THURSDAY, DECEMBER 27, 1877

THE METROPOLITAN SEWAGE

THE question of the effect of the main outfall sewers of the metropolis on the reaches of the Thames below London has occupied the attention of engineers not only since the completion of the works, but throughout the long series of years when those works were under consideration. Some persons qualified to make accurate observations and draw correct deductions from them, asserted that large masses of deposit were directly due to these outfalls, and were daily increasing in magnitude, while others, demanding equal confidence in their statements, asserted that no such deposits existed—in fact, that the sewage outfalls tended to improve the bed of the river by increased scour; thus the bulk of engineers for a long time held diverse views or suspended judgment on the subject, while the general public, not knowing whom to believe, trusted it would turn out all right in the end. Inasmuch as the Metropolitan Board of Works is bound, under the Thames Navigation Act of 1870, to keep the Thames free from banks and other obstructions to the navigation due to the flow of sewage from their outfalls, and to carry on all dredgin g operations required for that purpose, at their own expense, the vision of the possible cost of these works to the London ratepayer is unlikely to be pleasing; still less could any interference with the highway to the most important port in the world be tolerated by the Board who were looked to for its preservation. In 1869, the metropolitan main outfalls having been opened in 1863–64, the Home Secretary appointed Mr. Rawlinson to hold an inquiry on the reported silting up of the Thames, which was then causing great alarm; such, however, was the contradictory nature of the evidence, that the result was almost nugatory, and the question still remained in abeyance. In the course of the last summer the Thames Conservators requested Capt. Calver, R.N., F.R.S., to direct his attention to the subject, and report to them thereon. Before pointing out the conclusions arrived at by Capt. Calver,[1] it will be as well to direct attention to the part of the river under consideration. The northern outfall is situated immediately above Barking Creek, which forms the embouchure of the river Roding, and is about two miles below Woolwich; the southern outfall is about 2¼ miles lower, or 4½ miles below Woolwich.

In the face of the fact that this special inquiry was held and many competent witnesses examined with the sole object of determining whether or no the sewage outfalls have caused a silting up of the river in their neighbourhood, or the formation of shoals and mudbanks, and that so many observations and statements have since been made with the same view, it seems perfectly monstrous that the question should still remain unsettled. In the report now before us we have the last contribution on the subject, or perhaps, with more fairness it might he said the last but one, as since its publication Sir Joseph Bazalgette has addressed to the daily press a letter containing a direct denial of many of the conclusions there arrived at. When professional experts differ so entirely not only in their conclusions, but also in the facts upon which these conclusions are based, we see no other course open but to appeal to the cooler and more unbiased judgments of pure science.

[1] "Report upon the Discharge of Metropolitan Sewage into the River Thames at Barking Creek and Crossness." By Capt. E. R. Calver, R.N., F.R.S.

In comparing the analyses of Thames mud from various parts of the river, given in Capt. Calver's report, embodying a series taken in 1867, and another in 1868, by the late Dr. Letheby, with those given by Dr. W. A. Miller, and Dr. W. Odling in 1869, so close an agreement is manifest that a safe conclusion can be drawn from them. The analyses are as follows:—

Average Percentage Composition.

	1867	1868	
Organic matter	15·00	14·19	Dr. Letheby.
Mineral ,,	85·00	85·81	,,
	100·00	100·00	

On these analyses Dr. Letheby remarks that the above percentage proportions did not differ materially from the quantities of organic and sewage matters which he found suspended in water at London Bridge, and in the mud at London Bridge, Chelsea, and Westminster, when the sewage was discharged at low water. The next table gives nearly identical results from the analysis of the mud at the outfalls in 1867, and those of the suspended matter in the Thames water at Greenwich, Woolwich, and London Bridge in 1862, by the same chemist. Now, Dr. W. A. Miller so far agrees with these results that in his evidence, given at the inquiry before referred to in 1869, he states the percentage of organic matter in the mud taken from Barking Creek to be 16·2, from the Thames between Chiswick and Westminster, 15·8, and further, that of these two quantities 3·1 and 3·05 respectively consists of nitrogen, and finally, in answer to the question: "But there is nothing special and differing in the mud at Barking from the ordinary mud of the River Thames?" he says: No, the composition is as nearly the same as may be. With these observations Dr. Odling's evidence closely agrees.

Here, then, we have an agreement which nobody appears to dispute, and which leads inevitably to the conclusion that the great bulk of noxious putrescible matter left uncovered at low water throughout the whole of the tidal portion of the Thames owes its deleterious character mainly, if not entirely, to the presence of sewage matters.

Having carefully pointed out and established this identity of composition, Capt. Calver proceeds: "It is, however, equally necessary to prove that there is enough of this material in the sewage discharged from the outfalls to account for the large accumulations of it which have found a resting-place in the Thames channel." Here we are met by estimates differing in the wildest manner, and varying from 35 to 100 grains per gallon, and again to nearly double that amount, but fortunately we are here even given material for a trustworthy estimate. In the table of analysis given by Prof. Williamson of samples taken from the northern outfall in September of this year we find 108·01 and 151·45 grains per gallon as the actual amount of suspended solid matter at different times, the samples being collected in fine weather. Now abundant evidence has been given at various times, showing that after heavy rain the sewage contains an amount of solid impurity equal to, if not greater than, that in the fine-weather flow; thus there can be no doubt that the lower of these two figures is not in excess of the average. Capt. Calver takes the amount at 100 grains per gallon, and multiplying by the daily discharge quoted as 120 million gallons, he obtains a result of 279,225 tons per annum. This probably does not exceed one-half the true amount, as the water supply of the metropolis alone reaches the amount assumed for the daily discharge, and the rainfall over the drainage area gives nearly an equal amount, which, for the reason just stated, must be taken into account. We thus appear to have at command upwards of half a million tons of suspended matter discharged into the Thames in each year, which is amply sufficient to account for the deposits observed. Thus we read in the report that "Mr. Leach (the engineer of the Thames Conservancy Board) reported in December (1871) that a deposition of 7 feet 9 inches of mud had formed between the upper end of the southern embankment and the White Hart Draw Dock, Lambeth; that another bank 100 feet wide and 6 feet thick occupied the river-frontage of St. Thomas's Hospital, &c. By July of last year a material portion of these masses had been cleared away by excessive rainfalls." Are we to be left to the mercy of such an unpleasant remedy as the floods of last autumn to abate a nuisance of such magnitude, threatening, as it does, the existence of such an institution as St. Thomas's Hospital, and showing how soon we may return to the unsanitary state of affairs that existed twenty-five years ago? We have purposely avoided dealing with an equally important part of Capt. Calver's report, in which he points out the danger of the silting up of the navigable channel of the Thames below London, as he has not shown that the sectional area, though varying from year to year, has at any point permanently diminished, still the destructive elements have been shown to exist, and the forces which now hold them in equilibrium may at any time be thrown out of balance and the evil creep on imperceptibly if once the eyes of the public are closed to its existence. Without going into the question of the value of the sewage estimated by the highest authorities at 1,000,000*l.* per annum, thus not only wasted but employed as a powerful obnoxious agent, enough has been shown from the report before us to, we hope, show the suicidal folly of discharging sewage wholesale and unpurified into tidal rivers. Yet even now a scheme is under consideration for the collection of the sewage from a large area in the Thames Valley and for its discharge into the tidal waters of the Thames. We believe that a careful perusal of Capt. Calver's Report will dispel from the minds of the Thames Valley Joint Board all hopes of a satisfactory though expensive solution of their difficult problem being arrived at in this manner. As a remedy for the state of things he has shown to exist Capt. Calver recommends that in pursuance of the powers they possess the Conservancy Board call upon the Metropolitan Board to dredge away the obstructions they have caused; this may be indispensable at present and may be an unavoidable and constantly recurring expense until some profitable scheme is devised for utilising the metropolitan sewage; in the meanwhile the example of the inhabitants of Abingdon, as shown by the letter of their medical officer of health in the *Sanitary Record* of November 30, shows the inutility of other towns in the valley of the Thames striving to follow the example of London, and further increasing its difficulties. We learn from Dr. Woodforde's letter that the whole of the sewage of the town of Abingdon is purified by filtration through natural soil being frequently absorbed by one acre of land, and that the amount of organic and inorganic impurity contained in the effluent water after passing through the land is far less, in some cases less than one half that contained in the well water used for drinking purposes in the town. As this unprecedented result has been obtained on land of a character which exists in abundance throughout the Valley of the Thames we think that the towns situated therein have not far to look for the solution of their difficulties.

BOTANY IN GERMANY

Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. A. Pringsheim. Elfter Band. Erstes und Zweites Heft. (Leipzig: W. Engelmann, 1877.)

THE second decade of volumes of the Jahrbücher is now begun, and up to the present shows no sign of any falling off from the high standard of excellence attained by the former parts It is somewhat remarkable that such a work can be carried on successfully. Profusely illustrated (having about 500 plates in the ten vols.), and containing papers of great merit, it is at once evidence of the marvellous botanical activity of the Germans, and the energy of their publishers. A glance at the list of papers in the ten volumes shows that the Jahrbücher contain papers that have become classical, and have been contributed by men who have risen to the highest eminence in botanical science. Comparatively few of the papers are contributed by Russians or Italians, hence this one work may be looked on as almost wholly the result of German research. The papers contributed are chiefly morphological and physiological, although occasionally one having immediate bearings on taxonomy is introduced. There can be little doubt that the German university system tends greatly to foster original research, not only in botany, but in all other departments. The botanical institutes, with laboratory, garden, and herbarium attached, the way in which the students are induced not only to learn but to work under the superintendence of the professor, the whole system of private teachers and mode of promotion of the professors fosters research, and gives a thoroughness and heartiness to the work. In certain departments of botany, Britain is second to none with her Hooker, Bentham, and Darwin, but when we consider the enormous "microscope" power of Britain, we cannot help thinking that much of it goes to waste. There must be hundreds of microscopists residing near our coasts, yet what do we know of the reproduction of our algæ? A glance at the "Botanischer Jahresbericht" shows how few British botanists there are, and also that each contributes comparatively few papers per year. But quality is better than quantity—work slowly and well. The time is no doubt coming when we may look for increased botanical activity, perhaps the union of botanical studies to medicine has had something to do with the comparative depression, and if botany becomes a preliminary instead of a purely professional study by becoming more diffused, a greater taste for the subject may arise.

Prof. Pringsheim contributes the first paper, one part dealing with the interesting subject of the budding of the fruit of mosses, the second on the alternation of generation in the Thallophytes, a subject suggested by the first part. If the seta of the ripe fruit of the moss be cut into pieces, and the pieces cultivated on wet sand, protonema threads will grow from the cut portions, and produce the usual buds, exactly like protonema threads developed from the spores or stem and leaves of mosses. The anatomical connection of the protonema with the tissue of the seta can be observed in good longitudinal sections. Not all the cells can give rise to protonema, but only those of the middle zone, situated between the peripheral cortical cells and the central bundle. These cells contain abundance of reserve matter, such matter being found in many parts of the moss-fruit. The product of protonema by the seta of the moss is to be compared to the budding of the prothallium of ferns described by Farlow. Pringsheim figures in the two plates illustrating the paper, the protonema developing from the seta of *Hypnum serpens*, *H. cupressiforme*, and *Bryum caespitosum*, and he shows the stem and seta to be identical structures.

The second part of the paper, on the alternation of generation in Thallophytes, is difficult to follow without illustrations, as it takes for granted that the reader is acquainted with all the recent researches on the lower plants. Pringsheim distinguishes between sexual alternation of generations and vegetative alternation of generations (sprosswechsel), the fructification and vegetative propagation. All the generations of Thallophytes (as well as of the Cormophytes) begin with one free cell (the spore). The generations in the Thallophytes represent free individual plants, while in the Cormophytes the generations remain in organic connection and in their individual sequence appear only as two portions of one series of developments. From this it follows that the "fruits" of Thallophytes never have the value of a "generation," and also that where the development is due to sexual influence, they are only sexually influenced organs of the female plant. Such parts are the fruits of *Florideæ*, also apparently the Perithecia and Apothecia of Ascomycetes, which do not behave differently from the calyptra of the moss or the thickened tissue (gewebepolster) of the prothallum, in which the embryo of the vascular cryptogams is developed. Pringsheim believes that in the trichogyne and ascogon the influence of fertilisation is spread from cell to cell until it reaches the spores, just as in mosses and ferns the reverse process occurs, and the influence spreads from the fertilised germinal cell to the archegonium. Carpospores and ascospores are therefore to be regarded not as sexually-produced spores of a sexually-produced generation, but as truly sexually-produced spores, developing in the sexually-influenced organ of the mother plant.

The second paper, illustrated by two plates of diagrams, and occupying nearly half the part, is by F. G. Stebler, "Researches on Leaf-growth." The numerous observations made on *Allium Cepa*, *Secale cereale*, *Triticum vulgare*, *Cucurbita melanosperma*, are detailed at full length, and the following summary of the result of the paper is given at the conclusion. The leaf begins to grow very slowly, then growth becomes more rapid until a maximum of rapidity is reached; then growth becomes slower and slower until at last it ceases. The leaf thus behaves like other growing parts of plants. The growth of the linear monocotyledonous leaf is basipetal. The apex zone of the leaf ceases earliest to grow, then succeeding zones in basipetal order, until lastly the growth of the basal zone terminates the growth of the entire leaf. Most productive of increase in length is the growth in the basal zone, but at different times the maximum activity is in different zones, the absolutely greatest zone of growth proceeding in succession from the upper part of the leaf to the lower. The maximum period of growth of the whole leaf is the sum of the maximum periods of all the zones.

The linear monocotyledonous leaves examined in reference to alternations of growth by day and night showed a daily periodicity of growth, the growth diminishing as the intensity of the light diminishes. The maximum of growth corresponds to the greatest intensity of light; the minimum is observed to occur shortly before sunrise. The cause of the daily periodicity of growth is assimilation; as assimilation increases the growth increases; as it diminishes the growth diminishes.

The same daily periods of growth were observed in etiolated linear monocotyledonous leaves in the dark, the external conditions being constant. The periodicity has thus been transmitted.

In the dicotyledonous leaves observed the daily periods were modified, so that after the maximum of growth was reached in the forenoon a retardation took place, and a gradual diminution of the growth till the following morning before sunrise. At daybreak the growth rapidly increases again to reach a maximum in the forenoon. If the intensity of the light is small the maximum is later of occurring than if the light be very intense.

The maximum of the day periods of growth of the dicotyledonous leaf is due to the assimilation. The retardation during the day occurring after the maximum of growth (but not the maximum of light) has been reached, is due to the action of the light.

The third paper occupying the remainder of the part is by Dr. Celakovsky, and is entitled, "Teratological Contributions to the Morphological Import of the Stamens." It is illustrated by three plates. Considerable uncertainty still exists as to the morphological value of the different parts of the stamen, but more especially of the anther. The difficulty does not exist in regard to the pollen-bearing caulomes, but there are still difficulties in those cases where the stamens are modified leaves. Whether the question can be settled by the study of the development alone is a matter of doubt, even after the valuable researches of Warming and Engler on the subject; and it appears likely that the most important results may be expected from the careful study of the numerous abnormalities of stamens so constantly met with. The scientific study of the teratological developments of stamens must therefore be looked upon as of the highest importance, and Celakovsky—already well known by his teratological researches, here describes and figures the changes (phyllody) of the stamens of *Rosa chinensis*, *Dictamnus albus*, and in the double flower of *Camellia japonica*.

There are two important questions to be answered. 1. Are the pollen-sacs mere enlargements of the leaf-substance of the staminal leaf, or are they special developments somewhat like "emergences"? 2. Do these sacs belong to the under side, upper side, or both sides of the leaf; or are there differences of position in different plants?

Cassini and Roeper held that the pollen-sacs were cavities in the leaf-parenchyma, two forming on each side of the leaf, so that the margin of the leaf corresponded to the suture between the sacs. Mohl considered this view only to hold for certain cases, as the Euphorbiaceæ, and found, what Bischoff had already pointed out, that in all examples examined, as in poppy, rose, and nigella, the four pollen sacs were placed on the *upper* side of the leaf, and that the margin of the leaf ran along the two posterior or lower loculaments. Mohl did not consider the sacs as "emergences," and differing morphologically from the true leaf, as he says that the connective represents the central portion of the modified leaf, while the loculaments are the thick swollen lateral halves, which become contracted in length and breadth. Mohl considered that in the plants with extrorse anthers both the loculaments of each anther lobe were developed on the under side of the leaf. Alexander Braun pointed out in 1851 that the anthers were produced by doubling of the lamina (Ueberspreitung). This view was confirmed by Wydler in 1852, who compared the anther to the abnormal double lamina in the leaf of Bignonia.

Sachs considers the anthers to be appendages of the leaf. He compares each loculament in the anther of Cycads and Cupressineæ to Sporangia; the four pollen-sacs in the Metasperms being "emergences" from the upper side of the leaf, those of the Archisperms from the lower side. Braun still further examined the subject and confirmed his original views, namely, that the pollen-sacs do not belong to a simple leaf, but to one with a double lamina, the doubling due to the formation of an "emergence" (in Karl Schimper's, not in Warming and Sachs' sense). The two upper anther sacs belong to the "emergence," the two posterior to the original lamina of the leaf. Celakovsky in the paper now before us departs from the views published by him in *Flora* for 1874, and fully confirms the views of Braun and Wydler.

The second part of vol. xi. contains five papers by Pfitzer, Koch, Reinke, and Reinsch. Dr. Pfitzer's paper is on the rapidity of the current of water in the plants. It contains an elaborate series of researches, the first on the movement of leaves due to the absorption of water by the stem and root; the second by means of solution of lithium. Dr. McNab's experiments are extended and confirmed, but the astonishing rapidity of 22 metres per hour was observed in *Helianthus annus*, the greatest rapidity observed by Dr. McNab being 40 inches per hour in *Primus Lauro-cerasus*. Pfitzer also uses a solution of soluble indigo carmine 4 parts to 1,000, and finds that it is superior to solution of lithium, as it can be detected at once instead of using the spectroscope.

The second paper is by Dr. Ludwig Koch, on the development of the seeds of Orobanchaceæ. The development of the anatropal ovule, with one integument is described, and the development of the embryo. This agrees with the description given by Hanstein, of the embryo of Capsella. The endosperm is formed of divisions of embryo-sac, which contains antipodal vesicles before fertilisation. The third and fourth papers are by Prof. Reinke, both on the development and reproduction of algæ, of the genera Phyllitis, Scytosiphon, Asperococcus, and Bangia, the observations having been made at the Zoological Station at Naples, during the winters of 1875 and 1876.

The last paper is by Reinsch: "Observations on new Saprolegnieæ, on parasites in cells of Desmedicæ, and on the 'Spinous Spheres' in Achyla." A number of new species and genera are described and fully illustrated.

W. R. M'NAB

MOVING DIAGRAMS OF MACHINERY

Patent Working Drawings. By H. and T. C. Batchelor (London: Macmillan and Co.)

ALL who are engaged in the teaching of kinematics and of applied mechanics must often have it brought forcibly before them the difficulty that exists in making even comparatively simple mechanical motions intelligible to students by means of ordinary drawings and diagrams, while the more complex motions and combinations can hardly be treated of at all profitably without the aid of working models, which are very expensive, and take up a great deal of space. Again, inventors and the proprietors of patented mechanical inventions, are often at a loss to explain to unscientific or uninitiated persons the advantages of their systems, and costly working models have to be resorted to in order to avoid the mystification which ordinary mechanical drawings often produce in the minds of those not accustomed to them, or who are not versed in the principles of mechanics.

To supply this recognised need of teachers and others, Messrs. H. and T. C. Batchelor have designed and worked out a most ingenious system which combines the mechanical movements of a model with the flatness and clearness of a diagram. The name "Working Drawings" applied to these diagrams is somewhat misleading, especially to engineers and others accustomed to this term as having a distinct and special meaning, namely, drawings made for and used by the workmen employed upon the construction of machinery to work from. Working drawings are essentially drawings for the workshop, and that is the universal acceptation of the word. The meaning attached to it by Messrs. Batchelor is, however, very different; it is drawings which will work moving diagrams. This sense is, perhaps, more critically correct, but as another meaning is the generally accepted one, we cannot but think that it would have been wise if a name had been given to these diagrams more descriptive of what they are. They are, in fact, moving diagrams or sectional working models of machines, the fixed parts being lithographed as a background upon a firm cardboard mount, and the moving parts being also lithographed on card, but cut out and jointed together by most ingenious mechanical contrivances; the whole being no thicker than a sheet of stout cardboard.

The perfection of the centres upon which the various parts revolve or are pivoted together must be seen to be adequately appreciated, for while these centres allow perfect ease of motion to all the parts, they are absolutely steady and without the slightest shake. It is this system of centring that constitutes the patent by which Messrs. Batchelor's drawings are protected. The pivots are made entirely of card and paper cut in a most ingenious manner, by which both freedom and steadiness are insured. Nor are the centres the only parts of these drawings interesting for their ingenuity; the contrivance for holding down the sliding parts is equally good. It consists of a band of thin paper passing over the sliding part, and printed exactly like the part it covers, so that it is invisible except on close examination.

The first of these drawings which is before us is a diagram in illustration of the action of the "trunk engine," the characteristic feature of which consists in making the piston-rod hollow and of sufficient internal diameter to allow the connecting-rod to be attached at one end direct to the piston, and to oscillate within the trunk, the other end embracing the crank-pin. By this means the crank shaft can be brought nearer to the cylinder, considerable space thereby being saved, and the alternative system by which direct connection between the piston and crank is effected, viz., the oscillating cylinder, is avoided, with its more complicated valve gear and expensive construction.

The double trunk system represented in the drawing was the invention of the late Mr. John Matthew, who for many years was a partner in the eminent firm of Messrs. John Penn and Sons, and it is the system upon which, almost without exception, the large screw engines of Messrs. Penn are constructed, with which so many of the ships in her Majesty's navy are fitted.

There is nothing to be desired in the execution of the diagram before us, of which the name of Messrs. Maclure and Macdonald is a sufficient guarantee. It is lithographed in white upon a blue ground, and all the parts come out with singular distinctness. We could have wished that, in the choice of an example for illustration, a more modern design of engine had been selected. The eccentric rod, with its lattice bracing, is that employed in the old beam engines, and a trunk engine made to the drawing before us could hardly work, for the crank pin is evidently inserted into one of the spokes of the fly-wheel, and unless projecting to an impossible extent, the trunk could not clear the wheel; this could very easily have been remedied by showing the "throw" of a crank behind the connecting-rod, which would have aided rather than detracted from the clearness of the diagram.

While thus criticising the particular design of engine selected for representation, we can only express admiration of this most ingenious system of illustrating mechanical motions and the action of machines. For educational purposes it will be of the highest value, and there are many of the examples in Reuleaux's masterly work upon the "Kinematics of Machinery,"[1] so ably translated by Prof. Kennedy, to which it might with great advantage be applied.

We feel sure that Messrs. Batchelor's drawings will be a great boon to inventors for explaining their inventions to others; and as supplementary to scientific evidence in disputed patent cases and other litigation, they will be found of value.

C. W. C.

[1] "Theoretische Kinematik."

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.]

Oxygen in the Sun

FROM the time of the discovery by Prof. Draper of the presence of oxygen in the sun down to the present moment I have devoted most of my leisure time to the consideration of the question as to why the oxygen lines should appear *bright* while the metallic lines should appear dark in the solar spectrum. I was led into this inquiry under the firm belief that the new fact made known by Dr. Draper might lead to a modification of existing views of the sun's atmosphere, and it was consequently with the greatest pleasure that I read in last week's NATURE a communication from Dr. Schuster on this subject.

The views which I have arrived at being in my opinion hardly matured enough for publication, I reserve further statement at present, but will so far anticipate as to say that the explanation which I am disposed to maintain necessitates the assumption that oxygen possesses two different spectra—a low temperature band-spectrum and the well-known line-spectrum of high temperatures. This assumption I thought warranted by the behaviour of other non-metals as made known by the researches of many spectroscopists, but more particularly by those of Salet and Lockyer, and endorsed by the low temperature absorption spectra of the metals discovered by Roscoe and Schuster, Lockyer and Roberts. The recent research of Dr. Schuster, however, has now placed this assumption in the position of a fact, and all who have followed recent spectroscopic advancement will recognise the value and importance of this last discovery.

As Dr. Schuster's explanation of the brightness of the oxygen lines differs fundamentally from that which I am inclined to hold, and as he considers this view warranted by the result of his investigation, I will beg permission to make a few brief remarks upon the chief points of difference between us, being convinced that their discussion cannot fail to elicit opinions of interest to all concerned in the progress of solar physics. This ventilation of opinion is the more necessary as views very similar to, if not identical with, those of Dr. Schuster's had occurred to me and had been abandoned for reasons which I will now explain.

According to Dr. Schuster, "the temperature of the sun, at some point intermediate between the photosphere and the reversing layer" is the same as that at which the spectrum of oxygen changes; that is to say, if I rightly interpret these words, *above the photosphere* the temperature is such that we get the line spectrum of oxygen and *above the reversing layer* the temperature is such that we get the band ("compound line") spectrum. This state of affairs would doubtless account for the reversal of the "compound line" spectrum which Dr. Schuster has now shown to be present in the solar spectrum, but I fail to see at present how it is to be reconciled with the *bright* line oxygen spectrum. Let us consider the conditions more closely. All observers agree in placing the reversing layer at the base of the chromosphere—the present hypothesis necessitates a space between the photosphere and the reversing layer—*i.e.*, a space sufficiently extended to contain the incandescent oxygen giving the line spectrum. Neglecting for the present the antagonism between these views, let us assume that such a space exists, and for the sake of simplicity let us also neglect the other elements which may be present. Now it cannot be assumed that the supposed zone is *higher* in temperature than the photosphere—it *might be* of the same temperature, but, being above the photosphere it would more probably be at a *lower* temperature. Let us, however, make the assumption most favourable to Dr. Schuster's view, viz., that the hypothetical zone is of the same temperature as the photosphere. Then we have a zone of oxygen exterior to the photosphere and of the same temperature as this last region, and above the oxygen the cooler reversing layer. Thus the light of the photosphere passes unchanged through the oxygen zone, and we should see no dark lines corresponding to the line-spectrum of this gas.—As a matter of fact, however, the oxygen lines are *bright*—hence it must be at a higher temperature than the photosphere, or we must be looking through an enormous stratum of it, a stratum thick enough for the radiation of the gas to overpower the fierce glare of the photosphere behind it, and both these views have been shown to be untenable.

December 21 R. MELDOLA

Oxygen in Sea-water

AT p. 267 of the second volume of the "Voyage of the *Challenger*," Sir Wyville Thomson writes:—

"Mr. Buchanan drew the conclusion in explanation of the small amount of oxygen at depths of 300 fathoms and upwards, 'that animal life must be particularly abundant and active at this depth, or at least more abundant than at greater depths.' In other words, that a permanent condition, probably of all conditions the most unfavourable to animal life, is produced and maintained by its excess."

"This is entirely contrary to experience."

The words in inverted commas are part of a sentence in a short report in NATURE (vol. xvi. p. 255), of a paper which I read before the Royal Society of Edinburgh, on the results of the analysis of so many of the samples of air extracted during the cruise, from sea-water of different sources, as I was able to accomplish before my connection with the work of the expedition ceased. I will not encroach on your valuable space by anticipating the discussion of the bearing of my observations and those of others on the question of the greater or less abundance of animal life at different depths in the sea; but as the above quotation, from its fragmentary character, is somewhat misleading, both as to the nature of the belief which I expressed and my grounds for holding it, I must ask you to give place to the concluding sentences of the above report:—

"It is evident from these figures[1] that between 200 and 400 fathoms there is a great consumption of oxygen going on, and, as it is difficult to conceive its being consumed otherwise than by living creatures, the conclusion is forced on us that animal life must be particularly abundant and active at this depth, or, at least, more abundant than at greater depths; for at less depths there is more opportunity for renewal of the oxygen by reason both of the greater proximity to the surface and of the existence of vegetable life. This conclusion was borne out by the numerous experiments made by Mr. Murray with the tow-net at intermediate depths, which went to prove the existence of abundance of animal life down to 400 fathoms, vegetable life never extending to much below 100 fathoms. Below 400 fathoms life is sparingly met with."

It will be seen that the only independent experience which exists, namely, Mr. Murray's observations with the tow-net at different depths, is in favour of the conclusion at which I arrived. J. Y. BUCHANAN

10, Moray Place, Edinburgh, December 13

[1] A table of the mean amounts of oxygen in a hundred parts of oxygen and nitrogen contained in waters from different depths

On some Peculiar Points in the Insect-Fauna of Chili

FOR some years past I have been particularly interested in some points in the entomology of Chili and the extreme southern portion of South America, which, although known to most entomologists who have made special groups their study, have never yet been, so far as I know, even more than casually alluded to in works on geographical distribution, and are ignored in the principal ones. I allude to the occurrence in that part of the world of well-marked palæarctic or nearctic forms not found otherwise in America south of Mexico, and utterly unknown in the southern hemisphere in the Old World.

I have collected a not inconsiderable amount of data concerning this subject, and have the intention of addressing a circular to zoologists and also to botanists, asking for further information.

I will here allude to such familiar genera as *Carabus* amongst beetles and *Argynnis* and *Colias* amongst butterflies. *Carabus* is very abundant in species in the palæarctic region, poor in the nearctic, and reappears (for the whole world) only in Chili. The distribution of *Argynnis* and *Colias* is similar, only that they are about equally abundant in the two northern regions, and of *Colias* it appears probable that a single species occurs in Peru, but this exception only proves the rule.

In the *Trichoptera*, or Caddis-flies, a group of insects in which I am especially interested, there is even a still more striking case. The typical family, *Limnophilidæ*, comprising those insects the larvæ of which manufacture the cases of twigs and straws, so abundant in our ponds and ditches, and which is so rich in species in northern regions, is not, with the exception stated below, known south of Mexico in the New World nor south of the Himalayas in the Old; but I have several species from Chili, Araucania, and the Falkland Isles.

I could already multiply parallel instances, but have said enough to prove my case.

Confessedly I have, at present, only crude theoretical notions on the causes of this anomalous distribution. It might be said that these insects are the remains of a former Antarctic glacial epoch. But if this be so, then we must presuppose the existence of former Arctic and Antarctic faunas similar in details; all other evidence tends, I think, to disprove this. It may truly be said that, owing to the non-existence of large tracts of land towards the south pole at all comparable with those that exist towards the north, we are not in a position to acquire sufficient data, yet we have the continent of Australia and the large islands of New Zealand extending somewhat far south, and they furnish us with no indication whatever of forms parallel with those found in Chili.

It has occurred to me as just possible, that at the conclusion of the northern glacial epoch a few stragglers, instead of wending their way northward, mistook the points of the compass and went southward. But there remains this great difficulty, viz., that, with one possible exception, there are no indications of these forms on the northern portions of the Andes of South America.

I call attention to this subject as one deserving far more consideration than it has hitherto received, and with the idea that, by ventilating it in NATURE, I may receive additional information on a point that greatly interests me.

39, Limes Grove, Lewisham R. MCLACHLAN

Arctic Auroræ

IT will probably interest some of your readers to know that in reply to a communication lately addressed by me to the Admiralty I am informed that Captain Sir George Nares reports that although the auroral glow was observed on several occasions between October 25, 1875, and February 26, 1876, true auroræ were seldom observed, and the displays were so faint and lasted so short a time that the spectroscopic results were not considered worthy of a special report. Although the citron line was seen occasionally, on only two occasions was it well defined, and then for so short a time that no measure could be obtained. A report is preparing with a view to compare the auroral displays with magnetic disturbance, meteorological changes, and other phenomena which will include the few spectroscopic observations obtained. J. RAND CAPRON

Guildown, December 24

Insects and Artificial Flowers

IN a late number of NATURE a short account is given of some experiments recently made by Prof. J. Plateau, of Ghent, as to insects being deceived by artificial flowers. The nature of these experiments is not given, but the result would appear to have been of a somewhat negative character. In connection with the subject the following incident will not, I think, be considered uninteresting. I was coming by one of the lake steamers from Como to Menaggio, in September, 1875, and saw a humming-bird hawk moth, *Macroglossa stellatarum*, fly to some bright-coloured flowers on a lady's hat on deck, and hang, poised over them for a short time, and then fly away. During the process it made one of those short familiar darts off, for a moment, and then returned, after the manner of the moth when disturbed, and it remained long enough to convince me that it had tested the flowers and found them wanting. Another incident comes across my mind while writing this, which, though it does not exactly bear upon the point, yet is of a somewhat kindred nature. I was crossing from Harwich to Antwerp in August of the same year, and as the weather was fine, and the boat crowded, I remained on deck all night. About 4 o'clock in the morning I saw what appeared to be a bird or a bat flying rapidly about the rigging. As I was watching it the funnel of the steamer poured forth a thick column of black smoke, owing to the fresh coaling it had just received. Off went the creature as soon as it perceived the change, or, at all events, as soon as the change took place, and flew for some time in and about the smoke, now darting through it, close to the funnel mouth, and then letting itself be borne along with it, for some distance, as if in sport, looking very strange and weirdlike in the process.

After awhile, as the full daylight broke, it left the smoky region above and came down towards the deck, and I then discovered it to be neither bird nor bat, but a specimen of the death's-head moth, *Sphinx Atropos*, whose flight I then witnessed for the first time. After running the gauntlet of several of the passengers, who tried to catch it with their hats, it settled somewhere on the spars or woodwork of the boat and escaped, perhaps to renew its flight in a similar manner the following day.

Highfield, Gainsborough, December 21 F. M. BURTON

The Selective Discrimination of Insects

MAY I be permitted to remark on Mr. Bridgman's communication in NATURE (vol. xvii. p. 102)? He says he has collected pollen grains of different kinds washed from the thigh of an *Andrena nigro-ænea*, and varying in colour from orange-red to white. The true inquiry as to the discrimination of insects is not as to the colour on distinct kinds of pollen, but their homogeneity in respect of fertilisation.

The remark I made implied, rather than expressed, that bees and butterflies visited only those plants the admixture of the pollens of which induced fertilisation. In this respect and in this only, it appears to me, the investigation of the subject becomes of importance. No fact of natural phenomena is without use and without instruction; there are *no hap-hazards* in nature. If Mr. Bridgman, or other naturalists, can show the admixture of the diverse grains of pollen collected by him from the thighs of the creature named would not induce the fertilisation of the plants from which they were collected, then the discriminatory fact assumed is dispelled and the peculiarity observed by Mr. Forbes and myself, and doubtless by others, becomes of little value. The colour of the pollen grains is of no importance in the inquiry, as observation shows; the discriminatory fact, if it has any importance, is not as to variations in colour, but the collection of the pollen from distinct species of plants the admixture of which *would not induce fertilisation*. If it be proved that the admixture of the collected pollens are only such as induce fertilisation, then a natural phenomenon is disclosed of great importance. This is the fact I imagine Sir John Lubbock meant when he advised the pursuit of the inquiry.

I am still of opinion that it is *odour*, not colour, which is the attractive element. It is so with carrion birds and the blow-flies which collect on the fœtid arum. In phenomena one particular law appears to be repeated in all the natural kingdoms. The same rule is also to be observed in physics. S. B.

Sunbury-on-Thames

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF A.D. 418, JULY 19.—Philostorgius, in his "Epitome of Ecclesiastical History," relates that while Theodosius the Second was a youth, on July 19, at the eighth hour of the day, the sun was so greatly eclipsed that the stars were seen, and while the sun was thus hidden there was seen in the sky a light in the form of a cone, "which some ignorant people called a comet"; and he goes on to describe the supposed differences in the appearance of the phenomenon from that of a comet, particularly remarking that it resembled the flame of a torch, subsisting of itself, without any star to serve as a base, and adding particulars of its track and duration. That the object thus singularly discovered during a total, or nearly total, eclipse of the sun, was really a comet as the "ignorant people" supposed, is proved by the records in the Chinese Annals. The eclipse to which reference is made by Philostorgius took place on July 19, A.D. 418. The comet of that year is stated in Pingré's *Cometographie* to have been discovered in the 10th moon, commencing November 15, in which he follows the Jesuit, Couplet, but the account given by Mr. Williams, on the authority of the She Ke and Ma Twan Lin, dates the appearance of the comet on day Kang Tsze of the 5th moon, when it was situate in Ursa Major; on September 15 it was on the confines of Leo and Virgo; "it was bright, and gradually lengthened until it was 100 cubits in length." Philostorgius also refers to the passage of the comet through Ursa Major, and says it continued visible until the end of the autumn.

It may interest some readers to have particulars of the eclipse, during which it is recorded that a large comet was first discovered. The following figures depend upon a very similar system of calculation to that applied to other ancient eclipses, described in this column:—

G.M.T. of Conjunction in R.A. 418, July 18, at 23h. 3m. 17s.

Right Ascension	118° 30′ 48″
Moon's hourly motion in R.A.	38 34
Sun's ,, ,, ,, ...	2 32
Moon's declination	21 23 10 N.
Sun's ,, ...	21 2 41 N.
Moon's hourly motion in decl.	3 40 S.
Sun's ,, ,, ,, ...	0 28 S.
Moon's horizontal parallax	59 38
Sun's ,, ,, ...	0 9
Moon's true semi-diameter	16 15
Sun's ,, ,, ...	15 48

At Constantinople these elements give a very large eclipse, commencing at 0h. 5m. and ending at 2h. 50m. local mean time, magnitude, 0·95; at a short distance to the south the eclipse would be total.

VARIABLE STARS.—The following are geocentric minima of Algol and S. Cancri during the ensuing two months, so far as they are visible in this country. They are expressed in Greenwich mean time, and are calculated from Prof. Schönfeld's elements:—

ALGOL

	h. m.		h. m.
Dec. 31	12 29	Jan. 28	4 41
Jan. 2	9 18	Feb. 5	19 8
,, 5	6 7	,, 8	15 58
,, 16	17 23	,, 11	12 47
,, 19	14 13	,, 14	9 36
,, 22	11 2	,, 17	6 26
,, 25	7 52	,, 28	17 43

S. CANCRI

	h. m.		h. m.
Dec. 31	10 20	Feb. 7	8 51
Jan. 19	9 35	,, 26	8 7

ASTRONOMICAL PHENOMENA IN 1878.—The principal astronomical occurrence of the next year is the total solar eclipse of July 29, which traverses British Columbia and the United States; the American astronomers will doubtless give a good account of it, and it is reported they are likely to have coadjutors from this side of the Atlantic. There will be a transit of the planet Mercury on May 6, visible in this country to past the time of the nearest approach of centres, and a lunar eclipse on August 12, magnitude 0·6, wholly visible here. Mars will be occulted by the moon on the evening of June 3, and the second-magnitude star σ Sagittarii on the afternoon of October 30; on November 10 the moon traverses the Pleiades. A return of Encke's comet to perihelion also takes place in the summer, but not under favourable circumstances for observation, and the comet of short period detected by Tempel on July 3, 1873, will again arrive at perihelion late in the spring. Saturn's rings disappear on February 6, but reappear on March 1, according to Bessel's elements.

FERTILISATION OF GLOSSOSTIGMA

THE following letter to Mr. Darwin has been forwarded to us by him for publication:—

"Museum, Auckland, October 23, 1877

"My Dear Sir,—I forward to you a copy of a paper on the fertilisation of *Selliera*, one of the *Gooaeniaceæ*, which perhaps you may care to glance over. When I wrote it I did not know of your notes on *Leschenaultia*, published in the *Gardener's Chronicle* for 1871. In both plants the pollen is shed before the expansion of the flower, and neatly collected in the indusium, but in *Selliera* the stigma is situated within the indusium, and by its gradual upward growth after the flower expands slowly forces out the pollen, which is then transferred by insects to older flowers. When mature, the stigma protrudes considerably beyond the indusium. This appears to differ entirely from what takes place in *Leschenaultia*.

"I have recently been much interested with the curious irritability displayed by the stigma of *Glossostigma elatinoides*, one of the Scrophularineæ. The style is dilated towards its apex into a broad spoon-shaped stigma, which, when the flower expands, is closely doubled over the four stamens, entirely concealing them from view. If the front of the bent part of the style is touched it at once springs up, uncovering the stamens, and moves back to the upper lobe of the corolla, to which it becomes closely applied. In this position it remains for a few minutes, and then slowly moves back to the stamens and curves over them as at first. It appears to me that this irritability of the stigma is simply a contrivance to insure cross-fertilisation, for an insect crawling into the flower must inevitably touch the stigma, which would then uncover the stamens. On withdrawing, the insect would be certain to dust itself with pollen, but it would not by this effect the fertilisation of the flower, for the stigma would be then closely applied to the upper lobe of the corolla, entirely out of its way. If the insect were, however, to visit another flower it is evident that it must come into contact with the stigma at its first entrance and would doubtless leave some pollen thereon. The movement of the stigma is remarkably rapid, and its apex must pass through an angle of at least 180°. I have been unable to find a record of a similar case, or of so pronounced a degree of irritability in the stigma of any plant. The movement of the lobes of the stigma in *Mimulus* is much weaker, and is through a much less angle. Yours faithfully,

"T. F. CHEESEMAN

"Charles Darwin Esq., F.R.S."

A TELEPHONIC ALARUM

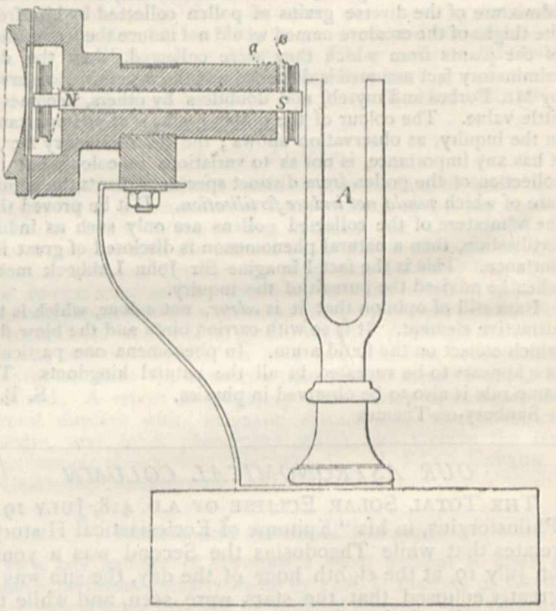
THE speaking of the telephone is admittedly so weak that it can only be caught by keeping the instrument in immediate contact with the ear. Hence there is transmitted through the telephone in its present form no sound which would be intense enough to announce to any one who was in a large room and who did not hold the telephone close to his ear, that a message was about to be sent from the transmitting station. The consequence is that a warning apparatus must be attached to the telephone, so that there may be no fear of missing a projected telephonic conversation.

It is clear that the conducting wire of a telephone can be used to sound a bell as an alarum by means of a current from a galvanic battery, and thereby the defect referred to would be supplied. But the necessary apparatus would considerably raise the price of fitting up a telephone apparatus, and besides, one most important property of the telephone, viz., producing the required electric current automatically, would be partly lost. I have, then, invented another warning apparatus, which, I believe, is quite workable.

Hitherto telephones have been so constructed that only one pole (N in the figure) of the magnet is effective; I now use also the second pole S, by providing it with a coil of wire, which is simply inserted in the circuit behind the first coil. (The dotted lines in the figure will explain this connection; the two ends *a* and *β* are connected with the binding screws fastened to the telephone; from this the circuit goes to the second telephone.) Before this pole of the magnet may be very easily set up a tuning-fork, A, which, with the telephone, is simply fixed on a resonance case, B; this arrangement should be made both at the transmitting and receiving stations, and both forks should be in unison. If now the sending station wish to signal that a conversation is to be begun, the fork of that place will be sounded with a fiddle-bow; the currents thereby induced in the coil are powerful enough to set the fork of the receiving station in such intense vibration that the sound may be distinctly heard in a large room; warned by this signal a person can in the usual way put the telephone to his ear and listen to the words from the transmitting station. And so *vice versâ*.

I have made an experiment in a large room, when about 100 people were present, and all could hear the sounds of the fork, which in the manner described was set in vibration by a second fork in a distant room. The two forks were König Ut_4; lower forks give less clearly heard tones; with higher forks I was unable to make any experiment, since I had not two similar ones at my disposal.

Let me mention two other experiments which I have made. The first is of importance in connection with the question as to how the clang-tints of tones are reproduced through the telephone. In one of the two telephones described substitute for the Ut_4 fork a higher one, and sound this by means of a fiddle-bow, and there will be heard with another inserted telephone of the ordinary construction tones of even 12,000 double vibrations per second, a sign that the variations of the magnetic condition of a magnet perceptibly occur, even when the forces producing these variations change their size 24,000 times in



a second. This result moreover was not to be expected, since, as is known, magnetic polarisation requires time to accomplish. Whether these higher tones are comparatively weaker than the deeper cannot be determined, but probably this is the case.

In another experiment I used the telephone to test the electric vibrations indicated by Helmholtz and others, which are produced by the opening of the primary current of an induction apparatus in the induced coil, when the ends of the latter are connected with the armatures of a condenser. For this purpose I inserted the telephone in the circuit between coil and condenser, and observed the effect when the current in the inducing spiral was opened.

When the ends of the induced spiral were not connected with the condenser, I heard a dull report in the telephone; when, again, these ends were connected with the condenser, this report was accompanied by a shorter, higher sound, whose vibration-number might perhaps be determined by a musical ear; a proof of the existence of the vibrations mentioned in the last case. The observations were made with a telephone, the iron membrane of which was very thin and had a very deep tone.

W. C. RÖNTGEN

THE NEW PARIS TRANSIT CIRCLE[1]

OF the numerous instruments with which Leverrier enriched the Paris Observatory during the twenty years of his direction, the last which he was able to see completely installed was the new transit circle. This instrument was not, like all the others, constructed at the expense of the State; an inscription on the marble pillars that support it informs the visitor that it was presented to the Observatory by the generous munificence of M. Raphaël Bischoffsheim. This is not the only gift of M. Bischoffsheim to astronomy; the Observatory of Lyons is also indebted to him for its fundamental instrument.

The project of erecting a new meridian circle at the observatory goes back to the time of the debate raised before the Academy of Sciences on the subject of the transfer of the observatory to a site outside Paris. Those who would not admit the legitimacy of the complaints made by the adversaries of the present situation of the observatory, were obliged to admit that the great meridian room, constructed in 1830 by Arago, did not offer any of the guarantees necessary to observations of great precision. The thickness of the walls and of the double roof of that room, the small breadth of the openings, the nearness of the observatory buildings, the difference of level between the two faces north and south, must necessarily affect the equilibrium of the neighbouring layers of the atmosphere and hinder them from taking that horizontality which admits of the correction of the observations from the influence of refraction.

Since the astronomer cannot get rid of this troublesome influence, his first business ought then to be to reduce it to conditions in which it may be possible to calculate the effect. Thus what strikes the visitor admitted to the new meridian circle of the Observatory is the small building in which it is placed. In the middle of a green lawn rises a hut made entirely of sheet-iron, the roof formed of two plates which, by sliding upon rollers, may be separated from each other, and leave all the upper part of the building open. The walls are formed of two envelopes of thin iron, between which the air freely circulates, thus maintaining the whole structure at the temperature of the air itself. Large windows may also be opened, and the observer and the instrument be thus placed in the same conditions as if the observations were made in the open air. All these conditions are to avoid as far as possible the disturbances arising from atmospheric refraction, the greatest source of inaccuracy in astronomical observation. The only obstacle which may yet be a hindrance to perfection in the conditions of observation is the presence of those beautiful trees which make the terrace of the observatory a magnificent garden, but which store up the warm air during the day and slowly distribute it during the night. No doubt some day the astronomers will be obliged to sacrifice to the precision of their observations the enjoyment of this beautiful foliage.

The meridian circle is composed, as its name indicates, of two instruments: the meridian telescope, intended, by its association with an astronomical clock, to fix the moment of the passage of a star across the meridian of the place of observation, and the mural circle, which gives the measure of the angular distance of this same star from the pole or the zenith. When, forty years ago, Gambey constructed the two meridian instruments of the Paris Observatory, so justly celebrated and on the model of which those of most other observatories have been designed, he had to reconcile, by prodigies of skill, the lightness resulting from the means of construction then in use, with the rigidity of the parts necessary for precision of observation. It is the alliance of these two almost contradictory qualities which renders so interesting the instruments of that celebrated artist and especially his machine for dividing the circles, which the Baron Séguier has restored in the galleries of the Conservatoire. But there resulted from this at first the necessity of separating the measure of the two co-ordinates of the stars—the instant of the meridian passage and the polar distance. There also resulted the necessity which Gambey was under to fix on his mural circle of two metres in diameter, a telescope altogether insufficient in optical power.

A simple glance at the great meridian circle of the observatory, the western equatorial, the great telescope, the new instrument of M. Bischoffsheim, all from the workshop of the great mechanician, M. Eichens, shows the revolution which has been effected in the processes of construction. In place of instruments formed of pieces of sheet brass connected by simple screws or even soldered together, we have the bodies of the telescope of cast-iron bolted on axes of cast-iron and steel, strong and elegant in appearance; circles of bronze cast in a single piece and protected against all deformation by numerous cross-bars. It is the art of the engineer applied to the construction of astronomical instruments, with the power given by the choice of metals and the thickness of pieces, and the precision which the employment of engineerlng tools secures.

This revolution was begun in England about 1847 by the Astronomer-Royal, Sir George Airy. In 1863, M. Leverrier successfully installed a meridian circle greater still than that of Greenwich, and intended, like it, for the observation of the small planets. But these gigantic instruments, veritable siege-guns of long range, since they reach the farthest depths of the heavens, want, simply on account of their weight, one essential quality—they are not reversible. Whatever be the rigidity of the pieces, the instrument is subject, in each successive position, to flexions necessarily unequal, which the astronomer must investigate and measure in order to correct his observations. But this investigation and this measurement can only be made by turning round the instrument. It will be understood, in fact, that the apparatus, directed successively to the same point of the sky, first with one of its faces up, then the same face below, gives, if it is really perfectly rigid but elastic, two results differing equally from the truth, one *minus* and the other *plus*, so that the mean of the two observations gives the exact position of the star. It is this which may be expected from the new meridian circle of M. Bischoffsheim. Fig. 1 represents the telescope upon its car, which serves to raise it above its pillars and to turn it right round by a movement of rotation around a vertical axis.

Since 1852 M. Brunner has constructed small portable instruments answering to these conditions. Improved by his sons, by M. Rigaud, and by M. Eichens, these meridian circles are now only used in geodesic expeditions. In 1868 M. Eichens constructed for the observatory of Lima a reversible meridian circle, the telescope of which was 2·30 m. in length, and the object-glass 20 cm. in free opening. It is this model, successively improved, which has become, in the hands of the able constructor, the meridian circle of Marseilles (1876), and the circle given by M. Bischoffsheim (1877). The object-glass of the first was made by Léon Foucault, the two others are by M. Ad. Martin. The new observatory of Lyons, in the establishment of which M. André took an active part energetically sustained by the Administration, will soon possess a similar meridian circle, a little smaller (telescope of 2 m., object-glass of 14 cm. aperture, by M. Praczmowzki), the expense of which is borne by M. Bischoffsheim.

The illustrations which we give then show the perfected model meridian circle employed in observatories for the determination of the celestial co-ordinates of the stars. To be able to understand the use of the various parts of the instrument, it will suffice to describe a complete observation of a star.

Some minutes before the passage of the star across the meridian, the astronomer gives to the telescope such

[1] From an article in *La Nature* by M. C. Wolf.

an inclination that the star, carried on by the daily movement, will cross the field of the instrument. For this purpose the interior circles fixed on the axis of the telescope carry a rough scale which may be seen by means of a pointer telescope fixed on the east wall. A clamp which clasps the edge of this circle serves to fix the instrument. The observer then places himself on the observing chair in the position indicated on Fig. 2. The star soon appears, enters the field of view on the west and proceeds towards the east side. With the star the observer sees in the field of view a network of spider threads stretched vertically and traversed by a horizontal thread. Listening to the beats of the clock, he notes the second and the fraction of a second at which the star passes under each of the vertical threads; the mean of these times is the precise moment of the passage across the middle thread. At this same moment he slightly displaces the telescope by a

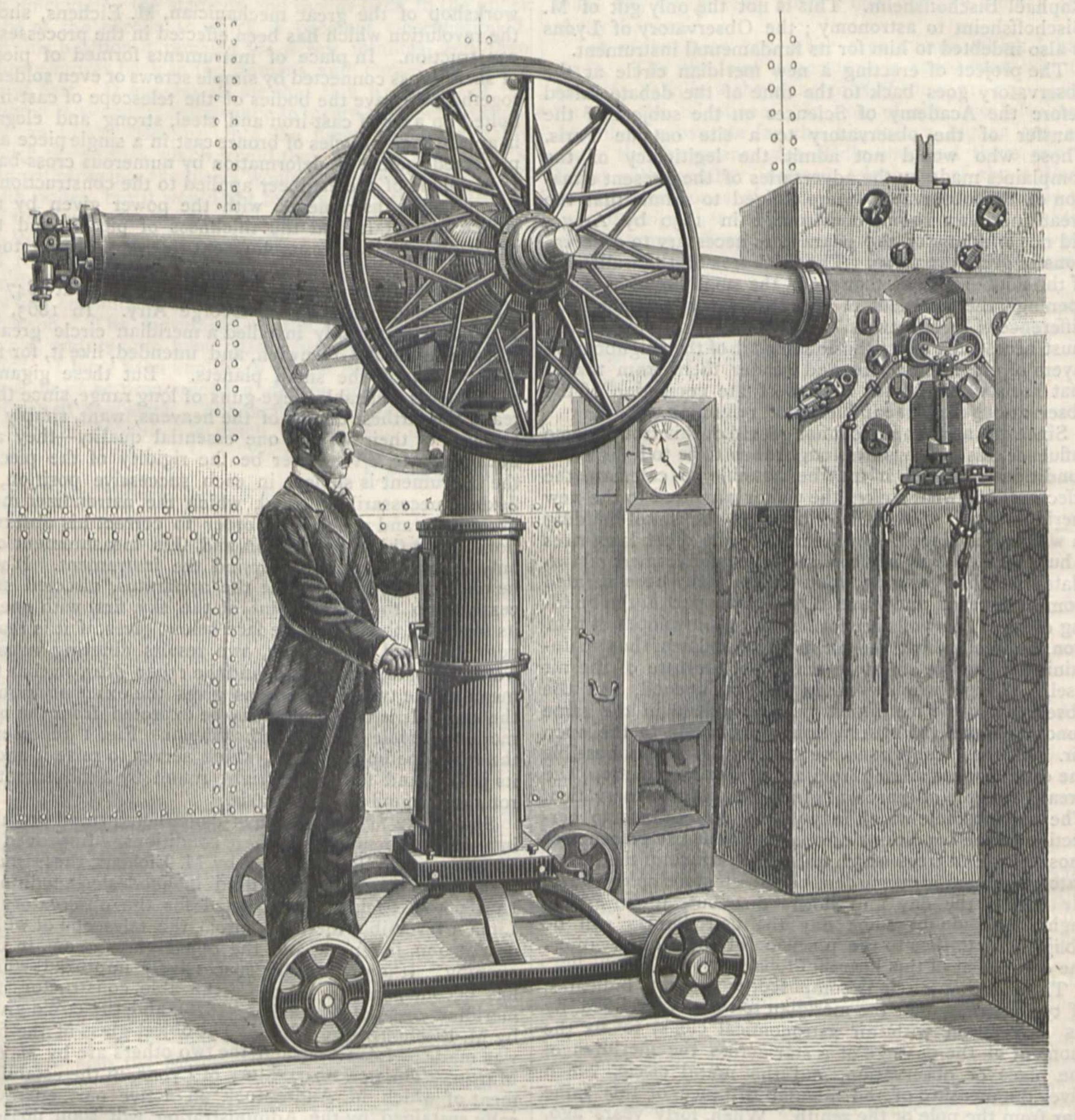


FIG. 1.—Reversing Apparatus.

movement given to the clamp and brings the star under the horizontal thread. The direction of a line determined by the crossing of this thread and the middle vertical one and the optical centre of the object-glass is that along which the star is seen at the moment of its passage across the middle thread. To fix this direction it is necessary to connect it with two points of an absolute fixity. For this purpose the telescope is provided with a circle of a metre in diameter, the limb of which is very finely and very exactly divided; this turns with the telescope in front of six microscopes permanently fixed to the east pillar. M. Eichens has adopted for these microscopes the arrangement devised by Sir George Airy for the meridian circle of Greenwich. The tube of each of these is formed by the side of a hole pierced in the block of marble which forms the upper part of the pillar; the positions of these microscopes is then permanently fixed to that of the wall,

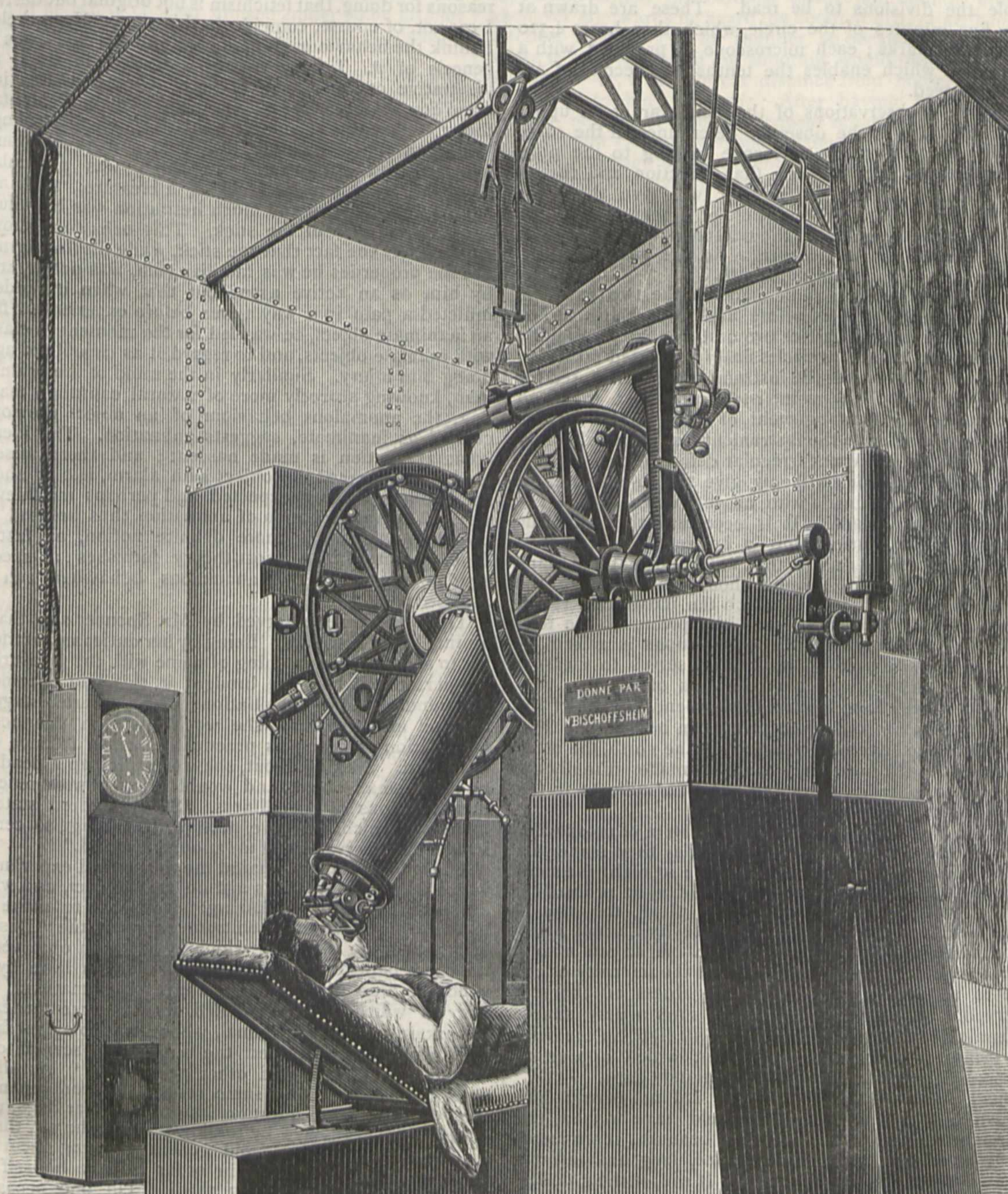


FIG. 2.—Taking an Observation.

and can only change by a displacement of the wall itself. Other orifices admit to the circle the light of a lamp and enable the divisions to be read. These are drawn at every five minutes of the circle, which then bears 4,320 equidistant marks; each microscope is provided with a micrometer which enables the tenths of a second of arc to be observed.

If now, by observations of the pole star at its upper and lower transits, the observer determines in the same way the direction of the telescope looking to the pole, the angle comprised between that direction and that of the telescope directed to the star will give the polar distance of the star. If by means of a mercury bath he determines the direction of the telescope when its optical axis is vertical, he will ascertain in the same way the distance of the star from the zenith.

These observations may be made in the two positions which the telescope takes before and after being turned round. This is why it carries two cast-iron circles roughly graduated and two brass circles finely graduated on silver, which on the reversal of the instrument are substituted for each other before the pointer-telescope and the fixed microscopes. The arrangement of these circles insures a perfect symmetry to the instrument, an essential condition if we wish to prevent irregular deformations.

But these operations will only give the co-ordinates of the star if they are made with an instrument set in the meridian of the place. It is necessary then that the telescope should turn round a horizontal axis, that it should be perpendicular to that axis, and that the plane which it describes in turning should pass through the pole of the earth. A level, which the illustration represents resting by two forks upon the pivots of the telescope, but which during the observations is raised by means of a crane fixed to the ceiling, serves to measure and correct the inclination of the axis of rotation. By turning it upon a long support the perpendicularity of the optical axis on the axis of the pivots can be assured. Two supports are to be constructed, one on the north, the other on the south; the latter only has been made. Finally the astronomical observation of the pole star indicate if the last of the three conditions is fulfilled.

A word on the illumination of the system of cross wires visible in the eye-piece. During the day they stand out on the clear background of the sky; at night the same effect is obtained by means of a ray of light proceeding from a gas-lamp fixed on the west pillar, the rays of which are sent towards the eye-piece by a small prism fixed in the middle of the telescope. A screen with a variable opening, or cat's-eye, permits the intensity of the light to be proportioned to the brightness of the star observed. Finally, for very weak stars a very simple mechanical arrangement suppresses all light in the field, and brings it to bear on the wires, which appear as luminous lines on a background absolutely dark.

The long illness of M. Leverrier did not permit him to push on, so actively as he would have wished, the preliminary investigations of this beautiful instrument, among which we must mention one, long and difficult —the divisions of the two circles. It will, without doubt, be facilitated by this circumstance, that, traced by means of the dividing machine constructed by M. Eichens, the lines present a regularity and a finish altogether favourable to precision.

The astronomers of the observatory will hold it a point of honour to take advantage as soon as possible of the magnificent apparatus which they owe to the generosity of M. Bischoffsheim.

FETICHISM IN ANIMALS

MR. HERBERT SPENCER, in his recently published work on the "Principles of Sociology," treats of the above subject. He says: "I believe M. Comte expressed the opinion that fetichistic conceptions are formed by the higher animals. Holding, as I have given reasons for doing, that fetichism is not original but derived, I cannot, of course, coincide in this view. Nevertheless, I think the behaviour of intelligent animals elucidates the genesis of it. I have myself witnessed, in dogs, two illustrative cases." One of these cases consisted in a large dog, which, while playing with a stick, accidentally thrust one end of it against his palate, when, "giving a yelp, he dropped the stick, rushed to a distance from it, and betrayed a consternation which was particularly laughable in so ferocious-looking a creature. Only after cautious approaches and much hesitation was he induced again to lay hold of the stick. This behaviour showed very clearly the fact that the stick, while displaying none but the properties he was familiar with, was not regarded by him as an active agent, but that when it suddenly inflicted a pain in a way never before experienced from an inanimate object, he was led for the moment to class it with animate objects, and to regard it as capable of again doing him injury. Similarly in the mind of the primitive man, knowing scarcely more of natural causation than a dog, the anomalous behaviour of an object previously classed as inanimate, suggests animation. The idea of voluntary action is made nascent; and there arises a tendency to regard the object with alarm, lest it should act in some other unexpected and perhaps mischievous way. The vague notion of animation thus aroused will obviously become a more definite notion, as fast as development of the ghost-theory furnishes a specific agency to which the anomalous behaviour can be ascribed."

The other case observed by Mr. Spencer was that of an intelligent retriever. Being by her duties as a retriever led to associate the fetching of game with the pleasure of the person to whom she brought it, this had become in her mind an act of propitiation; and so, "after wagging her tail and grinning, she would perform this act of propitiation as nearly as practicable in the absence of a dead bird. Seeking about, she would pick up a dead leaf or other small object, and would bring it with renewed manifestations of friendliness. Some kindred state of mind it is which, I believe, prompts the savage to certain fetichistic observances of an anomalous kind."

These observations remind me of several experiments which I made some years ago on this subject, and which are perhaps worth publishing. I was led to make the experiments by reading the instance given in the "Descent of Man," of the large dog which Mr. Darwin observed to bark at a parasol as it was moved along a lawn by the wind—so presenting the appearance of animation. The dog on which I experimented was a Skye terrier—a remarkably intelligent animal, whose psychological faculties have already formed the subject of several communications to this and other periodicals.[1] As all my experiments yielded the same results I will only mention one. The terrier in question, like many other dogs, used to play with dry bones by tossing them in the air, throwing them to a distance, and generally giving them the appearance of animation, in order to give himself the ideal pleasure of worrying them. On one occasion, therefore, I tied a long and fine thread to a dry bone and gave him the latter to play with. After he had tossed it about for a short time I took an opportunity when it had fallen at a distance from him and while he was following it up, of gently drawing it away from him by means of the long and invisible thread. Instantly his whole demeanour changed. The bone which he had previously pretended to be alive now began to look as if it really were alive, and his astonishment knew no bounds. He first approached it with nervous caution, as Mr. Spencer describes, but as the slow receding motion continued, and

[1] See especially an article on "Conscience in Animals," in *Quarterly Journal of Science* for April, 1876.

he became quite certain that the movement could not be accounted for by any residuum of the force which he had himself communicated, his astonishment developed into dread, and he ran to conceal himself under some articles of furniture, there to behold at a distance the "uncanny" spectacle of a dry bone coming to life.

Now in this, and in all my other experiments, I have no doubt that the behaviour of the terrier arose from his *sense of the mysterious*, for he was of a highly pugnacious disposition, and never hesitated to fight an animal of any size or ferocity; but apparent symptoms of spontaneity in an inanimate object which he knew so well, gave rise to feelings of awe and horror which quite enervated him. And that there was nothing *fetichistic* in these feelings may be safely concluded if we reflect, with Mr. Spencer, that the dog's knowledge of causation, for all immediate purposes, being quite as correct and no less stereotyped than is that of "primitive man," when an object of a class which he knew from uniform past experience to be inanimate suddenly began to move, he must have felt the same oppressive and alarming sense of the mysterious which uncultured persons feel under similar circumstances. But further, in the case of this terrier we are not left with *à priori* inferences alone to settle this point, for another experiment proved that the sense of the mysterious was in this animal sufficiently strong of itself to account for his behaviour. Taking him into a carpeted room I blew a soap-bubble, and by means of a fitful draught made it intermittently glide along the floor. He became at once intensely interested, but seemed unable to decide whether or not the filmy object was alive. At first he was very cautious and followed it only at a distance, but as I encouraged him to examine the bubble more closely, he approached it with ears erect and tail down, evidently with much misgiving; and the moment it happened to move he again retreated. After a time, however, during which I always kept at least one bubble on the carpet, he began to gain more courage, and the scientific spirit overcoming his sense of the mysterious, he eventually became bold enough slowly to approach one of the bubbles and nervously to touch it with his paw. The bubble, of course, immediately vanished; and I certainly never saw astonishment more strongly depicted. On then blowing another bubble, I could not persuade him to approach it for a good while; but at last he came and carefully extended his paw as before with the same result. But after this second trial nothing would induce him again to approach a bubble, and on pressing him he ran out of the room, which no coaxing would persuade him to re-enter.

One other example will suffice to show how strongly developed was the sense of the mysterious in this animal. When alone with him in a room I once purposely tried the effect on him of making a series of horrible grimaces. At first he thought I was only making fun; but as I persistently disregarded his caresses and whining while I continued unnaturally to distort my features, he became alarmed and slunk away under some furniture, shivering like a frightened child. He remained in this condition till some other member of the family happened to enter the room, when he emerged from his hiding-place in great joy at seeing me again in my right mind. In this experiment, of course, I refrained from making any sounds or gesticulations, lest he might think I was angry. His actions, therefore, can only be explained by his horrified surprise at my apparently irrational behaviour—*i.e.*, by the violation of his ideas of uniformity in matters psychological. It must be added, however, that I have tried the same experiment on less intelligent and less sensitive terriers with no other effect than causing them to bark at me.

I will only add that I believe the sense of the mysterious to be the cause of the dread which many animals show of *thunder*. I am led to think this, because I once had a setter which never heard thunder till he was eighteen months old, and on then first hearing it I thought he was about to die of fright, as I have seen other animals do under various circumstances. And so strong was the impression which his extreme terror left behind, that whenever afterwards he heard the boom of distant artillery practice, mistaking it for thunder, he became a pitiable object to look at, and, if out shooting, would immediately bolt home—or, if at a great distance from home, would endeavour to bury himself. After having heard real thunder on two or three subsequent occasions, his dread of the distant cannons became greater than ever; so that eventually, though he keenly enjoyed sport, nothing would induce him to leave his kennel, lest the practice might begin when he was at a distance from home. But the keeper, who had a large experience in the training of dogs, assured me that if I allowed this one to be taken to the battery, in order that he might learn the true cause of the thunder-like noise, he would again become serviceable in the field. The animal, however, died before the experiment was made. GEORGE J. ROMANES

RUHMKORFF

WE regret to record the sudden death on December 20, at Paris, of Henry Daniel Ruhmkorff, whose name is so closely connected with the history of magneto-electricity. He was born in Hanover, Germany, in 1803, and but little is known of his early life. In 1819 he wandered to Paris, and obtained a position as porter in the laboratory of Prof. Charles Chevalier, at that time one of the leading French physicists. Here he displayed a remarkable fondness for electrical apparatus, as well as ingenuity in its arrangement, and was enabled shortly after to start a modest manufactory of physical apparatus. Through the efforts of Chevalier and the excellence of the work performed, the business was rapidly extended. In 1844 Ruhmkorff brought out his first invention, a convenient thermo-electric battery. Soon after he turned his attention to magneto-electricity, especially the production of the induced currents, discovered by Faraday in 1832. A long series of experiments resulted in the appearance, in 1851, of the famous "Ruhmkorff coil," with its later modifications, the most important piece of apparatus in this branch of physics. With this powerful adjunct the electrician was enabled to obtain sparks 18 inches in length, pierce thick plates of glass, and carry out a vast variety of experiments. The invention was rewarded by a decoration and medal at the Exhibition of 1855, while in 1858 it received the first prize of 50,000 francs at the French Exhibition of Electrical Apparatus. Since then the manufacture of the coils and of electrical machines in general has assumed enormous dimensions, and the leading physicists of Europe are well acquainted with the dingy little bureau in the Rue Champollion, near the University. Personally M. Ruhmkorff was of a quiet, dignified appearance, and despite the disadvantages of his early life, he enjoyed the friendship of the leading Parisian *savants*, and was an honoured member of the French Physical Society. M. Jamin delivered an address over the grave, in which he stated that Ruhmkorff died almost a poor man, because he had spent all his earnings on behalf of science and in works of benevolence.

LIQUEFACTION OF OXYGEN

THE number of the permanent gases is rapidly diminishing. We have had occasion recently to refer to M. Cailletet's successful attempts to compress nitric oxide, N_2O_2, methyl hydride, CH_4, and acetylene, C_2H_2, to the liquid form. The list of non-compressible gases was thus reduced to three, viz., hydrogen, nitrogen, and oxygen. Within the past week M. Raoul Pictet has succeeded in obtaining the last-mentioned gas in the liquid state, an event which is certainly one of the most novel and interesting in the chemical progress of the expiring year.

The *Journal de Genève* of December 23] gives the following account of the experiments :—

One of the most interesting physical experiments of our time has just been made at Geneva with rare success in the laboratory of the Society for the Manufacture of Physical Instruments. M. Raoul Pictet has succeeded in obtaining, by means of ingeniously combined apparatus, the liquefaction of oxygen gas. The following is the process by which the curious result was obtained :—

By a double circulation of sulphurous acid and carbonic acid, the latter gas is liquefied at a temperature of 65° of cold, under a pressure of from four to six atmospheres. The liquefied carbonic acid is conducted into a tube four metres long; two combined pumps produce a barometric vacuum over the acid which is solidified in consequence of the difference of pressure. Into the interior of this first tube containing solidified carbonic acid is passed a tube of a slightly less diameter, in which circulates a current of oxygen produced in a generator containing chlorate of potash and the form of which is that of a large shell thick enough to prevent all danger of explosion. The pressure may thus be carried to 800 atmospheres.

Yesterday morning (December 22), all the apparatus being arranged as described, and under a pressure which did not exceed 300 atmospheres, a liquid jet of oxygen issued from the extremity of the tube, at the moment when this compressed and refrigerated gas passed from that high pressure to the pressure of the atmosphere.

The great scientific interest of this experiment is that it demonstrates experimentally the truth of the mechanical theory of heat, by establishing that all gases are vapours capable of passing through the three states—solid, liquid, and gaseous. Only twenty days ago M. Cailletet, as we have said, succeeded in liquefying the bioxide of nitrogen, under a pressure of 146 atmospheres and at a temperature of 11° of cold. After the experiment of M. Raoul Pictet there remain not more than two elemental gases which have hitherto escaped the attempt at liquefaction—hydrogen and nitrogen.

The experiment above described was to be repeated on Monday and subsequent days, with some slight changes in the processes and the arrangement of the apparatus.

NOTES

SOME interesting experiments with the telephone have been made by Mr. W. H. Preece between Dublin and Holyhead through the submarine cable. Conversation was freely maintained and songs were sung on each side and heard and appreciated on the other. The articulation was excellent, but muffled, as though the speakers spoke through respirators. This is what might have been expected from the static induction of the cable. It is the longest actual cable yet spoken through, its length being sixty-seven miles.

AT their last sitting the enlarged Council of the Paris Observatory were occupied in considering the question of the position of French meteorology. M. Dumesnil, the representative of the minister, was obliged to silence some members of the minority who were assailing the character of some of the physicists having the control of the Observatory and the transmission of the warnings to the sea-ports. A large majority rendering justice to the ingenuity displayed and to the highly scientific nature of the warnings, passed a vote recommending the administration not to alter the present condition of things at the Observatory.

DR. CARLO GHINOZZI, Professor of Medical Clinic at the Istituto Superiore of Florence, for many years colleague and afterwards successor of Prof. Bufalini, died on Saturday, the 15th instant, at the age of 66 years.

IN Bonn a committee has been formed consisting of leading citizens and Professors Bauerband, Kekulé, and Proschel, of the University, for the purpose of erecting a monument to the late Prof. Jacob Noeggerath, whose death last September we briefly alluded to at the time. Prof. Noeggerath was born in Bonn October 10, 1788, and since the foundation of the university in 1818 had been connected with it as Professor of Mineralogy. As a successful teacher of the natural sciences he acquired an unusually widespread fame, and the majority of the present Prussian mining officials pursued their studies under his direction. His general scientific researches touch on a number of interesting geological questions, such as the formation of basalt, &c.; but his chief efforts were directed to an exhaustive study of the mineralogy and geology of Rhenish Westphalia, the results of which are to be seen in the magnificent mineralogical collection at Bonn, and the rapid development of the mining interests in this district. As a favourite writer of popular works on scientific subjects, he contributed in no small degree to the general taste for this class of literature now prevalent in Germany.

THE expedition sent out by the Dutch Geographical Society for the exploration of Sumatra has met with a severe check by the sudden death of its leader, M. Schouw Landvort. His extensive knowledge, indomitable perseverance, and great powers of endurance, fitted him eminently for the position, these qualitie being notably evidenced by the bold journey across the middle of the island, through hitherto unknown regions, in the company of natives only, which we had occasion lately to chronicle.

AT the meeting of the Council of the Zoological Society on Wednesday last week, the president, the Marquis of Tweeddale, proposed that the silver medal of the Society should be awarded to Mr. Robert Hudson, F.R.S., in acknowledgment of the valuable services he had rendered to the Society for the fifty years that he had been a Fellow thereof. The motion was carried unanimously at the full meeting of the Council.

THE organisation of public instruction in France is undergoing an exceedingly beneficial change. A decree, published in the *Journal Officiel* of December 17, establishes a representative Council of Public Instruction under the title of "Comité Consultatif." The committee is divided into three different sections corresponding to the three divisions of public instruction in France, primary, secondary (grammar schools), and superior (universities). Each section is to appoint its president and secretary. The three sections in general session are to be presided over by the minister. Some of the members are appointed by the minister to serve during a period of five years, others are members *ex officio*. The minister cannot elect any who are not members of the teaching body or of the Institut. The directors of the administration of primary, secondary, or superior instruction are *ex officio* members of their respective sections. They meet yearly at a certain fixed period. The opinion of the committee is not binding, but it must be taken on a number of matters, such as bills which are to be presented to Parliament, modification of programmes, &c. Another decree appoints the members of the three committees. Among these are many names well-known to science, as MM. Laboulaye, Würtz, Claude Bernard, Vulpian, Gavarret, Chevreul, Faye, Berthelot, Milne-Edwards, Puiseux, and Desains.

THE following are the probable arrangements for the Friday Evening Meetings at the Royal Institution, before Easter, 1878:—January 25, Prof. Huxley, F.R.S., "William Harvey;" February 1, Wm. Henry Preece, C.E., "The Telephone;" February 8, Matthew Arnold, "Equality;" February 15, P. L. Sclater, F.R.S., "Zoological Distribution and some of its Difficulties;" February 22, Prof. Roscoe, F.R.S.; March 1, Richard Liebreich, M.D., "The Deterioration of Oil Paintings;" March 8, Prof. Goldwin Smith, "The Influence of

Geographical Circumstances on Political Character;" March 15, Lord Rayleigh, F.R.S.; March 22, Prof. Tyndall, F.R.S.; March 29, Prof. Dewar, F.R.S.; April 5, Sir John Lubbock, Bart., M.P., F.R.S.; April 12, Sir Joseph Dalton Hooker, C.B., Pres. R.S., "The Distribution of Plants in North America."

PROF. BARFF begins his juvenile lectures at the Society of Arts next Wednesday. His subject is "Coal and its Components."

VOLCANIC eruptions are threatening Iceland again. The last number of the *Skuld*, published in Eskifjödur, states that on the evening previous an unprecedented heat was suddenly felt, so strong that the inhabitants thought themselves in the vicinity of a vast conflagration. The phenomenon was followed by alternate gusts of rain and showers of volcanic ashes accompanied by subterranean rumblings.

THE German Government has lately named a new steamer after the well-known meteorologist, Prof. Dove, of Berlin, in recognition of the advantages accruing to navigation from his many observations and discoveries.

THE Italian Geographical Society has received news from Signori Martini and Cecchi, who have penetrated into Shoa. There is no intelligence of the Marquess Antinori and the engineer Chiarini, whose fate causes grave anxiety.

THE Geographical Society of Paris held a banquet last Saturday to commemorate the fifty-seventh anniversary of its foundation. Among the toasts which were given we must notice that of Mr. Gordon Bennett, the enterprising director of the *New York Herald*, who originated Stanley's fruitful mission, and the King of the Belgians, by MM. Levasseur and de Lesseps.

NEW halls of exhibition for antiquities have been opened in the Louvre. An interesting anthropological exhibition will be opened on January 15 at the Palais de l'Industrie. It will be confined to the discoveries made in South America by the several scientific missionaries sent to that region by the French government. The exhibition will be open only till March 1.

WE have received from Messrs. De la Rue and Co. some specimens of their exquisitely-printed Indelible Diaries, Pocket Diaries, Memorandum Books, and Calendars for the coming year. Our readers have doubtless already supplied themselves with one or other of these. If not, the following statement will recommend the Pocket Diary to every lover of science:—We not only find everything that one finds generally in such a pocket companion, but, under the careful editorship of Mr. Godward, the amateur astronomer is supplied with information as to astronomical phenomena, including the times of rising, southing, and setting of the five principal planets, and the illuminated discs of Venus and Mars, and occultations visible at Greenwich. The physiographer finds meteorological averages of mean temperature, rainfall, and barometer, hints as to weather forecasts, and the magnetic elements. Physical data are not forgotten, and the conversion of metric measures into British inches and centigrade readings into Fahrenheit are given. The geographer and statistician have also facts stored up for them which will certainly be often referred to in the course of the 8,000 odd hours which make up the year. One thing, and one thing only, we miss—the old three-page article and exquisite steel engraving which brought home to everybody the latest thing of mark in the progress of the sciences of observation.

WE learn with pleasure in perusing the last pamphlets sent to us by Capt. Howgate on his intended Polar Colony, that the use of small pilot balloons has been recommended to Mr. Sherman, the meteorologist of the preliminary *Florence* expedition. The method practised by M. de Fonvielle in the beginning of 1877 at Secretan's workshop for ascertaining the altitude of clouds and the direction of the winds by throwing ballonets into the air, has been improved upon in America and will be used regularly in arctic work. This success has led MM. de Fonvielle and Secretan to prepare instructions for the above purposes, in the hope of extending the use of these ballonets to the bringing of news from ships in danger or expeditions severed from the civilised world either by sandy wastes or icy solitudes. A number of examples cited in recent works on ballooning may be regarded as an indication that the old mode of throwing bottles into the sea may be replaced by a new method equally simple and having at least a thousand more chances of success.

CAPT. HOWGATE'S scheme for Polar colonisation has been brought before the Council of the Paris Geographical Society, and it is expected that a resolution favourable to the contemplated expedition will be adopted in time to be sent to America before Congress has come to a final decision on that important object.

AN interesting discussion arose at the last meeting of the Anthropological Institute, on the contents of the small oval pits which have been discovered in the neighbourhood of some of the shafts at Cissbury. The president, Mr. John Evans, pointed out marks on the bone of a small ruminant, probably a roebuck, which indicated that it had been used in the process of weaving. A carding-comb, a terra-cotta bead, large enough to serve as a spindle-whorl, and a loom-weight of chalk were found in the same pit. Lord Rosehill mentioned that chalk weights were also met with in Mr. Tindale's pit at Cissbury, and some were now in his museum. Mr. Park Harrison was of opinion that the little pits were graves, but they appeared to have been disturbed at a remoter period and used for more than one interment. The potsherds found in them were of various dates, some being of a type more common on the Continent than in this country.

WE notice the appearance of the first two of the three divisions of the *Jahresbericht für Chemie* for 1876, which completes the report of physical, inorganic, organic, vegetable, and physiological chemistry, leaving the analytical, technical, mineralogical, and geological portions for the closing number. Prof. Fittica, of Marburg, is still editor-in-chief, and he is assisted by C. Böttinger, C. Hell, H. Klinger, A. Laubenheimer, E. Ludwig, A. Naumann, F. Nies, H. Salkowski, Z. H. Skraup, K. Zöppritz, G. Schultz, and W. Weyl, the latter two replacing K. Birnbaum and A. Michaelis in the editorial *corps* of the preceding year. The publication of the *Jahresbericht* has been much more prompt since the appearance of Prof. Staedel's *Jahresbericht für die reine Chemie* in 1873, which although confined exclusively to pure chemistry, renders a tolerably complete report for each year in the following September.

THE two last numbers of the *Isvestia* of the Russian Geographical Society contain a very interesting account, by Dr. Wojeikoff, of his travels in Japan, made during the summer of last year. Besides a vivid description of the country visited, and of its inhabitants, the reader will find in these papers many interesting data as to the physical characteristics of the land, with many determinations of heights, the climate, the products, &c. Two separate papers are devoted, one to the exterior trade of Japan, and the other to the population and its dependence upon agriculture, as compared with other countries.

THE Moscow Society of Friends of Natural Science has undertaken various anthropological researches for the exhibition which will take place at Moscow in 1879. One of them was made in the Ryazan government by M. Néfédoff, who has already discovered and excavated ten unknown and very interesting *koorganes* (mounds) in Kasimov district. He has found there eleven human skeletons with many ornaments, some of them in bronze, representing snakes, heads of various animals, &c.; and a comparison of the Ryazan skulls and ornaments with those excavated in the Moscow and Meriaks *koorganes*, proves that they belong to quite a different people. Altogether the discovery promises to be of great importance. Another gentleman sent by the same society, M. Bensengr is busily engaged in making anthropological measurements and ethnographical descriptions of the Ryazan Tartars.

AT the meeting of the St. Petersburg Society of Naturalists on December 9, M. Polyakoff—returned from a journey to Western Siberia, the Altai, and Alatan Mountains—read a report on the interesting question as to the state of Central Asia during the glacial period. After having described the boulder-clays, boulders, and morainic deposits he met with during his journey, as well as the present characters of the flora and fauna of the country, he concluded in favour of a complete glaciation of Central Asia during the last ice-period.

WE notice a valuable Russian work, just published by M. Mushketoff, "Materials for a Knowledge of the Geology and of the Mines of the Zlatoust Mine District in Southern Ural." It is the result of careful study, contains many new and valuable data, and is accompanied by an elaborate geological map.

AT the last meeting of the Russian Geographical Society on December 8, Prof. Ujfalvy, of the Paris High School of Eastern Languages, who was sent by the French Government on an anthropological mission to Central Asia, made a very interesting communication on his work in the Russian provinces of Orenburg, Fergana, and Turkistan. After a careful study of the Bashkirs, he arrived at the conclusion that this people are the original stock of the Madjars; that the Mescheryacks are intermediate between Bashkirs and Ostyacks, and that the Tepteri are true Tartars. The conclusions arrived at as to the various peoples of Turkistan are more complicated and could not be briefly stated; but the learned professor has collected many important data, and has obtained valuable photographs, collections of old coins from Turkistan, of stone implements from Siberia, &c.—At the same meeting M. Minaieff referred to the work he has compiled, by order of the society, on the tracts of Central Asia occupying the upper parts of the Amu-daria. The work is divided into three parts: geographical, ethnographical, and linguistic, the former being the richest, and sums up all we know at present about those lands.

COL. GORDON has lately entered into a contract with Messrs. Yarrow and Co., of Poplar, for four steel steamers of small draught. He intends exploring the Albert Nyanza and the rivers flowing into it. The steamers are to be carried as far as possible by water, and are to be composed of several portable pieces of about 200 lbs. each, to be put together on arrival at their destination. Col. Gordon and his party are reported to be in good health.

SINCE the beginning of last year a new scientific journal has appeared at Christiania (Cammermeyer) under the title *Archiv for Mathematik og Naturvidenskab*. It is edited by Herrer Sophus Lie, Jakob Worm Müller, and G. O. Sars. The journal is published in four yearly parts which form a volume of about 500 pages. We have received the first seven parts, and may congratulate the editors and publishers on the decided step of progress which the appearance of this journal evidently marks in the history of Norwegian science. Amongst a number of mathematical papers by Herr Sophus Lie, and others of minor interest, there are some interesting geological treatises by Herr Karl Pettersen, viz., on the orography of Norway, on the geology of the Salten fjord, on the giant's cave near the Lavangen fjord in the neighbourhood of Sandvorf, and on the fjords of Northern Norway. Herr S. A. Sexe has contributed two papers on some old coast-lines and on the direction of the winds in the so-called "stille Belt." Herr Amund Helland is the author of a treatise on the ice-filled fjords of Northern Greenland, and of an elaborate account of the varying quantities of chlorine present in the sea-water of the German Ocean, the Atlantic, and Davis' Straits. Herr G. O. Sars contributes an interesting note on the scientific expeditions in the Atlantic during 1876, and some detailed researches on the invertebrate fauna of the Mediterranean (with plates.) Herr J. Worm Müller gives some notes on Malassez's method of estimating the number of red corpuscles in blood as well as on the relation between the number of red corpuscles and the colouring power of blood. Of the remaining papers we note—a metallurgical paper by E. Münster: on the influence of the eccentricity of the orbits of heavenly bodies upon the quantity of heat they receive from the sun, by H. Geelmuyden; and two zoological notes, one by J. Koren and D. C. Danielssen, the other by Herman Friele.

THE additions to the Zoological Society's Gardens during the past week include a Greater Sulphur-Crested Cockatoo (*Cacatua galerita*) from Australia, presented by Miss Rosetta Cohen; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, presented by Mr. Alex. F. Baillie; a Mocassin Snake (*Tropidonotus fasciatus*), born in the Gardens.

CERTAIN MOVEMENTS OF RADIOMETERS[1]

NEARLY two years ago Mr. Crookes was so good as to present me with two of his beautiful radiometers of different constructions, the discs of one being made of pith, and those of the other of roasted mica, in each case blackened with lampblack on one face. With these I was enabled to make some experiments, having relation to their apparently anomalous movements under certain circumstances, which were very interesting to myself, although the facts are only such as have already presented themselves to Mr. Crookes, either in the actual form in which I witnessed them, or in one closely analogous, and have mostly been described by him. Although it will be necessary for me to describe the actual experiments, which have all been repeated over and over again so as to make sure of the results, I do not bring forward the facts as new. My object is rather to endeavour to co-ordinate them, and point to the conclusions to which they appear to lead.

I do not pretend that these conclusions are established; I am well aware that they need to be further confronted with observation; but as I have not leisure to engage in a series of experiments which would demand the expenditure of a good deal of time, and have lately been urged by a friend to publish my views, I venture to lay them before the Royal Society, in hopes that they may be of some use, even if only in the way of stimulating inquiry.

In describing my experiments I will designate that direction of rotation in which the white face precedes as positive, and the reverse as negative. It will be remembered that, under ordinary circumstances, radiation towards either radiometer produces positive rotation.

1. If a glass tumbler be heated to the temperature of boiling water, and inverted over the mica radiometer, there is little or no *immediate* motion of the fly, but quickly a *negative* rotation sets in, feeble at first, but rapidly becoming lively, and presently dying away.

2. If after the fly has come to rest the hot tumbler be removed, a *positive* rotation soon sets in, which becomes pretty lively and then gradually dies away as the apparatus cools.

3. If the tumbler be heated to a somewhat higher temperature, on first inverting it over the radiometer there is a *slight positive* rotation, commencing with the promptitude usual in the case of a feeble luminous radiation, but quickly succeeded by the negative rotation already described. If the tumbler be heated still more highly, the initial positive rotation is stronger, and lasts longer, and the subsequent negative rotation is tardy and feeble.

4. If the pith radiometer be treated as in § 1, the result is the same, with the remarkable difference that the rotation is positive instead of negative; it is also less lively.

[1] Paper read at the Royal Society, December 20, by Prof. G. G. Stokes, Sec. R.S.

5. But if the tumbler be removed when the fly has come to rest, it remains at rest, or nearly so.

6. If the tumbler be more strongly heated, positive rotation begins as promptly as with light. In this case the tumbler must not be left long over the radiometer, for fear the vacuum should be spoiled by the evolution of gas from the pith.

7. If the tumbler be heated by holding it over the spout of a kettle from which steam is issuing, and held there till the condensation of water has approximately ceased, and be then inverted over the pith radiometer, the bulb is immediately bedewed, and a *negative* rotation is almost immediately set up, though sometimes, just at the very first moment, there is a trace of positive rotation. The negative rotation is lively, but not lasting; and after fifteen seconds or so, is exchanged for a positive rotation, which is not lively, but lasts longer.

8. If the tumbler be lifted when the negative rotation has ceased, and the dewed surface be strongly blown upon, a lively but brief positive rotation is set up.

9. To produce positive rotation by blowing it is not *essential* that the bulb be wet. If it be merely warm, and the circumstances are such that the fly is at rest for the moment, or nearly so, blowing produces positive rotation, though much less strongly than when the bulb is wet.

10. If the tumbler be heated as in § 7, and inverted over the mica radiometer, the rotation is positive, as when the tumbler is dry.

11. If the tumbler or a cup be smoked inside (to facilitate radiation), heated to a little beyond the temperature of boiling water, and inverted over the pith radiometer, a positive rotation is produced; and if, when this has ceased, which takes place in a couple of minutes or so, the heated vessel be removed, a negative, though not lively, rotation is produced as the apparatus cools.

12. These results do not seem difficult to co-ordinate so far as to reduce them to their proximate cause.

As regards the small quantity, if any, of heat radiated directly across the glass of the bulb, the action of which was experimentally distinguishable by its promptitude, both radiometers behaved in the ordinary way.

13. As regards the mica radiometer, when the bulb gets heated and radiates towards the fly the fly is impelled in the negative direction *as if* the white pearly mica were black and the lampblack were white. And there is nothing opposed to what we know in supposing that such is *really* their relative order of darkness as regards the heat of low refrangibility absorbed and radiated by the glass; for the researches of Melloni and others have shown that lampblack is, if not absolutely white, at any rate very far from black as regards heat of low refrangibility. On the other hand, glass and mica are both silicates, not so very dissimilar in chemical composition, and it would not therefore be very wonderful, but rather the reverse, if there were a general similarity in their mode of absorption of radiant heat, so that the heat most freely radiated by glass and accordingly abounding in the radiation from *thin* glass such as that of the bulb, were greedily absorbed by mica. The explanation of the reversal of the action when heat and cold were interchanged is too well known to require mention.

14. With the pith radiometer, when the bulb as a whole is heated, and radiates towards the fly, the impulse is positive, though less strong than in the case of the mica (§ 4); and when the bulb as a whole is cooler than the fly the impulse is negative (§ 11).

But to explain all the phenomena we must dissect the total radiation from or towards the bulb. When I first noticed the negative rotation produced by a heated wet tumbler, I was disposed to attribute it to radiation from the water, which possibly the glass of the bulb might be thin enough to let pass; but when I found that hot water in a glass vessel outside, even though the glass of it were thin, produced no sensible effect, and that blowing on the heated bulb when it was dry produced a similar effect to blowing on it when dewed, though of much less amount, I perceived that the moisture acted, not by direct radiation from it, and in consequence of a difference of quality between the radiations from glass and water, but by causing a rapid *superficial* heating of the bulb; and, similarly, the blowing on the dewed surface acted by causing a rapid superficial cooling. When the dry tumbler radiates to the bulb, the radiation is absorbed at various depths; the absorption is most copious, it is true, at the outer strata, but still the change of temperature is not by any means so much confined to the immediate surface as when we have to deal with the latent heat of vapour condensed on it, or obtained from it by rapid evaporation.

Hence, thin as is the glass of the bulb (about 0·02 in. thick), we must still, in imagination, divide it into an outer and inner stratum, and examine the effects of these separately. The heat radiated by either stratum depends only on its temperature, but the radiation from the outer, on its way to the fly, is sifted by passing through the inner, and the portion for which glass is most excessively opaque is in great part stopped. It appears from the observed results that the residue acts decidedly negatively, while when the bulb is pretty uniformly heated there is positive action. We may infer that if it were possible to heat the inner stratum alone it would manifest a very decided positive action.

15. In the struggle between the opposing actions of the outer and inner strata we see the explanation of the strange behaviour of the pith radiometer. In the experiment of § 7 the outer stratum at first shows its negative action, but quickly the inner also gets heated, partly by conduction from the outer, partly by direct radiation from the tumbler, and then the inner prevails. In the experiment of § 5 the whole bulb cools, partly by radiation, partly by convection, while the fly remains warmer; and the slightly greater coolness of the outer than of the inner stratum makes up for the superiority of the inner when the two are equally cool, so that the antagonistic actions nearly balance, and slight causes, such as greater or less agitation of the air, suffice to make the balance incline one way or other. That the inner stratum *would* prevail if the two were about equally cooled may be inferred from the behaviour of the radiometer when the bulb is pretty uniformly heated (§§ 4, 11), or shown more directly by cooling the bulb with snow, when a negative rotation may be obtained.

16. The complete definition of a radiation would involve the expression of the intensity of each component of it as a function of some quantity serving to define the quality of the component, such as its refractive index in a standard medium, or its wave-length, or the squared reciprocal of the wave-length.[1] The experimental determination of the character, as thus defined, of a radiation consisting of invisible heat-rays is beset with difficulties, at least in the case of heat of extremely low refrangibility; and in general we can do little more than speak in a rough way of the radiation as being of such or such a kind. It is obvious that the behaviour of radiometers by itself alone affords no indication of the refrangibilities of the kinds of heat with which we have to deal; nevertheless, by combining what we know of the behaviour of bodies in respect to radiations in general (especially luminous radiations, which are the most easily studied) with what we observe as to the motions of radiometers, we may arrive at some probable conclusions.

17. We may evidently *conceive* a series of ethereal vibrations of any periodic time, however great, to be incident on a homogeneous medium such as glass, and inquire in what manner the rate of absorption would change with the period; though whether we can actually *produce* ethereal vibrations of a very long period is another question, seeing that we can only act on the ether by the intervention of matter, and are limited to such periods of vibration as matter can assume when vibrating molecularly, in a manner communicable to the ether, and not as a continuous mass, in the manner of the vibrations which produce sound. We may inquire whether, on continually increasing the period of vibration, the glass (or other medium) would ultimately become and remain very opaque, or whether, after passing through a range of opacity, it would become transparent again, on still further increasing the period of the incident vibrations.

18. This is a question the experimental answer to which, as it seems to me, could only be given, in so far as it could be given at all, as a result of a long series of experiments, of a kind that Melloni has barely touched on. A variety of considerations, which I could not explain in short compass, lead me to regard the second alternative as the more probable, namely, that, on increasing the periodic time, homogeneous substances in general (perhaps even metals, though this is doubtful) become at last transparent, or at least comparatively so. The limit of opacity, in all probability, varies from one substance to another; and the lower it is, the lower would be the lowest refrangibility of the radiation which the same substance is capable of emitting.

19. In what immediately follows I shall suppose accordingly that glass is strongly absorbing through a certain range of low

[1] A map of the spectrum, constructed with the squared reciprocals of the wave-lengths for abscissæ, would be referred to a natural standard, no less than that of Angström, which is constructed according to wave-lengths; while it would have the great advantage of admitting of ready comparison with refraction spectra, the kind almost always used.

refrangibility, on *both* sides of which it gradually becomes transparent again.[1] Imagine a spectrum containing radiations of all refrangibilities with which we have to deal; let portions of this spectrum on the two sides of the region of powerful absorption for glass be called *wings* of that region, and let left to right be the order of increasing refrangibility. Then the spectrum of the radiation from a thin plate of glass, if it could be observed, would be seen to occupy the region of chief absorbing (and therefore emitting) power and its wings. The spectrum of the radiation from the outer stratum of the bulb of the pith radiometer, after transmission through the inner, would consist of two wings, with a blank, or nearly blank, space between; it would resemble, in fact, a widened bright spectral line, with a dark band of reversal in its middle, save that, instead of being confined to extremely narrow limits of refrangibility, the central space and its wings would be of wide extent. It follows from the experiments that, in the complete radiation from glass, the portions of the spectrum called the wings together act negatively, the portion between positively. It does not, of course, follow that each wing acts negatively, but only that the balance of the two is negative. When the tumbler is heated a little over 212° there is a slight positive action from radiation which passes directly through the bulb. The circumstances lead us to regard this as an extension of the right wing; for it comes from a depth, measured from the inner surface of the bulb in glass, *i.e.*, not counting the intervening air, somewhat greater than the thickness of the wall of the bulb; and we know that the more a solid body is heated, the higher, as a rule, does the refrangibility of the radiation which it emits extend, and the greater the proportion of rays of high to those of low refrangibility. It is simplest, therefore, to suppose that the action of the right wing, like that of the space between the wings, is positive, and that the observed negative action in the experiment of § 7 is due to the excess of negative action of the left wing over positive action of the right. In the mica radiometer the experiments indicate no such difference of action in the different layers of the bulb as in the case of the pith radiometer. Hence taking, in accordance with what now appears to be made out to be the theory of the motion of the radiometer, the direction in which the fly is impelled as an indication which is the warmer of the two faces of the discs, and that again as an indication which is the darker with respect to the radiation to which it is exposed, we arrive at the following results as regards the order of darkness of the substances for the three regions into which the spectrum of the incident radiation has been supposed to be divided, the name of the lighter substance being in each case placed above that of the darker:—

	Left wing.	Region of intense absorption by glass.	Right wing.
From pith radiometer ...	Lampblack.	Pith.	Pith.
	Pith.	Lampblack.	Lampblack.
From mica radiometer ...	Lampblack.	Lampblack.	Mica.
	Mica.	Mica.	Lampblack.

Hence, on descending in refrangibility, the order of darkness of the two substances o either pair is at first the same as for the visible spectrum, and at last the opposite; and the reversal of the order takes place sooner with mica and lampblack than with pith and lampblack. The order falls in very well with that of the chemical complexity of the three substances.

20. The whole subject of the behaviour of bodies with respect to radiant heat of the lowest degrees of refrangibility seems to me to need a thorough experimental investigation. The investigation, however, is one involving considerable difficulty. We can do little towards classifying the rays with which we are working unless we can form a pure spectrum. A refraction spectrum is the most convenient; but the only substance known which would be approximately suitable for forming the prism, lens, &c., required for such a spectrum, and for confining liquids, is rock-salt, of which it is extremely difficult to procure perfectly limpid specimens of any size; and even rock-salt itself, as Prof. Balfour Stewart has shown, is defective in transparency for certain kinds of radiant heat. Then, again, the only suitable measuring-instrument for such researches, the thermopile, demands a thorough examination with reference to the coating to be employed for absorbing the incident radiation. Hitherto lampblack has been used almost exclusively for the purpose; and it is commonly assumed, in accordance with certain of Melloni's results, that lamplack absorbs equally heat-

[1] It may be noticed that this supposition, which, as appearing the more probable, is adopted for clearness of conception, is not essentially involved in the explanation that follows, which would hardly be changed if the "left wing" were not terminated on the left.

rays of all kinds. But the experiments by which Melloni established the partial diathermancy of lampblack prove that rays exist for the absorption of which that substance is unsuitable.

On calling on Mr. Crookes after the above was written, I was surprised to find that all his mica radiometers behaved towards a heated glass shade in the opposite way to that he had given me, going round positively instead of negatively. Mr. Crookes showed me and gave me a specimen of the kind of mica he employs as eminently convenient for manipulation. It is found naturally in a condition resembling artificially roasted mica. It is not, however, quite so opaque for transmitted light, nor of quite such a pearly whiteness for reflected light as that which has been artificially roasted at a high temperature. The mica radiometer that Mr. Crookes first gave me, which I will call M_1, was, Mr. Gimingham told me, the only one they had made with roasted mica.

Mr. Crookes was so kind as to give me, for comparative experiment, a mica radiometer, which I will call M_2, made from the natural foliated mica. It revolves a good deal more quickly than M_1 under the influence of light; it also gets more quickly under way, indicating that the mica is thinner. When covered with a hot glass it revolves positively, as already remarked; there is, however, but little negative rotation when the glass is removed.

The difference in the thickness and condition of the mica sufficiently explains the difference of behaviour of M_1 and M_2. Any radiant heat incident on the white face that reaches the middle of the mica, whether it afterwards is absorbed by the mica or reaches and is absorbed by the lampblack, tends to heat the second or blackened face more than the first, and therefore conspires with the heat incident on the lampblack, and absorbed by it, to produce positive rotation; and the smaller thickness and less fine foliation of the natural mica are favourable to the transmission of radiant heat to such a depth.

P.S.—It might be supposed at first sight that the change of rotation from negative to positive (in § 7) was due, not to a change in the conditions of absorption, but to the circumstance that the inner surface of the bulb had become warm by conduction, so as to be warmer than the surfaces of the fly instead of colder. For we now know that the "repulsion resulting from radiation," as in some way or other it undoubtedly does result, is an indirect effect, in which radiation acts only through the alterations it occasions in the superficial temperatures of the solids in contact with the rarefied gas; and it might be supposed that when the inner surface of the bulb passed from colder than the fly to warmer, the direction of rotation would, on that account alone, be reversed. This, however, is not so. If bulb and fly are at a common temperature, and the instrument is protected from radiation, the fly remains at rest whether the common temperature be high or low. If a small portion of the total surface in contact with the rarefied gas be warmed by any means, repulsion takes place, through the intervention of the rarefied gas, between the warmed surface and the opposed surfaces, if not too distant; if it be cooled, the result is attraction. It does not matter whether the surface at the exceptional temperature belong to the fly or the bulb. The former takes place in the ordinary case of a radiometer exposed to radiation, the latter in that of a radiometer at a uniform temperature and protected from radiation when a small portion of the bulb is warmed or cooled, in which case the part at the exceptional temperature repels or attracts the disc irrespectively of its colour or the nature of its coating.[1] Suppose now that the fly is being warmed by radiation from without, the bulb being cool, at least at its inner surface. Let A, B be the two kinds of faces of the discs, and suppose A to be the better absorber of the total radiation. Then A will be the warmer, and therefore will be more strongly repelled than B. Suppose now that the bulb is heated till its inner surface becomes warmer than the fly. Then the fly will still be receiving heat by radiation, to some extent also by communication from the gas; but this will be the same for both faces. Hence if A be still the better absorber of the two (A, B), A will be the warmer, and being less below the tem-

[1] Theoretically there would be a minute difference of temperature, produced, other circumstances being alike, by the difference in the absorbing or emitting power of the two faces of a disc, as regards the radiation which is the difference between the radiations from or towards the affected portion of the bulb and the same portion at the normal temperature. But this, and the repulsion or attraction corresponding to it, would be only a small quantity of the second order, the main effect being deemed one of the first order.

perature of the interior surface of the fly will be less attracted, or, which is the same, more repelled. Hence, whether the inner surface of the bulb be cooler or hotter than the fly, a reversal in the direction of rotation while the fly is being heated, indicates a reversal in the order of absorbing power of the two faces, and that, again, shows that the order is different for different components of the total radiation, and that the ratio of the intensity of those components has been changed.

It is perhaps hardly necessary to observe that the radiometers mentioned in this paper are of the usual form—that is to say, that their arms are symmetrical, so far as *figure* is concerned, with respect to a vertical plane passing through the point of support. Accordingly the rotation which is attained, for instance, with a radiometer with concave disks of aluminium, alike as to material on both faces (of which kind, again, I owe a beautiful specimen to Mr. Crookes's kindness), has not been referred to. This rotation, depending on the more favourable presentation to the bulb of the outer (and therefore nearer and more efficient) portions of the fly on the convex than on the concave side, has nothing to do with the one isolated subject to which the present paper relates, namely, the elucidation of the peculiar behaviour in certain cases of certain kinds of radiometers, by a consideration of the heterogeneous character of the total heat-radiation.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LEEDS.—By the liberality of the Worshipful the Drapers' Company, the Council of the Yorkshire College are prepared to appoint an instructor in coal mining at the stipend of 100*l.* per annum and half the students' fees. A portion only of the instructor's time will be required. The fuller conditions and duties of the office may be learned from the secretary. Applications and testimonials must be received on or before January 18.

LEXINGTON, U.S.—In connection with the Centennial, efforts have been made in the United States to raise an endowment fund for Washington and Lee University, at Lexington, Va. The institution dates from colonial times, and was endowed, while it was still only an academy, by Washington and other soldiers of the Revolution. Among other recent benefactors of the University is Mr. L. J. McCormick, of Chicago, who has offered to give his magnificent telescope, made by Alvan Clark, of Cambridge, U.S., at a cost of 50,000 dols., provided the institution would raise the necessary funds to equip and maintain it. The trustees have not yet been able to do anything towards the acceptance of this proposal. It would be a great misfortune if the conditions could not be complied with, and we hope that the suggestion that the ladies in various parts of the States should take the matter up will be complied with; there is no doubt if they make up their minds to success they will succeed.

BERLIN.—The great Prussian university is closely competing now with the Leipsic University in point of attendance. In the calendar which has just appeared we notice that the number of matriculated students during the present winter amounts to 2,839, an increase of 600 on the summer semester. They are divided among the faculties as follows: theological 168, legal 1,163, medical 345, philosophical 1,163. There are 210 foreigners in the list, including 42 from America. Besides these matriculated students, there are 2,200 other persons in attendance on the lectures, belonging to the various technical and art schools of the city. The corps of instructors numbers 210, nearly half of whom are in the philosophical faculty.

BONN.—The winter attendance at the University is 859, an increase of sixty-two on the preceding semester. The philosophical faculty includes 375, the legal 219, the medical 126, the Catholic theological, 89, and the Evangelical, 50.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, December 6.—Prof. Allman, president, in the chair.—Messrs. J. N. Fitch, J. S. Gamble, F. S. Piggott, A. B. Stewart, and Prof. Macoun were elected Fellows.—Mr. Thiselton Dyer exhibited portions of the "Nam-mu" tree, which grows in Yunnan, 25°–26° N. lat. The Chinese nobility greatly prize its wood for building purposes and for making coffins, and enormous columns in tombs of the Ming dynasty, 300 years old, are still extant. Supposed to be teak, it probably rather belongs to the Lauraceæ, the leaves closely resembling those of *Phœbe pallida.* Mr. Dyer also exhibited a seed of *Entada scandens*, and another of an anonaceous plant (*Cyathocalyx Maingayi?*) found in the cœcum of *Rhinoceros sumatrensis* from Chittagong, and dissected at the Zoological Gardens, Regent's Park; and he likewise showed fruits of *Oncocarpus vitiensis* from the crop of a fruit-pigeon (*Carpophaga latrans*).—Attention was afterwards called by Mr. Dyer to the fruit-head of an Indian *Pandanus* made into a brush, the fibrous tissue of the drupes forming the bristles, and this instrument was said to be used to scrape cloth, like our teazle (*Dipsacus*).—Flowers and foliage of Cinchona (*C. calisaya*, vars., *Josephiana* and *Anglica*) grown in the garden of Mr. J. Elliot, at Tottenham, were exhibited by that gentleman, whose researches among the quinine-bearing trees are already well known and appreciated.—Mr. Moggridge read a note on the occurrence at Wallis Down, a heath near Bournemouth, of *Dabeocia polifolia.*—A paper on certain organs of the Cidaridæ was communicated by Mr. Chas. Stewart, who illustrated, amongst others, the subjoined points of his recent investigations. Among the sea-urchins the families Diadematidæ, Echinometridæ, and Echinidæ, have long been known to possess external branchiæ; but the existence of such in the Cidaridæ has been denied by Müller, though insisted on by Alex. Agassiz. Mr. Stewart finds in *Dorocidaris papillata* five organs corresponding to branchiæ, but situated internally. The water bathing these interior gills finds ingress and egress by a crevice near the "compasses," the peculiar mechanism of the teeth and jaws producing the temporary opening in question. As respects the pedicellariæ of Cidaridæ, where the jaw ends in a terminal hollow fang, there is an additional orifice to that at the tip, besides two glands in the vicinity; he suggests this to be a poison apparatus, comparable to the falces of the spider, and poison sac and tooth of venomous serpents.—The Secretary read a paper by Dr. I. Bayley Balfour, "Observations on the genus *Pandanus.*" Few families of plants present more difficulty in their elucidation than the Pandanaceæ; this by variability of species, difficulty of procuring the male flower, with little character in the leaves, while the fruit loses its distinctive features in drying. The Screw-pines had attracted the notice of the early voyagers, but their descriptions are confused. To Rumphius we owe the name Pandanus, though his account and figures are poor compared with Reede's of a century before. Linnæus, though indicating a plant under the name *Bromelia sylvestris*, omitted the genus *Pandanus*, a want supplied by his son. Afterwards, as species increased, many new genera were unnecessarily introduced, which Dr. Balfour is now inclined to reject; even Brongniart's New Caledonia genera do not claim acceptance. *Pandanus* runs over a great breadth of longitude, viz., from east tropical Africa through the Mascarene Islands, India, Indian Archipelago, and Australia, to the Sandwich Islands. The East Archipelago and the Mascarenes are centres whose species do not commingle. There succeeds in this paper other facts and an extensive list of names and references to all the Pandani known.—The substance was given of a report on a small collection of insects obtained by Dr. J. C. Ploëm, in Java, with description of a new species of *Hoplia*, by Chas. O. Waterhouse, of the British Museum.—The Secretary read a communication by Dr. J. Stirton, viz., "Notes on the Rev. Mr. Crombie's paper on the Lichens of the *Challenger* Expedition," and another note by Dr. R. C. A. Prior, relative to the migration of wild geese, purported to have passed from North America to the African coast.

Physical Society, December 15.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected Members of the Society:—W. E. Ayrton, J. M. Cameron, J. W. Clark, J. E. Judson, B.A., H. N. Moseley, M.A., F.R.S., Lord Rayleigh, M.A., F.R.S., W. N. Stocker, M.A., and H. T. Wood.—Mr. C. W. Cooke read for the author, Prof. S. P. Thompson, a paper on permanent Plateau films, and exhibited the process of their formation. After a brief enumeration of the various attempts made by Plateau himself, Schwartz, Mach, Rottier, and others, most of which are described in the work of Plateau, the author described his own experiment on the subject. As the result of these he concludes that the best films are obtained by using a mixture of 46 per cent. of pure amber-coloured resin, and 54 of Canada balsam, which should be heated to from 93° to 95° C. The frames for forming the films are made of brass wire 0·3 mm. in diameter, and when thicker wire is employed they are found to be irregular in consequence of the retention of heat by the metal. The films are obtained by simply introducing these frames into the heated mixture, and they harden almost immediately on exposure to the air; but better results are obtained by slow drying in an air bath heated up to 80° C., and allowed to cool. In proof of the toughness of the films it was mentioned that a flat circular film 4 cm. in diameter, had supported a 50-gramme brass weight at its centre.—Mr. Sedley Taylor then exhibited some experiments in illustration of a paper on the colours exhibited by vibrating liquid films which he has recently communicated to the Royal Society.—Dr. Guthrie exhibited a simple lecture illustration of the action of the telephone. Two similar coils of wire are placed one on the end of a bar magnet, and the other on a soft iron core. A tin disc about three inches in diameter is suspended by two threads almost in contact with one end of this latter, and when a similar disc is brought, at regular intervals, against the end of the magnet which is provided with the coil, a distinct movement of the first-named disc is observed which can be easily increased by properly timing the movement of the inducing disc.

Anthropological Institute, November 27.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—The election of five new members was announced.—Major-Gen. A. Lane Fox, F.R.S., exhibited various objects from Istria and Scinde.—The Director read some notes on Socotra, by Capt. Hunter, R.N., in which some of Lieut. Wellstead's statements about that island were criticised.—A paper on the Záparos, by Mr. A. Simpson, was then read, in which many interesting observations of these tribes of "Equador" were recorded. Their wonderful tracking powers, abstention from heavy meats, such as tapir and peccari, curious mode of training hunting-dogs, were described. Their enjoyment in the destruction of life, human or animal, with the exception of the alligator, which they will not touch themselves, was very marked. The Napos do not resemble them in this respect. The Záparos are very disunited, and wander about in separate hordes, the worst of which are the Supinus. Courtship is sometimes carried on by a silent invitation by the suitor to his elect to cook his food. If rejected, he tries elsewhere. The Záparos are described as of a happy and cheerful disposition, very superstitious, believing in an evil spirit, and very poor and almost nude.—A paper on the Malayo Polynesians, by Rev. S. J. Whitmee, was then read, in which the author noted the high social position of women in the Samoan group, as compared with their place among the *black* Polynesians. The existence of hereditary ranks and titles among the *brown* Polynesians seems to the author to indicate a former higher condition. The author referred to the difficulties experienced by missionaries in obtaining the true versions of the native poems and myths, and noted the custom of preserving the myths in poetry as well as prose, the two versions acting one as a check on the other, and so preserving the correctness one of the other. In the discussion, Major-Gen. A. Lane Fox, Mr. Blackmore, Mr. Hyde Clarke, and others, took part.

Entomological Society, December 5.—J. W. Dunning, vice-president, in the chair.—Mr. W. L. Distant exhibited specimens of the rare species of Hemiptera-Heteroptera, *Tetroxia beauvoisii*, and *Oncocephalus subspinosus*, from the West Coast of Africa.—Mr. F. Smith exhibited a fine series of *Macropis labiata*, male and female, captured near Norwich by Mr. I. B. Bridgman.—Mr. Smith also exhibited a specimen of *Rophites quinquespinosus*, a genus and species new to the British Hymenoptera, captured near Hastings by the Rev. E. H. Bloomfield.—Mr. Meldola exhibited three fine photographic enlargements of micro-photographs (two being of parts of insects) taken by Mr. Edward Viles, of Pendryl Hall, Wolverhampton. The photographs, which had been exhibited at the recent exhibition of the Photographic Society were 30 × 24 in., while the original negatives were 3 in. square.—Mr. Meldola likewise performed an acoustical experiment illustrating the action of the stridulating apparatus in the *Phasma* (*Pterinoxylus*), an account of which had been given to the society by Mr. Wood Mason at the last meeting.—Mr. Wood Mason made further remarks on the structure of the stridulating organ in scorpions.—Mr. F. Smith mentioned a case of stridulation occurring in a British species of *Curculionidæ* (*Acalles*).—Mr. Dunning called attention to a striking case of mimicry recorded by Mr. Neville Goodman in the *Proceedings* of the Cambridge Philosophical Society for February, 1877, the mimic being a species of *Laphria*, and the model, the well-known Egyptian hornet, *Vespa orientalis*.—Mr. F. Smith read a paper containing descriptions of new species of hymenopterous insects from New Zealand, collected by Prof. Hutton at Otago.—Mr. A. G. Butler communicated a paper on the Lepidoptera of the Amazons, collected by Dr. James W. H. Trail during the years 1873 to 1875.—Dr. Sharp communicated the following papers:—Descriptions of eight new species and a new genus of Cossonides, from New Zealand, and descriptions of some new species and a new genus of Rhyncophorous Coleoptera, from the Hawaiian Islands.

MANCHESTER

Literary and Philosophical Society, October 16.—Mr. E. W. Binney, F.R.S., president, in the chair.—The President exhibited to the meeting some coal-measure plants and other organic remains from Spain. From the character of the fossil organic remains and the nature of the strata he was led to believe that the coal-field of Puertollano was of the same geological age as our true English coal-measures.—Mr. M. P. Pattison Muir, F.R.S.E., exhibited and gave a description of a modified form of Hofmann's apparatus for determining vapour densities.—Note on an edible clay from New Zealand, by M. M. Pattison Muir, F.R.S.E. The author lately received from Mr. R. E. Day, a small specimen of a clay which is greedily eaten by the sheep in a certain district in New Zealand. The clay was brought by Mr. Day from Simon's Pass Station, Mackenzie Country, South Island. It there forms a range of low bare hills: the sheep (merino sheep) eat very considerable quantities of the clay without appearing to be any the worse for it. It is supposed by the shepherds that the clay must contain salt, and that it is to supply the deficiency of this article of food that the sheep resort to the earth. The analysis shows that very probably the shepherds are right:—Silica, 61·25; alumina, 17·97; ferric oxide, 5·72; calcium oxide, 1·91; magnesium oxide, 0·87; alkalies (as chlorides), 3·69; organic matter, 1·77; water, 7·31 = 100·49.—On the decomposition of calcium sulphate by alkaline chlorides; a contribution to agricultural chemistry, by M. M. Pattison Muir, F.R.S.E.—On some thionates, by H. Baker, student in the Owens College. Communicated by Prof. C. Schorlemmer, F.R.S.

VIENNA

Imperial Academy of Sciences, October 18.—On the chemical nature of peptone and its relation to albumen, by M. Herth.—On the addition of prussic acid to urea, and on the action of trichloric lactic acid on urea, by M. Cech.—Transformation of cyanamide into ammelide, by MM. Cech and Dehmel.—New discoveries on the negative heliotropism of above-ground parts of plants, by M. Wiesner.—On Fraunhofer's rings, Quetelet's stripes, and allied phenomena, by M. Exner.

October 25.—On the connection of n straight lines in the plane, and on properties of the triangle and two propositions of Steiner therewith connected, by M. Kantor.—On the structure and the growth of some forms of mould-fungus, by M. Hassloch.—On the development of the small pollen-plants of *Colchicum autumnale* L., by M. Tomaschek.—On the secular acceleration of the mean motion of the moon, by M. v. Littrow.

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