seconds would produce a deviation of 40 centimetres in the middle of the tunnel. Besides the verification indicated, another was made by a system of arrows continued from the point where the tunnel-axis was calculated to intersect the observer's base line in the direction of the line forming with the base the angle given by calculation. This measurement by arrows was continued as far as Goeschenen, and led precisely to the mouth of the tunnel.

When measuring the angles of the triangles, M. Gelpke also took the vertical angles, and was able to calculate the difference in altitude of the tunnel-mouths. The level moreover was ascertained directly, and connected with the great European triangulation of precision, which passes directly over the Col of St. Gothard.

The direction and level having been thus obtained, observatories were placed near the tunnel-mouths, to serve as direction-points for the miners. At Airolo there was no difficulty in fixing the observatory, the site being suitable; but at Goeschenen, where the direction of the tunnel forms with the narrow and deep valley a very acute angle, there was not a sufficient line of sight, and it was necessary, in order to have one, to traverse two projecting rocks with two small bores 115 and 93 metres long, and only then could observations satisfactorily be made either as to direction or level.

In each observatory was placed a large theodolite, and after a very large number of angular measurements and astronomical verifications, arrows and levelling-marks were placed in the exact direction of the tunnel, so that at every moment during the first stage of the tunnelling works the direction in which the miners were advancing could be verified.

In spite of the success of the verifications of M. Gelpke's labours, the managers of the St. Gothard Company thought it right to recommence the triangulation in 1874, employing another engineer, M. Koppe, and a different system. Instead of limiting himself to summits in the neighbourhood, and having moderately-sized triangles

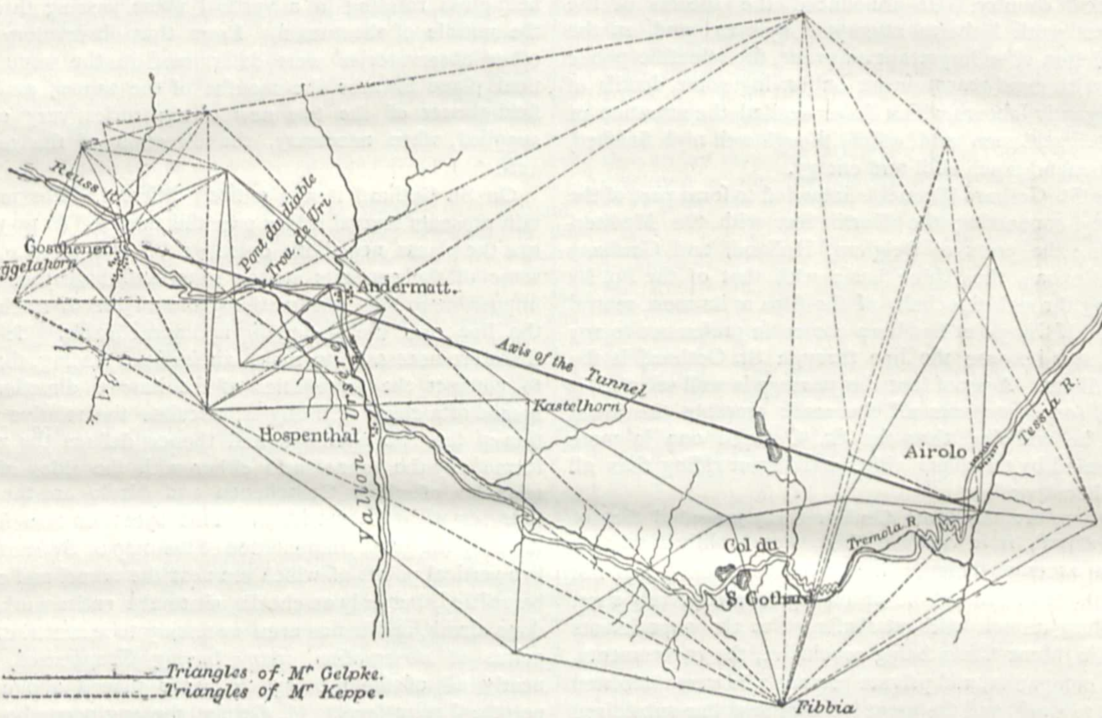


FIG. 1.—Plan of Triangulation for the St. Gothard Tunnel, $\frac{1}{133333}$.

(the longest side being under 7,000m.), M. Koppe preferred to use as large triangles as possible, in order to connect together by a minimum number of intermediary stations the two tunnel openings. No doubt, since these openings are at the bottom of valleys, a certain number of triangles was necessary to descend from the heights; but the sides of triangles of from 10 to 15 kilometres connect directly the signals on the heights above Goeschenen with those of the heights near Airolo, and several triangles for the purpose of verification were laid down. The two operations proving the accuracy of the works were (1) the correspondence *within two seconds* of M. Koppe's triangulation with that of M. Gelpke; (2) the following almost direct and very interesting verification. M. Koppe started

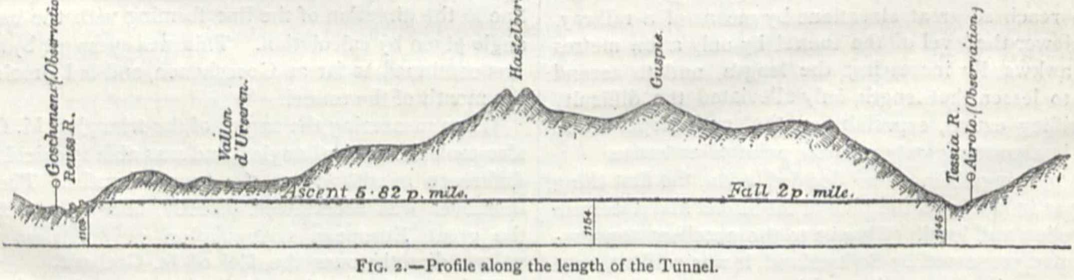


FIG. 2.—Profile along the length of the Tunnel.

a system of arrows from Airolo northwards along the axis of the tunnel over the mountain, following the direction given by the observatory to the highest attainable point near Kastelhorn, where he erected a mast. Being unable to reach this point from Goeschenen by going south, on account of the local difficulties, he started northwards also from that opening, in the direction of the continuation of the tunnel-axis. He thus ascended the flanks of a mountain till he could observe in the distance the mast he had erected near Kastelhorn. Then observing the level of Goeschenen with a theodolite, he moved his field-glass in a vertical plane, and directing it upon Goeschenen, he was delighted to see his mast almost in the centre of his field of vision, at a distance from his estimated axis of about 15 centimetres. It might therefore be assumed that that axis traced in air is of great accuracy.

As to the operations for verifying the direction within the tunnel, they are in theory very simple. So long as the boring was not far advanced it was given directly by the direction-points by means of field-glasses fixed in the observatories. At Goeschenen this was done till they had bored to a depth of 1,300 metres, but at Airolo the atmosphere was saturated with vapours, and thus they could

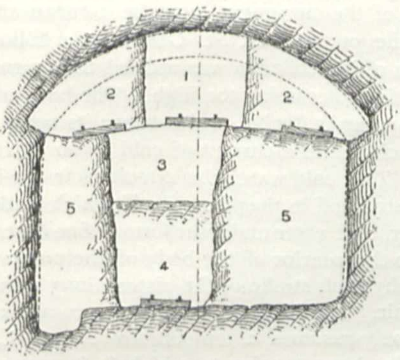


FIG. 3.—System of excavation, $\frac{1}{200}$. 1, Advance Gallery; 2, Side workings; 3, 4, "Cunette de Strosse"; 5, "Strosse."

not advance more than 600 m. Beyond those limits, therefore, it was necessary to have several stations. By means of the observatory, lamps were fixed in the exact direction of the axis. Then, a theodolite being placed behind the lamps in a line with them, its telescope was reversed, and the direction thus determined from the bottom of the boring. Lamps were then placed in this line of direction on hooks fastened to the roof of the tunnel. They could thus advance and increase the numbers of observing stations as they found necessary, either from the increased length of the boring or the want of transparency in the air. This operation, apparently very simple, is in reality very long and complicated, and to effect it various instruments were made use of which were improved during the works. As to the lamps, after trying different modes of lighting, they came back to ordinary petroleum lamps with round wicks, which were placed on stands carefully hung up and centred, and on which the instrument was then placed. To effect communication between the different stations of the instrument and lamps, experience soon proved that the simplest and most economical plan was to use a field telegraph, which was lengthened as they advanced.

Direction-points were given to the contractor every 200 metres, and they were fixed by producing the direction of the preceding points of reference. Twice or thrice a year, however, the operation of tracing the direction from the observatories was recommenced, as if that work had never previously been done; only when passing under each hook which had been fixed in the previous operations they compared the new result with the preceding, and thus ascertained the changes of direction. In the northern boring the greatest deviation was two centimetres, and that of the south, owing to the vapours, seven.

The result of that operation was a tunnel 14,920 metres long, starting on the north at the level 1,109, rising at the rate of 5·82 in a thousand to the level 1,152, which was attained about the middle of the tunnel, and a small level being left in case of any possible error in levelling, it fell at the rate of 2 in a thousand to the level 1,145 at the Airolo station.

Having thus indicated how the direction of the tunnel

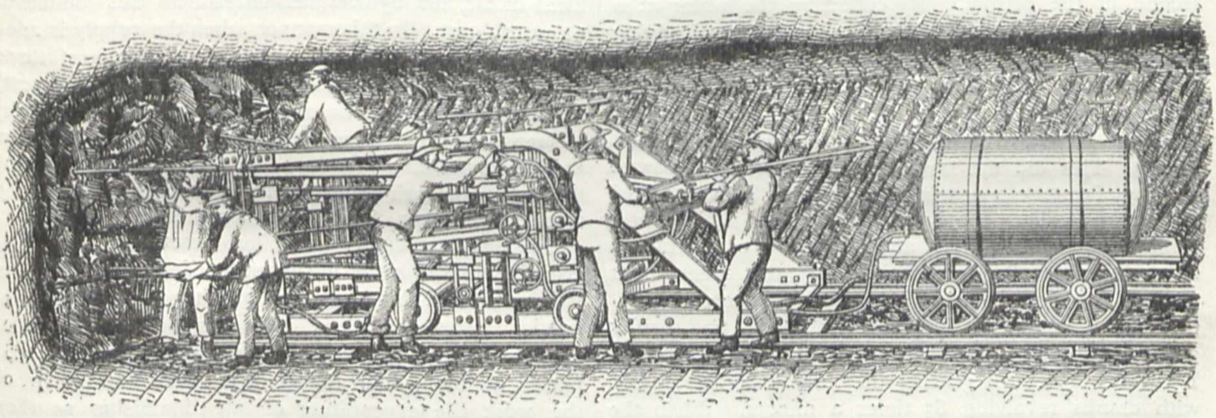


FIG. 4.—The borers at work.

was traced, let us examine the procedure employed in the works properly so called.

After the experiment made in Mont Cenis, they had only to follow it and at the same time take advantage of the improvements since made on the system; the two principles without which the work would have been impossible being mechanical perforation of mine-holes and the transmission of force by compressed air.

Without the English invention of machines for boring rocks, the opening of a long tunnel without shafts or external openings would have occupied so long a time that no company could undertake it. But, thanks to the experiments made elsewhere, M. Louis Favre, the contractor of the tunnel, was able to utilise those excellent boring machines which have powerfully assisted in hastening the work. His object was constantly in the direction of suppressing hand-boring, and he almost attained it by making use of several models of machines which cannot be described here, but which are distinguished by the names of their inventors, Dubois and

François, Ferroux, Mackean, and Turrettini. All these machines have one common object, to project forcibly, and with blows of the greatest rapidity, a tool against the rock, and, as it is drawn back, force it to rotate slightly on its axis. Some of the machines advance by an automatic movement; others require a workman to push them. But to keep these boring machines in action some considerable motive power is necessary, and in the Alps that motive power can only be water. The question, then, is How to collect it and transmit it into the tunnel?

Close by the Goeschenen opening runs the River Reuss, which has just passed under the legendary "Devil's Bridge," and precipitates itself into its narrow and rocky bed with an average fall of 10 per 100, forming a volume of water which is never less than 1,000 litres per second. M. L. Favre utilised a large rock standing in the channel to fix his dam, below which the water is received in a reservoir to deposit its sand and gravel. Thence it is led to the machines by a cast-iron pipe ·85 m. in diameter and 800 m. long. The useful fall from the pipe is 85 m., and it can supply 1,200 litres a second. This water works four Girard wheels with horizontal axes. Their diameter is 2·4; they revolve 160 times a minute, and are each of 250 to 280 horse-power. When turning a horizontal axis with cranks and wheels attached, the number of revolutions is reduced to 80 per minute.

At Airolo, the nearest river being the Ticino, which has no rapid descent, it would have required a very long canal to procure enough of fall. M. Favre therefore followed the advice of engineers who had studied the matter and assured him he would find enough water by making use of the Tremola, a torrent close by the tunnel, which falls into the valley with an average descent of 20 per 100, and is said to supply at least from 300 to 400 litres a second at the bottom. By collecting the water at a height sufficient to allow of the receiving reservoir giving a useful fall of 180 m., the necessary motive power could thus be found. The works were completed in 1872, but in February, 1873, the supply of the Tremola fell to 100 litres, and there was not sufficient power to turn the water-wheels. This did not last long, it is true, but the same scarcity of water reappearing next year, M. Favre resolved to form an auxiliary supply by collecting the Ticino water 3 kilometres up the river, and thus have a new fall in order to meet all eventualities. By this means four tangential water-wheels with vertical axes are turned; they are of bronze, with a diameter of 1·2 m., and revolve 350 times a minute, each being of about 200 horse-power. By means of conical gearing they turn a horizontal axis with cranks revolving 85 times a minute. When the supply of the Ticino is insufficient, it is made to act on a smaller number of wheels; and then on the vertical axles, which are no longer turned by that stream, there are other water-wheels slightly different on which the water of the Ticino is brought to bear.

The power communicated to the horizontal axis at each opening is transmitted into the tunnel by means of the remarkable system already employed at Mont Cenis, and the inventor of which is Prof. Colladon of Geneva. This engineer conceived the idea of using for that purpose compressed air. The immense advantage gained by it is the transmission of a motive power, whatever the temperature or distance may be; moreover, it serves for ventilation and the supply of fresh air.

But the compression of air develops great heat, which injures the machine. At Mont Cenis, to avoid this heating, the air was compressed by means of pistons of cold water, which were themselves pushed by ordinary pistons moving up and down in pump cylinders. In order that the piston pushing the water should move it without splashing it must itself be moved very slowly, and the quantity of air required being considerable, the slowness of the movement must be compensated by the size of the pumps. At St. Gothard the following new invention of M. Colladon was utilised to prevent the temperature from becoming too high. The body of the compression-pump is double, and between its outer and inner cylinder circulates a current of cold water introduced by pumps. This cold water also circulates in the hollow rod of the piston and in the piston itself, which is also hollow. Moreover, cold water under the form of fine dust is injected even into the interior of the body of the pump, and then expelled by each stroke of the piston along with the compressed air. In this manner air compressed at 8 atmospheres only assumes a temperature of 32° C., which is lowered in the reservoirs to which it is exposed before passing into the tunnel. By this ingenious arrangement we can give much more rapidity to the pistons and proportionately diminish the volume of the pumps.

At Airolo and Goeschenen there are fifteen such pumps, divided at each place into five groups of three. They are horizontal, and their pistons are set in motion by beams connected with the cranks of the main axis. They serve a double purpose: at the bottom of each pump cylinder there are two expiration valves and one for supply. The Airolo pumps are of ·46 m. interior diameter, and at each stroke of the piston (through ·45 m.) a pump receives 71 litres of air, and by compression reduces this volume to a seventh part. In this manner, when the supply is adequate (160 litres for each wheel), four groups of three pumps (one of the five groups being generally left at rest) receive in twenty-four hours 208,000 cubic metres of air, whereas the volume required is 104,000. At Goeschenen the pumps are of ·42 m. diameter, and at each stroke of the piston (through ·65 m.) 87 litres are received; but the number of strokes of the piston is rather less.

The compressed air is sent into reservoirs, where it is cooled, and the water in suspension deposited. Thence it is conducted by tubes into the tunnel. These conducting tubes are pipes of hammered iron of ·20 m. diameter, which are bolted together end to end, and placed all along the tunnel. At each new stage of the work the expenditure of air diminishes as they advance, and the diameter of the pipes is accordingly reduced to ·14, then to ·10, till at last they terminate in india-rubber tubes of 5 centim., which supply the compressed air for working the advanced gallery.

The process being now known, let us examine how the work is organised. The tunnel being 8 m. wide and 6 m. high above the rails, since it must be vaulted, it is necessary to make a clearance as high as 6½ m. above the rails. The first thing is to make a preliminary or advanced boring 2½ m. high and 2½ m. broad. For this first boring M. Favre adopted the Belgian system, according to which the preliminary gallery is entirely at the top of the tunnel. To open this boring, six perforating machines were arranged on a cast-iron stand placed on rails. These machines, first of all, perforated six holes in a horizontal direction. Then shifting the points, they made six new holes, and again a third set and a fourth set. This ought to have produced twenty-four perforations; but as there was always some delay from the change of drills or other hindrances, they seldom had more than eighteen or twenty. As the tools striking on the rock became very hot, and much dust was produced in the holes, they required to be constantly moistened by a jet of water. The water was carried on a train behind the cast-iron stand, and, by means of an india-rubber pipe to convey compressed air, was projected forcibly in several jets.

The holes bored were generally 1 m. deep. When the face of the rock was, in the opinion of the head miner, sufficiently perforated, the stand was drawn back and the holes were charged with a mixture of dynamite and clay. Then they were fired by means of slow matches, so arranged that the central holes should explode before the others. After that they broke the large fragments, loaded trucks with the rubbish, and rolled them towards the opening; and thus the gallery was advanced about 1 m. Then placing rails in front, the stand with its boring-machine was brought back, and the mining recommenced.

The advance was more or less difficult according to the nature of the rocks, but on the whole the contractor was fortunate in this respect. The rock was hard, but its hardness was almost always suitable for perforation. About three-quarters of an hour were necessary to make a hole 1 metre deep; and under favourable circumstances four operations would be made in twenty-four hours, that is to say, an advance of 4 metres on each side. The most favourable rocks were granite, gneiss, mica gneiss, and mica schist. The layers, which were almost vertical, lay from east to west, and were therefore at right angles to the direction of boring. There were, however, three unfavourable circumstances which greatly hindered the works at certain times:—(1) The infiltrations of water in the Airolo tunnel during the first months of the operations, and in such quantities that a regular river flowed from the southern opening. Fortunately dynamite is not affected by water, and after boring several hundred metres the infiltration stopped. (2) Rocks of exceptional hardness were met with from time to time blunting the best drills, and scarcely an advance of 1 metre a day could be made. (3) In the Goeschenen gallery, at 2,700 m. from the mouth, they came upon a bed of rock entirely disaggregated, where they could only work with the pickaxe and were afraid of being buried. Under the enormous pressure of the mountain the props were crushed, and even the arches of masonry overthrown. At this part the advance was from 30 to 40 m. a month, and it continued for more than four months. There was some danger even of the rock falling behind where the workmen were engaged, and so isolating them and all who were beyond. In order to strengthen this dangerous part it was necessary to employ a special system of arches strengthened with iron.

When the advanced boring was completed, it was enlarged on the right and left. After that was done, they proceeded to build the arches of the roof, and then dug to the level of the tunnel's base a trench of about 3 m. wide, called the Cunette de Strosse. It is not dug in the middle, so as to leave as long as possible the way clear on the higher level. Then all is removed that remains on the right and left of the trench, and which is known as *Strosse*.

These different excavations are almost all done by perforations, and the holes being bored downwards, the work is more easy, whether for boring or exploding.

The transport of rubbish and materials had to be performed as often as possible by more powerful agents than manual power or horse-power. Steam-engines were out of the question, the air being already vitiated by the constant percussion of the boring-drills. The compressed air was employed to move the locomotives, just as if they were acted upon by steam. It was collected in reservoirs placed on the locomotive trains, and by simply turning a cock the machine was moved or stopped. But as the air of the atmosphere did not furnish a "course" sufficient, except by means of enormous reservoirs, they constituted "compressors" of the same system as those already in use, but which compressed the air to fourteen atmospheres. With so considerable power the locomotives were sufficiently supplied by ordinary reservoirs.

Charge of the works was handed over to the contractor, M. Favre, in October, 1872, on condition of completing them within eight years; should they occupy nine years, a heavy penalty was attached. On February 29, 1880, the two advanced borings met with great accuracy. By a mistake the general direction only was taken, and therefore the exact amount of error was not ascertained, but it could not have exceeded 10 centimetres (or less than 4 inches)! This meeting did not take place in the middle of the tunnel, but at a point about 600 m. nearer Airolo than Goeschenen. The newspapers fully reported the event, the joy of which was greatly mingled with sorrow on account of the death of Louis Favre, the energetic and intelligent contractor, who was to have presided at the ceremony, after having organised and directed all the details, and at the very moment when he was about to realise the aim of his efforts. He died suddenly in the tunnel, the offspring of his labours, on July 19, 1879. Born in 1826 in Chêne, near Geneva, he left his native place as a journeyman carpenter, and by his intelligence and talent returned to Switzerland thirty years afterwards to be intrusted with the greatest undertaking of the present time. As he had thoroughly well organised everything, the works were continued without him, and also completed; but when shall we find such another man to begin again such another undertaking?

There is still much to do in the tunnel—rocks to clear away, mason-work to be built, &c.—but now the ground is known, and there is no fear of being able to complete the tunnel within the stipulated time. But what purpose would it serve? The lines of approach could only be finished long after the tunnel, being much less advanced. It is proposed, however, to have carriages running next winter between Goeschenen and Airolo, driven by atmospheric locomotives. That would no doubt be an advantage, but would the result be worth the great exertions necessary?

Much has been said of the extreme heat which prevails in the tunnel, and there is no doubt it is almost intolerable, being 32° to 35° C., and is injurious to men, and especially to horses. But when the tunnel is entirely cleared out and there are no more dynamite explosions, a current of air will set in; and as there are only 3 kilometres more than at Mont Cenis, there is no reason why, in respect of ventilation, it should be more dangerous.

We may therefore confidently utilise this magnificent way of communication, and be proud of living at the period during which it has been opened.

Geneva ADOLPHE GAUTIER, Engineer

COLLOIDS

The Influence of Colloids upon Crystalline Form and Cohesion. By W. M. Ord, M.D. (London: Stanford, 1879.)

THE series of researches which Dr. Ord has put into the hands of the public in the volume before us possess a double value, as dealing with problems in molecular physics of the deepest intere t to the physical investigator and of the highest importance to the surgical practitioner.

The starting-point of Dr. Ord's work is to be found in very remarkable research made more than twenty years ago by Mr. George Rainey, on the spherical forms assumed by carbonate of lime and other crystalline substances when deposited in the midst of gummy or colloidal liquids. The process by which this assumption of a globular form is effected Mr. Rainey termed "molecular coalescence." He also assigned the name of "molecular disintegration" to another process by which the conditions are reversed, and which breaks up the spheres into forms possessing a structure more nearly approaching a crystalline character. The most important of his deductions was undoubtedly the conclusion that the rounded forms of organised bodies depended on physical and not on so-called vital conditions. If solutions of gum arabic (containing malate of lime) and concentrated carbonate of potash are placed together in a bottle with as little mixing of the two as possible, the most perfect microscopic spheres are slowly deposited. They exhibit both concentric and radial markings, and in polarised light present a distinct "cross." They consist of carbonate of lime for the most part, but inclose portions of gum also. When plunged into stronger solutions of gum these spheres lose their globular arrangement and break up into radial lines, and subsequently into smaller particles. This is Mr. Rainey's fundamental fact; and others entirely analogous have been observed by Harting, Guthrie, and Montgomery, with different substances, and by somewhat different processes. Mr. Rainey was of opinion that these artificial spheres were the exact analogues of the globular masses detected by the microscope in bone, shell, in the testa of crustaceans, as in the tail of the shrimp, for example, and in ossified tendons, and he proceeded to argue that by a purely physical process in which the colloidal environment was concerned, not only bone, but starch granules and even the crystalline lens of the eye were formed. To these fundamental experiments Dr. Ord has himself contributed parallel observations on the disintegration of crystals of uric acid, carbonate and oxalate of lime, murexide, &c., which, when inclosed in gelatin, glycerine, or glycerine jelly, lose their sharpness of outline and transparency of substance, and progress by degrees towards the spherical form.

Dr. Ord now points out the very important relation held by these obscure molecular processes to the production of spherical and spheroidal concretions of calcareous matter in the renal and urinary organs, and he has sought to establish this relation by two lines of investigation: (1) by microscopic observation of calculi and other urinary deposits obtained under certain morbid conditions; (2) by synthetically obtaining identical forms from the various salts, phosphates, urates, &c., in the presence of colloids under varying conditions of temperature and hydration.

The substances thus investigated were uric acid, urates, oxalate of lime, phosphates, and carbonates; and the colloids employed were gum, gelatin, albumin, grape-sugar, starch, &c. The experimental results were throughout compared with the microscopic observations of Thudicum, Beale, and Prout, and a very large proportion of the forms observed in nature were artificially reproduced, thus affording pregnant suggestions as to the varying circumstances which prevailed in their natural production. Many of these comparative observations are of considerable interest. Thus we learn, on p. 55, that the collospheres of uric acid are always very small and homogeneous when deposited in urine, though they are rare; and a "dumb-bell" form is still rarer. Both are found in albuminous urine of small density. The experiments with watery solutions of egg-albumin always gave large and brilliant spheres. The conclusion is that the presence of small quantities of urea may retard the formation of the collospheres of uric acid. This supposition is strengthened by the known effect of urea in small quantities in modifying the crystal form of chloride of sodium. Another deduction is of equal moment. Two-thirds of all urinary calculi are composed of, or start from, a nucleus of uric acid. This uric acid would be quite unlikely to cohere in globules without the presence of a colloidal body—the mucus which would undoubtedly be present. "To make calculi of uric acid without colloids would be as hopeless a task as making ropes of sea-sand." This should be remembered in attempting to prescribe a regimen for suspected calculous disease. Whenever the urine is for any length of time purulent and ammoniacal, the formation of calculus is to be looked for. This conclusion is confirmed by a case given in detail by Dr. Ord, in which paralysis led to renal disease, and which he sums up as follows:—"First comes the paralysis leading to retention; retention permits decomposition and the formation of carbonate of ammonia; then come cystitis and the mixture of mucus and albuminous fluid with the triple phosphate and the spherical urates; and so a calculus is formed." Some of the calculi figured by Dr. Beale—notably those of dumb-bell form—were experimentally found to be reproduced by a scarcely suspected substance—oxalate of lime. It was further shown that pre-existing crystals may be resolved into dumb-bell forms in two ways: either by the formation of a dumb-bell within the crystal, or by a disintegration of the crystal and its subsequent conversion in mass into a non-crystalline, homogeneous dumb-bell.

Catching at a suggestion of Rainey's that the peculiar action of the colloid resulted from its *viscosity*, Dr. Ord conceived that some independent evidence for or against this notion might be afforded by the influence of the apparent viscosity of the magnetic field. Without appending any criticism on the admissibility or otherwise of this analogical piece of reasoning, we will simply narrate the results of putting the question to the test of experiment. When oxalate of lime was deposited in a gelatin plug between the poles of horseshoe magnets, "there was an extraordinary increase in the size of all the forms, crystalline and non-crystalline, where the plug or gelatin was subjected to the action of magnetism, but there was no production of new forms or greater tendency to sphericity." Similar experiments with a large electro-magnet yielded crystals which in several cases *appeared to have their axes slightly twisted.* This observation, if confirmed, and if presenting any assignable relation between the direction of magnetisation and that of the alleged axial twist, would be in the highest degree interesting. Up to the present moment, so far as we are aware, no crystal presenting tetratohedral dissymmetry or optically active in the polarimeter has been procured by artificial synthesis. Is it possible that Dr. Ord's observation contains the germ of the method by which we may hope to procure the synthesis, not of the active tartrates and sugars only, but of quinine and other alkaloids also? Experiments with electric currents were also tried, but proved less satisfactory, though the electrolytic actions set up produced several unexpected results.

Later chapters in Dr. Ord's book are devoted to renal and biliary calculi other than those mentioned—including a very singular case of an indigo calculus—and to a short scheme for the qualitative examination of calculi, which contains valuable hints to the general practitioner.

Concerning the production of the collospheres themselves there does not appear to be any one assignable cause. Harting dwells strongly on the influence of the "nascent" state in which the crystalloid body is deposited by double decomposition within the colloid. This term will probably fall out of use by chemists so soon as they perceive that it is a term convenient only as a cloak for ignorance. A more satisfactory point is made by Dr. Ord in the suggestion that there exists a relation yet undiscovered between hydration and the colloidal state; the hydrate of fresh uric acid being a colloid. Dr. Ord is of opinion that hydrated colloids and strong solutions of very soluble salts alike prolong the colloidal state of certain crystals, thus favouring the production of spheroids. Dehydration, which in certain cases appears to determine the production of spheroidal forms, is obviously inadmissible as the cause in the majority of cases. Nor does the difference of crystalline form between one crystal-system and another appear to affect the collospheric condition, in which absolutely no smallest modifications attributable to this possible cause can be detected. Solubility undoubtedly has much to do with the matter, since insoluble crystalline substances yield the best spheroids; but by evaporation and by deposition from hot strong solutions even sulphate of copper and ferrocyanide of potassium can be thus obtained. We must therefore fall back upon the conclusion that the one important factor in the production of the collospheric condition is the influence of the colloid. Mr. Rainey, who came to this conclusion, attributed this action to the "viscosity" or tenacity of the colloid fluid; and hence he associates with true colloids such substances as glycerine (which is a true crystalloid) and other viscid substances. Dr. Ord, on the other hand, is disposed to regard the influence of the colloid as "a result of intestinal molecular movement inherent to the constitution of the colloid."

Arrived at this point, however, we cease to perceive any definite coherence between the various speculations which follow and in which the effects of pressure, of strain, and of hypothetic spiral waves, are mixed up with Brownian movements and chemical interaction. It is a pity that the all-important bearing of surface-tension at the boundary of two media, and of the elegant and instructive researches of Plateau, including his production of liquid spheroids, is not once alluded to, even in the remotest manner, by Dr. Ord. For our own part, we are disposed to attribute a very large portion of the influence which determines the production of these collospheres of solid matter to the same molecular actions as those which produce the surface-tensions between solids and liquids, and which cause the rise of liquids in capillary tubes and the production of liquid spherules in the experiments of Plateau.

In conclusion we must not omit to quote one experiment of Dr. Ord, that in which the rapid production of the collospheres is conducted under conditions suitable for lecture demonstration. A solution of pure urea of density 1·026 usually throws down shining white flakes of nitrate of urea on the addition of an equal bulk of strong nitric acid. If, however, a little egg-albumin be added to the urea solution before the nitric acid is added, spheres are formed of the greatest beauty, and appear "floating like snowballs" in the yellowish liquid.

OUR BOOK SHELF

A Guide for the Electric Testing of Telegraph Cables. By Capt. V. Hoskiær. Second Edition. (London: E. and F. N. Spon, 1879.)

THIS very unpretentious but very useful little manual has reached a second edition, and now reappears with several valuable additions. In his original preface the author states that he does not expect an electrician to discover anything new in its pages. Be that as it may, the electrician will acknowledge the debt he owes to Capt. Hoskiær for the precision and brevity with which all his directions concerning the practical details of testing are given. Without philosophising or going into mathematical reasons of why and wherefore, he gives the necessary formulæ in the shape most useful for practical calculations; and the necessary tables of logarithms, trigonometrical functions, and temperature coefficients are sufficiently complete to save reference to other more extended works. The twelve lithographed diagrams leave nothing to be desired in point of clearness.

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.*]

The Antiquity of Oceanic Basins

IT seems to have escaped Dr. Carpenter's notice[1] that, in a Report on the results of the Deep-sea Dredgings of Mr. Pourtalès

[1] Lecture before the Royal Institution, January, 1880.

for 1866, 1867, 1868, Prof. Agassiz[1] had already called attention to the probable great antiquity of the oceanic basins.

Dr. Carpenter seems also to have overlooked the series of physical observations of the depths of the sea commenced by the United States Coast Survey[2] in 1850, and carried on without interruption to the present day.

The statement made by Mr. Wild[3] that the deepest sounding of the *Tuscarora* is not trustworthy, because "no sample of the bottom was brought up," is apparently endorsed by Dr. Carpenter, who says: "The sounding wire of the United States ship *Tuscarora* twice broke without reaching bottom . . . . at depths considerably exceeding 4,000 fathoms." This should be modified by stating that the *wire broke while reeling in* twice, once the bottom was not reached, and five casts were made over 4,000 fathoms, bringing up each time a specimen of the bottom.[4] Capt. Geo. E. Belknap, of the *Tuscarora*, says,[5] speaking of the casts beyond 4,000 fathoms in depth: "The wire parted at the last two and deepest casts. . . . the result of momentary carelessness on the part of the men at the *reeling-in wheel*."

The method of sounding with wire has now been in use long enough to show that even if the *Tuscarora* had not brought up a single specimen of the bottom during her whole trip, and if the wire had *invariably broken while reeling in*, we could not for that reason alone have rejected those soundings as inaccurate.

Those who have sounded with wire know that the instant the sinker has touched bottom is recorded on deck, and the precise depth is then known, whether the cylinder is brought up or not. There is no more reason for rejecting the deepest sounding of the *Tuscarora* of 4,655 fathoms than for rejecting the 480 other casts which are accepted because a bottom specimen came up.

Cambridge, Mass., April 5 ALEXANDER AGASSIZ

On the Alum Bay Flora

IN the list of fossils appended to the paper upon the Alum Bay flora, brought before the Royal Society by Baron von Ettingshausen and reported in NATURE, vol. xxi. p. 555, the new species have Ett. and Gard. attached to them, implying that Ettingshausen and myself are their authors. It is only fair to Ettingshausen to state that I had no share in making the determinations, and to myself, that I accept them simply as provisional. Associated as he is with me in the work upon the British eocene floras, he felt that he could hardly publish preliminary work connected with it in any other way. I completely disagree with him, however, as to the utility of publishing new specific names unaccompanied by drawings or descriptions of any kind, and think that a simple list of genera, with the number of new species in each, would have been unattended with any inconvenience. He appears to me to attach altogether undue weight to mere priority in nomenclature, and the existence of such provisional lists, far from aiding research, must prove a serious difficulty to our fellow workers. In the highly probable event of an author being unable to come from some distant country to examine the specimens themselves, is he, for instance, to forbear naming every undescribed species of such common Tertiary genera as *Ficus*, of which eight new and unpublished species are in the list, of *Celastrus*, of which there are five, or of any other of the some fifty genera containing new specific names? He could not safely name even any indeterminable leaf or fruit, for fear it might be one of the long list of Phyllites or Carpolithes for which Ettingshausen has devised specific names.

[1] *Bulletin* of the Museum of Comp. Zoology, 1869, vol. i., No. 13.

[2] Coast Survey Reports, 1850 to present day; also Bibliography of Biological Results (*Bull.* Mus. Comp. Zool., vol. v., No. 9, 1878).

[3] "Thalassa," 1877, p. 15.

[4] "Deep-Sea Soundings in the North Pacific obtained by the United States ship *Tuscarora*" (Washington: Hydrographic Office, 1874, No. 54, p. 30):—

1874.	Fathoms.	
June 11	4,643	Wire broke; *bottom not reached.*
,, 17	4,340	Yellow and clay brown mud.
,, 17	4,356	Yellowish mud and sand and specks of lava.
,, 18	4,041	Yellow and clay-coloured mud and gravel.
,, 18	4,234	Rocky; point of cylinder came up battered.
,, 18	4,120	Yellow and clay-coloured mud mixed.
,, 18	4,411	No specimen; wire broke (*while reeling in*).
,, 19	4,655	,, ,, ,, ,,

[5] *United Service Magazine*, July, 1879.

But were our supposititious author to go on with his work, in spite of this "sword of Damocles," would Baron Ettingshausen claim priority and deprive the man who had first figured and *published* descriptions of them, of the pleasure of christening them in accordance with his views and wishes? If not, *cui bono?*

To show the purely provisional light in which the list must be regarded, I may mention that, unfortunately just as the Baron left England, a large collection, that of the late M. Watelet from the Grès du Soissonnais, came into my possession, and seems, on a cursory examination, to contain a preponderance of species identical with those of Alum Bay. None of Watelet's published species appear in the list of the Alum Bay flora, which therefore must of necessity be considerably modified to include them. The same may be said of the flora of Gelinden, of which a large series has also reached me.

Again, even in the only section of plants yet worked out by us for the palæontographical memoir, the ferns, discrepancies occur. Two ferns occur in this Alum Bay list which are not included in our fern flora from that locality. These are inserted on the authority of Heer, who states that he has seen them from Alum Bay; but as on the occasion of that gentleman's visit or visits to England many years ago the floras from the different localities had not been systematically collected, and were generally mixed together in museums, in the same drawers and cases, and cannot always be identified by the matrix, I prefer to adhere to the opinion of that indefatigable collector, Henry Keeping, who lived within a short distance of Alum Bay, and to my own, Mr. Mitchell's, and all other workers' experience, that no fern but *Marattia* is found there. At all events, if they are to be included in the Alum Bay flora, they should be so with reserve, especially as Prof. Heer's ideas as to the position of the localities and their ages are so hazy that he puts the Alum Bay leaves in the "Bartonisem" (above, if anything), or about 1,000 feet too high, and thinks that Bournemouth is somewhere in the Isle of Wight.

An illustration of the inconvenience caused by publishing names without proper figures and descriptions occurs to me. Heer named a small fern fragment which he supposed to be from Alum Bay, *Asplenium martinsi.* This name has got into works by Saporta and Crié, who have each tried to fit ferns of their own into Heer's meagre description. Neither had seen the original, nor could they give any information, and it was only after several attempts to obtain it that Ettingshausen received a rough sketch from Heer showing conclusively that the "species" in question was a fragment of the abundant and well-known *Anemia subcretacea* of Sézanne. I do not even now know whether it was upon this fragment or some other that Heer wrote that he had "seen this form" (*Anemia subcretacea*) from Alum Bay.

J. STARKIE GARDNER

Negritoes in Borneo

HAVING had inquiries addressed to me as to the existence of a Negrito race in Borneo, I think it may be useful to recall attention to, and possibly save from oblivion, a statement on this subject which was published by Windsor Earl in the *Journal* of the East Indian Archipelago. Mr. Earl says that a Capt. Brownrigg, who had been shipwrecked on the east coast of Borneo, informed him (*J.E.I.A.*, No. 9) that he had lived several months at a town some distance up the Berau River, and that during his stay the town was once visited by a small party of men from the interior, "who must have been of the Papuan race" (*sic*). He described them as being short, strongly-built people, black in complexion, with hair so short and curly that the head appeared to be covered with little knobs like peas; and with many raised scarifications over the breast and shoulders. He described them as being on good terms with the people of the town, mostly Bugis, and as supplying them occasionally with jungle produce.

Of this account it may be remarked that Mr. Earl would not have retailed it unless he had had some confidence in the credibility of his informant—that, so far as it goes, it is curiously circumstantial—and that these people are said to have come exactly from that district in Borneo where we might expect *à priori* to find Negritoes if they existed at all.

Whilst on the subject of Borneo, may I suggest that ethnologists should make a more sparing use of the term "Dyak" when treating of the Malay Archipelago? It should only be applied to tribes who themselves use it as the distinctive appellation of their people. As more than one tribe so uses it, there should always be prefixed some word still further limiting its application in each particular case. As employed by Malays, who are followed both by Dutch and English travellers, the word has scarcely better standing-ground in a scientific terminology than has "Alfuro."

The following fact with regard to the Sea Dyaks may be of interest. When Europeans first entered Sarawak the Kayans, properly so called, were dominant in the great Rejang River, and the Sea-Dyaks were strictly confined to the Batang Lupar, Saribas, and Kalakah rivers. Now the Sea-Dyak population of the Rejang is some 30,000, and the Rejang Dyaks are rapidly occupying the Oyah, Mukah, and Tatau rivers further up coast. On the original Sea-Dyak rivers the people always use the expression "we Dyaks" when they mention their own race; but on the Rejang the expression "we Iban" will invariably be heard—the explanation being that the Kayans habitually designate Sea-Dyaks as "Ivan" among themselves, whence the Dyaks have applied the name; but having no v-sound in their language, they say "Iban." The Kayan proper is rich in v-sounds. I have been informed, though I cannot vouch for the accuracy of the statement, that "Ivan" in Kayan is a term carrying with it a sense of opprobrium. However this may be, it is remarkable that so large a section of the Sea-Dyaks, who are so thoroughly dominant in Rejang, and are in constant daily communication with their original seat in the rivers to the westward, should in the course of some thirty years have come to habitually speak of themselves by the name given them by their foes. And it is the more surprising because the Sea-Dyaks generally give new names of their own to the geographical features of the district into which they immigrate.

Papar, North Borneo A. HART EVERETT

Seeing by Electricity

WE hear that a sealed account of an invention for seeing by telegraphy has been deposited by the inventor of the telephone. Whilst we are still quite in ignorance of the nature of this invention, it may be well to intimate that complete means for seeing by telegraphy have been known for some time by scientific men. The following plan has often been discussed by us with our friends, and, no doubt, has suggested itself to others acquainted with the physical discoveries of the last four years. It has not been carried out because of its elaborate nature, and on account of its expensive character, nor should we recommend its being carried out in this form. But if the new American invention, to which reference has been made, should turn out to be some plan of this kind, then this letter may do good in preventing monopoly in an invention which really is the joint property of Willoughby Smith, Sabine, and other scientific men, rather than of a particular man who has had sufficient money and leisure to carry out the idea. The plan, which was suggested to us some three years ago more immediately by a picture in *Punch*, and governed by Willoughby Smith's experiments, was this:—Our transmitter at A consisted of a large surface made up of very small separate squares of selenium. One end of each piece was connected by an insulated wire with the distant place, B, and the other end of each piece connected with the ground, in accordance with the plan commonly employed with telegraph instruments. The object whose image was to be sent by telegraph was illuminated very strongly, and, by means of a lens, a very large image thrown on the surface of the transmitter. Now it is well known that if each little piece of selenium forms part of a circuit in which there is a constant electromotive force, say of a Voltaic battery, the current passing through each piece will depend on its illumination. Hence the strength of electric current in each telegraph line would depend on the illumination of its extremity. Our *receiver* at the distant place, B, was, in our original plan, a collection of magnetic needles, the position of each of which (as in the ordinary needle telegraph) was controlled by the electric current passing through the particular telegraph wire with which it was connected. Each magnet, by its movement, closed or opened an aperture through which light passed to illuminate the back of a small square of frosted glass. There were, of course, as many of these illuminated squares at B as of selenium squares at A, and it is quite evident that since the illumination of each square depends on the strength of the current in its circuit, and this current depends on the illumination of the selenium at the other end of the wire, the image of a distant object would in this way be transmitted as a mosaic by electricity.

A more promising arrangement, suggested by Prof. Kerr's experiments, consisted in having each little square at B made of silvered soft iron, and forming the end of the core round which the corresponding current passed. The surface formed by these squares at B was to be illuminated by a great beam of light polarised by reflection from glass, and received again by an analyser. It is evident that, since the intensity of the analysed light depends on the rotation of the plane of polarisation by each little square of iron, and since this depends on the strength of the current, and that again on the illumination of the selenium, we have another method of receiving at B the illumination of the little square at A. It is probable that Prof. Graham Bell's description may relate to some plan of a much simpler kind than either of ours; but in any case it is well to show that the discovery of the light effect on selenium carries with it the principle of a plan for seeing by electricity. JOHN PERRY

Scientific Club, April 21 W. E. AYRTON

Musical Sounds within the Ear

I SHOULD like to know how far the musical sounds, which we sometimes hear within our ears, are of different pitch in different persons. From repeated observations I find that my left ear gives G, and the right one B. A friend of mine, who is a good performer on the violin, finds F and A respectively.

It is perhaps not without interest that in some parts of Germany (at least in Silesia) people believe these sounds to be indicative of one's being talked about, and that the sound ceases as soon as one thinks of the person who is supposed to do so.

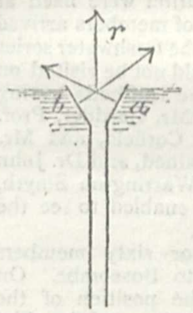
Carácas, March 18 A. ERNST

Ice Filaments

"THE comb-shaped masses of ice of fibrous structure" mentioned by your correspondent, in explanation of the inquiry made by the Duke of Argyll, are observed every winter in the southern portion of the United States, especially on the sloping sides of a path or country road where the surface-earth has been removed, and the natural clay sub-soil is not rendered compact by being trodden. The conditions requisite for its abundant production are a sudden reduction of temperature below the freezing-point when the clay soil is thoroughly saturated with water. When this occurs at sunset, the next morning, if the night continues favourable, will disclose a vast collection of fibrous filaments, from two to six inches in height, rising from the soil in close juxtaposition, generally holding aloft in their caps portions of the soil, the longest crystals appearing when the soil is free from surface-loam.

I have frequently given to my class this explanation of the phenomena.

The capillary tubes of the soil are all filled up to the surface with water. The sudden reduction of temperature freezes the water at the *surface*, but does not chill it within the soil below 32° F. The consequence is that this expansion, caused by congelation at the upper extremity of the capillary tube, compresses the walls of the tube externally, and causes the mouth of the tube at the surface to assume a conical shape, as in diagram. The congelation of all the water within the conical cavity causes pressure normal to the surface of the cone at *a* and *b*, and hence produces a vertical resultant, *r*, that raises the cone of ice. Capillary action immediately fills the little cavity with water, which in turn is frozen and elevated by the expansion force of its congelation. The filament thus grows in this simple way from its base. The soil in which these fibrous crystals or filaments form is never frozen; thus proving the correctness of the explanation.



They are formed very rapidly. I have on more than one occasion, when a sudden chill at sunset would start them growing, listened to the crackling of the little ice-crystals as they would break loose from each other, being pushed up by this expansive force.

I infer the filaments of ice formed on rotten wood are due to a similar cause, and that they will not be formed unless the reduction of temperature is quite sudden. That is, if the reduction of temperature is so gradual that the water somewhat below the surface in the cylindrical portion of the capillary tube is frozen, the crystals will not be elevated, but the ground will be frozen. WM. LEROY BROUN

Vanderbilt University, U.S.

Ophiolepis mirabilis

In a review of Lyman's description of the Ophiurans of the *Challenger*, which appeared in Nature, vol. xxi. p. 513, I was much surprised to find a criticism relating to a remarkable species which I described last year in the *Proceedings* of the Linnean Society.

The reviewer informs your readers that *Ophiolepis mirabilis*, Duncan, is a true Ophiopholis, having all the structures of the genus, and that it is allied to *O. aculeata*. This simple statement leaves the impression that I have made a mistake, and that I am ignorant of a well-known form. I therefore extract the following from the *Proc.* Linn. Soc., vol. xiv., Zool., p. 479, 1879:—

"*Ophiolepis mirabilis*.—This common species has the disk of Ophiolepis as diagnosed by Müller and Troschel; that is to say, the scales, which are of good size, and the large radial shields are environed by rows of small scales, as by belts. But the upper arm-plates have also the supplementary rows of small scales around them, and there are also large accessory side-pieces. Moreover, there are hooks on the side arm-plates. This mixture of Ophiolepian and Ophiopholian characters is very interesting, and this species, I consider, renders the abolition of *Ophiopholis* as a genus inevitable." In fact the beautiful Ophiuran is a synthetic type, and I prefer its teachings to authoritative statements. P. Martin Duncan

Hastings, April 9

The Stone in the Nest of the Swallow

Your correspondent, P. P. C. Hoek (Leiden), will find the information which he asks for under this heading (vol. xxi. p. 494) in an article which appeared in the *Zoologist* for May, 1867, entitled "An Inquiry into the Nature and Properties of the Swallow Stone and Swallow's Herb," by J. E. Harting

24, Lincoln's Inn Fields, London, W.C., April 14

The Songs of Birds.—Mr. C. C. Starling asks to be informed of any book or paper which treats upon the musical properties of the songs of birds.

Dew Claws.—A. N. asks if any correspondent familiar with wild species of *Canis* can tell him whether the rudimentary hind toe is invariably present, or, if not, in what proportion of individuals, and whether it has bony function with the metatarsus?

THE EASTER EXCURSION OF THE GEOLOGISTS' ASSOCIATION TO THE HAMPSHIRE COAST[1]

The head-quarters of the Association were fixed at Bournemouth. A large number of members arrived before Easter, and were able to explore the freshwater series to the west of Bournemouth, which could not be visited on the excursion. An excavation into the leaf-beds having been opened a few days previously by Mr. Gardner, Prof. Morris, Dr. Henry Woodward, Prof. Corfield, and Mr. Birch, a number of fine leaves were obtained, and Dr. John Evans, Prof. McKenna Hughes, Mr. Warrington Smyth, Prof. Bonney, and many others, were enabled to see the leaves *in situ* and the method of work.

On Easter Monday some fifty or sixty members assembled, and the party proceeded to Boscombe. On the way the director pointed out the position of the Bournemouth series in the eocene formation, and the chief geological features of the coast. Far to the west could be traced the cliffs whence had been obtained a rich dicotyledonous flora, shed apparently from forest trees, which clothed the hilly slopes of the right bank of the eocene river. It is remarkable to notice in how many respects this flora differs from those found nearer Bournemouth, most notably so in the total absence of palms. The next mass of cliffs is almost unfossiliferous, and from its confused bedding is now conjectured to present a transverse section of the actual bed, silted up, of the old eocene river. Between this and Bournemouth, for nearly a mile, extends the eastern series of leaf-beds, containing the remains of a more tropical flora, derived, perhaps, from low-lying country on the left bank of the old river. Among the palms, which are abundant, can be recognised such genera as Phœnix, Calamus, Iriartæa, Sabal, &c., and among the ferns, species scarcely differing from such magnificently tropical forms as *Osmunda javanica*, *Chrysodium aureum*, *Gleichenia dichotoma*, *Lygodium dichotomum*, &c. Beyond these cliffs, skirting the downs of nearly vertical chalk, are the Lower Bagshot beds, in which the well-known leaf-beds of Creech Barrow, and Alum and Studland Bays are situated. A very small portion, however, of the fresh-water Bournemouth series could be actually examined on Monday, for the chief object in view was to investigate the recently-discovered marine series, described for the first time in the pages of the *Journal* of the Geological Society less than twelve months ago. The passage from the one series to the other was well seen, although from the absence of slips and consequent inaccessibility of the beds, few fossils could be obtained. The beds are mostly dark sandy clays, highly charged with lignitic matter, and they contain in places well-preserved fruits and teredo-bored wood. The evidence of their marine origin is amply demonstrated by the presence of casts of Bracklesham mollusca, masses of oysters, bryozoa, and crustaceans. Overlying them are the clean white sands, with flint shingle beds, of the Boscombe series. These eocene shingle beds, from the perfectly-rounded form of the pebbles composing them, show the former prevalence of heavy surf upon the old shore-line. In many cases the condition of the silex is wholly or partially changed into a soft, white, chalk-like mass, entirely free from carbonates however, and much speculation was indulged in concerning the nature of this change. The party having been joined by Dr. Alman, president of the Linnean Society, and Mr. Pike, owner of the vast china-clay pits near Wareham, the curious Honeycomb chines were explored, and the zone of nipadites pointed out, crowded in places with the empty husks of fruits which had floated out to sea. At another point proteaceous leaves and tubular borings of annelids, filled in with horizontally-disposed lignitic matter, were noticed. On the way to Hengistbury Head it became apparent that as the fresh-water beds present a transverse section across a vast river channel, so the marine beds present a similar section through a great eocene beach which formerly sheltered a stagnant lagoon. These towards the east are seen to be composed of larger and larger shingle, whose well-rolled appearance indicates the distance it must have travelled. Attention was particularly called to the resemblance of the Boscombe series to the so-called Upper Bagshots of the London Basin.

Arriving at the Headland, after skirting its base and examining its remarkable geology, the party somewhat rapidly made their way through the heather on the summit, past the prehistoric double wall and ditch, and across the Stour and Avon by ferry to Christchurch, where Mr. George H. Birch gave a most interesting historical sketch of the priory.

The second day was devoted to the cliffs between Mudiford and Hordwell. The main features of the coast were well seen as the haze lifted. The sequence of the beds from Hengistbury to Highcliff was pointed out by the director, and the Barton clays and sands, the Upper Bagshots and Headon beds of Hordwell were examined, and numerous fossils collected. During the short stay for lunch Prof. Morris favoured the party with an address, in which he clearly placed before them the data for the correlation of these beds with those of the rest of Europe, and sketched in eulogistic terms the work of those who have made it possible to trace the history of their deposition. The members reached Lymington in time to return to London or Bournemouth by the 5.50 train.

The excursion, which was unusually largely attended, was keenly enjoyed, owing to the magnificent weather and the beauty and interest of the country traversed.

[1] Director, J. Starkie Gardner, F.G.S. &c.

DEEP-SEA DREDGING AND LIFE IN THE DEEP SEA[1]

III.

HOW is it that the general absence of ancient forms from the deep sea is to be accounted for? It is hardly probable that the struggle for existence in the great depths is very severe. The fact that so helpless an animal as a Pycnogonid can grow to a length of two feet points to the existence of easy conditions of life. Even if the struggle in the deep sea were as great as in shallow water we might have expected that it would extinguish these different forms from those which it exterminated near the shores. It seems on the whole probable that the deep sea may have been entirely devoid of life during the earlier geological epochs. The modifications existing in deep-sea animals as adaptations to their special modes of life are not much more important than those exhibited by animals inhabiting caves of comparative recent origin, such as Proteus or those living in the deep waters of the large lakes of Europe, which are also of no great antiquity, such as the air-breathing water-snails, which, from the necessities of deep-water life, have adapted their lungs to aquatic respiration. A long time has not therefore been required for these modifications to take place.

It has been, I believe, commonly assumed that the water of the ocean was originally fresh, or nearly so, and that it became gradually salter as the rivers brought into it salts as part of the products of denudation. But surely the primitive sea must have been highly charged with saline matters of all kinds.

When the earth was still intensely heated, the whole of the water now on its surface must have been present as gas in its atmosphere, at first no doubt dissociated, but afterwards as aqueous vapour. Since if the sea-bottom and continents were smoothed down to a uniform level, the sea would still suffice to cover the entire earth to a depth of over 1,000 fathoms, aqueous vapour equal to a layer of water of that thickness must have existed in the atmosphere and have produced a pressure of more than a ton on the square inch at the earth's surface. To this pressure must have been added that produced by all the other vapours with which the primitive atmosphere must have been filled. As the earth cooled the water condensed on the coolest spots from time to time, boiled, and rose as vapour again. Mr. Mallet[2] conjectures that the first water formed on the earth's surface may have been even as hot as molten cast-iron. At last permanent seas were established. The waters of these heated to an intensely high temperature under great pressure must have dissolved salts in abundance from the freshly consolidated earth's crust, and being constantly in a state of ebullition as the pressure diminished at the surface with the growth of the seas, or the temperature of the earth's surface varied in different places, must have taken up vast quantities of rock-matter in suspension and become thickly charged with volcanic mud. Intensely hot rain must have fallen on the land and have washed down more salts and mud into the sea. The whole ocean must have consisted of a vast mass of seething mud.

It must have required a protracted period for the ocean to become clear, and for its deposit, which was perhaps somewhat like the present deep-sea red mud, to settle, and possibly the deeper water long remained uninhabitable, being overcharged with various gases and salts and suspended mud. In connection with the question of the probable development of the earliest forms of life in heated water holding abundant salts and gases in solution, it is of importance to note that various algæ at present thrive in very hot mineral springs in various parts of the world.[1]

To this original deposit of mud on the deep ocean floor the deposits which have since been formed possibly bear but a slight proportion in thickness, for it must not be forgotten that all the Globigerina mud and other organic deposits now in course of formation on the sea bed are ultimately derived from the land. The Globigerinæ merely distribute the lime washed down by the rivers more evenly over the ocean floor, by concentrating it in the substance of their shells. The organic muds are in their origin products of denudation, and if the whole land now above the sea were washed into the ocean and evenly distributed a deposit of only about 500 feet in thickness would result.

FIG. 16.—*Umbellula Greenlandica.*

I shall, in conclusion, speak of some of the physiological conditions of life in the deep sea. Deep-sea animals, as a rule, have either no eyes at all or have very large eyes. As an example may be cited the crustacean, *Astacus zalencus*, most closely allied to the common crayfish which Prof. Huxley has lately made illustrious. It is from 450 fathoms. It has no eyes at all, but one of its nippers is extraordinarily long and delicate, and possibly the animal uses it to feel its way with, as a blind man uses his stick. There are also abundant hairs on the animal's surface, which are probably organs of touch.

Many deep-sea crustacea, however, and fish have very large eyes indeed, evidently for the purpose of making

[1] Friday Evening Lecture delivered at the Royal Institution on March 5, by H. N. Moseley, F.R.S., Assistant Registrar of the University of London. Continued from p. 572.

[2] R. Mallet "On the Probable Temperature of the Primordial Ocean of our Globe." *Quart. Journ.* Geol. Soc., 1880, p. 115.

[1] See "Notes by a Naturalist," pp. 36, 383, 410.

use of some small quantity of light which must exist in all depths. In the absence of sunlight the only other source of light must be the phosphorescence of certain of the deep-sea animals themselves. Dr. Carpenter, Sir Wyville Thomson, and Mr. Gwyn Jeffreys came to this conclusion after some of their early deep-sea dredgings. There can be little doubt of its correctness.

Here (Fig. 16) is a deep-sea Alcyonarian, *Umbellula*

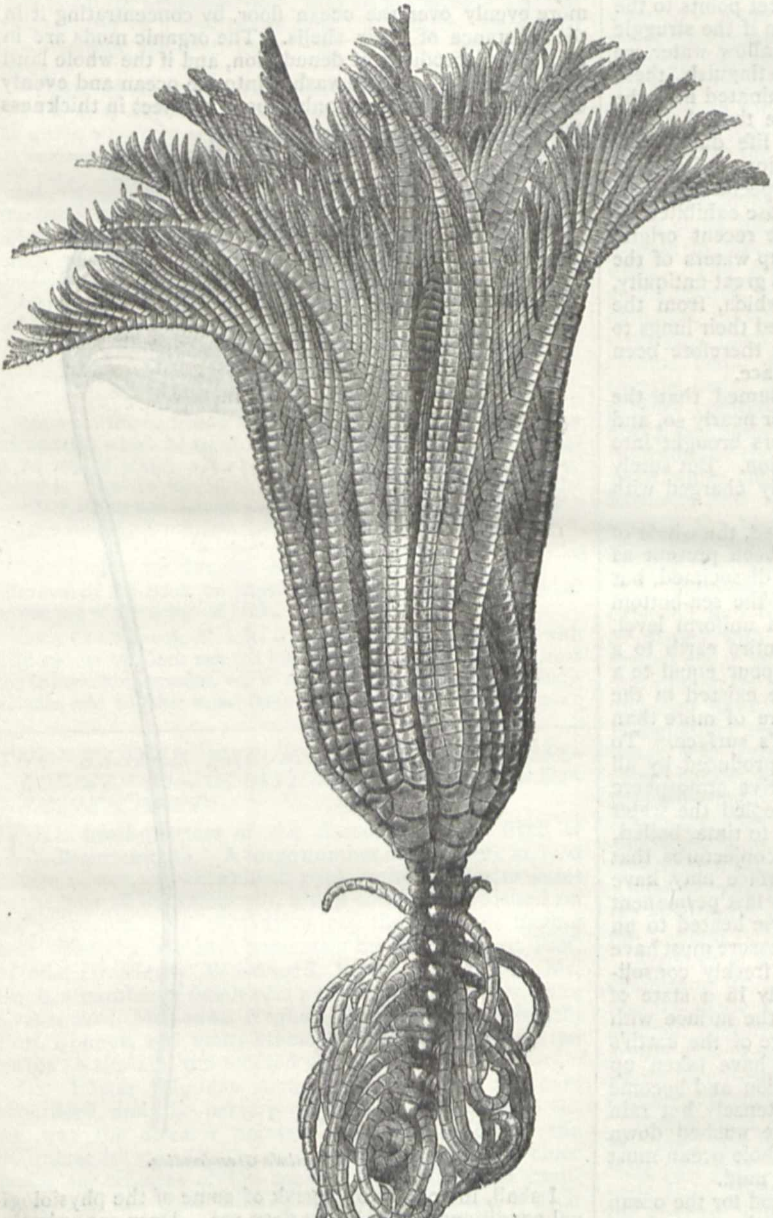


FIG. 17.—*Pentacrinus maclearanus*, Wyville Thomson.

Greenlandica, so named because it was first obtained off Greenland, being, like the deep-sea fish I exhibited, an example of a deep-sea form extending into shallow water in high latitudes. Umbellula consists of a bunch of polyps supported on the end of a long flexible stem, which is cut off short in the figure. This specimen was from 2,175 fathoms between Madeira and the Spanish coast. When it came to the surface it emitted a most brilliant phosphorescent light, as did also many allied forms dredged in deep water. No doubt these animals, like their congeners in shallow water, emit light in the deep sea; and the deep-sea animals with eyes probably congregate round them or grope their way in the gloom from one bunch to another as they lie scattered over the bottom, just as we half-feel, half-see our way from lamp-post to lamp-post at night in a London fog. Some lose their way, as we do sometimes, and get into shallow water, and a good many deep-sea animals have been from time to time picked up near the shores at Madeira and elsewhere, and have found their way into museums as great rarities.

No doubt the sense of touch is the one mainly relied on by most deep-sea animals. Very many are provided with special organs of touch, such as long hairs, or in the case of fish enormously long fin-rays. Unfortunately we did not examine the organs of hearing of any of the deep-sea animals which we dredged. Nearly all were far too precious to be dissected. Possibly some of the animals have the sense of hearing highly developed.

There is plenty of oxygen for respiration in the deep sea. Mr. Buchanan found that water from even 4,475 fathoms contained 4·06 cubic centimetres of oxygen to the litre, nearly as much as at the surface, where, however, the amount varies greatly, rising to as much as 7 cubic centimetres per litre. The smallest amount observed was ·6 of a cubic centimetre per litre in a depth of 2,875 fathoms. Even this amount would probably support life, for Humboldt and Provençal showed that tench could still breathe, though with difficulty, in water containing only one-third of that quantity of oxygen.

Life must be very monotonous in the deep sea. There must be an entire absence of seasons, no day and night, no change of temperature. Possibly there is at some places a periodical variation in the supply of food falling from above, which may give rise to a little annual excitement amongst the inhabitants.

There being no plants in the deep sea except parasites, the ultimate sources of food must be derived entirely from above, from the falling to the bottom of dead surface animals and plants, and of the *débris* washed from the shores. Sea-water acts as a most efficient preservative of animal tissues, and possibly at no very great depths and at the deep-sea bottom Bacteria may be entirely absent, so that decomposition in the form in which we are commonly acquainted with it does not there take place at all.

From experiments which I made on the rate of sinking of a dead Salpa I found that it would reach the bottom at 2,000 fathoms in four days, in which time it would even at the surface be hardly decomposed at all. It would thus afford good food to the bottom animals.

A large quantity of shore *débris* and vegetable matter carried down by rivers reaches the deep-sea bed. We found leaves, branches, and fruits in deep water, and one of the latter had its interior still fresh, and was full of animals feeding upon it. Mr. Agassiz found in depths of over 1,000 fathoms orange and mango leaves, sugar-cane, nutmegs, and land-shells in profusion, and many hermit-crabs actually inhabiting tubes of bamboo instead of shells. We found a land-crab once in 450 fathoms; no doubt it had drifted out hanging on to some floating object, and had sunk to the bottom, being unable to swim.

The numbers of animals to be found in the deep sea decrease rapidly in proportion as the depth exceeds 2,000 fathoms, and very probably the greatest depths have very little life in them. We at present know only of Rhizopods as inhabiting them. Even in depths of less than 2,000 fathoms the shallower waters are most productive, and probably the deep-sea fauna is most abundant not far from the upper limit of its range. It is here, in from 200 to 400 fathoms, that such forms as Pentacrinus are most numerous. Here in many places these animals, a few years ago the greatest of rarities, cover the sea-bottom, thickly set like trees in a forest, still as abundant as ever they were in geological times. It is probably scarcity in supply of food which limits the quantities of animals in great depths. No doubt food is always most abundant near the coasts.

Some animal forms appear to be dwarfed by deep-sea conditions of life. Others attain under them gigantic proportions. It is especially certain crustacea which exhibit this latter peculiarity, but not all crustacea, for the crayfish-like forms in the deep sea are of ordinary size. I have already referred to a gigantic Pycnogonid dredged by us. Mr. Agassiz dredged a gigantic Isopod eleven inches in length. We dredged also a gigantic Ostracod. The increase in size depends probably on lack of enemies rather than on abundance of food.

The unhappy deep-sea animals have not escaped their parasites in their cold and gloomy retreat. The tube of the Cerianthus, of which I showed a figure, was full of Nematode worms. Crinoids are beset by a Myzostomum, and one deep-sea shrimp was found with a parasitic Gordian worm coiled up inside its body, filling it almost entirely. I have already described the vegetable parasites of corals.

The existence of colour in deep-sea animals is a very interesting fact. Some of the animals, as for example many of the fish, have lost their colour in the dark, and have become simply black or white. Others are most brightly coloured, having retained through countless generations the colouring of their shallow-water ancestors. Some, like the deep-sea shrimps, which are almost always of an intensely bright red colour, seem to have developed a special amount of colouring in the depths. The phosphorescent light of deep-sea Alcyonarians, when examined by the spectroscope, is seen to consist of red, yellow, and green rays only. Hence only these colours would be effective in the deep sea, and no blue animals were dredged from any considerable depths.

Colouring matters however need not always have a decorative object in existence. Certain chemical compounds formed within the bodies of animals for various physiological purposes may happen to have a peculiar action on light so as to be coloured, but this colour-producing property may be a waste or bye-product, so to speak, and only be turned to advantage by certain animals as a subsequent improvement. The fact that our own blood is red is probably an instance in point. In most mammalia the blood is entirely in the dark throughout the animal's life, and never acts on the light so as to exhibit its colour, which is to these animals useless. In ourselves the colour has been turned to advantage for decorative purposes. The colouring matters of some deep-sea animals may thus be retained, because the substances yielding the colours are necessary for the well-being of the animals, and these substances happen to be coloured, just as sulphate of copper is blue, though chemists seldom employ it because of its colour.

As an example of the vividness of deep-sea colouring matters, may be cited that of Pentacrinus. Here (Fig. 17) is a Pentacrinus dredged from 400 fathoms near the Azores. The animal may be briefly described as a star-fish turned upside down and set on a stalk. When freshly dredged Pentacrini are put into spirit, their colouring matter dissolves out and tinges the spirit of an intense purple red. (The light was thrown upon the screen through a solution of this colouring matter in spirit, from specimens of Pentacrinus dredged in 650 fathoms.) The colour is a most beautiful red. It is red when acid, but when a few drops of ammonia are added to it, it turns to an intense green. Very probably this colouring matter is as ancient as the genus Pentacrinus itself.

The colouring matter yields a well-defined absorption spectrum. The acid solution (Fig. 18) shows two dark bands in the yellow and a faint one in the green, and the alkaline green fluid a dark one in the red, with two fainter

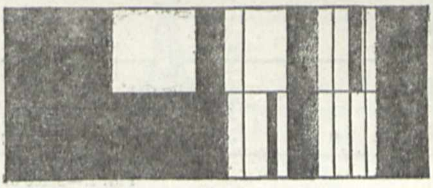


FIG. 18.—Spectra of the acid and alkaline solutions of the colouring matter of Pentacrinus in spirit. The acid spectrum above and the alkaline below. The fine lines are solar lines.

ones in the yellow and green. By means of this double set of lines this colouring matter can be almost certainly identified, although its chemical composition has never been investigated.[1]

A good many other colouring matters of deep-sea animals give well-marked absorption spectra, and can be similarly identified, and it is most interesting to find that the very same colouring matters found in deep-sea animals occur also in allied shallow water and surface forms. Thus numerous deep-sea corals and sea-anemonies are tinged of a madder-red colour by the same pigment, which is abundant in many jelly-fish which float on the sea surface. The red colouring matter of the deep-sea shrimps is also identical with that which occurs in smaller quantities in nearly all the microscopic crustacea with which the sea surface is crowded.

In conclusion I would merely impress upon you again that the most important subject now remaining to be investigated with regard to deep-sea life is the range of life at the various depths between the surface and the bottom of the ocean.

A MAGNETO-ELECTRIC GYROSCOPE

THIS is the name of an apparatus invented by M. W. de Fonvielle, editor of *Electricité*, after having witnessed an experiment by M. D. Lontin. This gyroscopic machine was exhibited by M. de Fonvielle to the Royal Society on the 15th inst., when a paper by him was read by Prof. Stokes. The instrument can now be seen at Elliot's, St. Martin's Lane.

The object of the apparatus is to demonstrate new properties of induction currents brought into play in a magnetic field, and which give a continuous rotatory motion to movable pieces of iron of various forms (Fig. 1). The apparatus consists essentially of a galvanometric frame of any shape. In the first model which has been brought over to England the galvanometric frame is a rectangular one, above which is placed a horseshoe-magnet, supported by a vertical axis round which the

[1] See H. N. Moseley, "On the Colouring Matters of Various Animals, especially Deep-sea Forms" (*Quart. Journ. Micro. Sci.*, vol. xvii., new ser., p. 1).

magnet can be placed in any azimuthal position which may be required. The galvanometric frame has been so constructed that the the horseshoe-magnet may be taken away and be replaced by any number of bar-magnets laid flat upon its upper side. It is possible also to place other bar-magnets underneath the frame in a space arranged for that purpose, or to place the two magnets laterally, or on the left and right side of the frame (Fig. 2).

To produce a continuous movement of rotation with

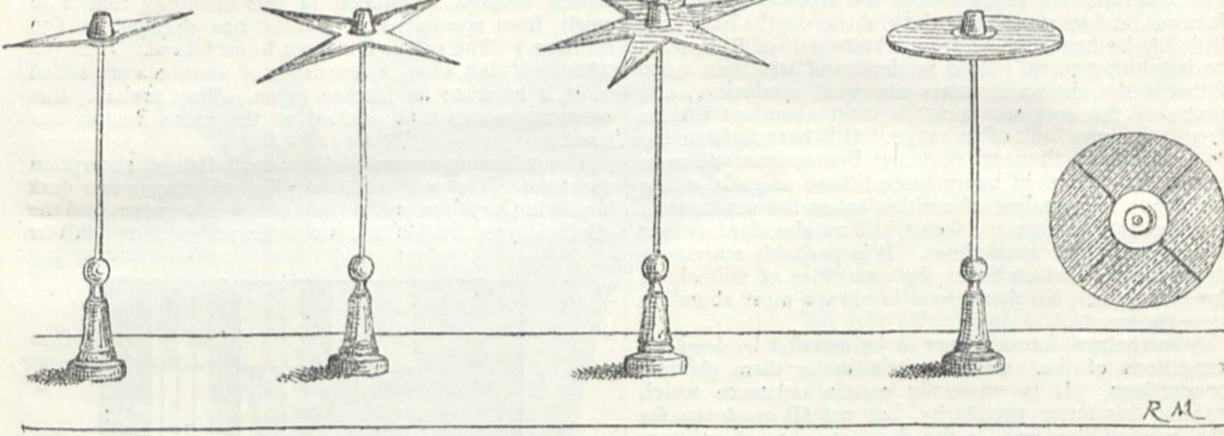


FIG. 1.—Some of the soft iron rotators used in the gyroscope.

the intervention of the magnets it is sufficient to place any movable piece of iron in equilibrium by means of a block on a vertical axis (Fig. 3) inside of the frame, and to send the current of induction into the coil. When, by

FIG. 2.—Rotating induction machine surmounted by a magnet. The operator is introducing a magnet into the space underneath the frame.

means of a commutator, the direction of the primary is changed, the direction of the motion of rotation is reversed. The same phenomenon is observed when we transfer the poles of the acting magnets from left to right, or *vice versâ*. But no sensible alteration even in the velocity is observed when we place the bar-magnets underneath which have been originally placed on the top, or *vice versâ*. If the magnets are too near or too far from the rotating piece of iron, the motion ceases, which, under favourable circumstances, might acquire a very great velocity. The rotation is also stopped when the magnets are placed in a direction perpendicular to the frame.

The movable piece may be placed also at a small distance laterally without the rotation ceasing to take place. It may be also placed on the top, and be rotated by the influence of the bar magnets placed underneath.

The same phenomena may be obtained very easily with a bar electro-magnet of which one pole is presented at a distance of several metres. In this case the experi-

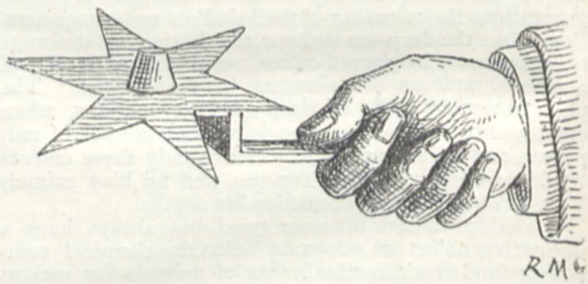


FIG. 3.—Movable piece balanced on its axis, ready to be introduced into the galvanometric frame.

ment is very curious, and looks like a conjuring trick, as two or three movables can be rotated at once. A mere change in the pole presented produces a change in the direction of rotation as well as the displacement from one side of the axis to another. But the operator must be careful not to approach too near, otherwise, the power of the electro-magnet being too great, the action of the induction-current is absorbed, and no motion at all is observed. If the operator is quite near, the movable pieces of iron are attracted magnetically, and fly from the pivots where they have been rotating, to the pole of the electro-magnet. To ascertain the velocity of the movable pieces of iron it is advisable to have them painted half in white and half in black, so that they become grey as soon as the velocity is six or seven turns in a second.

It is possible also to obtain motion without the presence of magnets by guiding an impulse to the movable by an exterior force, and M. de Fonvielle and M. Lontin insisted on that particular point in the memoir they have presented jointly to the Paris Academy in offering a theory of these curious phenomena. The rotation is not always the same, but once it is determined in a solid sufficiently balanced it continues indefinitely in the same direction. The direction does not change when we reverse the direction of the currents by means of a commutator.

The possibility of producing the same movement by means of movables of any form whatever, in presence of magnets or without their action, and notably of two spirals constructed of a flat wire and wound in an opposite direction, appears to the inventors to demonstrate that the rotatory action is exercised individually on each molecule of iron, and that the total impulse must be regarded as the integral of the individual impulsive actions. This remarkable property appears to furnish a very simple means of completely explaining all the circumstances of these curious phenomena by means of the known laws of induction, and to dispense with having recourse to any new hypothesis. It is sufficient, in fact, to remark that the molecule of iron acts in its movement of rotation in two different ways in each of the two nearly equal currents of induction which successively traverse the spires. In fact, during the whole continuance of the two phases of rotatory movement which the galvanometric frame brings closer together, each molecule of soft iron increases the intensity of the current which affects it, and which the inventors call *positive*, independently of its real direction, in order to fix the idea; at the same time it diminishes that of the current which repels it, and which they call *negative*, for the same reasons. In the two other phases of its movement the same molecule diminishes the intensity of the positive current, which then tends to draw it back, and increases that of the negative current, which turns it away from the frame. The actions exerted in the two phases of the movement, that is, in the total extent of the plane described by the molecules, tend then to keep up the continuous rotation, which progressively increases in speed until it reaches that which corresponds to the absolute intensity of the attractions or repulsions exercised by the currents induced, by the energy of the inductive current, the value of the friction of the resistance of the arc, &c.

When we bring the pole of a magnet into action, it is clear that its influence determines in each of the molecules of the movable object a transient magnetisation which strengthens the induction currents produced in the spires in the cases in which it is concordant, and which paralyses them in the opposite case. It hence results that, in presence of a permanent magnetic centre, the movement is possible only in a direction determined by its position and its nature. MM. De Fonvielle and Lontin believe that this principle applies even to the action of the earth.

When we change the position of the active pole in relation to the axis of rotation, the rotation changes its direction; but the pole of the magnet may be placed above or below, to right or left, without the rotation changing its direction. The two poles of a bar or a horseshoe magnet combine to accelerate the movement when they are placed in the direction of the frame; but if we place the magnet in a perpendicular direction, all movement is, as a rule, rendered impossible. It is the same with near position; in proportion as we approach it to that limit of position, the rotation in general will be found to slacken. It is clear that a magnetisable body so strongly tempered as not to have the capacity of being magnetised and demagnetised to the given extent, will remain insensible to these successive dynamic reactions, and consequently immovable, and that it is necessary to employ the softest possible iron in the construction of the movable objects. The same phenomena, especially with the spiral, may evidently be produced if we place it about the frame. They are accompanied, especially with the full disk, by a strident sound, by alternate magnetisations and demagnetisations. Their production appears to the inventors a new confirmation of the theories which they have advanced.

We must add that the coil used is of a peculiar construction, but that at least some of the phenomena can be observed without any Ruhmkorff's machine at all, but with an interrupter of the current from the battery.

It is impossible to say at present if the apparatus may be rendered serviceable as a motive power. But it may be used at all events not only as exhibiting a new mode of action, but as a balance to make a comparison of the force of several magnets, by placing them in opposition at various distances.

NOTES

McGILL University, Montreal, has long held the lead in the cultivation of natural science among the colleges of Canada, and has already large collections, which are still further to be increased by the liberality of Mr. Redpath, a member of the Board of Governors, and Principal Dawson. Mr. Redpath proposes to erect on the College grounds, and on a site which will harmonise with the arrangement of the existing buildings, a stately and beautiful edifice for a museum of geology and natural history, primarily for the use of the professors and students of the college, but also for the benefit of the public generally. The building is to be detached and practically fire-proof, and while it will be an ornament to the college grounds, will be fully up to the present idea of museum buildings in regard to space, light, and means for study and illustration. It will accommodate the whole of the present collections of the University, and will enable them for the first time to be fully accessible to students. In connection with Mr. Redpath's gift, Principal Dawson purposes to present to the University the whole of his private collections in geology, embracing the types of the species which have been described by him in his papers and other publications. In some departments of the geology of the Dominion, as in the fossils of the Pleistocene and Carboniferous, and in the geology of the Maritime Provinces, these collections are believed to be the most important extant.

THE International Congress of Anthropology and Prehistoric Archæology holds its next meeting at Lisbon, on September 20–29, this year. Several important questions concerning the prehistoric archæology of Portugal will be discussed. Excursions will be made to several places of archæological interest.

A UNIVERSAL exhibition of prehistoric German anthropology will take place at Berlin in August. All the German States have been invited to join in this exhibition, which will comprise objects chosen from every museum in Germany. Prof. Virchow is at the head of the Committee appointed to organise the necessary details.

THE Institution of Mechanical Engineers holds its ordinary general meeting to-day and to-morrow, April 23, at 7.30 p.m. The following papers will be read and discussed:—"Remarks on Chernoff's Papers on Steel," by Mr. William Anderson, of Erith; "On Permanent Way for Tramways, with special reference to Mechanical Traction," by Mr. J. D. Larsen, of London; "On Water-Pressure Engines for Mining Purposes," by Mr. Henry Davey, of Leeds; "On Electric Lighting" (second paper), by Dr. John Hopkinson, F.R.S., of London

Sir Henry Bessemer, F.R.S., has been presented with the freedom and livery of the Turners' Company.

Among Mr. Murray's list of announcements are: "The Circumnutation of Plants," by Charles Darwin; "The Life and Discoveries of David Livingstone," by Dr. W. G. Blaikie; "Unbeaten Tracks in Japan," by Miss Bird; "Siberia in Europe: a Naturalist's Visit to the Valley of the Petchora," by Mr. H. Seebohm; "Eastward Ho! Journal of a Naturalist and Botanist in the Forests and Swamps of New Guinea," by Mr. F. W. Burbidge.

The State of Connecticut has taken to heart the teachings of science with regard to the prevalence of colour-blindness, and its General Assembly has passed an Act authorising the State Board of Health to prepare rules and regulations for the examination and re-examination of railroad *employés* in regard to colour-blindness and visual power, and prescribing the method in which, and the intervals at which, such examinations shall be made. The Board is annually, in the month of May, to recommend two or more medical experts to make the necessary examinations and the Governor, on or before July 1 following, is to appoint two of these gentlemen, who will issue certificates. The Act further makes provision for inflicting penalties on any railway company employing a person who is not in possession of a regular certificate of freedom from colour-blindness, or whose certificate shall at any time have been revoked by the examiners. May we hope that our new Government will impose some such sweeping reform upon our reckless railway companies?

The *American Art Review* publishes an account by Mr. F. W. Putnam of an attempt at archæological imposture. In the course of an excavation made in December last in a small burial mound at Yalaha, Lake Harris (on the Ocklawaha), Sumter County, Florida, there was found a little figure of ordinary brick clay, identical in design with many of the statuettes so common in the Egyptian tombs, and known under the general name of Osirids. This little figure was sent to Mr. F. W. Putnam for examination at the Peabody Museum in Cambridge, U.S., and after a careful study he has little hesitation in saying that all the facts he has been able to gather bear witness against its genuineness, in so far as attributing it to the workmanship of the builders of the Florida mounds is concerned. Nor can it be made a link in the chain of supposed evidence by which it is sought to establish a belief in a connection of the early nations of America with the people of Egypt. That the discoverer is entirely clear of all attempt to impose on the public, Mr. Putnam states, is self-evident, as he was suspicious of the antiquity of the object the moment he took it from the mound, and sent it to the Museum in order to have its character determined, if possible. At the same time the finder states that, so far as he observed, the mound had not been disturbed before he commenced to dig. It would seem from this as if a careful "plant" had been made for the purpose of imposing on any one who might happen to open the mound, which, from the large number of relic-hunters who annually visit Florida, would probably be very soon. The front part of the statuette has the appearance of having been cast in a mould made by pressing an original Egyptian specimen in plaster, face down, while the back of the figure shows signs of having been cut with a knife when the clay was still soft. The freshness of the surface of the object tells perhaps more than anything else against its antiquity. In fact to one used to looking at and handling ancient pottery this little statue has the appearance of having just been taken from a place where it had been carefully kept from dirt and from hands since the moment it was made. In connection with this "Osirid" from Florida, it has been stated, Mr. Putnam says, that similar objects were recently found in South America, and are now publicly exhibited in the National Museum at Buenos Ayres. This museum is said to possess, "among the antiquities taken from tumuli in the pampas, mummies, images, and sarcophagi, as fresh, and decorated with as brilliant hieroglyphics, as any exhibited in the famous galleries of the Louvre or other foreign museums containing Egyptian collections." In the published reports of the museum in question, so far as accessible, Mr. Putnam does not find any such objects mentioned. He has therefore taken steps to ascertain the trustworthiness of the report. In regard to the antiquity of the mounds in Florida, Mr. Putnam states that while many of the burial mounds are unquestionably very ancient, it is particularly to Florida we turn to prove the continuance of mound-building by some Indian tribes down to a time long after the appearance of Europeans in that region. In support of this statement we have historical evidence, and also the fact that in many of the mounds in Florida there have been found objects of European origin, such as glass beads, iron implements, glazed pottery, and ornaments of brass, silver, and gold.

A fish and Fishing Exhibition on an unusually large scale was opened at Berlin on Tuesday by the Crown Prince of Germany. The exhibition seems to be admirably arranged, the worst represented country being England. The correspondent of the *Daily News*, animadverting on this, says: "It is a great pity that England sends almost nothing. As far as I can hear, the entire blame rests with the English Government, which refused to give any support or encouragement to the affair. The consequence is that out of the fifty-three whose names were down as exhibitors only seventeen have appeared, which means really that if Mr. Buckland had not sent a rather interesting collection of casts and Messrs. Bartlett and Sons fair specimens of rods and fishing-tackle, the English Department would have been virtually empty. I believe this is the first time that England has been last on the list in an International Exhibition, and I could not help remarking that the Crown Prince was very much surprised at this as he passed through the English Department. Switzerland beats us entirely. The public here evidently expected a great deal from England. However, they must console themselves with the very interesting collection which the United States have sent over."

A stalactite cave, with prehistoric animal remains, is stated to have been discovered near Menhadu, on the Romano-Hungarian frontier.

Prof. Prosdocismi, of the Este Museum, who discovered a prehistoric cemetery on the slope of the hills overlooking that town, has unearthed in the same vicinity eighty-two tombs, forty-four of them violated apparently during the Roman period, the rest untouched, with all their pottery and bronzes. The urns are of three periods, some coloured black, with linear ornamentation, others adorned with circles and wavy lines; others with alternate bands of red and black. Some of the accessory vases might serve as elegant models for modern potters. The bronze ornaments are also very interesting, and a onze chest bears three designs, comprising in all seventeen warriors and a priest, seven animals (horses, oxen, stags, birds, and a dog), several plants, and a kind of chariot with a man seated in it. The Professor considers these the finest prehistoric remains in Italy. Remains of lake-dwellings have been discovered in a peat-bog near Milan, and in a street in Milan excavations for a house have brought to light what are believed to be vestiges of the old Roman theatre.

In a paper read at the American Philosophical Society Mr. Horatio Hale gave an interesting account of his acquaintance with the various Indian tribes collected on the Canadian reservation at Brantford, east of London, Upper Canada; of the most distinguished surviving chief of the Six Nations, Sakayenkwaraton (disappearing mist), known to the English as John S. (smoke)

Johnson, now eighty-seven years old. His son, Chief George Johnson, bears the official title of the one of the original fifty council chiefs whom he represents. Mr. Hale described the formation of the confederation, three centuries ago, and testified to the accuracy of Mr. Morgan's history of it. He then described the "Book of Rites," which, after two centuries of verbal tradition, was reduced to writing by some one connected with the early missions. Two copies exist, and Mr. Hale is obtaining a translation of it. It is the only known American aboriginal piece of literature north of Mexico. It has many archaic words, and is engrossed, in an old-fashioned current English hand, in a common schoolboy's copybook. He also described the wampum belts of the confederation, partly preserved by the Onondagas in New York, and partly among the Indians in Canada. He told of his discovery that the Tutelos were not an Iroquois tribe, but were allied to the Dakotahs or Sioux of the West. Their first seat was in North Carolina. Brainard reported that Tutelos, Iroquois, and Delawares lived together at Shamokin, Pa., speaking three entirely different languages. The syntactical position of the personal pronoun before, after, or betweeen any two syllables of the verb allies the Tutelo language with the two dialects of the Dahcotah, and separates it from all the other Indian dialects. But Tutelo seems to be older than the Dahcotah. So also Huron (Quebec) was older than Iroquois (Six Nations); and Delaware older than Chippeway. It looks as if the movement was from east to west, and not from west to east.

FROM an inquiry as to cetaceans which have perished on the coasts of the Mediterranean and of the West of France during the years 1878 and 1879 M. van Beneden (*Bull. de l'Acad. Roy. de Belgique*, No. 2) finds there were two species of Balænoptera on the former, *Musculus* and *Rostrata*, and two *Giphius cavirostris*, which was believed to have disappeared some years since as a living species. On the west coast of France there perished three *Balænoptera musculus*, one *Megaptera boops*, and one Ziphioid female, whose rudimentary teeth are unknown.

DR. LYON PLAYFAIR has been re-elected representative in Parliament of the Universities of Edinburgh and St. Andrews.

IN reference to the note on the Walker Prize, awarded to Dr. Leidy (NATURE, vol. xxi. p. 451), we should state that the sum awarded to Prof. Agassiz was, like that awarded to Dr. Leidy, 1,000 dollars.

THE sums placed at the disposal of the French Minister of Agriculture and Commerce for the purpose of encouraging research and experiments as to the best way of dealing with the phylloxera, amounted, in 1879, to 500,000 francs, and this will be increased during the present year by supplementary grants to 969,750f. Of this amount 200,000f. are devoted to the treatment of diseased vines in the districts specified by the superior commission, while 250,000f. will be given to doubling the grants voted by the various departmental and municipal bodies. Societies and companies formed for the investigation of the disease will also be assisted by bonuses to the aggregate amount of 300,000f. A further sum of 100,000f. is set aside towards encouraging the propagation of American vine stocks and the distribution of new plants and cuttings from the Agricultural School at Montpelier. Rewards to the amount of 100,000f. will be given for furthering microscopic researches, while 50,000f. are left for dealing with individual cases.

A CHICAGO agricultural journal gives an account of the largest plough ever known to be made, which has been recently turned out by an Illinois firm of agricultural machinery-makers for use on the St. Louis, Iron Mountain, and Southern Railway. It is attached to a platform car of a construction train in such a way as to cut its ditch a sufficient distance from the railway line. It will make one mile of ditch 2 feet deep and 3 feet wide, every four hours, thus doing the work of about 1,000 men. The beam is made of swamp oak, and is 8 inches by 14 inches, the land side being made of bar iron 8 inches wide and 1½ inch thick, which had to be forged expressly for the purpose. Its total weight is 1,700 lb.

FROM China we hear that Mr. Molesworth has lately gone up the Yangtsze-kiang, in order to open coal-mines in the Nganhiong province, where he hopes eventually to introduce foreign machinery.

THE additions to the Zoological Society's Gardens during the past week include a Brown Bear (*Ursus arctos*) from Asia Minor, presented by Commander Atwell Lake, R.N.; two Common Wombats (*Phascolomys wombat*) from Tasmania, presented by Dr. J. C. Cox; two Nutmeg Finches (*Munia undulata*) from India, a Chestnut-breasted Finch (*Donacola castaneothorax*) from Queensland, presented by Mrs. Hylton Joliffe; a Himalayan Bear (*Ursus tibetanus*) from North India, deposited; an Eyra Cat (*Felis eyra*) from South America, a Short-nosed Perameles (*Perameles obesula*), a Stanley Broadtail (*Platycercus icterotis*) from Australia, two Scaly-breasted Parrakeets (*Trichoglossus chlorolepidotus*) from New South Wales, a Blue-crowned Hanging Parrakeet (*Loriculus galgulus*) from Malacca, two Red-naped Fruit Pigeons (*Carpophaga paulina*) from Celebes, an Elate Hornbill (*Buceros elatus*) from West Africa, two Black-necked Swans (*Cygnus nigricollis*) from Antarctic America, three Wheatears (*Saxicola œnanthe*), a Meadow Pipit (*Anthus pratensis*), European, five Eyed Lizards (*Lacerta ocellata*), South European, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE SOUTHERN COMET.—There appears now to be no reasonable doubt that the comet which has attracted so much attention in the southern hemisphere is identical with the great comet which was observed in almost all parts of the habitable world in March, 1843. Dr. Gould succeeded in obtaining observations for position at Cordoba up to the evening of February 19, though the head was then only recognised "as a scarcely perceptible whiteness in the field of the large equatorial of 28⅓ centimetres aperture." On the following evening it was not distinguishable, though having a good ephemeris, the observers knew that it must be in the field of the telescope, which fully accounts for the comet having been unsuccessfully sought with large instruments in this hemisphere, amongst others with Mr. Common's powerful reflector at Ealing. Dr. Gould publishes the following parabolic elements founded upon his observations of February 6, 12, and 18; an orbit deduced by Mr. Hind from the Cordoba places of February 6, 9, and 14, which is annexed, will be seen to differ in no material degree from Dr. Gould's. The longitudes are for 1880·0.

	GOULD	HIND
Perihelion passage G.M.T. ...	Jan. 27·6188 ...	Jan. 27·62195
Longitude of perihelion ...	279 52 11 ...	279 45 1
,, ascending node	6 10 30 ...	5 55 37
Inclination	35 20 21 ...	35 22 52
Log. perihelion distance ...	7·739364 ...	7·745636
Motion	Retrograde ...	Retrograde

On comparing the accounts so far received of the appearance of the comet of 1880, while in some general features, as the great length and narrowness of the tail, &c., there is a close resemblance to the descriptions of the comet of 1843, it will be found that the head at least was probably more brilliant in the first days after perihelion in 1843, though as in the present year its brightness very quickly faded. We do not hear of the comet having been detected in broad daylight near the sun, soon after the perihelion passage as in 1843, when it was remarked only a few degrees distant from the sun's limb, at various places in the United States, in Italy and off the Cape of Good Hope; in that year the comet crossed the sun's disk without being observed in transit. In the present year the comet has not transited the sun, but according to Mr. Hind's orbit would have the following positions within the twenty-four hours after perihelion :—

Greenwich mean time.	h.	Right Ascension.	North Polar distance.	Distance from Sun's centre in R.A.	Distance from Sun's centre in N.P.D.
		° ′	° ′	° ′	° ′
January 27,	22	309 49	109 33	− 0 32	+ 1 15
28,	0	310 1	109 49	− 0 25	+ 1 32
28,	2	310 13	110 4	− 0 17	+ 1 48
28,	4	310 26	110 18	− 0 10	+ 2 4

The following expressions for the comet's heliocentric co-ordinates to be combined with the X, Y, Z of the *Nautical Almanac*, apply to the same orbit, and may be useful to some of our readers in the other hemisphere who have occasion to learn the right ascension and declination of the nucleus in examining their observations of the track of the tail before the head was visible :—

$$x = r\,[9{\cdot}99922] \sin (v + 8^\circ\ 39'{\cdot}7)$$
$$y = r\,[9{\cdot}99079] \sin (v + 279\ 22{\cdot}6)$$
$$z = r\,[9{\cdot}32714] \sin (v + 82\ 39{\cdot}9)$$

r is the radius-vector and v the true anomaly.

A NEW COMET.—A telegram to the Astronomer-Royal notifies the discovery of a comet at Ann Arbor, Michigan, on April 6, at 11h. Washington time, in right ascension 7h. 20m., and declination 84° 25′; daily motion in right ascension, − 30m.; in declination, − 48′; tail, 3′. From a telegram to the Academy of Sciences at Vienna, the name of the discoverer would appear to be Schäberle.

GEOGRAPHICAL NOTES

THE gold medals of the Royal Geographical Society have just been awarded as follows:—1. To Lieut. A. Louis Palander, of the Swedish Royal Navy, in recognition of the services he has rendered to geographical science as commander of the *Vega* in the Swedish Arctic expedition under Prof. Nordenskjöld, during which he safely navigated the ship along the unsurveyed shores of the Asiatic continent for nearly 3,000 miles, and took the leading part in charting the coasts of Northern Asia. 2. To Mr. Ernest Giles for leading four great expeditions and several minor ones in Australia, and for his route-surveys, geological and botanical collections, and published descriptions of his various journeys. A gold testimonial watch was voted to Bishop Samuel Crowther for his services to geography in the Niger region during the past forty years, and for having aided various expeditions on that river between 1841 and 1857. Prof. Nordenskjöld, having received a gold medal in 1869, was, we believe, in accordance with all precedents, ineligible for another, but, as we have already recorded, he has been elected an honorary corresponding member—an honour accorded to Mr. H. M. Stanley under analogous circumstances. The Council of the Society of Arts at their last meeting elected Prof. Nordenskjöld an honorary life member of the Society in consideration of the services rendered to science by his recent explorations.

MOUNT NAIGUATÁ, in the Venezuelan coast-chain near Carácas, and at a short distance towards the east from the Silla, was ascended by a party from Carácas, August 24 last year. Its height was for the first time carefully determined by Lic. A. Aveledo and Dr. Man. V. Diaz. The barometrical reading on the top was 551·20 mm. (reduced to freezing point), thermometer being at 13° C. At the same time observations made in Carácas (Colegio de Santa Maria) gave: Barom. 683·44 mm.; therm. 24°·8, which makes Naiguatá 1,852 metres higher than the lower station, and as this place is 930 metres over sea-level, the total height of Naiguatá is 2,782 metres, or 9,130 Eng. feet. Dr. A. Ernst made botanical and other collections. The rock, wherever it is not covered by vegetation, is amphibolic gneiss, with much quartz, and therefore very hard. A swift (*Chætura zonalis*, Sclat.) was frequent on the top, but it could not be discovered whether it was nesting there. In the stomach of one specimen a large number of wasps were found. Of beetles there were some *Platyomus*, a *Pterostichus*, and a few *Chrysomelidæ*, crowded together with some wasps in a narrow cleft between two large stones. Not very far from the top a specimen of what appears to be the moss insect described by Belt ("Naturalist in Nicaragua," p. 382) was caught, clinging to the bark of a stem, which was thickly covered with *Neckera undulata*, Hedw., the likeness between the insect and the branches of the moss being indeed very striking. No butterfly was seen, though there was an abundance of flowering plants; nor were there any land-shells, which was to be expected, on account of the total absence of limestone. Amongst notable plants growing on the higher part of the mountain the following may be mentioned:—*Arenaria nemorosa*, H.B.K., *Acæna*, sp.n. (allied to *A. cylindrostachya*, Cav., and *A. macrorhiza*, Hook.), *Berberis aurahuacensis*, Ch. Lem. (after Sir J. D. Hooker; the plate in Van Houtte's, "Flore des Serres," iv. tab. 334, however, does not agree; it looks very much like *B. guilache*, Tr. et Pl.), *Liabum hastifolium*, Poepp., *Hieracium avilæ*, H.B.K., *Gnaphalium incanum*, H.B.K., *Myrsine ciliata*, H.B.K. (the only woody plant which reaches the top), *Sphacele*, sp. n., *Siphocampylus microstoma*, Hook., *Anthericum coarctatum*, R. et P., *Arthrostylidium longiflorum*, Munro, *Epidendrum alpicolum*, Reichb. (several specimens were seen with racemes ten to twelve inches long, and sending forth a sweet vanilla-like smell). It must be remembered that Mount Naiguatá was ascended some years ago by the late Mr. James Mudie Spence, of Manchester, and his party.

WE take the following from the *Times of India*:—It is said that Major Biddulph, stationed on the Kashmir boundary, has prepared a report upon the customs, the languages, and the folk-lore of the singular communities among whom he has been residing for a long time. "From Major Biddulph's peculiar advantages and opportunities may be expected," says the *Pioneer*, "a complete account of people who are a survival of the old Aryans, from whom all civilised mankind of the present day is probably descended. Surgeon-Major Bellew, meanwhile, has been examining a few men from the cantons on the south-west of Dardistan, peopled by a similar race, who in one respect are still more interesting, for their country has never yet been visited by a civilised traveller. But in appearance and language they closely resemble the Dards, and, unlike them, have not embraced the creed of their Mahomedan neighbours. The tongues spoken in all these hills are, for the most part, Aryan; not descended from Sanskrit, and, indeed, of earlier origin than that classical language. On the northern slopes of the mountains Parsee words prevail in the southern cantons. Some of the words resemble Greek, some Latin, some those of modern Europe. They make, and freely consume, grape wine, something like a crude Burgundy. Those who are not Mussulmans believe in one God, but employ the intercession of minor powers, represented by images. They also occasionally canonise great men whom they have lost by death. They are usually monogamous, opposed to divorce, and strict defenders of the chastity of their unmarried girls. These latter have blue, grey, or hazel eyes; black hair is the exception amongst them, and, when young, they are of such remarkable comeliness as to be in great demand in the slave markets of adjacent countries. Authentic information concerning these interesting races cannot but be anxiously awaited by all who realise the nature of the questions involved."

THE Rev. C. T. Wilson, of the Church Missionary Society's Nyanza Expedition, who has just arrived in England *viâ* Egypt, from Lake Victoria, will read a paper on "Uganda and its People," at the meeting of the Royal Geographical Society on Monday next. Great interest will attach to this meeting, owing to the expected presence at it of the three Waganda chiefs who have accompanied Mr. Wilson as a deputation from King Mtesa.

FURTHER details are to hand as to the projected Italian Antarctic Expedition, under Lieut. Bove and Commendatore Negri. Committees for subscriptions have been started in the chief Italian towns and the colonies, and it is hoped the expedition will be ready to sail in May, 1881. From the Shetlands the expedition will steer to the south-west, and endeavour to penetrate a line of land which was observed by Dallman, a Hamburg whaler, some few years since. Thence a movement will be made towards the land where Bellinghausen marked the lofty capes of Alexander and Peter, and the western lands observed by Wilkes in 1839. At this point a serious discussion must arise as to future movements. It would be desirable to coast along the land of Bellinghausen if there were any appearance of a "continued mass," steam for the back of the islands which Wilkes believed to exist, and thus enter on the south of Ross's Sea, where the winter might be spent. Should, however, expectation be disappointed, the winter might be passed on the Bellinghausen land, and preparations might be made for entering Ross's Séa. The voyagers think that with a strong vessel it would not be impossible to penetrate beyond Ross's Sea, and complete the studies which were made of the flora, the fauna, and the mineralogy of the Antarctic region. Having examined these lands and seas, it is proposed to move towards Adele, discovered by D'Urville in 1840, and here it is thought it might be possible to land and winter. Continuing their course to the west, they intend running along by the "Southern Continent," where the existence of land is certain, and endeavour to penetrate through the ice, as did D'Urville, Wilkes, and Ross. The hope is that canals in the ice might be found through which they might attain a remote latitude, or, running along them when massed into a continent, arrive at Kemp or Endermet, where they could pass the second winter.

A JAPANESE paper states that the Swedish skipper Johannesen, who has already done a good deal of exploration in the Spitzbergen seas, is to set out this month from Yokohama in the steamer *Nordenskjöld* to make the North-East Passage in an opposite direction to that taken by the *Vega*, viz., from Behring Straits to Europe.

PROF. NORDENSKJÖLD reached Copenhagen at the end of last week, in the *Vega*, and was received on landing by the acclamations of 20,000 persons. Every one has united to do him and his companions honour, from the Royal Family downwards.

THE death is announced, on the 16th inst., of Mr. Robert Fortune, well known as a botanical collector in China and Japan, and author of several volumes describing his travels in those countries in search of new plants. It was he who introduced the tea-plant from China into the North-West Provinces of India. He was born in 1812.

ON THE EMPLOYMENT OF THE PENDULUM FOR DETERMINING THE FIGURE OF THE EARTH

MY object in writing this paper is principally to draw attention to the course which the employment of pendulums has taken, from the time when Richer's first experiment at Cayenne, in 1672, attracted the attention of Newton; and to show in what respect the present aspect of the question is different from that which successive observers, as well as writers upon the subject, have at various times taken. It is no part of my object to discuss the observations themselves, or to discriminate between them, still less to enter upon any investigation of the figure of the earth, except incidentally in alluding to the conclusions which different writers have accepted. But as it is nearly impossible—perhaps not altogether desirable—to hold no independent opinions, I may add that I hope to be able to influence the future course of such operations in a certain direction which will be recognisable as we proceed.

The literature of the subject is very extensive,—not so much in respect of the pendulum itself, or of the use which has been made of it, as on account of the intimate relation which the laws which govern its motion have to larger questions. It is the discussion of these that experiments with the pendulum have influenced, and in general it is only with reference to such influence that the experiments have been instituted, described, and considered; and that in close connection with other operations of wholly different character. It is thus nearly impossible to have a thorough knowledge of the history of pendulum operations without at least a general acquaintance with the history of geodesy, and of that part of astronomical and mathematical literature which deals with the probable forms and constitutions of cosmical bodies. This would be less the case than it is if some of the many writers on the figure of the earth had written less exclusively from their own point of view, and (at any rate in writing of pendulum operations) had dealt more fully with the historical aspect of this particular branch of the general subject: I mean in the modern sense of the word; describing not only the sequence of experiments, but also the development of the comprehension of the questions in issue. I have felt the want of this myself so strongly that now that a somewhat protracted study has partly supplied that want I am fain to attempt this review, in aid of those who may have to prosecute the work.

It is of course impossible to present the course of pendulum operations without continually referring to their intention. At the same time one learns at last that, with one or two exceptions, the intention itself was not well grasped by those who conducted the experiments. Indeed one may almost say that even on the part of those who directed them the intention is not very clear; or more correctly, that it was more confined than we now might wish had been the case. Laplace was not perhaps the first to give utterance to the opinion that the anomalies noticeable in pendulum results were probably due rather to inequalities of figure than to errors of observation. Nevertheless it is with something of surprise, considering that the importance of his opinion lay latent so long as a practically unrecognised consideration, that we find him saying as follows:—"We shall here remark that the same anomalies. . . arising without doubt from the irregularity of the parts of the earth, are also perceived in the observed lengths of pendulums." That such irregularities existed was doubtless always a suspicion, but the fact was very slow of being recognised, and to this day it does not govern the observations.

In reviewing the course of pendulum operations then we must be prepared to put this aside as a fact which has not entered into account. It may be strange, but such is the case. It follows that a very considerable portion of the discussions and calculations, based on results which I am very far from wishing to impeach, must also be set aside as almost entirely without present value other than as evidence that the breadth of the question had not been measured.

If the absence of a true appreciation of the influence of local irregularity is apparent in the narrowness of the discussion of individual observations, or of small groups of results; it is also noticeable in the rejection of many, on the sole ground that the methods of observation were inferior; without any proof being adduced that the probable error was greater than the probable effect of local irregularity. This may be taken as indicating that there was also on the part of those who set themselves to review the produce of experiment a reluctance to accept as facts the irregularities which now we recognise as necessary concomitants.

Here again it follows that we must be ready to turn aside from conclusions which are seen to rest on the exclusion of an important consideration. But it by no means follows that, in thus finding reason on all hands to go back to the original sources, and to discard more or less summarily much which has been at one time or another accepted as legitimate deduction, there is any occasion to slight those deductions. Mere trials as they have often been, they have served many purposes which we cannot disdain, and (in ways which it is vain now to examine) have placed us in the more advanced position. There is one thing however which they must have no power to effect, and that is to obstruct us in further advance.

At the same time I confess that, for my own part, I cannot turn over the innumerable pages of vain calculations without profound regret that they represent so much labour—not thrown away—but without further use. I would give instances, but perhaps it is better to refrain. If anything could excuse it, it would be the hope of saving some other learner from spending time over them, and that object can perhaps be otherwise secured.

From another point of view I have also been led to perceive a want of distinctness in the plan of operations, which accounts for an otherwise inexplicable diversity in the individual contributions. In studying the history of these operations we are reminded of an edifice which presents different styles; and parts which make up a whole, not so much after any known design as casually. The two principal styles, to continue the metaphor, have indeed a common element which is a key to the whole construction; but though we can perceive that it is there in every part, and was present to the mind of every worker, it scarcely ever amounts to an expressed design by which future work is to be regulated. I allude to the absolute and differential methods. As we cannot properly appreciate the value of the work which has been done without understanding the relation in which these stand to each other, it is necessary to preface the merely historical account by a description of those methods in their relation both to the general purpose and to each other.

The conception of the earth as an oblate spheroid probably preceded the first use of the pendulum as an instrument by which its oblateness could be proved and measured. But the uncertainty which characterised geodetic measures—an uncertainty so great that it was, at a later date, actually the subject of vehement controversy whether the elipticity was not prolate—was such that Richer's discovery (in 1672) that the length of a pendulum beating seconds at Cayenne was notably less than that of a pendulum beating seconds at Paris, was from its very simplicity and conciseness, a revelation which promised inestimable consequences.

This was not the first time that a measurement of the seconds pendulum figures in the annals of geodesy. Picard had two years before adopted the idea—subsequently, towards the close of the next century, so nearly being given final effect to—of making the seconds pendulum the unit of length. But although he did actually determine the length at Paris, and thus unwittingly laid the first stone of the "absolute" method, it was not until the length had also been ascertained at another place and in another latitude that the firstfruits of the method could be gathered.

It is conceivable (though unlikely) that the pendulums used in the two places were the same. But this is of no consequence, since their lengths were different—the time of oscillation being constant.

Precisely the same result, in respect of variation of attractive force, would have been deduced had the same pendulum, of unchanged length, been used; the fact of observation in this case being a diminution of rate.

In the one case we have the absolute method; in the other we should have had the differential method. But in the former we have not only an indication of the variation of the force, but also a measure of its magnitude in terms of the measured lengths. Hence the designations.

It is well to remark here that unless the measurements at the two places refer directly, or can, by intermediary scales, be referred ultimately, to one and the same standard, there is no exact comparison, and no certain result. Further, that if, from any cause, the standard of reference is of unknown length, or, technically speaking, lost, the result is differential only. Thus we see that observations which were, in intention, of the absolute class, may fall into the differential class for want of reliable connection with existing standards of length.

Measurement of the absolute force of gravity at the earth's surface by means of a pendulum was doubtless contemplated by astronomers anterior to Richer's discovery of its variation. It is therefore scarcely permissible to recognise in the prosecution of this discovery the determination of the absolute force as a principal object. The object was distinctly to ascertain the variation at different parts of the surface. Such being the case, we must admit that the absolute method was not the simplest for the purpose. In due time this was recognised. It is doubtful to whom is due the credit of earliest perceiving that the measurement of the pendulum might be dispensed with, provided means were supplied of securing a constant length. Graham and Campbell (in 1732), and Bouguer (in 1735), were the earliest in the field;[1] and Bradley (in 1736) in describing Campbell's use of Graham's pendulum at Jamaica, very decidedly recommends the use of a pendulum of constant length. Bouguer and Godin at the same period were also using pendulums which were measured. From this time forward to the present day both methods have been practised.

I confess myself unable to frame arguments in favour of the measured pendulum, as an instrument for its purpose, which can at all account for its prolonged use. I mean of course if we are to presume an intimate acquaintance, on the part of the observer, with the meaning of his labours. Considering how fragmentary and scattered, and often unimportant, not to say mistaken, are the early writings on the use of the pendulum, it is scarcely invidious to say that such a presumption is sometimes gratuitous. The men who made scientific voyages in those days doubtless had something else to do than to study. Moreover, study was not very feasible when books were scarce and libraries few. Nevertheless one cannot help a curiosity as to the charm which protected the absolute method. Was it precedent?

There is something seductive, it must be admitted, in the conciseness and completeness which attends an absolute determination, as contrasted with the dependence of a differential one. To adjust a pendulum to such a length that it will beat seconds, and then to measure its length against a portable standard whose length has been ascertained—this is conclusive. When done, the worst that can happen to the instruments used is no worse than prevention of further use. This is true; and if the observer is full of confidence in the accuracy of his measurements, unconscious of the errors that lurk in reductions, and innocent of the insidious nature of instrumental mischances, why should he surrender the security of present gain? Nevertheless these flaws exist, and ultimately they are recognised and found irremediable. The more honour to those who foresee and provide against them, in preparing instructions, in conducting experiments to elucidate difficulties, or in multiplying observations by which the work of others may be consolidated.

[1] Mairan, in the same year, suggested it independently, and Lacondamine advocated and used one frequently.

Much of the doubtfulness which attaches to the earlier work is due to want of that knowledge which experience brings. Among the most important causes of error must be reckoned the imperfect comprehension which formerly existed as to the retarding influence of the air on the swinging pendulum. It was of course known that a body being lighter when suspended in a fluid than when in air, the oscillations of a pendulum when swung in air would be slower than when swung in a vacuum. No account was taken of this at first, but the time came when the effect was calculated by determining the diminution of weight due to flotation. Ultimately it was shown that there was also retardation due to the disturbance of the surrounding air, and it was surmised that this depended, not only on the bulk of the air displaced by the pendulum, but on the form of its surface as well. Of course it was immediately apparent that the old results must undergo some correction depending on the forms of the pendulums used; and equally, of course, the want of precise descriptions was then felt in a way the original observers never contemplated.

The difficulty was perhaps less real than apparent. As I have already pointed out, the magnitude of the force was not the object of the experiments, except as a means of inter-comparison. Hence any shortcoming which invalidated the determination of the absolute magnitude without rendering impossible that of the relative force, was of no real consequence. It was only necessary to abandon the idea of retaining a set of results in the absolute class, and to consider them as differential only; the *sine quâ non* being that such set were taken with one pendulum or with pendulums of the same size and construction. But I cannot remember a single instance in which the difficulty has been met in this way.

I have pointed out that the idea of measuring the force at any place absolutely arose anterior to that of measuring it relatively, and was afterwards retained as a means of securing the relative measure. Whatever interest attached to the determination of the force of gravity for itself, it is pretty certain that it went for little in the experiments which succeeded Richer's. It is therefore remarkable that even after the simpler differential method had been inaugurated it should still have held its ground. It is probable that Picard's idea[1] of a base or standard of length dominated to some extent the subsequent line of investigation. At any rate it is to his experiments that we must look for the rudiments of the instrument. I am[2] unfortunately unable to refer to the original memoirs in which these experiments are recorded, but I gather from other accounts that all the experimenters aimed at as near an approach to a "simple" pendulum as possible, and that even the celebrated form which Borda adopted in 1792 scarcely differed at all from the earlier ones. The following notice of Borda's experiments by Lalande, in his "Histoire Abregé de l'Astronomie," is noteworthy as sustaining the view which I am led to take of the vitality of the absolute method:—

"Le décret de l'Assemblée nationale qui, le 8 mai, ordonna la réforme des mesures en France, en indiquant le pendule à secondes pour mesure primitive, exigeait que la longueur du pendule fût déterminée avec une nouvelle précision. En 1735 Mairan avait fait ses observations avec bien du soin; mais alors on ne pouvait guère l'assurer d'un quinzième de ligne. Borda espéra obtenir une précision bien plus grande par des moyens nouveaux; il l'entreprit donc cette année à l'Observatoire, avec des instruments faits d'après ses idées par C^en Lenoir, et il en résulta enfin une détermination du pendule de 36p. 8l. 60 réduite[3] à la température de 10°; et dans le vide; ce résultat, qui est à un cinquantième de ligne, et mieux encore, a été obtenu avec un pendule de douze pieds de long."

The old form of pendulum consisted of a weight, of simple geometrical form, suspended by a fine wire or fibre. In Borda's the weight was spherical and attached by adhesion to a cup to which the wire was fastened. The object of this was to vary the position of the ball without detaching the wire. This attachment seems to be the *only* part of consequence which was without precedent.

In all these forms it is particularly to be noticed that the ball was made heavy and the suspension light and fine. Although the obvious intention of this was to attain to the

[1] Priority, in point of time, is due to Wren in this matter; but Mouton, from whom Picard got it (and perhaps also Huygens), no doubt evolved it independently, though some years later.

[2] This article was written in Calcutta, and no library there possesses the early volumes of the Paris Acad. *Memoirs*.

[3] Please to remark the absence of intelligible meaning in this. What is it that is reduced? and why?

nearest practical model of a simple pendulum, I cannot help seeing in it also an unconscious recognition that there was *resistance* to be met in the air as well as buoyancy. It is not credible that the resistance of the air to a body moving through it was not thought of, though it is intelligible that the effect of such resistance was so underrated or misunderstood as to be supposed insignificant.

Having mentioned Borda's pendulum in this connection, I will add that I do not find grounds for assenting unreservedly to the practice of assigning to him so large a share in the merit of inventing the measurable pendulum. He improved somewhat on an already existing form, and experimented with greater accuracy, if not with greater care, than his predecessors; but we must, I think, in justice associate Picard's name, and still more Graham's, with that form.[1]

About the year 1786 Whitehurst, carrying out an abortive idea of Hatton's, presented to the Royal Society, and afterwards withdrew and published independently, a paper describing experiments with a pendulum of the ball-and-wire construction, the use of which depended on a change in the length of the wire. I have not had access to the original paper, but if we may trust Saigey's account the experiments were singularly correct in their result. In all experiments with very long pendulums—indeed whatever be the length, but especially with long ones—the ultimate precision turns on the measurement of the distance between the upper and lower planes. The liability to error in such measurements is much better understood now than it was in Whitehurst's time, and it is somewhat doubtful if the correctness of his result may be accepted as an argument in favour of his method. But even if we had not ground for anticipating advantages in a method which secures, to some extent, the elimination of certain errors, the fact that Bessel adopted the same method in preference to employing either Borda's or Kater's pendulum, some forty years later, would go far to require that full recognition should be accorded to Whitehurst.

The result of the use of the single ball-and-wire pendulum, in whatever form, depends ultimately, as I have said, on the accuracy with which the distance can be measured between the point of support and the lower contact plane. The measurement, and perhaps also the distance itself, will vary with the temperature. The length of the pendulum also, and therefore its rate, will vary either with the temperature (if the suspension is by a wire) or with the dampness of the air (if by a fibre). If the temperature varies much during the time occupied by the experiment, the effects will be so complex—owing to the difference of the masses of the wire, the scale, and the support—that great precision can scarcely be expected. To some extent the uncertainties are eliminated when the experiment takes the form of a comparison between the rates of two pendulums of different lengths, but otherwise identical. In any case, however, there is an element of uncertainty peculiar to the quest of *length* distinct from that which is peculiar to the quest of *rate*.

It is not, I think, possible to form a correct conception of the progress of the research which was prosecuted by the help of the pendulum—still less to understand its present aspect—without grasping firmly the idea that the use of the absolute pendulum contemplated two distinct objects which had no essential connection, viz., the force of gravity and the figure of the earth; while the use of the differential pendulum contemplated one only of these. Of course I do not mean to imply that this distinction was not perceived; but I do suggest that no small portion of the difficulties which have attended the research are traceable to the frequent absence of a sufficient perception of the independence of the two quests. From the time of Graham, Bradley, and La Condamine, to the present day, while the ostensible main purpose, has been one, the methods have been two; and one of these was encumbered with a hardly acknowledged second purpose whose presence created a set of difficulties from which the single-minded purpose and method were free. This is so obvious, so well known, that it seems almost an impertinence to bring it forward. It is not so, however. Fully recognised and admitted as it must be allowed to be, the fact remains that notwithstanding the comparative failure of the absolute method and the acknowledged success of the differential, the tendency at this day is still to have recourse to the former, although the second purpose is now scarcely thought of as a real desideratum, the whole interest centring in variation, and variation only.

Let us consider the two objects of the absolute method separately; or rather, let us consider that one especially which is its peculiar object—the *length* of the equatorial seconds pendulum.

What is the equatorial seconds pendulum? It is a simple pendulum beating seconds under the force of gravity at, or near, the equator. We are obliged to add the qualifying words, because it is certain that the rate varies along the actual equator. It is necessary, therefore, either to specify some spot, or to define in some other way the force to be designated. How is this to be done?

As I remarked at the beginning of this paper, I do not propose to approach the question of the figure of the earth more nearly than is necessary. But it is perfectly clear that as soon as it is admitted that the form which we are to study by means of a pendulum actuated by gravity is to be expressed as a more or less complicated mathematical equation, the force of gravity enters as a principal variable. The limits and law of variation it is not necessary here to attempt to define. All that is necessary is to perceive that there will be one or more maxima at or near the equator, one or more minima at or near the pole, and as many means as we choose to invent functions expressing what may be called means.

The idea of a seconds pendulum as a definite length rests on the idea of gravity as a definite force. The idea of an equatorial seconds pendulum as a determinable length rests on the idea of equatorial gravity as a determinable force. We are therefore driven to consider in what sense, and by what means, it was, and perhaps is, supposed determinable.

Had France been an equatorial country there need be no doubt that, not Paris, but some equatorial village, would have been chosen as the site of experiment to be repeated again and again as time went on. But as this was not the case French philosophers went some thousand miles and sojourned for years in an equatorial country, in search of this stone. And in after time, when confirmation was wanted, voyagers experimenting in foreign latitudes made a point of getting as near to the equator as possible.

Gradually the idea of determining gravity *by experiment at the equator* gave place to the idea of determining it by experiment elsewhere. The summit lost its immediate attraction in the interest of perfecting a road to it. By degrees it became apparent that there was no summit, or at any rate that the summit could only be designated by a careful study of all the approaches; and lastly, that it was in fact only an idea. Only an idea, and that an undefined one.

The history of physical research is full of instances of this kind of baffled inquiry. From a distance the goal is clear, distinct, definite, precise; we can in thought put a finger on it. We approach, and the aspect changes, foreshortened distances extend, and small things become great; forms are changed, and though we penetrate into the very midst of what we ran for, we recognise it no longer. Had we run open-eyed we should have been prepared for the transformation and have realised better the success.

So it is with equatorial gravity. What was seen at a distance *was* that very idea, which, close at hand, we cannot readily define.

Some such difficulty appears to be the explanation of the vigour with which the more concrete idea of the actual force at a definite spot was grasped; and perhaps we may recognise in the almost extravagant pretensions of the London and Paris seconds pendulums a sense of retreat from, and abandonment of, the hopeless equatorial representative.

But in falling back from the equator upon Paris and London there was no abandonment of the length of the seconds pendulum as a linear standard. This came somewhat later, when the difficulties of precise determination even at one and the same spot were more apparent. Meanwhile the local lengths were retained as provisional units.

This appears to me to be the key of the position. It was anticipated that the exact relation of gravity at Paris and at

[1] On another point also, namely that of the general association of Borda's name with the invention of the method of coincidences, I am glad to find myself not alone in demurring. Legentil distinctly says that he followed Mairan in this; and Meyer alludes (1865) to the same misconception when he says: "Die von Mairan erfundene Methode der Coincidenzen, die gewöhnlich de Borda zugeschrieben wird" (*Pogg. Ann.*, cxxv. p. 182, note). The merit due to Borda in this connection would seem to be limited to his employment of a cross mark on the clock-pendulum as an object with which the thread of the free pendulum was to be in concert—just as Kater afterwards used a white disk and an opaque slip; out of which in after years arose a somewhat complicated and very differently understood question of precision.

London to gravity at the equator would eventually be known, and meanwhile a base of connection was wanted. It is perfectly true, as I have already said, that an absolute determinination is eminently satisfactory, and (theoretically) can stand by itself; but practically they rarely did so. It is perfectly true that if the length of a pendulum is actually measured and its rate observed, an independent determination is made; but practically the determination was almost always relative. The pendulum was generally not so much measured as to its actual length, whatever that might be, as adjusted to a certain length such as (very commonly) had been previously done at Paris or London. The distinction is very clear in some cases, less so in others. But, generally speaking, the determination has as good a right to be classed among the differential ones as among the absolute.

Consider the case of Graham's pendulum as used by Campbell at Jamaica. It was purposely designed to be adjusted *to the same length*. Or, again, consider Legentil's. He was constantly testing and adjusting the length by means of a *règle en fer*, and the only kind of measurement which took place was that of examining the equality from time to time of the length of his *pite* fibre.

It appears to me to be entirely beside the mark to insist that his *règle* or gauge had been compared or measured. It was used as a gauge and not as a measuring scale.

The same applies in nearly all cases. A gauge is always found to have been used, and some constant addition or subtraction made for the calculated position of the centre of oscillation.

That which gives to all the older determinations their apparently absolute character is that the result is *expressed* in linear measure. Considering the exceedingly doubtful character of the linear element so introduced, it is practically certain that the only chance of utilising any of these is to get back to the *observed rate* if possible, and to treat them all as merely differential.

Let it not be supposed that we shall lose anything by this. As things now stand, observations which were essentially differential and often good of their kind are under the cloud of doubtful reduction, caused by the endeavour to kill two birds with one stone. Experience has shown that this is barely possible even now, with vastly better means. Common sense suggests that it was vain before.

I have hitherto been speaking of the last century. The aspect changes somewhat as we enter the present one. Scarcely a trace remains of the absolute force of gravity as a real object. The idea of a linear standard is still active, but evidently doomed. What will be left as the *motive* for absolute determination, in preference to differential? I confess that I can give no answer. Anxious as I have been, and am, to learn and to understand the whole of this subject; careful as I may be to catch at every indication of an unexpressed idea latent in the mind; it is in vain that I try to find a *raison d'être* for absolute pendulum operations at the present day. It would be impossible to say this and not imply dissent from the views of those who advocate their prosecution, and I am well aware that such views *are* advocated by a section of the Continental geodesists. But I seem to be unable otherwise to find a solution. A year has elapsed since this paper was written—all but these two sentences—and I have learnt nothing to change my opinion.

J. HERSCHEL

NOTE ON SOME EFFECTS PRODUCED BY THE IMMERSION OF STEEL AND IRON WIRES IN ACIDULATED WATER [1]

DURING a discussion upon a very interesting paper by our president, "On the Durability of some Iron Wire," I mentioned a fact which I had lately observed, viz., that steel or iron wires immersed for a few minutes in acidulated water containing one tenth sulphuric acid became excessively brittle. Our president has since kindly asked me to make a few more experiments on this subject, and to embody them in the form of the present note.

Upon repetition of these experiments I have found that this brittleness is no mere accidental result, due to some flaw in the steel or iron wires, but that the resulting brittleness is invariable in all kinds of steel as well as iron. Nor is the effect due to any specific proportions of sulphuric acid to the water; nor, in fact, as we shall see later, to any particular acid. The effects, however, seem confined to steel and iron; as by similar treatment I have as yet obtained no perceptible effect on copper or brass. At first I was inclined to believe that the effects were due primarily to a change in the molecular structure; but a more extended series of experiments has led me to adopt entirely the view taken by my friend Mr. W. Chandler Roberts, who predicted that the effects were most probably due to the absorption of hydrogen.

[1] Read before the Society of Telegraph Engineers, April 14, by Prof. D. E. Hughes.

I have tested these wires in my induction balance, but can find no change whatever in its magnetic conductivity, nor any change which would be the equivalent of those produced by heat, strain, torsion, or tempering; but there are very evident results produced: if the conditions of the experiments are such as to favour the absorption of hydrogen. For instance, if we reduce the proportion of sulphuric acid to one-twentieth, we find that it requires some thirty minutes' immersion to produce the full effect, a few minutes' immersion producing no perceptible result. If now we place an amalgamated zinc plate in the same liquid, and join the two extremities, we have an ordinary battery, where hydrogen is given off on the steel wire. Now as the hydrogen produced by the decomposition of the water is much more rapid than before, we find that a few minutes' immersion produces a far more brittle wire than could be obtained by hours of simple immersion, and we have the result free from any doubt as to its being a mere surface action; for if we immerse the wire alone, surface corrosion rapidly takes place, but by simply connecting it with the zinc the steel is perfectly protected, retaining its original bright surface, for any time, as long as it is so protected.

It is not absolutely necessary that we should join the zinc in the same cell, for if we pass a current from a few cells of an external battery through two steel wires as electrodes in sulphuric acid and water we find that both wires have become brittle, though in a very different degree, the wire connected with the zinc or negative pole remaining bright, although excessively brittle, whilst the one connected with the positive pole is much corroded, and but feebly brittle, with this arrangement. I find that sulphuric acid is no longer required, but that all acids, neutral salts, and ordinary water produce an active effect, the time required being simply as the conductivity of the liquids employed. When water or most neutral salts are used, we find the negative pole quite bright, but brittle, the positive pole much corroded, but not at all changed as regards its flexibility.

I believe that these effects are due to the absorption of hydrogen when the hydrogen is in the "nascent" state, for I have obtained no results by continued immersion in carburetted hydrogen gas (ordinary lighting gas), but when plunged into a medium containing the hydrogen just freed from its combination, its effects are most remarkable: for if we immerse a wire into sulphuric acid and water, say one-twentieth, the effects are slow, requiring at least thirty minutes; but if we let fall into this water some scraps of zinc hydrogen is rapidly given out, and by now immersing the steel wire in this gaseous liquid, taking care not to touch the zinc, we find that the steel becomes rapidly brittle, whilst its surface is free from corrosion, due no doubt to the protecting surface of surrounding hydrogen.

Hydrogen seems to permeate through the entire mass, for iron rods a quarter of an inch thick were equally affected, requiring more time, or in other words, a supply of nascent hydrogen sufficient for the larger mass; and once the wire has become hydrogenised (if we may be allowed the expression), it retains it under all circumstances of time and change of surrounding atmosphere: heat alone, of all the means I have tried, has any effect; and if we heat a wire to cherry red in a spirit lamp we find that it is completely restored to its primitive flexibility in a few seconds. This same wire, however, on being immersed in the accidulated water, rapidly becomes again brittle; we may thus at will render the same wire flexible by previously heating it, or render it exceedingly brittle by favouring its absorption of hydrogen.

I have remarked that a wire immersed in sulphuric acid and water of any proportion, say one-sixteenth, becomes more electro-negative than at the first instant of plunging. If we take amalgamated zinc as the positive element, and a steel or iron rod or wire for negative, we find that there is such a remarkable similarity of electromotive force between all kinds of steel and iron that we are forced to the conclusion that we are simply testing the electro-negative qualities of hydrogenised iron; the force being with amalgamated zinc ·56.

I noted here a remarkable fact, and which does not agree with the results of many authorities. I found that as soon as the iron rod had absorbed its maximum of hydrogen (a few minutes after being short-circuited), it became a constant cell, giving but small traces of polarisation when or after being short-circuited for hours at a time. There occurs, however, a slight diminution of electromotive force after a few days' hard work, being then ·52, due to the acidulated water becoming more neutral by the formation of sulphate of zinc and iron. If, however, we wish to restore its full electromotive force, we have only to short-circuit the cell for a few seconds, torrents of hydrogen will be given off, and its electromotive force becomes, on testing of its highest value, ·56.

If we short-circuit the hydrogenised iron cell for one minute, and at once test its electromotive force, we shall find at the first instant a certain amount of polarisation, about 10 per cent., but it rapidly recovers, being at its full initial force in ten seconds' repose; whilst carbon, platinum, and all other negatives yet tried, did not recover their polarisation in several minutes' repose.

Taking the Smee battery as the best example of depolarisation in a single liquid, and comparing the constancy of this cell with that of the hydrogenised iron, I find that according to Mr. Latimer Clark's experiments, in his work on electrical measurements, that the electromotive force of a Smee cell is 1·017, but when in action only ·446. Thus its electromotive force in action is less than that of the iron cell, and its polarisation some five times greater than that of iron.

I have submitted these results (rather hastily obtained) to our president, Mr. W. H. Preece, and he has kindly consented to have some exact measurements made of the electromotive force of hydrogenised iron, and its comparative freedom from polarisation with all other metals employed as negative elements in a single liquid cell, the results of which quite agree with those obtained by myself.

A practical application of iron as a negative may be mentioned. If we wish to purify mercury from any zinc, or any metal less negative than iron, we have only to place the mercury in dilute sulphuric acid, and then introduce an iron rod so that its lower portion shall make contact with the mercury, hydrogen is now freely and constantly given off by the iron, and this continues until all traces of zinc have disappeared; and as a proof of this, if after a certain time, when no hydrogen is given off, we simply touch the mercury with zinc for an instant, the hydrogen at once reappears, and continues until this small portion of dissolved zinc has been separated from the mercury.

In order to render evident the remarkable depolarising power of iron, we use in the same cell several negatives, such as carbon, platinum, silver, copper, and iron; and if we test these negatives separately for its initial electromotive force, we shall find them all superior to iron. But if we join all the negatives together, and short-circuit the whole with the zinc, iron alone will freely give off its hydrogen, whilst carbon will appear to be entirely inert, and if after this short-circuiting we insultate or separate the different negatives, we shall find on testing them that they are all polarised, carbon being the most so, and iron comparatively quite free, and at its initial giving the highest electromotive force.

In conclusion I may add, that if hydrogen seems to be an enemy of iron and steel, rendering it brittle, on the other hand it is perhaps its best friend in rendering it more negative, and whilst under its entire influence completely preserving it from oxidation or rust.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The debated question of a Natural Science Degree will again come before Congregation on April 27. It is proposed to create a new faculty and to allow students in the Science Faculty to pass a modified form of Responsions and Moderations, in which a modern language may be substituted for Greek, but in which the mathematics required will be more advanced than at present. The following is the proposed form of statute:—

"*Of the Faculty of Natural Science.*—1. There shall be a Faculty of Natural Science, in which two degrees shall be granted, viz., the Degree of Bachelor of Natural Science and the Degree of Master of Natural Science.

"2. Any person duly matriculated wishing to proceed to a Degree in Natural Science shall be deemed to be a Scholar in the Faculty of Natural Science as well as in the Faculty of Arts."

In the Natural Science "Responsions" candidates shall offer two books, either (1) one Greek and one Latin, or (2) one Greek and one German, or (3) one Greek and one French, or (4) one Latin and one German, or (5) one Latin and one French. A special knowledge of the grammar of the languages of the books selected will be required. The candidates will also be examined in arithmetic, in plane geometry, including doctrine of similar triangles, and in algebra, including quadratics and ratio and variation.

In the Natural Science "Moderations" candidates shall offer *three* books, one being some portion of a Greek or Latin historical or philosophical work. The mathematical part of the examination will include theory and use of logarithms, trigonometry as far as the solution of plane triangles, the rudiments of plane co-ordinate geometry, and the mechanics of solid and fluid bodies treated by elementary methods.

After passing "Moderations" the student will be at liberty to enter the Natural Science School or the Mathematical School in honours.

A GRANT of 75*l.* has been made from the Worts Travelling Scholars' fund to Mr. J. E. Marr, B.A., of St. John's College, Cambridge, to enable him to travel in Norway, Sweden, and the islands of the Baltic, and collect evidence and specimens bearing upon the classification of the Cambrian and Silurian rocks, with the understanding that specimens be sent by him to the University, accompanied by reports which may hereafter be published.

THE Queen has signed the charter of the new Royal Irish University, the successor of the Queen's University. The Senate is large and fairly representative.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, April 15.—The Rev. G. Henslow in the chair.—The Secretary read a paper for the Rev. R. Boog Watson, on the Mollusca of the *Challenger* Expedition (Part 5). Some thirty-five species are described and compared, whereof the greater part are new forms and belonging to the families Solenoconchia, Trochida, Rissœllidæ, Litorinida, and Cerithiidæ. The author observes that temperature even more than mere depth seems an important condition in molluscan life, while both prove barriers to distribution, though great length of time naturally helps escape from these barriers. Where barriers of depth and temperature do not check distribution there is no limit to universality of distribution, and such is the case with certain existing species; still there is no trace of especial, lasting, and progressive change.—A communication was read by Mr. N. E. Brown on some new Aroideæ, with observations on other known forms (Part 1). Of the former the specimens are contained in the Kew Herbarium, and the latter are annotations, chiefly supplementary to Prof. Engler's recent monograph of the order. While following Engler, the author has given preference to the classification of Schott. Among others several interesting new Bornean forms are described.—Prof. F. Jeffrey Bell next read a note on an abnormal (quadriradiate) specimen of *Amblypneustes formosus*, and afterwards Mr. Chas. Stewart exhibited and made remarks on another but differently abnormal specimen of the same species.—Prof. Bell, after a full description of his specimen, observes: that with more or less reason some naturalists have looked on the possession of other than five rays as a character of some specific value among the Asteridæ and Ophiurida, and have considered that, on account of its greater rarity among the latter, it is of greater value as a mark of distinction; but such a view must be taken with considerable limitation. The pentamerous arrangement of parts in the regular Echinida is, then, only disturbed in one example; information and specimens are, however, at hand to show how this may have happened. The rarity of any divergence from this five-part division, in face of the numerous variations which occur in the Echinodermata, will doubtless become more and more important as a factor in determining the genealogical history of the group.—A series of microscopic sections of pearls exhibiting many irregularities in structural detail were shown by Dr. J. Murie, and their several peculiarities explained.—Messrs. S. H. Wintle and George Bay (of Tasmania) were elected Fellows of the Society.

Chemical Society, April 1.—H. E. Roscoe, president, in the chair.—The following papers were read:—On betorcinol and some of its derivatives, by J. Stenhouse and C. E. Groves. The authors have extracted from *Usnea barbata* an acid provisionally named barbatic acid, which is probably dimethylevernic acid; by distillation it furnishes carbonic acid and betorcinol (or Borcin). Betorcinol melts at 163° C., giving a bright crimson colour with hypochlorites: its ammoniacal solution is rapidly coloured by exposure to air. Chlorine, bromine, and nitroso compounds were prepared and examined.—Note on chemical equilibrium, by M. M. P. Muir. The object of this paper is to describe a few measurements of the variations caused in chemical changes by modifications in the conditions of these changes, and to attempt to generalise some of the conditions of chemical equilibrium, looking at the phenomena from a dynamical point of view.—Preliminary note on the action of the new diastase Eurotin on starch, by R. W. Atkinson, Professor of Chemistry at Tôkiô. The author has studied in detail the interesting manufacture of "saki," the fermented liquor from rice; he comes to the conclusion that the ferment solution "koji" converts the starch of rice not into maltose and dextrin, but into glucose and dextrin. Analyses of the "mash" are given at various stages from the first to the twenty-eighth day.—Note on the products of the combustion of coal-gas, by L. T. Wright. In opposition to the paper recently read before the Society by Mr. Ridout, the author concludes that ozone is not formed by the combustion of coal-gas, and that the substance which gives the blue colour with iodide of potassium and starch is probably nitrous acid, as when the coal-gas and air are carefully freed from ammonia no blue colour is produced.—On polysulphides of sodium, by H. C. Jones. The author establishes the existence of the pentasulphide, which is probably a tetrathiosulphate; this is probably the highest sulphide. On heating it is converted into a tetrasulphide. The precipitate produced by the addition of the pentasulphide to cadmium salts contains cadmium sulphide and sulphur.—On the reflection from copper and on the calorimetric estimation of copper by means of the reflection cuprimeter, by T. Bayley. The author has shown that the light reflected from metallic copper contains all the elements of white light, but that the region of the spectrum to the red side of the D line is more intense than in the spectrum of the reflection from a white surface of equal illumination; the light transmitted by dilute solutions of cupric salts is deficient in those rays which the spectrum of reflection has in excess. It follows that if we look at a copper surface through a sufficient thickness of cupric sulphate solution the metal appears silver white. Upon these facts the construction of the reflection cuprimeter is based.—On pyrene, by Watson Smith and G. W. Davies. Crude material from Dr. Schuchardt was purified by crystallisation from petroleum spirit. Light yellow monoclinic crystals were obtained melting at 149°, having a vapour density of 6·912, calculated 6·999.—Analyses of the ash of the wood of *Eucalyptus rostrata* and *E. globulus*, by Watson Smith.—On the action of organo-zinc compounds on quinones (second notice), by F. R. Japp. The author has succeeded in isolating the substance $C_{16}H_{14}O_2$, and has studied its more important reactions. From various considerations he concludes that Graebe's views, giving to phenanthrene quinone the formula of a peroxide, are correct.

PARIS

Academy of Sciences, April 12.—M. Edm. Becquerel in the chair.—The following papers were read:—Nebulæ discovered and observed at Marseilles observatory, by M. Stephan.—On the explanation of MM. Lontin and de Fonvielle's experiment, by M. Jamin. He traces the effect to the magnetisation of the direct current being greater than that by the inverse.—On some compounds of halogen substances, by M. Berthelot. A thermo-chemical study.—The plague in modern times: its prophylaxy defective or *nil*: its limitation spontaneous, by M. Tholozan. Like other evils whose secret is unknown, it appears at one or several points, extends, reaches its acme, then diminishes, and ceases; and this independently, to a great extent, of sanitary measures.—Disinfection of vehicles by means of anhydrous sulphurous acid, by M. Fatio. This relates to the effects of a spray of the acid in waggons containing phylloxerised roots and plants.—On cyclotomic functions, by M. Lucas.—Reply to a note of M. Boussinesq, by M. Bresse.—Studies on chronometry; compensation, by M. Rozé.—On a new dynamometric indicator, by M. Deprez. He sought a mechanism giving a diagram from motion of the pencil only, the paper being at rest; the problem was to impart to a point C a motion proportional and parallel each instant to the resultant of the motions of two other points A and B. The pantograph meets this want (where the three points are in a line). Point A is attached to the piston of the indicator, point B (which must be guided in a straight line) to the cord commanded by the piston of the engine, and the crayon is at C. The arrangement has several advantages.—On the deformation of glass tubes under strong pressures, by M. Amagat. He shows by an experiment (with mercury) that the glass or crystal tubes, nearly 1 mm. internal diameter, and 10 to 12 mm. external, which he has used in experiments on the compressibility of gases, do not sensibly increase in volume under pressures up to 400 atm.—On some new experiments of magnetic attractions, by M. Ader. Of various substances tried (wood, paper, &c.), elder-pith was most attracted by a magnet. With a Jamin magnet capable of holding 100 kg., and having two small polar armatures 0·002 m. apart, he attracted at 0·03 m. distance a suspended pith ball 0·005 m. diameter. He could raise it at a distance of 0·004 m., and, once attracted, it was held, spite of shocks given to the magnet.—On the freezing-point of alcoholic liquors, by M. Raoult. For solutions containing 0 gr. to 10 gr. alcohol in 100 gr. water, the retardation of the freezing-point resulting from addition of 1 gr. of alcohol is constant and equal to 0·377°. The lowering of the freezing-point below zero is proportional to the total weight of alcohol dissolved in a constant weight of water (whence, probably, the alcohol here exists in the anhydrous state). For solutions with 24 to 51 gr. alcohol in 100 gr. water, the retardation of the freezing-point, on adding each gramme of alcohol, is constant and equal to 0·528°. The total lowering below zero is not proportional to the total weight of alcohol (so that the dissolved body is probably a hydrate of alcohol, at least between −10° and −24°). M. Raoult gives a table of freezing-points of various fermented liquors; these are always lower than for equivalent mixtures of alcohol and water. The freezing-point descends as the freezing progresses.—On two new silico-titanites of soda, by M. Hautefeuille.—On the examination of pyrites by the gravivolumetric method, by M. Houzeau. The sulphur in pyrites can be determined much more quickly by this method.—On the formation of tetramethylammonium, by MM. Duvillier and Buisine.—On the natural and mydriatic alkaloids of Belladonna, Datura, Hyoscyamus, and Duboisia, by M. Ladenburg.—On the existence of ammonia in plants, by M. Pellet. Operating with the normal plant, he found in beet-leaves (dry) 0·155 gr. ammonia per cent.; beet-seed, 0·168 and 0·216 gr.; beet-root (dry), 0·196 and 0·147 gr.; corn, 0·16 gr.; ordinary linseed meal, 0·188 gr. The regular existence of ammonia in plants is important, suggesting that magnesia and phosphoric acid penetrate them in the form of ammoniaco-magnesian phosphate.—On some facts relative to the gastric digestion of fishes, by MM. Richet and Mourrut. Experiments with fishes of the genus *Scyllum* and with *Lophia piscatorius* seem to show that there are very great differences in the stomachic mucus as to richness in pepsine. The acidity of liquids in the stomach is extreme. (The authors give results of a number of comparative artificial digestions.)—Analyses of chlorophyll, by M. Rogalski. His results agree with those of M. Gautier and M. Hoppe-Seyler.—On the formation of the shell in Helix, by MM. Longe and Mer.

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