LIBRARIES
UNIVERSITY OF WISCONSIN–MADISON

Nature. Vol. I, No. 20 March 17, 1870

London: Macmillan Journals, March 17, 1870

https://digital.library.wisc.edu/1711.dl/LBXITYVRTMAPI83

Based on date of publication, this material is presumed to be in the public domain.

For information on re-use, see:
http://digital.library.wisc.edu/1711.dl/Copyright

The libraries provide public access to a wide range of material, including online exhibits, digitized collections, archival finding aids, our catalog, online articles, and a growing range of materials in many media.

When possible, we provide rights information in catalog records, finding aids, and other metadata that accompanies collections or items. However, it is always the user's obligation to evaluate copyright and rights issues in light of their own use.

728 State Street | Madison, Wisconsin 53706 | library.wisc.edu

THURSDAY, MARCH 17, 1870

ON FLOATING MATTER AND BEAMS OF LIGHT

BEAMS of light may be employed to reveal the existence of floating matter in the air; or the floating matter may be employed to reveal the track of the beams.

When the beam is intense it becomes an extremely powerful searcher and revealer of the state of the air. Thus examined, the air of a room which in diffuse daylight appears absolutely pure is seen to be loaded with suspended matter. Many of the fine clouds developed in my experiments on the action of light upon vapours disappear utterly in diffuse daylight; while when the room is darkened and the light of an intense beam confined to the clouds themselves, they appear highly luminous. The eye is the real re-agent here. Rendered sensitive by darkness, and receiving light from the floating matter alone, the amount of light competent to produce a sensible effect is incalculably small. The power of the light to make an impression is moreover increased by the extension given to the body which emits it.

The mobility of these actinic clouds is in some cases quite extraordinary. The differences of temperature introduced by the act of decomposition often cause the clouds to assume forms of astonishing complexity and beauty. The clouds which thus shape themselves by internal action are also exceedingly sensitive to external action. Supposing a thin actinic cloud to fill the experimental tube, the whole of it being flooded with the light of a beam passing longitudinally through the tube, an instant's contact of the tip of a spirit lamp flame with the under surface of the tube causes the cloud to break upwards in a violent current, and to whirl itself into the most beautiful vortices right and left of the vertical line. The rapidity with which the heat passes through the thick glass and sets the cloud in motion is surprising. The warmth of the finger suffices to produce an effect feebler than, but substantially the same, as that produced by the spirit lamp flame.

In fact, the floating matter of the air, properly illuminated, might be converted into a thermoscope of surpassing delicacy. A little brown paper smoke was diffused in an ordinary glass shade; the track of the beam through it was much whiter than through the air. Hence the invasion of the smokeless air could be instantly seen by the darkness it produced. On introducing the hand at the open base of the shade, a violent uprush of air immediately occurred. The smoke was violently whirled about, and the course of the whirlwinds distinctly marked by the relative action on the light of the smoky and the unsmoky air. I was not prepared to see so small a difference of temperature produce so large and prompt an effect.

Nor is it necessary to introduce dark extraneous air to render the currents through nebulous matter visible. The current produced in the actinic cloud by the spirit flame forms a dark vertical septum between the two adjacent parts of the cloud. When the current curls to form a cyclone, the septum curls also, producing dark spirals through the illuminated nebulæ.

The late Principal Forbes often referred to the floating scum on water slowly flowing through a channel, the lateral parts of which are retarded by friction. Such scum, or froth, arranges itself in distinct striæ, separated from each other by comparatively free intervals. It is practically impossible to establish differential motions, either in solids or liquids, without producing some effect of this kind. Fibrous iron shows it; while in the atmosphere differential currents produce cirrus clouds. I have often watched the way in which the suspended matter of the turbid Arve at Chamouni traced itself through the water. Notwithstanding the tossing endured from the source of the Arveyron downwards, the mixture of mud and liquid was by no means perfect. In fact, every new obstacle which introduced differential motion introduced also the striæ, and destroyed all uniformity of mixture.

Five or six weeks ago I had a square chamber constructed, the upper half of which is glazed, its floor consisting of transverse rails, over which is placed a thick mat of cotton wool. The chamber has a brass chimney, in which a rose burner can be lighted. An upward current is thus established in the chimney, the air below entering through the cotton-wool to supply the place of that discharged by the flame. When the chamber is filled with the ordinary laboratory air, a beam sent through it tracks its course on the floating matter. When the flame is ignited, the air enters through the cotton-wool; but the consequence is not a uniform enfeeblement of the light of the beam. Perfectly dark striæ pass through the luminous track, and they sometimes bend and whirl so as to form gracefully curved streams of darkness. Even air urged from the nozzle of a bellows through the luminous track, shows a tendency to form those striæ, though it, like the water of the Arve at Chamouni, is filled with the same floating matter as that of the air through which it is urged.

On a recent occasion the following effects were described, and an attempt was then made to explain them:—

In a cylindrical beam, which powerfully illuminated the dust of the laboratory, was placed an ignited spirit-lamp. Mingling with the flame, and round its rim, were seen wreaths of darkness resembling an intensely black smoke. On lowering the flame below the beam the same dark masses stormed upwards. They were at times blacker than the blackest smoke that I have ever seen issuing from the funnel of a steamer, and their resemblance to smoke was so perfect as to lead the most practised observer to conclude that the apparently pure flame of the alcohol lamp required but a beam of sufficient intensity to reveal its clouds of liberated carbon.

But is the blackness smoke? This question presented itself in a moment. A red-hot poker was placed underneath the beam, and from it the black wreaths also ascended. A large hydrogen flame was next employed, and it produced those whirling masses of darkness far more copiously than either the spirit-flame or poker. Smoke was therefore out of the question.

What then was the blackness? It was simply that of stellar space; that is to say, blackness resulting from the absence from the track of the beam of all matter competent to scatter its light. When the flame was placed below the beam the floating matter was destroyed *in situ;* and the air, freed from this matter, rose into the beam, jostled aside the illuminated particles, and substituted for their light the darkness due to its own perfect transparency. Nothing could more forcibly illustrate the invisibility of the agent which renders all things visible. The beam crossed, unseen, the black chasm formed by the transparent air, while at both sides of the gap the thick-strewn particles shone out like a luminous solid under the powerful illumination.

But here a difficulty meets us. It is not necessary to burn the particles to produce a stream of darkness. Without actual combustion, currents may be generated which shall exclude the floating matter, and therefore appear dark amid the surrounding brightness. I noticed this effect first on placing a red-hot copper ball below the beam, and permitting it to remain there until its temperature had fallen below that of boiling water. The dark currents, though much enfeebled, were still produced. They may also be produced by a flask filled with hot water.

To study this effect a platinum wire was stretched across the beam, the two ends of the wire being connected with the two poles of a voltaic battery. To regulate the strength of the current a rheostat was placed in the circuit. Beginning with a feeble current the temperature of the wire was gradually augmented, but before it reached the heat of ignition a flat stream of air rose from it, which when looked at edgeways appeared darker and sharper than one of the blackest lines of Fraunhofer in the solar spectrum. Right and left of this dark vertical band the floating matter rose upwards, bounding definitely the non-luminous stream of air. What is the explanation? Simply this. The hot wire rarefied the air in contact with it, but it did not equally lighten the floating matter. The convection current of pure air therefore passed upwards *among the particles*, dragging them after it right and left, but forming between them an impassable black partition. In this way we render an account of the dark currents produced by bodies at a temperature below that of combustion.

This explanation has been found difficult. When the wire is white hot, it sends up a band of intense darkness. This, I say, is due to the *destruction* of the floating matter. But even when its temperature does not exceed that of boiling water the wire produces a dark ascending current. This, I say, is due to the *distribution* of the floating matter. The difficulty alluded to is probably to be referred to the brevity of the explanation. Imagine the wire clasped by the mote-filled air. My idea is that it heats the air and lightens it, without in the same degree lightening the floating matter. The tendency, therefore, is to start a current of clean air through the mote-filled air. Figure the motion of the air all round the wire. Looking at it transversely we should see the air at the bottom of the wire bending round it right and left in two branch currents, ascending its sides and turning to fill the partial vacuum created above the wire. Now as each new supply of air filled with its motes comes in contact with the hot wire, the clean air, as just stated, is first started through the inert motes. They are dragged after it, but there is a fringe of cleansed air in advance of the motes. The two purified fringes of the two branch currents unite above the wire, and, keeping the motes that once belonged to them right and left, they form by their union the dark band observed in the experiment. This process is incessant. Always the moment the mote-filled air touches the wire this distribution is effected, a permanent dark band being thus produced. Could the air and the particles under the wire pass *through* its mass we should have a vertical current of particles, but no dark band. For here, though the motes would be left behind at starting, they would hotly follow the ascending current and thus abolish the darkness.

It has been said that when the platinum wire is intensely heated, the floating matter is not only distributed, but destroyed. Let this be proved. I stretched a wire about four inches long through the air of an ordinary glass-shade resting on its stand. Its lower rim rested on cotton wool, which also surrounded the rim. The wire was raised to a white heat by an electric current. The air expanded, and some of it was forced through the cotton wool, while when the current was interrupted and the air within the shade cooled, the expelled air in its return did not carry motes along with it. At the beginning of this experiment the shade was charged with floating matter; at the end of half an hour it was optically empty.

On the wooden base of a cubical glass shade measuring eleven and a half inches a side, upright supports were fixed, and from one support to the other thirty-eight inches of platinum wire were stretched in four parallel lines. The ends of the platinum wire were soldered to two stout copper wires which passed through the base of the shade and could be connected with a battery. As in the last experiment the shade rested upon cotton wool. A beam sent through the shade revealed the suspended matter. The platinum wire was then raised to whiteness. In five minutes there was a sensible diminution of the matter, and in ten minutes it was totally consumed. This proves that when the platinum wire is sufficiently heated, the floating matter, instead of being distributed, is destroyed.

But is not the matter really of a character which permits of its destruction by the moderately heated platinum wire also? Here is the reply:—

1. A platinum tube with its plug of platinum gauze was connected with an experimental tube, through which a powerful beam could be sent from an electric lamp placed at its end. The platinum tube was heated till it glowed feebly but distinctly in the dark. The experimental tube was exhausted and then filled with air which had passed through the red-hot tube. A considerable amount of floating matter which had escaped combustion was revealed by the electric beam.

2. The tube was raised to brighter redness and the air permitted to pass slowly through it. Though diminished in quantity, a certain amount of floating matter passed into the exhausted experimental tube.

3. The platinum tube was rendered still hotter; a barely perceptible trace of the floating matter now passed through it.

4 The experiment was repeated, with the difference that the air was sent more slowly through the red-hot tube. The floating matter was totally destroyed.

5. The platinum tube was now lowered until it bordered upon a visible red heat. The air sent through it still more slowly than in the last experiment carried with it a cloud of floating matter.

If then the suspended matter is destroyed by a bright red heat, much more is it destroyed by a flame whose temperature is vastly higher than any here employed. So that the blackness introduced into a luminous beam where a flame is placed beneath it is due, as stated, to the destruction of the suspended matter. At a dull red heat, however, and still more when only on the verge of redness, the platinum tube permitted the motes to pass freely. In the latter case the temperature was 800° or 900° Fahrenheit. This was unable to destroy the suspended matter; much less, therefore, would a platinum wire heated to 212° be competent to do so. Such a wire can only distribute the matter, not destroy it.

The floating dust is revealed by intense local illumination. It is seen by contrast with the adjacent unilluminated space; the brighter the illumination the more sensible is the difference. Now the beam employed in the foregoing experiments is not of the same brightness throughout its entire transverse section. Pass a white switch, or an ivory paper-cutter, rapidly across the beam, the impression of its section will linger on the retina. The section seems to float for a moment in the air as a luminous circle with a rim much brighter than its central portion. The core of the beam is thus seen to be enclosed by an intensely luminous sheath. An effect complementary to this is observed when the beam is intersected by the dark band from the platinum wire. The brighter the illumination, the greater must be the relative darkness consequent on the withdrawal of the light. Hence the cross section of the sheath surrounds the dark band as a darker ring.

The following four paragraphs, though printed nearly two months ago, have not been published hitherto. Might I say that whatever my opinion on the subject of "spontaneous generation" may be, I purposely abstain from expressing it here? That expression shall be given at the proper time. I desire now to show the practical value of the luminous beam as an investigator of the state of the air.

The question of "Spontaneous generation" is intimately connected with our present subject. On this point a kind of polar antagonism has long existed between different classes of investigators. Van Helmont gave a receipt for the manufacture of mice, and it was for ages firmly believed that the maggots in putrefying flesh were spontaneously produced. Redi, a member of the famous Academy del Cimento, destroyed this notion by proving that it was only necessary to protect the meat by a covering of gauze to prevent the reputed generation. In 1745 two very able men, Needham and Spallanzani, took opposite sides in the discussion, the forming affirming and the latter denying the fact of spontaneous generation. At the beginning of our own century, we find on the affirmative side Lamarck, Oken, and J. Müller; and on the negative Schwann, Schultze, and Ehrenberg. The chief representatives of the two opposing parties in our day are Pouchet and Pasteur.

The method of inquiry pursued in this discourse will, I think, help to clear the field of discussion. The experimenters do not seem to have been by any means fully aware of the character of the atmosphere in which they worked; for if this had been the case, some of the experiments recorded would never have been made. For example, to make the destruction of atmospheric germs doubly sure, M. Pouchet, the distinguished supporter of the doctrine of spontaneous generation, burnt hydrogen in air and collected the water produced by the combustion. Even in this water he afterwards found organisms. But supposing he had seen, as you have, the manner in which the air is clouded with floating matter, would he have concluded that the deportment of water which had been permitted to trickle through such air could have the least influence in deciding this great question? I think not. Here is a quantity of water produced and collected exactly as M. Pouchet produced and collected his. This water is perfectly clear in the common light; but in the condensed electric beam it is seen to be laden with particles, so thick-strewn and minute, as to produce a continuous cone of light. In passing through the air the water loaded itself with this matter, and hence became charged with incipient life.*

Let me now draw your attention to an experiment of Pasteur, which I believe perplexes some of the readers and admirers of that excellent investigator. Pasteur prepared twenty-one flasks, each containing a decoction of yeast, filtered and clear. He boiled the decoction, so as to destroy whatever germs it might contain, and while the space above the liquid was filled with pure steam he sealed his flasks with a blow-pipe. He opened ten of them in the deep, damp caves of the Paris Observatory, and eleven of them in the courtyard of the establishment. Of the former, one only showed signs of life subsequently. In nine out of the ten flasks no organisms of any kind were developed. In all the others organisms speedily appeared.

Now here is an experiment conducted in Paris, which shows that the air of one locality can develop life when the air of another locality cannot. Let us see whether we cannot here in London justify and throw light upon this experiment. I place this large flask in the beam, and you see the luminous track crossing it from side to side. The flask is filled with the air of this room, charged with its germs and its dust, and hence capable of illumination. But here is another similar flask, which cuts a clear gap out of the beam. It is filled with unfiltered air, and still no trace of the beam is visible. Why? By pure accident I stumbled on this flask in our apparatus room, and on inquiry learnt that it had been a short time previously taken out of one of the cellars below stairs. Other flasks were in the same cellar. I had three of them brought up to me; they were optically empty. The still air had deposited its dust, germs and all, and was itself practically free from suspended matter. You can now understand the impotence of the air of the Paris caves. The observation illustrates at once the influence of the germs and the accuracy of Pasteur.

* In this case a polished silver basin was soldered to one end of a wide brass tube; the tube was filled with ice, the hydrogen flame was permitted to play upon the basin, and the water of condensation was then collected. Dr. Child also objects to Pouchet's experiment.

The air of the cellar was afterwards examined by the electric lamp. Though less heavily charged than the air outside, it was by no means free from particles. This was to be expected, because the door of the cellar was frequently opened. The flasks themselves were the true tranquil chambers; on their sides the dust had been deposited, and to them it firmly clung. To prove this several flasks about ten inches in diameter were filled with common air, corked, and laid upon a table in the laboratory. After two days' quiet they were optically empty.

Nor is it necessary even to cork the flasks; for with their mouths open the air within them is scarcely disturbed, certainly not displaced. Two days' rest on one of the laboratory tables suffices to deposit the organic dust and to render the open flasks optically empty.

I have had a chamber erected with a view to experiments on this subject. The lower half is of wood, its upper half being enclosed by four glazed window-frames. The chamber tapers to a truncated cone at the top. It measures in plan 3 ft. by 2 ft. 6 in., and its height is 5 ft. 10 in. On the 6th of February this chamber was closed, and every crevice that could admit dust or cause displacement of the air was carefully pasted over with paper. The electric beam at first revealed the floating dust within the chamber as it did in the air of the laboratory. The chamber was examined almost daily; a perceptible diminution of the floating matter being noticed on each occasion. At the end of a week the chamber was optically empty, exhibiting no trace of matter competent to scatter the light. But where the beam entered, and where it quitted the chamber, the white circles stamped upon the interior surfaces of the glass showed what had become of the dust. It clung to those surfaces, and from them instead of from the air, the light was scattered. If the electric beam were sent through the air of the Paris Caves, the cause of its impotence as a generator of life would, I venture to predict, be revealed.

It cannot, I think, be doubted that the method of observation here pursued is destined to furnish useful control and guidance in researches of this nature.

Royal Institution, March 14 J. TYNDALL.

HEREDITARY GENIUS

Hereditary Genius, an Inquiry into its Laws and Consequences. By Francis Galton, F.R.S., &c. (Macmillan & Co.)

In this book Mr. Galton proposes to show that a man's natural abilities are derived by inheritance, under exactly the same limitations as are the form and physical features of the whole organic world. Many who read it without the care and attention it requires and deserves, will admit that it is ingenious, but declare that the question is incapable of proof. Such a verdict will, however, by no means do justice to Mr. Galton's argument, which we shall endeavour to set forth as succinctly as possible. He first discusses the classification of men by "reputation," and from a study of biographical dictionaries and obituaries for certain years taken at wide intervals, arrives at the conclusion that not more than 250 men in each million, or 1 in 4,000, can be termed "eminent"; and he shows what a small proportion that is, by the well-known fact that there are never so many as 4,000 stars visible to the naked eye at once, and that we feel it to be an extraordinary distinction in a star to be the brightest in the sky. These "eminent" men are the lowest class he deals with. The more illustrious names are as one in a million or one in many millions; but unless a man is so much above the average that there is only one like him in every 4,000, he is not admitted into the ranks of the eminent men on whom Mr. Galton founds his deductions.

He next discusses the classification of men according to their natural gifts. He shows first, that each man has a certain defined limit to his mental as well as to his physical powers, and that this limit is in most cases soon discovered and reached. He next shows the enormous difference that exists between mediocre and high class men, by the evidence of examination papers; the senior wrangler at Cambridge, for example, often getting thirty times as many marks as the lowest wrangler, who must himself be a man very far above the average. Statistics show, that the number of imbeciles and idiots are about the same per thousand as the eminent men. He then applies Quetelet's "law of deviation from an average" (which will be new to many of his readers), and deduces from it, that if men are divided into sixteen equal grades of ability, eight above and eight below the average, the six mediocre classes will comprise nineteen-twentieths of the whole; while it will be only the sixth, seventh, and eighth above the average who will rank as eminent and illustrious men, and form about one in four thousand of the adult male population.

The next chapter relates to the important question on which, indeed, the possibility of any solution of the problem depends, of whether "reputation" is a fair test of "ability." The subject is very ably discussed, and it is, I think, proved, that notwithstanding all the counteracting influences which may repress genius on one side, or give undue advantage to mediocrity on the other, the amount of ability requisite to make a man truly "eminent" will, in the great majority of cases, make itself felt, and obtain a just appreciation. But if this be the case, the question of whether "hereditary genius" exists is settled. For if it does not, then, the proportion of mediocre to eminent men being 4,000 to 1, we ought to find that only 1 in 4,000 of the relations of eminent men are themselves eminent. Every case of two brothers, or of father and son, being equally talented, becomes an extraordinary coincidence; and the mass of evidence adduced by Mr. Galton in the body of his work, proves that there are more than a hundred times as many relations of eminent men who are themselves eminent, than the average would require.

Turning now to the concluding chapters of the book, we meet with some of the most startling and suggestive ideas to be found in any modern work. The law of deviation from an average enables us to determine the general intellectual status of any nation, if we are able to estimate the ability of its most eminent men, and know approximately the amount of the population. We have these data in the case of ancient Attica; and Mr. Galton arrives at the conclusion, that the Athenians of the age of Pericles were, on the lowest possible estimate, nearly two whole grades of ability higher than we are. With all our boasted civilisation, and the vast social and scientific problems with which we have to grapple; with all our world-wide interests, our noble literature, and accumulated wealth; the intellectual status of the most civilised modern nation is actually lower than it was more than two thousand years ago! Well may Mr. Galton maintain that it is most essential to the well-being of future generations that the average standard of ability of the present time should be raised. Not less striking is his exposition of the effects of prudential restraints on marriage, on the general character of a nation. If one class of people, as a rule, marry early, and another class marry late in life, the former have a double advantage, both in having on the average larger families, and in producing more generations in each century. But, by the supposition, it is the imprudent who gain this advantage over the prudent; and Mr. Galton therefore denounces the doctrine of Malthus, that marriage should be delayed till a family can be supported, unless the rule could be imposed on all alike. I hardly think that this argument is sound, and I doubt if the imprudent who make early marriages do, in the long run, increase more rapidly than the prudent who marry late. Increase of population depends less upon the number of children born, than on those which reach manhood; and I believe that the prudent man who has acquired some wealth and wisdom before he marries, will give to the world more healthy men and women, than the ignorant and imprudent youth, who marries a girl as ignorant and imprudent as himself. It is also to be remembered that the men who marry late often marry young wives, and have as good a chance of large families as the imprudent.

Mr. Galton traces the long-continued darkness of the Middle Ages, and our present low intellectual and moral status, to the practice of celibacy and to religious persecution. Whenever men and women were possessed of gentle natures, that fitted them for deeds of charity, for literature, or for art, the social condition of the times was such that they had no refuge but in the bosom of the Church; and the Church exacted celibacy. Those gentle natures left no offspring; and thus was the race of our forefathers morally deteriorated. The Church acted as if she had aimed at selecting the rudest portion of the community for the parents of future generations; and the rules as to fellowships at our Universities are a relic of this barbarous custom, being bribes to men of exceptional ability not to marry. Religious persecution acted in the same way. The most fearless, truth-seeking, and intelligent were year by year incarcerated in dungeons or burned at the stake; so that, by this twofold selection, human nature was brutalised and demoralised, and we still feel its hateful effects in the long-continued antagonism to the essential requirements of an advancing civilisation. These concluding chapters stamp Mr. Galton as an original thinker, as well as a forcible and eloquent writer; and his book will take rank as an important and valuable addition to the science of human nature.

ALFRED R. WALLACE

SPECTRUM ANALYSIS

Die Spectral Analyse in ihrer Anwendung auf die Stoffe der Erde und die Natur der Himmelskörper. By Dr. H. Schellen, director der Realschule I.O., Cologne. (Brunswick, Westermann, 1870. London: Williams and Norgate.)

THIS book contains an accurate and luminous account of the recent discoveries in celestial chemistry and physics, and especially of the researches of our countrymen Huggins and Lockyer. As regards the completeness of that portion of the work bearing directly upon terrestrial chemistry, readers will, I fear, be disappointed. The first division of the book is devoted to a description of the means employed for the artificial evolution of light and heat of great intensity, beginning with combustions in oxygen, and ending with the electric-light. The second division is headed "The simple and compound spectra in their application to terrestrial matter;" whilst in the third and most important division Schellen considers the application of spectrum analysis to the heavenly bodies. The illustrations throughout the work are good, though many of them are not new, and are borrowed, without acknowledgment, from other books.

With respect to the physical constitution of the sun, it behoves us in this, the infancy of our knowledge, to be very careful in drawing positive conclusions. In the first place, there is no doubt that whilst Kirchhoff's original theory must undergo certain modifications, it will remain in its grand features as having first pointed out to us the true physical condition of the sun. The discovery of the chromosphere by Mr. Lockyer, in which, as a rule, only the bright hydrogen lines are seen, together with the yellow mysterious line of unknown origin, renders it difficult for us, especially if we accept Frankland and Lockyer's conclusions respecting the excessive tenuity of the upper chromospheric layers, to suppose that an atmosphere containing iron and the other 13 difficultly volatilisable metals can exist outside the chromosphere of sufficient density to effect such a powerful selective absorption as we see in the darkness of Fraunhofer's lines. Hence we should be inclined to agree with Lockyer that the absorption does not take place, as Kirchhoff suggested, in a far outlying layer of solar atmosphere, or in what we term the corona, but that the dark lines are produced within the chromosphere. But, on the other hand, upon what known physical basis are we entitled to assume that the higher lying portions of the solar atmosphere consist almost entirely of glowing hydrogen gas, whilst the lower lying layers contain the more easily condensible gases of the other 14 elements? The well-known laws of gaseous diffusion (to say nothing of the cyclones of vast magnitude and of enormous rapidity, which Lockyer has taught us are constantly mingling up the various layers of solar atmosphere), forbid us to suppose that the lighter hydrogen gas can ascend whilst the heavier metallic gases remain quietly below. If the components of the solar atmosphere are gaseous, they must be uniformly, or nearly uniformly, mixed. How then can we account for the constant presence in the chromosphere of the hydrogen lines, whereas the lines of the other constituents of the solar atmosphere are scarcely visible, except in special cases of the occasional projection of the vapours of magnesium and other metals, whilst the absorption is to occur in a lower gaseous layer, having a totally different composition?

Another point to be remembered is, that according to the law of exchanges the fact of the existence of absorption *necessitates* the existence of a lower temperature in the absorptive medium than in the *media* (either above or below) in which such absorption is not exhibited, and which may either give continuous or broken spectra, according to the physical and chemical nature of the incandescent bodies. How then can the iron and magnesium vapour exist nearer to the white-hot body of the sun than the hydrogen and yet possess a lower temperature? I am here forcibly reminded of the plausibility of a suggestion thrown out by Kirchhoff, in a conversation with me a few weeks ago, viz.: that the upper regions of the solar atmosphere may be constantly illumed by discharges of electricity; that the incandescent hydrogen may be heated not from below but from within its own mass, either by continuous flashes of lightning or constant auroral discharges; and, indeed, Zöllner has noticed the flashing out of certain bright points in the prominences, which may possibly be caused by solar lightning.

We must also bear in mind that the existence in the sun of a solid or liquid white-hot nucleus, as originally assumed, is not proved by the results of subsequent research; inasmuch as we learn from the recent researches of Frankland, Lockyer, and Wüllner (as indeed we may do from much older experiments), that incandescent gases under certain physical conditions emit white light and yield a continuous spectrum. So that spectrum analysis does not give us any *certain* information as to the physical state of that portion of the sun's body from which the main portion of light and heat proceeds.

H. E. ROSCOE

OUR BOOK SHELF

Essays on Physiological Subjects. By Gilbert W. Child, M.A., F.L.S., F.C.S. Second edition, with Additions; pp. 293. (London: Longmans, Green, and Co. 1869.)

THE present edition of Dr. Child's work is by no means a mere reprint of the last. It has undergone considerable modifications, chiefly in the form of additions, which will tend to make it more acceptable to a large class of readers. There is an almost entirely new essay on "Some Aspects of the Theory of Evolution," in which he endeavours to show how this theory is related to religious belief. He believes its proper meaning and tendency to have been much misunderstood; that far from being an "atheistical" conception, it is in reality only the scientific form of natural religion. The subject of "Physiological Experimentation on Animals" is also considered, whilst the last and longest essay, also new, is entitled "Physiological Psychology," in which he endeavours to make known to persons whose chief interest is in psychological rather than physiological science, all the chief points in the anatomy of the nervous system, necessary to be understood before he could explain, as he also attempts to do, the principal physiological conclusions which have been arrived at concerning brain action and mind.

Monographie der Molluskengattung Venus, Linné. 1 *Band. Sub-genus Cytherea, Lamarck.* Von Dr. Eduard Römer. 4to. (Cassel, 1869.)

NATURALISTS have been divided into those of the field and those of the closet. The author of this monograph may be classed in the latter category; and he certainly shares the indefatigable industry of his countrymen. Such labour, however, when applied to subjects of natural history, sometimes tends to an excessive multiplication of species; and its utility is in that respect questionable. Professor Römer, in a critical examination of the species of *Venus* which he published in 1857, enumerated 145 species arranged in eight sub-genera. In the present work one only of these sub-genera is treated, and includes no less than 209 species. We may well ask, with Cicero, "Quousque tandem abuteris patientiâ nostrâ?" A common European species (*Circe minima*) is described in two sections under different names; and some of the author's new species seem to be merely the young of well-known forms. The method in which he subordinates this host of species is unusual. Eight sections of the sub-genus *Cytherea* are named and described; and the specific names are applied, not to the genus or even to the sub-genus, but to each section. The sectional name is used in a generic sense; so that *Venus meretrix* becomes *Meretrix meretrix*, and *V. Dione* is converted into *Dione Dione.* The description of species is not in every case consistent with the sectional characters. In the first section, *Tivela*, the shape is stated to be "trigona;" but in *T. nitidula* we find it is "ovato-elliptica," and in *T. nucula* "cordato-ovata." It would also be more convenient to have the descriptive characters given in the same order throughout. In the description of the first species colour takes precedence of sculpture; in that of the second species the order of these characters is reversed. The same confusion occurs as to the teeth and pallial scar as well as to other characters. But the excellence of the illustrations compensates to a great extent for the small blemishes which it is the unpleasant duty of a critic to point out. The plates are fifty-nine in number and contain many hundred figures, all of which are evidently truthful, admirably engraved, and exquisitely coloured. The monograph must be indispensable to collectors, who are better pleased with a redundancy than with a paucity of species. Dealers have the same feeling.

J. GWYN JEFFREYS

Abstracts of Two Papers on the Geography of Disease. I. The Geographical Distribution of Heart Disease and Dropsy in England and Wales. II. The Geographical Distribution of Cancer in England and Wales. By Alfred Haviland, M.R.C.S. Pp. 18. (London, 1869.)

THIS pamphlet contains a reprint of two papers, in which the author has endeavoured to map out the districts in which the particular forms of disease above-mentioned are most frequently encountered. The subject of his geographical distribution of disease is a most important one in its bearings upon the great question of the causation of disease. It is a subject, however, in which the facts should be many and the conclusions few, if he who draws them wishes to make lasting contributions to the science of medicine.

Leçons sur la Physiologie Comparée de la Respiration. Par Paul Bert. (Paris: Baillière. London: Williams and Norgate.)

IN his preface, the author to some extent apologises for the imperfections of this volume, on account of the difficulties, including "the singular penury of the laboratory over which he presided," attending the delivery of the course of lectures of which it forms the report. No apology, however, is needed for an interesting and admirable series of discourses on a difficult and yet important topic. The book does not profess to be a complete Treatise on Respiration, but rather treats fully of certain selected points, such as the respiration of tissues, the gases of blood, the respiratory mechanism in various classes of animals, asphyxia, &c., &c. The "graphic method" is employed throughout, by far the larger number of the illustrations being representations of various respiratory movements. We would especially call attention to the chapters on the respiratory movements of fishes, amphibia, reptiles, and birds, in which the graphic method brings out many singular and interesting facts. Even in matters of science, national characteristics come to the surface; and quite apart from the language, it is generally an easy matter to distinguish the work of a Frenchman from that of German or an Englishman. Prof. Bert's work is no exception to the rule, though we must add, with singular pleasure, that it is far more cosmopolitan than many of the writings of his fellow countrymen. The author has evidently studied and appreciated the labours of countries other than his own.

Systematische Beschreibung der bekannten europäischen zweiflügeligen Insekten. Von J. W. Meigen, Achter Theil, von Hermann Loew, Erster Band. Schmidt, Halle, 1869. (London: Williams and Norgate.)

PROFESSOR LOEW, who is to be regarded as the highest authority on European Diptera, contents himself in this book with supplying a sort of supplement to Meigen's great work on the insects of that order inhabiting Europe. It is, perhaps, to be regretted that he does not rather direct his efforts to the production of a complete systematic work on the subject, but he probably thinks that the time is not yet ripe for such an undertaking, and in the meanwhile the full and detailed descriptions of species detected since the publication of Meigen's last volume will be most welcome to entomologists. The present volume contains descriptions of 182 species of two-winged insects, belonging to various families from the *Tipulidæ* to the *Dolichopodidæ*, and especially of numerous forms of *Asilidæ* and *Bombyliidæ*. No fewer than 138 of the species are described as new, and the greater part of the remainder are species described by Professor Loew himself in various scattered papers.

Nachrichten von der K. Gesellschaft der Wissenschaften und der Georg-Augusts Universität zu Göttingen, aus dem Jahre 1869.

THE volume of "Reports of the Royal Society of Sciences and University of Göttingen," for 1869, which has lately reached us, contains a great number of papers of considerable value, for the most part relating to mathematics, physics, and chemistry. There are also some memoirs relating to literary antiquities, but natural history receives little attention, the only papers being a notice of some marine animals and their metamorphoses by Dr. E. Mecznikow, and a revision of the *Butomaceæ, Juncaceæ,* &c., collected by the Brothers Schlagintweit in Upper Asia by M. F. Buchenau, to which we may specially call the attention of botanists, as a good many new species are described in it.

Via Medica. A Treatise on the Laws and Customs of the Medical Profession, in relation especially to Principals and Assistants; with Suggestions and Advice to Students on Preliminary Education. By J. Baxter Langley, M.R.C.S., F.L.S. Third edition. (London: R. Hardwicke. 1869.)

THIS little work is full of most useful information concerning the subjects indicated on its title-page. That it meets a demand for information of this description is sufficiently indicated by the rapid sale of the two previous editions.

LETTERS TO THE EDITOR

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

The Geological Calculus

MR. WALLACE'S essay, completed in NATURE, No. 18, brings to the front the question whether or no we can measure the Geological Past by the historical unit of years. Have we any basis for fixing with any certainty the date of any geological epoch? Mr. Wallace's answer in the affirmative to this is worthy of a careful analysis, because it represents fairly the ideas current in the minds of many geologists.

There are three ways by which the attempt to solve the problem has been made:—(1.) The slow geological changes which have been noted during the period of history, such as the modification of coast line, the silting up of estuaries, and the like. This method Mr. Wallace very justly discards as being "too minute, too limited, and too uncertain to afford the basis of even any approximate measurement" of the geological past. (2.) The change in organic life. This also is a unit of measurement "which we have not yet been able to get; for the whole length of the Historical Period has not produced the slightest perceptible change in any living thing in a state of nature." Professor Huxley, in 1869, gave expression to very much the same view, in his Presidential Address to the Geological Society. This method, therefore, of approaching the problem may also be given up as hopeless. (3.) The excentricity of the earth's orbit which Mr. Croll has used, in making his ingenious computation of the lapse of time since the glacial period, on the hypothesis that the severity of climate at that time was due solely to astronomical causes, and not, as had been previously supposed, to changes in the physical geography of the earth. But, as Sir Charles Lyell argues, since the distribution of land and water and the course of marine currents now modify climate, they cannot be fairly supposed to have had no share in causing the severity of the glacial period. And therefore, the fact that they are ignored in Mr. Croll's computation, destroys its value as fixing the glacial date, although there may be astronomical reasons for a depression of temperature at certain times in the northern hemisphere without the aid of any terrestrial agent. This, indeed, is practically admitted by Mr. Croll, when he reduces the date of the last glacial period from 750,000 to about 80,000 years ago, because of the amount of sub-aerial denudation that has taken place since that time. There are, moreover, two fatal objections to any estimate that can be formed of the amount of denudation since the glacial period. First of all, the denudation now going on, over any wide area, has not yet been ascertained with anything like accuracy, and it acts unequally, even in any one limited region. Secondly, as we do not know the original thickness of the glacial deposits, or the extent to which the existing valleys were excavated in preglacial times, it is impossible to estimate the amount of denudation since that period, even if we had trustworthy data from our own experience. This third method, therefore, of measuring geological time, is not more satisfactory than the former ones.

Mr. Wallace, however, assumes in the second part of his essay that the *vera causa* of the glacial epoch was the high excentricity of the earth; and then he proceeds to reduce Mr. Croll's lowest estimate by 20,000 years, by using precisely that argument of observed change in physical geography which in the first part was discarded as "too uncertain." From this untenable standpoint hangs the following chain of reasoning.

"Now it is most important to observe that, for the last 60,000 years, the excentricity has been very small—for three-fourths of the time less than it is now. During this time the opposite phases of precession, each lasting 10,500 years, will have produced scarcely any effect on climate, which in every part of the earth will have been *nearly uniform for that long period.* But this is quite an exceptional state of things; for the curve of excentricity shows us that, during almost the whole of the last three million years, the excentricity has been high—almost always twice, and sometimes three and four times as much as it is now. If, therefore, Mr. Croll's theory be correct, there will have been a change each 10,500 years during this vast period (in all the extra-tropical regions at least) from a very cold to a very mild climate. This will necessarily have caused much migration, both of plants and animals, which would inevitably result in much extinction and comparatively rapid modification. Allied races would be continually brought into competition, altered physical conditions would induce variation, and thus we should have all the elements for natural selection, and the struggle for life, to work upon and develop new races. High excentricity would therefore lead to a rapid change of species; low excentricity to a persistence of the same forms; and, as we are now and have been for 60,000 years, in a period of low excentricity, *the rate of change of species during that time may be no measure of the rate that has generally obtained in past geological epochs.* Thus we should have explained the extraordinary persistence of organic forms during the historical period, as well as during the preceding Neolithic age, although slight changes of climate and of physical geograghy have undoubtedly taken place; and it would prove to be not so much the *usually* slow rate of organic change, as the fact of our living in the midst of an *exceptionally uniform climatic epoch*, that has hitherto prevented us from obtaining a measure of the average duration of species."

The major premiss latent in this argument is, that all climatal change from the glacial epoch to the present day has depended solely on the excentricity of the earth's orbit, a proposition which Mr. Wallace himself would be the last to endorse. If it be admitted that the alteration of a marine current here, or the elevation of a sea-bed there, be factors in climatal change, the estimate of 60,000 years in which they are not reckoned is without value. The study of the mammalia, of historic, pre-historic, and post-glacial times does not warrant the conclusion that the persistence of organic forms was "extraordinary," nor the recognition of an "exceptionally uniform climatic epoch." The mammals have exhibited on the whole a steady diminution *in size* from the post-glacial to the present day, owing probably to the fact that they have been worried off their feeding and hunting grounds by man. The non-development of new species during that time may be ascribed to its short duration as compared with past geological epochs, rather than to exceptional conditions of life caused by exceptional excentricity. The deposits in Britain since the glacial epoch are a mere surface film compared with those of previous geological periods. The steady northern retreat of the reindeer during historic times, taken in conjunction with its pre-historic range, testifies to a gradual increase of temperature in central and northern Europe, to say nothing of the historical evidence of the former severity of winter in Gaul and Italy from Cæsar's time to the present.

There is another point which ought not to be omitted. Mr. Scott Moore is quoted as maintaining that the group of mammalia commonly called "Quaternary" is pre-glacial because of the "striking fact, that none of the supposed pre-glacial gravels ever rest on the boulder clay, but always on an older rock, which could hardly have been the case in every instance were they all post-glacial." So far from this being true, the famous Bedford section proves, as Mr. Wyatt showed in 1860, that the mammaliferous and flint-implement producing gravels are post-glacial without the possibility of a doubt. On the Norfolk coast mammaliferous gravel overlies the boulder clay in certain places. The mammalia found in making the Ipswich tunnel were derived from a river deposit, clearly of later date than the boulder clay of the district. Very many other cases might be quoted that would show that this sweeping generalisation is without any foundation in fact.

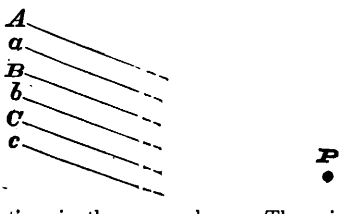
But can we measure geological time by the lapse of years? If so, we shall have solved a problem infinitely harder than that which has foiled the archæologists. Can they fix the date, say of the introduction of iron into Europe, or of the dawn of the age of bronze or of stone? No man would venture to answer yes. Modern historians are becoming more and more alive to the worthlessness of the so-called chronology of the Assyrian kings and of the Manethonian dynasties. If, then, we are ignorant of the date of any one of these events, which are, comparatively speaking, of yesterday, and we can simply tell that one succeeded another in a definite order, how can we reasonably expect to fix the date of any one period of the geological past? The attempt can only be made by forsaking those laws of rigid induction by which geology has become a science—by the assumption of a premiss which we have no right to assume. The strict interpretation of geological phenomena only warrants our saying that, one event, say the deposition of the chalk, took place in Europe after another,—the deposition of the Neocomian strata,—how much after none can tell. In other words, the geological "when" merely implies before and after, while in history the idea not only of sequence, but of the lapse of how long before and how long after, can be mastered. The attempt to fathom the geological past with our short historical sounding line has up to the present time resulted merely in estimates, varying according to the assumed basis in each by thousands of centuries, that have been about as valuable in geological theory as Archbishop Usher's chronology has been found in Biblical criticism. The problem is hedged in by innumerable difficulties, which cannot be overcome in the present state of science.

W. Boyd Dawkins

On the Diffraction Spectrum and Wave Lengths

The letter which I wrote about the diffraction spectrum has called forth several inquiries. I have been asked how it is that in that spectrum the position of a line depends only on its wave-length, and I may take the opportunity of answering these questions through your columns. First, however, let me state that the numbers given in the table in my letter of February 9 are copied, including the obvious errors, *verbatim* from the translation of Mossotti's memoir in vol. v. of Taylor's "Scientific Memoirs." I attach no importance to such analogies. Any analogy between these lines and interdependent notes of music must, I expect, be entirely accidental. The latest experiments made by Mr. Lockyer have shown that the lines given out by a gas vary according to temperature and pressure, and if these be caused by vibrations of the particles of the gas, whether atomic or molecular, the periods of these vibrations must be dependent on the temperature and pressure. A distinct numerical statement of this interdependence is a great desideratum in this part of science with a view of affording the materials for making and testing a mechanical theory of that interdependence. These are the great problems which demand our attention, as I take it, with respect to the fixed lines, and our ability to correlate light with other modes of force.

If a beam of homogeneous light be admitted through a small hole into a dark room, and fall on the opposite wall, it will illuminate only the portion of the wall directly opposite it. But if a series of fine parallel wires, or a fine grating be put across the hole, there will be seen ranged in a line perpendicular to the grating a series of nearly equal distant spots, or rather streaks of light. This is because the wires of the grating shut out those waves of light which at these points would destroy the other waves. To understand this more clearly, suppose A*a*, B*b*, &c., &c., to be a number of consecutive and equidistant points in the front of a wave coming through a small hole, A*a*, &c. These may, by the principle of Huyghens, be regarded each as the origin of a new wave, together having the same effect as the original



wave, all starting in the same phase. There is darkness at every point on an opposite wall, except directly opposite the hole, because of the interference of these elementary waves, which destroy one another everywhere except exactly in front. This is owing to the shortness of the waves and the number of sources. Let P be a point on the wall which we shall suppose sufficiently far from the hole in order that the lines PA, Pa, PB, Pb, &c., may be regarded as all sensibly parallel. On that account also each of these lines exceeds the one next it by the same amount. Now, generally, when the waves from A, *a*, &c. arrive at P, the wave from *a* will not entirely blot out that from A, but a residue will be left, which will be blotted out by other similar residues. But there will be certain points along the wall at which the waves will blot one another out in a peculiarly regular way. We shall suppose P to be one of these points, namely, that each of the distances A P, *a* P, &c., exceeds the one next it by exactly some odd multiple of the length of half a wave—say in this instance half a wave's length. Then in this case the disturbance originating at *a* will arrive at P exactly half a wave's length in advance of that from A, and the wave from A will be exactly blotted out, so to speak, by that from *a*; similarly that from B will be exactly blotted out by that from *b*; and so on. So that if I were to put wires at *a*, *b*, *c*, &c., so as to stop up the waves entering there, the waves from A, B, C, &c., would fall at P each undestroyed and assisting one another, each disturbance being an exact wave-length behind the other, and therefore all in the same phase. Thus light would be restored at P. And similarly at other points where the difference between A P, *a* P, &c., is three, five, &c., semi-wave-lengths. Light would be restored in the same way at no other places because the elementary waves do not at other points destroy one another in this peculiar alternative manner. Supposing *a*, *b*, *c*, &c., to be the centres of the wires, A, B, C, &c., to be the centres of the spaces between, a more refined consideration shows us that the position of P does not depend on the space A *a* being equal to the space *a* B, but only on the total length A B (or *a b*). Hence we have a very accurate method of determining the wave-length from the position of P. For if θ be the angle between the line drawn from the hole straight forward in the direction of the ray and the line drawn to P, then the difference between A P and B P, which is a wave-length, is also A B sin θ. So that to get the wave-length all that we have to do is to measure θ and A B. A B may be very accurately got by ruling the grating on glass with a machine, and counting the whole number of rules given and the total space which they occupy. Adaptations of this method may be made to suit circumstances, as for instance the grating may be attached to the object glass of a telescope used for measuring the angle θ. It is by such a method that the wave-lengths have been found for the principal fixed lines in the solar spectrum, and the wave-lengths of all other lines are determined from these by some formula or other which may best suit the views of the calculator; the constants of the formula being determined so as to constrain the formula to satisfy the truth at those fixed lines whose wave-lengths are got directly from the refraction spectrum. Uncertainty, therefore, prevails about all wave-lengths thus obtained, although, of course, the uncertainty must, from the method of calculation and the number of lines whose wave-lengths have been obtained from the diffraction spectrum, lie necessarily between narrow limits. These latter are the only wave-lengths, however, which have the recommendation of being due to direct observation; and the method of obtaining the wave-length by observation from the diffraction spectrum is one capable of such accuracy, that I have sometimes considered that the wave-length thus determined might be used as an absolute and recoverable standard of linear measure.

Of course, the preceding investigation must not be considered to be perfectly exhaustive in giving the whole character of the phenomena, which are explained by a more refined investigation, in which the space between each wire is regarded, not as the source of one, but of an infinity of waves, so that the application of Huyghens' principle becomes rigidly correct. By this means the difference of the spaces occupied by the wire and the opening is found to have no effect on the position of P, but a certain effect on the brightness there, which, under certain circumstances, causes one of the spectra entirely to disappear: an experimental result which, being thus deducible from the theory of undulations, gives a striking proof of the power of that theory to account for phenomena.

James Stuart

Trinity College, Cambridge, March 2

The Valuation of Liquid Town Sewage

If there is one thing that is more to be deprecated than another, it is the unnecessary importation of personalities into a scientific discussion, or indeed into any serious matter of business, and fortunately this is not now a common proceeding in this country. It was therefore with a feeling of unqualified disappointment that I read Dr. Paul's personal attack on myself in your columns.

Whenever I hear or read an opinion or statement which I believe to be erroneous, I endeavour to point out its fallacy, and shall always continue to do so. It is the undoubted right, as it is in some degree the duty, or every man to do this. But opinions may be criticised and condemned without any reflection on the man who holds them. This is all I did in Dr. Paul's case, and I certainly endeavoured to do it good-naturedly; at all events I not only made no reflection whatever on Dr. Paul's character, but I did not even mention his name. He had, in his article, hazarded an opinion on a question of practical farming, of which, so far as I know, he does not profess to have any personal knowledge or experience whatever. This opinion I knew from experience to be utterly erroneous, although exceedingly plausible. I therefore did my best to point out its fallacy, partly with a view of convincing him. He replies, not by arguments, but by a personal attack on me, the acrimony of which I deplore but cannot explain, and by re-asserting his opinion as "a fact long accepted as beyond question . . . . by authorities too numerous to name." In short, it appears to be "a fact" which the proverbial schoolboy ought to know; but I am never more pleased than when the schoolboy with his universal knowledge is brought out against me. It is generally a sure sign that the assumed "fact" has no other foundation than that intelligent youth's imagination.

In the present case I say that if a man buys sewage he should buy it as he would any other manure, on the basis of a chemical analysis. I say that this is the *only* safe and reliable basis on which to found a calculation of its money value. And I say that if a farmer puts so much money's worth of manure into his land, he is simply a bungler if he does not get it out again with its proper increment of profit, whether he buries it in his land by means of a plough, or of a spade, or of water. Dr. Paul, on the other hand, says that if a man pays so much money for so much sewage because it contains a certain percentage of ammonia he will be ruined. Why? Because of the water.

Of course we all know that water applied to certain crops in certain stages would spoil them; so would manure. If, therefore, a farmer applies sewage under such conditions, he is a bungler. His skill, as a sewage farmer, is shown in so arranging his land and crops as never to injure but always to benefit them by the application of his sewage. This is simply a question of good *versus* bad management. It is one to be decided on the farm and not in the laboratory. Incredible as it may appear to Dr. Paul, on taking a lease of the sewage of the town of Romford, although bound, under penalty, to use it every day in the year, I stipulated for an additional dilution of the sewage to the extent of twenty gallons of water per head of the population *per diem*, and this although I have not got the proportion of "twenty-five acres for every 100 persons," as Dr. Paul says I recommend, but which I never recommended, and should be the first to condemn.

Having defended my opinion, I will now, with your permission, defend my character. Dr. Paul regrets that I should "declare myself a partisan of one particular solution of the town-refuse problem." It may be that he has so recently commenced the study of this large question that he has, as yet, formed no opinion upon it except that by irrigation the value of the manure cannot be recovered; but I have laboured at it for many years, and it is not possible that in those years I should not have formed some very distinct opinions. Will any one else regret that I should "declare" what those opinions are? I think not; and I think that such a declaration is straightforward and honest, though why Dr. Paul should affect to think that I have made it now for the first time I cannot say. My views on this subject have been publicly expressed for many years, and have been so expressed in his presence. Dr. Paul complains that I desire "the promotion of a project at any price." I do. I desire to see sewage utilised all over the country, and by irrigation, if possible, because I believe it is the right thing, and I am gratified to find that my views are in exact accord with those expressed in the unusually careful and able report just published by the Royal Commission on Pollution of Rivers. But when I joined the Committee of the British Association on Sewage, I at once suggested that the scope of their inquiry should be enlarged so as to include a full investigation into every proposal for the utilisation of sewage which presented any appearance of practicability. I also suggested a source from which the necessary funds might be obtained. My suggestions were approved both by the Committee and by the Council of the Association, the funds have been in great part obtained, and the inquiry is about to be prosecuted. Whether Dr. Paul is justified in the personal attack he makes upon me, I will therefore leave to the judgment of your readers.

I will merely add, as regards my opinions, that they were condemned in the most unqualified and unmeasured language by Baron Liebig five years ago; but I have lived to prove Liebig mistaken in this instance, and, on the practical farming part of the question, I think I may claim to be a very much better authority than Dr. Paul. At all events, as the German philosopher said of the author of the Epistle to the Corinthians, "I do not agree with your Dr. Paulus," and I have yet to learn that such a disagreement involves the breach of any law, human or divine, although Dr. Paul is evidently firmly convinced that while *he* has an undoubted right to express "a foregone conclusion," such an expression on *my* part is a sign of great moral depravity. This is a common form of superstition, but it is scarcely scientific, and seldom adds much weight to a man's opinions.

W. HOPE

Parsloes, March 6

Transactions of Scientific Bodies

I WISH it were possible to induce our learned societies to be a little more liberal; it should be their aim to spread knowledge, not make it a luxury for the wealthy. I happen to wish to read a paper by Professor Tait on "Rotation," published in the Transactions of the Royal Society of Edinburgh. The only libraries I have access to are those of the British Museum and London Institution. At the Museum there is no volume of the "Transactions" later than 1864; at the London Institution no volume later than 1862; so that if I persevere in my intention of reading the paper, I must buy the volume containing it, for which I must pay 2*l.* 2*s.*—that is, I must buy thirteen papers I don't want in order to be able to read one which I do want: these include one on the temperature of newly-born children, and another on tetanus in cold-blooded animals.

All papers should be published separately; this would lead to a much wider diffusion of them, and the Societies would benefit by their increased sale.

London, March 7 G.

Sir. W. Thomson and Geological Time.

THE *North British Review*, for July last, thoroughly exposes the inaccuracy of the quotation from Prof. Thomson, referred to by your correspondent G.H., in its article devoted to the consideration of Geological Time.

J. S.

YOUR correspondent G.H. will find in one of Thomson's papers something very like the assertion "that there was a time when the earth rotated too swiftly for the existence of life," but expressed in a manner at once more precise and less pleonastic. "The existence of life" reminds me of a phrase which I heard a few days ago from a female beggar; she lamented that her husband had "fallen into habits that are habitual." Well; the required reference is the paper "On Geological Time," in the Transactions of the Geological Society of Glasgow, vol. iii. Part I. pp. 15 and 16 (§§ 19 and 20). A thousand million years ago, says Thomson, "there must have been more centrifugal force at the equator due to rotation than now, in the proportion of 64 to 49. . . . If the earth rotated seventeen times faster, bodies would fly off at the equator. . . . If you go back ten thousand million years ago—which, I believe, will not satisfy some geologists—the earth must have been rotating more than twice as fast as at present—and if it had been solid then [which he thinks improbable], it must be now something totally different from what it was." Such a state of things he seems to consider inconsistent with any organic life such as we know of. Surely the connection of this question with the argument from retardation by tidal friction is too plain to need exposition.

Ilford, March 11 C. M. INGLEBY

How large seems the Moon?

READING Mr. R. A. Proctor's communication under the above heading in NATURE of March 3rd, reminds me of an experiment I tried some time ago. I imagined I should get all sorts of answers to the question, varying from "a fourpenny-piece" upwards, without any particular size being more frequently pitched on than any other. I did not collect more than about twenty or thirty replies, but they were sufficient to show that, contrary to my expectation, *one to two feet* was assigned more frequently than other sizes. Mr. Proctor says the estimate of a foot in diameter assigns a distance of 115 feet to the moon. If he were to try to convince the observer of the soundness of this deduction, the latter would probably meet him with vehement reiteration that he only means the moon *looks* a foot large. It seems to me fairer to say that such a man thinks a two-foot rule 115 feet off, a fit and proper measure for celestial objects. I think many, who are aware of the futility of attempting to convey their ideas to other minds by these comparisons, yet involuntarily make them in their own. I am conscious of a lurking idea that the moon is more like a fourpenny-piece in size than anything else. The question is, what is the cause of the "personal equation" which determines for each individual the distance of the imaginary two-foot rule—and why is about 115 feet more common than other distances?

I think it probable the sort of objects with which a man is familiar in his daily life may have some influence on his judgment in this respect. The men I questioned were for the most part engaged in warehouse or out-door work. I should like to know what answers watchmakers or jewellers would give. This theory, however, fails to account for the different estimate formed by the same individual when the moon is high above, or on the horizon; but I imagine, in the latter case, the imaginary rule is superseded, or more properly modified, by the terrestrial objects which are in the field of vision with the moon.

Cardiff, March 7 GEORGE C. THOMPSON

Cuckows' Eggs

MAY I be permitted to make a few observations upon Mr. Sterland's letter in your issue of the 27th of January, relative to the cuckows' eggs' controversy.

In answer to Prof. Newton's query, "If the eggs in question were not cuckows', what birds laid them?" Mr. Sterland says, "My reply is simply that they were laid by the birds in whose nests they were found."

Besides the well-known fact mentioned by Mr. Newton (NATURE, p. 266), "that when birds lay larger eggs than usual the colouring is commonly less deep," which tells so strongly against Mr. Sterland, I will only mention the following instances.

1st. The egg No. 9 in the series given by Herr Baldamus, (see *Zoologist* for April 1868), which the Royal Forester, Mr. Braune, found *in the ovary* of a just-killed cuckow, and which "was coloured exactly like the eggs of Hypolais."

2ndly. The egg No. 26 in the same series, belonging to the collection of Dr. Dehne, described as a "light-greenish blue egg without any markings," and "might have passed for the egg of either species of the redstarts," which specimen "was *laid in a cage* by a cuckoo that was caught in a hay loft." (The *italics* are mine.)

3rdly. The two instances given by Mr. H. E. Dresser, (NATURE, p. 218) of two eggs of the cuckow "closely resembling" those "of the common bunting (*Emberiza miliaria*)," one found in a blackbird's, the other in a robin's nest.

Can Mr. Sterland explain away the 1st and 2nd instances? and how does he reconcile the 3rd instance with his affirmation? Will he venture to say that the two apparent bunting's eggs were laid by a blackbird and a robin respectively, or, will he risk the remark that a common bunting had taken a cuckow-like freak into its head and been laying its eggs in other birds' nests? As either alternative is too absurd to be worth a moment's consideration, we can only conclude that they are cuckows' eggs, unless there has been some mistake as to the nests from which they were taken—scarcely likely, if Mr. Dresser's remarks are carefully read.

Therefore I think Mr. Sterland must admit, if he accepts these facts as authentic, that the cuckow's eggs *do* vary to a large extent, and doing so, he has little foundation for doubting the identity of the specimens mentioned by Herr Baldamus as taken from nests whose eggs they resembled.

For my own part I have every confidence in the discrimination of that ornithologist, and am not afraid that he had been carried away by a pet theory that led him to *imagine* this or that egg taken "out of the nest of the hedge-sparrow or tree-pipit" to be a cuckow's merely because it is "an egg rather larger than the rest, but marked and coloured in a similar manner." If Mr. Sterland will carefully examine Herr Baldamus's evidence he will find that it is not of such a superficial character.

I agree with Mr. Sterland that it is certainly singular that British and Continental observers should come to such opposite conclusions as to this variation of the cuckow's egg in their respective countries; but this is no reason for impeaching (merely because our experience differs) the testimony of the eminent Continental oologists who affirm this extreme variation, and to some of whom Herr Baldamus's theory is probably unknown; as instance, in the two quotations by Prof. Newton (NATURE, p. 266) from *Des Murs* and *Degland et Gerbe*.

Can it be that such extreme variation really does occur on the Continent, and is yet comparatively absent in Britain? I leave it to abler hands than mine to discuss; but if it should prove so, it will be another feature in the already remarkable habits of the cuckow.

Tadcaster, Feb. 7. FRANCIS G. BINNIE

MR. RUSKIN ON RIVER CONSERVATION

IN his recent Friday evening discourse on Verona and its Rivers, at the Royal Institution, Mr. Ruskin, speaking of the Adige and the Po, said: "I want to speak for a minute or two about these great rivers; because in the efforts that are now being made to restore some of its commerce to Venice precisely the same questions are in course of debate which again and again, ever since Venice was a city, have put her Senate at pause—namely, how to hold in check the continually advancing morass formed by the silt brought down by the Alpine rivers. Is it not strange that for at least six hundred years the Venetians have been contending with those rivers at their *mouths*—that is to say, where their strength has become wholly irresistible—and never once thought of contending with them at their sources, where their infinitely separated streamlets might be, and are meant by Heaven to be, ruled as easily as children? And observe how sternly, how constantly the place where they are to be governed is marked by the mischief done by their liberty. Consider what the advance of the delta of the Po in the Adriatic signifies among the Alps. The evil of the delta itself, however great, is as nothing in comparison of that which is in its origin. The gradual destruction of the harbourage of Venice, the endless cost of delaying it, the malaria of the whole coast down to Ravenna, nay, the raising of the bed of the Po, to the imperilling of all Lombardy, are but secondary evils. Every acre of that increasing delta means *the devastation of part of an Alpine valley, and the loss of so much fruitful soil and ministering rain.* Some of you now present must have passed this year through the valleys of the Torcia and Ticino. You know, therefore, the devastation that was caused there, as well as in the valley of the Rhone, by the great floods of 1868, and that ten years of labour, even if the peasantry had still the heart for labour, cannot redeem those districts into fertility. What you have there seen on a vast scale, takes place to a certain extent during every summer thunderstorm, and from the ruin of some portion of fruitful land the dust descends to increase the marshes of the Po. But observe further—whether fed by sudden melting of snow or by storm—every destructive rise of the Italian rivers signifies the loss of so much power of irrigation on the south side of the Alps. You must all well know the look of their chain—seen from Milan or Turin late in summer—how little snow is left, except on Monte Rosa, how vast a territory of brown mountain-side heated and barren, without rocks, yet without forest. There is in that brown-purple zone, and along the flanks of every valley that divides it, another Lombardy of cultivable land; and every drift of rain that swells the mountain torrents, if it were caught where it falls, is literally rain of gold. We seek gold beneath the rocks; and we will not so much as make a trench along the hillside to catch it where it falls from heaven, and where, if not so caught, it changes into a frantic monster, first ravaging hamlet, hill, and plain, then sinking along the shores of Venice into poisoned sleep. Think what that belt of the Alps might be—up to four thousand feet above the plain—if the system of terraced irrigation, which even half-savage nations discovered and practised long ago in China and in Borneo, and by which our own engineers have subdued vast districts of farthest India, were but in part also practised here—here, in the oldest and proudest centre of European arts, where Leonardo da Vinci—master amongst masters—first discerned the laws of the coiling clouds and wandering streams, so that to this day his engineering remains unbettered by modern science: and yet in this centre of all human achievements of genius no thought has been taken to receive with sacred art these great gifts of quiet snow and flying rain. Think, I repeat, what that south slope of the Alps might be; one paradise of lovely pasture and avenued forest of chestnut and blossomed trees, with cascades docile and innocent as infants, laughing all summer long from crag to crag and pool to pool, and the Adige and the Po, the Dora and the Ticino, no more defiled, no more alternating between fierce flood and venomous languor, but in calm clear currents bearing ships to every city and health to every field of all that azure plain of Lombard Italy. . . . . Without in the least urging my plans impatiently on any one else, I know thoroughly that this which I have said *should* be done, *can* be done, for the Italian rivers, and that no method of employment of our idle able-bodied labourers would be in the end more remunerative, or in the beginnings of it more healthful and every way more beneficial than, with the concurrence of the Italian and Swiss Governments, setting them to redeem the valleys of the Ticino and the Rhone. And I pray you to think of this; for I tell you truly—you who care for Italy, that both her passions and her mountain streams are noble; but that her happiness depends, not on the liberty, but the right government of both."

CAPTAIN FRED. BROME

WITH great regret we have to record the death of Captain Fred. Brome, formerly Governor of the Military Prison on Windmill Hill, Gibraltar, and well known to many of our geological and archæological readers as the able and indefatigable explorer of the ossiferous caves and fissures of the rock.

His explorations, an account of which, so far as they related to the human remains and relics, was published in the Transactions of the Congress of Prehistoric Archæology for 1868, were commenced in April, 1863, and unremittingly continued, often under considerable difficulties, to December, 1868, when he was most unaccountably removed from the post he had so long and so well occupied.

The amount of labour and responsibility thus voluntarily undertaken by Captain Brome, solely in the interest of science, and without any personal motive whatever, can scarcely be imagined, nor can the value of the results obtained by him be easily over-estimated.

A more striking instance of self-devotion to a purely scientific object can nowhere be found.

The results of Captain Brome's work may be said to have afforded all, or nearly all, the knowledge we possess of the priscan population of the Rock of Gibraltar, and have added enormously to our materials for determining the nature of its quaternary fauna, as disclosed in the ossiferous breccia and other contents of the rock fissures, from the examination of which Cuvier truly anticipated that the most important information would be derived.

Captain Brome's death occurred, we are sorry to say, under very melancholy circumstances. Having been removed from the post which he had so long and so usefully filled, and for which, from his great experience, extraordinary energy, and high sense of duty, he was so admirably qualified, he was appointed, on coming to England, Governor of the Military Prison at Weedon. Here he hoped to find an asylum for his family, and some compensation for the sacrifices he had been compelled to make in leaving Gibraltar.

But this was not to be. Amongst the numerous reductions of late effected in our military establishments, the disestablishment of the prison at Weedon was one. The notice that his services would be no longer required was received by Captain Brome a short time since, and it seems to have so affected him, from the apprehension that his family would thus be deprived of all support—and this after a public service of thirty years—that, although a strong and vigorous man, he gradually sank, from mental depression, as it would seem, and he may truly be said to have died of a broken heart on the 4th March, leaving a widow and eight children, we fear wholly unprovided for.

A more melancholy case, and one more deserving of the sympathy of the scientific world, and, as we should venture to hope, of the consideration of the authorities at the War Office, it is impossible to conceive. G. BUSK

THE GEOLOGY OF THE HOLY LAND

IN the year 1866 the Duc de Luynes organised an expedition for investigating the physical geography and geology of the Holy Land and part of the surrounding territories. Narratives of some features of the explorations have already been given to the world, but it is only now that the first part of the geological report appears. M. Lartet, the geologist of the expedition, has chosen as the vehicle of publication for his memoir, the opening number of a new magazine—the *Annales des Sciences Géologiques*. Instead of confining himself to a record of what he personally accomplished, he has with much labour given a brief summary of the publications of previous writers, and has incorporated their results with his own, so as to present in a clear and connected form the sum of all that is at present known regarding the geology of the country between Lebanon and the Red Sea. Until the whole of the memoir is published it would be premature to pass judgment upon the position which it will ultimately take in the geological bibliography of Palestine. The present instalment, after its introductory and historical sections, passes on to describe the igneous and crystalline rocks, leaving the great limestone and later formations for a subsequent paper.

Viewed in the great scale, the geological structure of Palestine is remarkably simple. A long table-land or succession of table-lands, consisting for the most part of horizontal or gently inclined cretaceous and nummulitic limestones, is traversed by the valley of the Jordan, and cut through by transverse valleys, many of which are now quite dry. Stretching southwards into the peninsula of Sinai, these calcareous plateaux end against a mass of high rugged ground—the mountain-group of Sinai and Arabia—consisting of crystalline rocks. Here and there on the west side of the Jordan Valley, but much more markedly on the east side, the table-lands are roughened by rocks of volcanic origin. Everywhere there is evidence of vast denudation, whereby the plateaux have been cut into valleys and hills, and of a former climate when rain and river-water were much more developed than they are now.

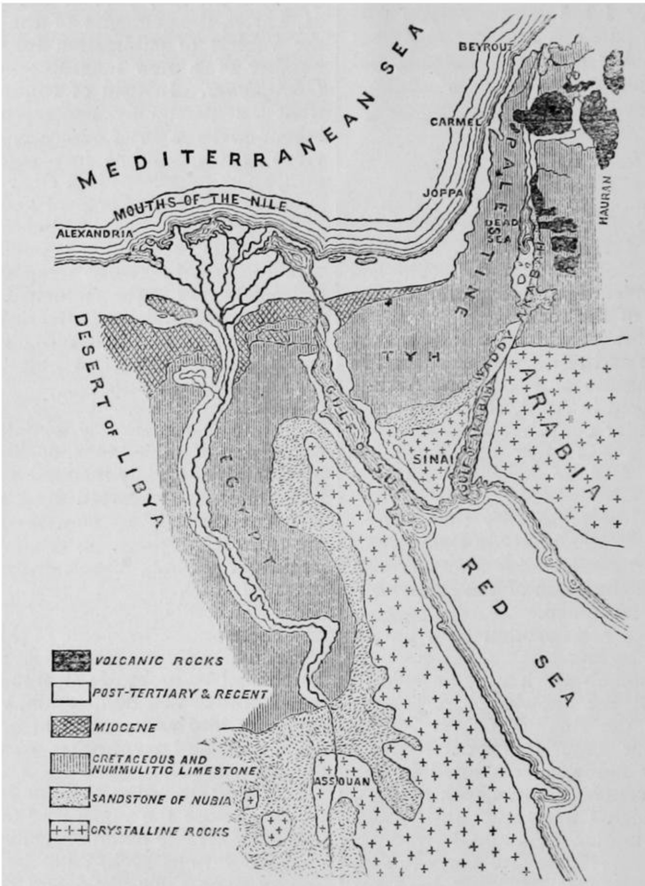
M. Lartet describes at some length the crystalline rocks which enclose the upper end of the Red Sea, and enters into considerable detail regarding the mineral differences of these various rocks; but he touches with tantalising brevity upon their geological relations—a fault, however, which he shares with all other writers who have treated of the geology of these regions. We only learn from him that there is a central nucleus of granite round which are folded successive zones of gneiss and various schists and slates, and that all these rocks are pierced by intrusive masses of porphyry, dicrite, melaphyre, serpentine, &c. From the granites and old intrusive rocks he passes, by what seems an abrupt and awkward transition, to the basalts and lavas, which are among the most recent of the geological formations of the country; and he then takes up the schistose rocks. This arrangement is much more a petrographical than a geological one. We cannot but think that it interrupts the chronological sequence of events which it is the business of a geologist to decipher and describe. The volcanic rocks were not erupted until the cretaceous table-lands had been long exposed to denudation. It would surely have been better, therefore, to have deferred the history of the eruptions until some account had been given of the rocks over which they took place.

There can be no doubt that Palestine and the surrounding countries have been the scene of powerful volcanic activity from a time which, though in a geological sense recent, is yet immensely remote when measured by human standards. In the Holy Land itself that action has now ceased, though abundant hot springs still testify to its recentness, while along the shores of the Red Sea there are even at this day some still active craters. On the west side of the Jordan Valley, the fertile plain of Esdraelon is strewed over with basaltic débris, while a little farther to the north and east, true *coulées* of basalt are found on the flanks of Little Hermon. Near Tiberias another *coulée* comes down from the west to the margin of the Lake; while still farther north stands the basaltic mass of Safed, which seems to have been the great volcanic centre of that district. But it is on the east side of the line of the Jordan and Dead Sea that the most remarkable display of volcanic rocks is to be found. The high grounds of Moab and Ammonitis are dotted with the sites of ancient volcanos, from which streams of lava have descended, sometimes even into the Dead Sea. The houses of Rabbath-Moab were built of these basalts. Farther north, from the heights of Ajlun, the eye travels over a vast basaltic plateau, which stretches north to the roots of Hermon and Anti-Lebanon, and eastward until it is crowned by the imposing volcanic mountain of Hauran. Some portions of these regions are dreary beyond expression—rough with dark rugged lava streams and scattered scoriæ, rendering the journey of a traveller slow and laborious. It is in this wild region that the freshest traces of volcanic phenomena occur; some of the cinder cones still remaining with their enclosed craters like those of the Puys of Auvergne.

The long period during which volcanic action continued to manifest itself is well shown by the fact that the older basalts are found capping limestone plateaux which have since been cut through by valleys descending into the Jordan and the Dead Sea, while these valleys have served as moulds into which the later lava currents have, in numerous instances, been poured. Such a sequence of events is a repetition of the well-known structure of Auvergne. There is a further resemblance to the volcanic district of central France in the singular freshness of some of the cones.

But of all the geological features of Palestine, that which possesses, perhaps, the strongest interest is the valley or depression which stretches from Lebanon along the line of the Jordan, the Dead Sea, and the Waddi Akabah to the head of the eastern arm of the Red Sea. Notwithstanding the yearly increase of travellers in the Holy Land and the rapid multiplication of descriptions of the country, it must be owned that no very satisfactory account has yet been given of the geographical structure and probable origin of this, the most remarkable depression upon the globe. In the present part of his report M. Lartet does not discuss this subject, nor does he enter into the existing contour of Syria or the causes which may have led to the drying up of the Waddys and the general desiccation of the climate. These questions he will probably discuss in the next portion of his valuable memoir, to which we hope to call attention when it appears.

ARCH. GEIKIE



GEOLOGICAL MAP OF PALESTINE AND LOWER EGYPT

NOTES

We are informed that it is probable that Dr. Sharpey, Prof. Huxley, and Sir John Lubbock will be among the members of the Royal Commission to inquire into the present state of Science in this country.

The Society of Arts has resolved to hold a morning conference on the same subject, which will probably take place on Friday week. The council of the Society has requested Lieut.-Colonel A. Strange to open the discussion with a paper, the title of which will be "On the inquiry by a Royal Commission into the relations of the State to Science." We understand that the Society intends to invite the attendance at the conference of Her Majesty's present and late Ministers, of all members of the Legislature known to be interested in analogous questions, of the councils of all scientific societies, and of learned bodies generally.

It is surely a sign of the times that we should be able to lay before our readers a scientific lecture delivered on a Sunday before a great audience, composed chiefly of the middle classes. The history—the all too short history—of English Sunday lectures is very curious and, withal, instructive. Some years ago the movement was commenced by lectures in St. Martin's Hall, which lectures, thanks to the activity displayed by "The Lord's Day Observance Society," were brought to a close somewhat suddenly. They were afterwards revived (such is the perfection of our English law) with impunity, by the simple process of enrolling the lecturers and their friends as a religious body! But many of those who had taken an active part in the origination of the lectures declined to shelter themselves under what they conceived to be an unworthy, as well as an unnecessary, subterfuge; and, believing that the law was really on their side, determined to take the earliest opportunity of obtaining experimental proof of the justice of their views. So we have had two movements—one, embodied in the Sunday Lecture Society, a lecture *pur et simple* being delivered each Sunday afternoon, and another, emphatically the working men's movement, in which the exact programme which was at first threatened with prosecution is reproduced. Both these movements have been in operation, and have been the means of doing much good, for some time past; and no attempt has been made to interfere with that "Free Sunday" which is of a good deal more importance to the working men of this country than even a "Free Breakfast-table." Surely one of those quiet victories by which each step in the march of real progress has been made good in English history, has been won.

A new mathematical journal, edited under the direction of MM. Chasles, Bertrand, Serret, Delaunay, Puiseux, Darboux, &c., is about to appear, under the name of *Bulletin des Sciences Mathématiques et Astronomiques*. Twelve parts will be issued yearly.

We have received from Mr. Murray a pamphlet on Compulsory Education, by the Hon. Dudley Campbell, which is clear enough in treatment, and sound enough in argument to do great good at the present time, when the Government bill is before the country. It is very hard for a man of scientific training to bear with the shifts and compromises with which politicians are too often apt to cover their own shortcomings and lukewarmness, but in the matter of compulsory education there is less halting than usual. Mr. Campbell well points out that people are vaccinated and otherwise dealt with compulsorily for the public weal, and he asks why should this system stop just at the point where our lamentable backwardness points in the clearest way for the necessity of Government action. The country not only asks for compulsory education, but we are convinced in time that the system of local boards will be swept away. In connection with this subject we would call special attention to the fearful state to which the voluntary system has reduced education in Birmingham, Leeds, Liverpool and Manchester, as recorded in the reports of Messrs. Fitch and Fearon just presented to Parliament.

The fund raised in this country for the benefit of the family of that great naturalist, the late Professor Sars, now amounts to 265*l.* 6*s.* 10*d.* Among the contributions from the provinces may be especially noticed those of Newcastle and Glasgow, showing the favour with which science is regarded in those places. Devonshire and Wales have hitherto given no sign, although it might have been expected that the last meeting of the British Association at Exeter would have left some impress of its visit. The French and Belgian subscriptions amount to 6283 fr. or 251*l.* 6*s.* 10*d.*

The Lectures of the present year at the Royal College of Physicians will be delivered at the College, Pall Mall East, at five o'clock on each of the following Wednesdays and Fridays:—Goulstonian Lectures, by Dr. Maudsley, March 18. "On the relations between body and mind, and between mental and nervous disorders." Croonian Lectures, by Dr. Sibson, March 23, 25, 30, "On aneurisms of the aorta." Lumleian Lectures, by Dr. J. R. Bennett, April 1, 6, 8, "On the natural history and diagnosis of intra-thoracic cancer."

Notices of motion have been given by Mr. Strutt for a return relative to the expenditure of the Meteorological Committee, voted in Class 4 of the Civil Service Estimates, 1869-70; by Mr. Grant Duff for a copy of report on measures adopted for sanitary improvements in India; by Mr. Macfie for a select committee to consider and report on the law relating to letters patent for inventions; by Mr. Mundella of an amendment to Mr. Macfie's motion.

A Natural History Society has just been formed at Winchester College. The subjects it embraces at present are botany, ethnology, and geology, these being the most easy of access under the existing circumstances. One of the main objects of this society is the formation of a museum, which, it is hoped, will tend to keep up an interest in scientific subjects among the members of the school. The meetings have been largely attended, and there is every reason to expect that the society will prove a lasting benefit to the college.

We must congratulate the Leamington Philosophical Society and its energetic president, Dr. O'Callaghan, upon the successful way in which they are doing the good work of fostering science in Warwickshire by means of lectures, and upon the fact that they can prevail upon such busy men as Mr. E. J. Reed and Sir Bartle Frere—to mention two of their recent lecturers—to help them. Mr. Reed's lecture on "Our Ironclad Navy" was given last week, and we hope to place some parts of it before our readers.

The prospectus of the forthcoming course of instruction at the Working Men's College is a most gratifying one. Besides classes in Art, History and Law, Languages and Mathematics, we have the following in physical science:—The Use of the Microscope, Mr. J. Slade; Astronomy, Mr. R. B. Litchfield, B.A.; Muscular Anatomy of the Human Body, Mr. J. Beswick Perrin. Besides these classes, there are free general lectures at 8.30 on Saturdays, among which we note four on Crystals, by Prof. N. S. Maskelyne, of the British Museum. All should feel grateful to those connected with this institution who, without fee or reward, devote their small spare time to the arduous work of teaching.

We regret to hear that Dr. Kirk, the indefatigable friend and former companion of Livingstone finds his efforts to send supplies to the latter paralysed by the presence of cholera on the East Coast of such severity that 10,000 have died in Zanzibar in the course of six weeks. He adds that "the scourge rages up and down the coast. At Quiloa, by the last accounts, there were 200 deaths a day among the slaves; when offered at one dollar a head they found no purchasers, so very worthless had slave property become from the disease. It is also going inland, which is strange, as it came to us from the interior, first showing on the coast at Pangani; now it goes in from Bogamoyo, and has reached Ugogo. Caravans on the route are stopped by death, ivory is left abandoned, and a party is being sent off to bring one large lot down, all the porters being dead in Ugogo. The expedition with valuable goods and a gang of men I sent off to assist Livingstone has been caught by it, and is at a standstill. Many of those I had engaged and paid considerable advances to are dead. There will be much loss this season, and Dr. Livingstone will come in for a share of it."

THE Working Men's Club and Institute Union have, with permission of the authorities, arranged for a series of visits to the national museums on Saturday afternoons, for the members of workmen's clubs. The important feature connected with these visits is, that in each case the party will be under the guidance of some gentleman specially qualified to afford instruction in some particular branch of science and art. A party of fifty workmen were thus enabled to pay a visit to the Egyptian Department of the British Museum on Saturday, under the guidance of Mr. Samuel Sharpe. We are informed that the "Club Union" will be very glad to have similar valuable services rendered by other gentlemen for visits to the national collections.

A LETTER has been received at Alexandria from Sir Samuel Baker, dated Khartoum, February 7, wherein he reports that 32 boats were collected together to convey him and his party to Gondokoro. With the last shipment of troops the total expeditionary force amounts to 700, including a battery of artillery. Mr. Higginbotham is reported to be within four days' march of Khartoum, having crossed the Nubian Desert. He has under his charge the steel steamers for the lake Albert Nyanza. Mr. Higginbotham has command of the rear expedition, and will follow Sir Samuel Baker immediately. All the members of the expedition are in good health and spirits.

THE *Pall Mall Gazette* states that the Arctic explorer, Mr. C. F. Hall, has, in a lecture given at Washington, developed his plans for a third voyage.

THE establishment of the first sewage farm in India has taken place in the vicinity of Madras. It is an experiment, but the results as given in the official report are most hopeful, both as regards the drainage of Indian towns and the profit likely to accrue from the operation. The site is an old swamp four feet only above the sea level; the soil is a stiff clay, mixed with much salt and a little sand—one of the worst possible soils for the purpose. The surface was levelled and protected from floods, and the sewage from Perambore barracks and a small portion of the adjacent village, after being raised 22 feet, is conducted in an open earthenware conduit, and floated over the surface of the ground. The total area is 37 acres, but about 2 acres only have been put under cultivation. The sewage is as thick as pea-soup, and sometimes more diluted. Its smell is overpowering close to the channel, but as it flows over the ground "it loses its offensiveness very soon." Various crops have been tried on the sewaged area. Guinea grass succeeds so well that its yield is at the rate of 88 tons of fresh grass, or 29 tons of hay per acre. The value is 58*l*. per acre. It is stated that grass will take any quantity of sewage, but that other useful crops, chiefly native vegetables, also succeed. Different plants require different treatment. Some of the best crops are native greens, which grow most luxuriantly and take a large quantity of sewage. The report states that the results to health have been satisfactory, and that one great source of disease has been removed. The application of the sewage of two other districts of the city was nearly completed at the date of the report.

CANDIDATES for the first chair of Algebra at the Paris Faculty of Sciences are requested to send in certificates.

MESSRS. MOXON AND CO. are preparing for publication a Dictionary of Science, edited by Mr. G. Farrer Rodwell. It will be uniform with Haydn's "Dictionary of Dates" and "Dictionary of Biography," and will comprise acoustics, astronomy, chemistry, dynamics, electricity, heat, hydrodynamics, hydrostatics, light, magnetism, meteorology, pneumatics, and statics. These subjects will be treated of by Mr. J. T. Bottomley, Lecturer on Natural Science in King's College School; Mr. William Crookes, Mr. Frederick Guthrie, Professor of Natural Philosophy in the Royal School of Mines; Mr. R. A. Proctor, Mr. Richard Wormell; and the Editor.

THE Paris Zoological Acclimatisation Society celebrated their seventeenth anniversary last week in the Hôtel de Ville. Dr. Hooker was unanimously elected an honorary fellow, and the following prizes were awarded:—The gold medal offered to the society by the Minister of Agriculture and Commerce to M. Carbonnier, for the introduction of Chinese fish; another gold medal to M. Vekemans, the director of the Zoological Gardens at Antwerp; grand gold medal to M. Alfred Grandidier for his travels in Africa and America; a prize of 500fr. for the theoretical researches of M. Verreaux on acclimatisation subjects; a similar prize for works of pure zoology to the late Professor Sars for his publication on the littoral fauna of Norway and the development of sea-fish; a prize of 200fr. was also awarded to M. E. Gayot for his essay on *Leporides*.

THE Liverpool Naturalists' Field Club held a soirée at the Royal Institution on the 11th instant, the Mayor and about two hundred members and friends attending. The principal objects exhibited, interesting in a natural history point of view, were a very large collection of British plants beautifully mounted by Mr. Gibson, sen., a member of the club; an interesting case of spiders found in the neighbourhood, preserved in spirits in flat glass bottles, and very effectually displayed by Mr. H. Higgins, son of the president; a selection of valuable shells lately presented to the Free Museum by Mr. Samuel Smith; young salmon; case illustrating anatomical structure of the elephant, &c.; the largest known Nudibranch and the largest known Foraminifer, both taken alive by Dr. Collingwood in the China seas—from the Free Museum; a case of sections of Brazilian creepers, showing curious abnormal structure, specific names unknown—by Mr. Robert Holland. Members of the Microscopical Society illustrated various subjects. A spectroscope and micro-spectroscope were worked by the secretary, Mr. Stearn; and the evening concluded with some chemical experiments bearing on Prof. Tyndall's "Dust and Disease" article in *Fraser*, by Mr. Davis, F.C.S.; the exhibition of Geissler tubes, a number of insects, &c.

AT a recent meeting, the Natural History Society of Montreal presented its medal to Sir William Logan, the distinguished geologist. The following resolution was passed:—"That this Society, in presenting its medal to Sir W. E. Logan, LL.D., F.R.S., although it cannot add appreciably to the many honours which he has received, desires to place on record, not merely on its own behalf, but on that of all the students of Natural Science in Canada, its high estimation of the value of his services in creating as well as directing the Geological Survey of this country, in promoting the development of its mineral resources, in stimulating and aiding the efforts of scientific institutions, and in extending throughout the world the name of Canadian science. We desire also to express our high appreciation of Sir William's admirable qualities, and our hope that he may be spared for many years to Canada and to science, and that the relief from official cares may give him the opportunity to pursue to completion the researches in scientific geology in which he is now engaged."

WE are glad to see that meetings are being held in support of Mr. W. S. Allen's motion in the House of Commons to open museums on week-day evenings. We know of no argument against the experiment, and we believe the experiment would be an entirely successful one.

THE infallibility of photographic reproductions, says the *Photographic Journal*, cannot be prized too highly. In the copying of elaborate tabular forms, containing numerous figures and intricate calculations, the aid of the camera is sometimes of the greatest importance. To obtain an exact and reliable copy of a complex document of this description by clerical means involves much time and labour, beyond the chance and risk of error; but with the camera a reproduction may be secured in a couple of hours, in which all the figures are exact, the totals correct, the calculations checked, the words spelt right, and to which the observation "certified a true copy" may be appended without hesitation.

THE Vicar of Cushendun, county Antrim, communicates the following to *Science Gossip*:—"The following incident was told me the other day by a resident, who vouches for the truth of it, and which happened close to his residence in Cushendun, county Antrim. A rat, nearly white with age, and blind, was frequently seen led to the water by a young rat, by means of a straw, of which the old rat held one end and the young rat the other. This incident corroborates a similar statement given by Jesse in his 'Gleanings of Natural History.'"

SCIENTIFIC SERIALS

THE new number of the *Zeitschrift für Biologie* (VI. i.) contains an interesting paper by Subbotin "On the Physiology of Fats." Towards an answer to the question—Is there in the animal organism any direct passage of fat from the alimentary canal to the cells of adipose tissue? a lean dog was fed for a month on meat, spermaceti, and common fat. Of the 1,000 grms. of spermaceti swallowed, 800, at least, were absorbed; but the merest trace only of spermaceti could be found in the fatty tissue of the body at the close of the experiment. Spermaceti, therefore, though absorbed and consumed in the economy, is not stored up unchanged. Hence there is a presumption that the same is the case with other fats (though it is obvious that many possible events might negative the presumption). Towards solving the further question—Are fats formed in the body out of proteids? a dog reduced to the utmost leanness was fed on leanest meat and palm oil (palmitin and olein) for twenty-five days, during which he gained three kilos in weight. The fat of the body was, at the close, found to contain 13·9 per cent. of stearin, though none had been taken. A very considerable quantity of stearin had therefore been formed in the body. A very lean dog was fed for six weeks on leanest meat, and a soda soap made with palmitic and stearic acids only. At the end of the experiment, the dog having gained over three kilos in weight, the fat of the body was found to consist of 53·6 per cent. palmitin, 13·4 per cent. stearin, and 33 per cent. olein. A large quantity of olein had therefore been formed in this case. But if olein was thus formed, possibly the palmitin and stearin were likewise formed from proteids, and not by synthesis of the fatty acids with the glycerin of the economy. Subbotin further points out that olein is more abundant in the sub-cutaneous than in the deep-seated fat, possibly on account of a less energetic transformation of proteids in the cooler surface regions. So also in cold-blooded animals the fat is proportionally richer in olein.—The same number contains a long paper by Vierordt, in which that distinguished physiologist continues his researches on the connection between the delicacy of touch and mobility of any part of the body. In this memoir he confines his attention to the arm from the shoulder downwards, working upon data provided by his pupils Kottenkamp and Ullrich, who have made a study of the sense of touch in all parts of the arm, to a much greater extent and with much fuller detail than did Weber, and whose elaborate results are given in a paper immediately preceding Vierordt's. In the arm Vierordt finds striking illustrations of his hypothesis that the delicacy of touch in any point in a region of the body which is moved as a whole, is proportional to the distance of the point from the centre of movement of the region. There are also hygienic papers by Pettenkofer and others on the cholera epidemic of 1865 at Gibraltar, and typhus and drinking water at the barracks at Neustift.

The *Journal of Botany*, New Series, No. 1 (double number for January and February, 1870), contains the following articles:—"Suggestions on the 'Species' question as regards *Rubus*," by the Hon. J. B. Leicester Warren; "Notes on *Quercus Wallichiana* Lindl," by Dr. Hance; "Descriptions of four new Chinese *Crassulaceæ*," by Dr. Hance; an interesting and important "Review of the contributions to Fossil Botany, published in Britain in 1869," by Mr. Carruthers, containing references to all papers on vegetable palæontology published in Britain during that period, with observations and a synopsis of the genera and species described in them. "Cinchona cultivation in Bengal," being an official report from Mr. C. B. Clarke, officiating superintendent of the Botanic Garden at Calcutta, and in charge of the Cinchona cultivation in Bengal, for the year ending March 31, 1869. It appears from this report that during the year the number of plants of Cinchona at the Darjeeling Government plantations was increased by 673,654, making a total (including those in private plantations) of upwards of three million plants, covering 965 acres. By far the larger number of plants are of the species *C. officinalis* and *succirubra*, with a few of *C. Calisaya* and *micrantha*, *C. Pahudiana* being considered worthless. The tallest plant of *C. succirubra* is 19 feet high, and of *C. officinalis* 11¼ feet. There is besides a small plantation of *C. succirubra* at Nunklow, in the Khasia Hills. "Review of the genus *Narcissus*," by Mr. J. G. Baker. An abridgment and translation by Mr. A. W. Bennett of Van Horen's paper in the *Bulletin de la Soc. Roy. de Bot. de Belgique*, "On the hibernation of Lemnaceæ," showing the production in several species of duck-weed of submerged leaves adapted to live through the winter. "The genus *Ascobolus*," by M. Boudier, from the *Annales des Sciences Naturelles*. Reviews of books, proceedings of societies, and shorter articles.

The *Geological Magazine* for the present month (No. 69) opens with an article by the editor on the Liassic Pterodactyle (*Dimorphodon*) as described by Professor Owen in the volume of memoirs just published by the Palæontographical Society. The most important paper in the number is one by Mr. Poulett Scrope, "On the character and composition of lavas," and next to this, in general interest, two articles on "Faults in Strata," by Mr. W. T. Blandford and Mr. G. H. Kinahan.—Two of the remaining papers are by lady geologists, namely, a description of the Pleistocene deposits of North Shropshire, by Miss Charlotte Eyton, and a notice of vegetable fossils in the Water Blain iron mines in South Cumberland.—A paper on "The Water-bearing Strata in the neighbourhood of Norwich," by Messrs. Taylor and Morant, contains some remarks upon sand-pipes in the chalk; and the final article is the conclusion of Mr. Davies' paper "On the Millstone Grit of the North Wales Border."—The usual notices of memoirs, reports of proceedings, letters, and miscellaneous intelligence make up the rest of the contents.

Revue des Cours Scientifiques, March 8.—This number contains a report of the congress of German naturalists and medical men at Innspruck; a paper on "Alsace during the Tertiary period," read by M. Delbos at the Mulhouse Conferences; a review of M. Gréhant's researches on the excretion of urea and on the respiration of fish, also a fifth list of subscribers to the Sars fund.

The *Revue des Cours Scientifiques* for the 12th inst., contains an admirable lecture by M. Wolf, of the Paris Observatory, on the Figure of the Earth, and a translation of Dr. Tyndall's lecture on Dust and Disease.

Moniteur Scientifique, February 15.—This number contains several abstracts of papers on dyeing materials, by Messrs. Hofmann, Martius, and Weidel.—A paper descriptive of the peat at Avigliana, near Turin, by MM. Kopp and Fino, shows that, in the air-dried state, its efficacy as fuel is little more than one-fourth that of coke.—In a paper by M. Fremy on nitrous acid, he points out the production of nitrous oxide by the reaction of nitrous acid with sulphurous acid, regarding this as one of the causes of loss in the manufacture of sulphuric acid, when there is an excess of sulphurous acid in the chambers. In the reaction of nitric acid with nascent hydrogen, he has ascertained the production of another substance besides nitrous acid and ammonia. By means of sodium amalgam he obtained it in large quantity but not sufficient for complete examination. Arsenious acid and arsenites give, with sodium amalgam, a similar product. Both the nitrogen and arsenic compounds are characterised by a remarkable reducing power. M. Fremy is continuing his examination of the subject.

THE FOREFATHERS OF THE ENGLISH PEOPLE *

THE English people of the present day present two types of physical structure, which are extremely different in their most marked forms, though they pass into one another by every shade of gradation. The one type is tall, fair-complexioned, yellow or red haired, and blue-eyed; the other, short, dark-complexioned, black-haired, and black-eyed. The two types and their intermediate gradations are, at present, to be found side by side in most parts of the British Islands; but there is a marked predominance of the fair type in the eastern half of Britain. The languages spoken by the English people have, at the present time, no relation to these two physical types; English speakers and Celtic speakers belonging no less to the one type than to the other. Nor are the two Celtic dialects, Cymric and Gaelic, confined to people of the one or the other physical type, as both the types described are exhibited in their extreme forms among Welshmen, Highlanders, and Irishmen.

The earliest historical records of the nature of the population of Britain, furnished by Cæsar, Strabo, and Tacitus, take us back nineteen hundred years, and show that, at that time, the physical characters of the population might be described in the same language as at present. The people of South-eastern England and of Caledonia were certainly tall, fair, and blue-eyed, with hair varying from yellow to red in hue; while, in South Wales, they had dark hair and complexions, resembling the Spaniards of that day. But there was a wonderful difference in language between the ancient and the modern inhabitants of these islands, inasmuch as all these people of Britain, so far as we know, spoke the Cymric dialect of the Celtic tongue; while it is probable, though we have no absolute knowledge on this point, that in Ireland they spoke Gaelic. Thus, at the time of the Roman invasion, the outward physical characters of the population of these islands were much what they are now, though the language spoken was, probably, altogether Celtic. And there was no parity between the distribution of the Cymric and Gaelic dialects of the Celtic and that of the two physical types, any more than there is now between English and Celtic and the fair and dark stocks by which those languages are spoken. If we confine our attention to the British Islands, therefore, we have absolutely no means of ascribing any special physical characters to the Celtic-speaking people. A British, or Irish, "Celt" might be tall or short, dark or fair, rounded-headed or long-headed; and the remark of Professor Max Müller that it is as rational to speak of a dolichocephalic language as of a Celtic skull is, for the "Celts" of Britain, perfectly justified.

Whence was this Celtic-speaking people, with its two contrasted dark and fair forms, which inhabited Britain nineteen hundred years ago, derived? The position of the British Islands is sufficient to suggest the extreme probability that it migrated from Europe, the eastern and the southern faces of these islands being within easy reach of the shores of those countries which are now Norway, Denmark, North Germany, Holland, Belgium, and France. And the probability suggested by the facts of geography becomes converted into a certainty by those of ethnology and of history.

In the first place, if we turn to the existing population of the continent of Europe and Asia, we shall at once recognise our two physical types—the fair and the dark. From Norway to North-eastern France the predominant constituents of the riverain population of the North Sea and of the British Channel are tall, fair-haired, and blue-eyed. In North-western France the proportion of short and dark people increases, until, in Southern and South-western France, they are the chief constituents of the population. A traveller who should set out from the Orkney Islands and call at every port in the North Sea, and who then should make a land journey from the mouth of the Elbe to that of the Don, would find the people with whom he met to be generally, and in many regions exclusively, of the fair type. On the other hand, if he set out from Galway and cruised along the western coasts of these islands, and of France and of Spain and the north shore of the Mediterranean, he would find as marked a predominance of the dark type. In fact, the population of the southern and western parts of France, of Spain, of the Ligurian shore, and of Western and Southern Italy, is as generally dark as that of North Germany is fair.

There is no reason to think that climatal conditions have had anything whatever to do with this singular distribution of the fair and the dark types. Not only do the dark Celtic-speakers of the Scotch Highlands lie five or six degrees farther north than the fair Black-foresters of Germany; but, to the north of all the fair inhabitants of Europe, in Lapland, there lives a race of people very different in their characters from the dark stock of Britain, but still having black hair, black eyes, and swarthy yellowish complexions.

Thus, having regard only to physical characters, the population of Europe falls into three broad bands, which run in a rough way from west to east. In the north is the zone of the black-haired, black-eyed Mongoloid Lapps. In the south is the zone of the people who resemble the dark type of the British Islands, and who have been called *Melonochroi*; between them lies the broad belt of fair people, who have been termed *Xanthochroi*. And if this were a mere natural history question, the facts I have mentioned would allow us to draw but one conclusion as to the origin of the population of these islands—namely, that the dark type has been furnished by immigrants from the Continental *Melanochroi*; the fair type by immigrants from the Continental *Xanthochroi*. But history and philology have every right to be heard in such a matter as this; and I must now try as well as I can (for I am neither historian nor philologer) to put before you what they have to say.

What history tells us, so far as it goes, is quite in accordance with the suggestions of biology. It is certain that, from the fifth century to the tenth a vast number of people from North Germany and Scandinavia poured into the British Islands on all sides, but, as might be expected, most persistently and numerously into the eastern moiety of Britain. They brought with them languages which may properly and conveniently be termed dialects of Teutonic, in contradistinction to the indigenous dialects of Celtic. Out of the North German dialects the language usually known as Anglo-Saxon was developed, and from it, by subsequent modification and absorption of, for the most part, Scandinavian, Celtic, and French elements, has grown English. The invasion which thus changed the language of Britain introduced no new element into the physical conformation of the people, so far as stature and complexion are concerned, though it may have done so in the matter of cranial conformation. It is unquestioned that Saxons, Danes, and Norsemen were alike a tall, fair-haired people; and their immigration strengthened the Xanthochroic element of our population, but added nothing new, unless it were a longer form of head. It is a very remarkable circumstance that the skulls of the existing Scandinavians—and of the Allemanni and Saxons, if not of the whole of the ancient Germans—are long, while those of the South Germans and Swiss of the present day, and those which very probably belonged to the ancient Belgæ, are round. Thus, to put the matter in another way, tall stature, fair hair, and blue eyes, in a native of Britain, are no evidence of his descent rather from the primitive Celtic-speaking, than from the immigrant Teutonic-speaking, element of our population, or the reverse. He is as likely to be a "Celt" as a "Teuton;" a "Teuton" as a "Celt."

But history teaches us more than this. There is the clearest evidence that the Gauls—the Celtic-speaking people who burnt Rome nearly four centuries before our era—belonged to the fair type, and neither by their stature, their complexions, the colour of their eyes or their hair, were distinguishable from such Teutonic-speaking people as the Goths, who sacked Rome four centuries after it: and that, for these eight centuries at any rate, North-western, Central, Eastern Europe, and the western part of Central Asia were occupied by a tall, fair, blue-eyed people* who were known by the names of Celtæ, Belgæ, Germani, Venedi or Wends, and Alani, according to the districts which they occupied and the languages which they spoke.†

Thus, when history first makes known the Celtic language to us, it is in the mouths of a people extremely similar in their outward appearance to the Germans and the Slavonians; and when the affinities of the Celtic, the Teutonic, and Slavonic languages are worked out by the philologer, they are all found to belong to the same great group of Aryan languages. The argument to be drawn from the physical affinity of the Celtic-speaking with the

* A Lecture delivered by Prof. Huxley, in St. George's Hall, on Sunday, March 13, and revised by the author.

* The story told by Suetonius, that Caligula tried to pass off some tall Gauls for Germans, by making them redden their hair, is often quoted to prove that the hair of the Gauls differed from that of the Germans. But as the Germans themselves were in the habit of reddening their hair artificially, the force of the argument does not appear.

† Those who have any doubts upon this subject had better consult the great work of Kaspar Zeuss, "Die Deutschen und die Nachbarstämme," published thirty years ago; or the excellent discussion, mainly based upon Zeuss, in Pritchard; or the instructive essays of Brandes and De Belloguet.

Teutonic-speaking people is therefore supported and intensified by the linguistic affinities between the Celtic and the Teutonic tongues; and philology concurs with history in testifying to the ethnic unity of the Celtic-speaking people on the left bank of the Rhine, with the Teutonic-speaking * people to the eastward. In their clothing, in their arms, in their houses, in their employment of horses and wheeled carriages, no differences of moment obtain between the Celtic-speaking and the Teutonic-speaking people of old Europe; nor in their fashion of government, their social organisation, their morality,† or their theology, do there seem to be any greater differences than are readily accounted for by the fact that the Teutonic-speaking nations were more remote from the corrupting influences of wealth and civilisation. The Tonga islanders of Mariner's time offered the same contrast to the Tahitians that the Germans of Tacitus do to the Gauls, but no one would dream, on that ground, of declaring them to be of different races.

Hence, there can be no reasonable doubt, that the fair element of the Celtic-speaking population of these islands 1,900 years ago was simply the western fringe of that vast stock which can be traced to Central Asia, and the existence of which on the confines of China in ancient times is testified by Chinese annalists. Throughout the central parts of the immense area which it covers, the people of this stock speak Aryan languages—belonging, that is, to the same family as the old Persian or Zend, and the Sanskrit. And they remain still largely represented among the Affghans and the Siahposh on the frontiers of Persia on the one hand, and of Hindostan on the other. But the old Sanskrit literature proves that the Aryan population of India came in from the north-west, at least 3,000 years ago. And in the Vedas these people portray themselves in characters which might have fitted the Gauls, the Germans, or the Goths. Unfortunately there is no evidence whether they were fair-haired or not.

India was already peopled by a dark-complexioned people more like the Australians than anyone else, and speaking a group of languages called Drawidian. They were fenced in on the north by the barrier of the Himalayas; but the Aryans poured from the plains of Central Asia over the Himalayas, into the great river basins of the Indus and the Ganges, where they have been, in the main, absorbed into the pre-existing population, leaving as evidence of their immigration an extensive modification of the physical characters of the population, a language, and a literature.

Italy is to the Alps what Hindostan is to the Himalayas. The Po is its Ganges. Four centuries B.C. it was peopled mainly by the dark and short stock represented by Ligurians, Etruscans, and old Italians. The Gauls poured into it over the north-western passes, and settled in Cis-Alpine Gaul, modifying the physical characters and the language of the population, but becoming lost eventually in the great Roman nationality. And, doubtless, in more ancient times, the Aryan-speaking ancestors of these Celtæ and Belgæ had similarly made their way through the Hercynian forest or along the shores of the North Sea, into Gaul, and thence into Britain. The correspondence of the names of places in Gaul and ancient Britain fully confirms Cæsar's statement that the Belgic Gauls had, at some comparatively recent time, colonised south-eastern Britain in great numbers. But the primitive colonisation of Britain from the mainland by the fair people is doubtless of extreme antiquity.

I have now, I believe, accounted for the fair Celtic-speaking population of ancient Britain. There remains the problem, Why did Britain contain another Celtic-speaking population, of a totally different type?

The key to this riddle is, I believe with Dr. Thurnam, De Belloguet, and others, afforded by history and philology. History, which tells by the mouths of Cæsar, Strabo, and Tacitus, that the Aquitani, who lived beyond the Garonne, were a small and dark people like the Iberians, who spoke a language different from that of Gaul. Philology, which tells us that this language was the Euskarian, represented by the modern Basque, which is unlike every other European language, and which once covered a vastly greater area than it now occupies—the great majority of the people who once spoke it having acquired other languages.

* I use this phrase without prejudice to the much-debated question, Did the Germans of Cæsar and Tacitus speak "Deutsch" (not 'Dutch,' *pace* Mr. Freeman) or Celtic? and with the greatest respect for the champions of both "Keltenthum" and "Deutschthum." It is enough for me if nobody doubts the "Deutschheit" of the Goths and Alemanni.

† The grossest immorality with which the Gauls are charged may well enough have been imported by the Greeks of Massilia along with other products of Greek civilisation.

Thus, once more, physical and philological ethnology properly viewed, concur. The physically distinct stock turns out to be linguistically distinct—to have, in fact, all the ethnological characters of a distinct race.

In Spain, and within the boundaries of the old Aquitania, the Euskarian language lingers only among a fragment of the population, though the Spaniards and southern Frenchmen retain, to a great extent, the dark complexion and short stature of the Melanochroic stock. In Britain the same process of extinction seems to have been consummated as far back as the time of Tacitus. For from what has been said, it can hardly be doubted that the Silures and the dark type in general were the outliers of the continental Euskarian-speaking dark type, just as the British Belgæ, and the fair type in general, were the offshoots of the continental Celtic-speaking fair type. And just as in Western and Middle Gaul, and in Spain, the Celtic-speaking fair people had, even in the time of Cæsar, largely supplanted and absorbed the dark stock; so, in Britain, it is to be supposed that it had altogether absorbed it, and that the dark stock had given up their Euskarian for the Celtic language.

All these reasonings may be put into the form of a probable hypothesis, as follows:—The chain of the Alps, the densely wooded highlands of Central Europe known in old times as the Hercynian forest, and the broad Rhine in its lower course, form a natural rampart between the vast central plains of Eurasia and Western and Southern Europe. Before England was peopled by the ancestors of its present population, the latter region, including the north shore of the Mediterranean, Spain, and Gaul (and perhaps the shores of the Baltic) were occupied by people of the dark type, who may, by possibility, have been the chief people of the so-called bronze age in those parts. These people occupied the British islands wholly or in part, and were, very probably, at first their sole occupants. And in Spain, France, and Britain they spoke Euskarian dialects.

During this time the fair stock, with its Aryan languages, wandered over the great Eurasiatic plain to the east of the rampart, from Poland to the frontiers of China, and from Siberia to those of Persia and India. But at length the fair people found their vast plains too narrow, or the luxuries beyond its natural barriers too tempting, and they began to overflow—as Celtic-speakers into Western Europe; as Zendic and Sanskritic speakers into Persia and Hindostan. The Celtic-speaking fair people, passing into Gaul, partly extirpated and partly mixed with the pre-existing dark Euskarian-speaking population, imposing their language and habits on all the northern, middle, and eastern parts of Gaul, and extending widely into Spain. From Gaul they passed into Britain, and Celticised it still more completely; so that, though much of the old blood of the dark stock remained, its language vanished.

The Teutonic-speaking people were simply another wave of the same great Aryan ocean of Central Eurasia. They treated the Celtic-speakers exactly as the latter had treated the dark stock, and before another century has passed the Celtic language will probably be as much a thing of the past in these islands as the Euskarian is.

If this is a fair picture of the general course of events, it furnishes the explanation of the fact from which we started, namely, the presence in the British Islands of two distinct ethnical elements—a fair and a dark.

T. H. HUXLEY

ASTRONOMY

Ephemeris of the Satellites of Uranus

By A. Marth, Esq.

Angles of Position at 8^h Greenwich Mean Time

1870 Mar.	Ariel. °	Umbriel. °	Titania. °	Oberon. °
17	250	243	277	159
18	114	162	229	135
19	330	71	189	106
20	181	347	154	75
21	35	257	114	44
22	256	172	65	18
23	120	86	22	355
24	335	357	346	333
25	186	274	310	308
26	41	182	263	278
27	264	101	216	246
28	126	8	179	216

1870 Mar.	Ariel. °	Umbriel. °	Titania. °	Oberon. °
29	340	288	144	192
30	191	193	100	169
31	47	115	51	146
April				
1	271	19	11	120
2	132	302	336	90
3	345	205	297	58
4	196	128	249	29
5	53	31	204	5
6	278	314	169	343
7	137	217	132	320
8	350	141	86	293

The Apparent Distances vary between the Limits.

Ariel	15″	and	12″
Umbriel	21″	,,	16″
Titania	35″	,,	27″
Oberon	46″	,,	36″

BOTANY

Floating Leaves of Marsilea

PROF. HILDEBRAND has noticed that if a plant of *Marsilea quadrifolia* (a species of the genus which furnishes *Nardoo*), is sunk beneath the surface of the water, so that all the leaves are more or less deeply covered, those leaves which are fully developed at the time of immersion, remain unchanged, while those which are not so far advanced, undergo a remarkable change; the petioles gradually lengthening in succession according to their position on the stem, and soon over-topping those which were already formed. At first the four leaflets do not increase, but they soon begin to enlarge, and by the time the surface of the water is reached, they exceed in size the ordinary leaves, forming a four-rayed star on the surface. While the petioles of the ordinary leaves are stiff, so that they stand erect out of the water, these floating leaves are weak and flexible, like those of water-lilies, allowing the leaf to maintain its position on the surface with the rise and fall of the water. Their upper surface is shining and coated with wax, so that the water flows off them. If immersed in deeper water, the petioles will lengthen still further, even to the extent of three feet. In these cases the formation of the organs of fructification appears to be suppressed. In the ordinary aërial leaves, stomata are found on both sides of the leaf in about equal numbers; in the floating leaves, on the other hand, the under side is entirely destitute of stomata, while on the upper surface they are about three times as numerous as in the aërial leaves; thus resembling *Nymphæa*, *Hydrocharis*, and other plants. (Botanische Zeitung.)

Alternation of Generation in Fungi

M. GAURIEL RIVET records in the *Bulletin de la Société Botanique de France* a remarkable illustration of this phenomenon in some very interesting observations on the "rust" of cereals. He finds that the Fungus which causes one of the common forms of this disease, *Puccinia graminis*, will not reproduce itself, but that if the spores are sown on the leaves of the common berberry, they give rise to the well-known orange spots of *Œcidium Berberidis*, generally considered as a fungus belonging to an entirely different group. The spores of the *Œcidium*, on the other hand, do not reproduce itself, but the *Puccinia*, thus furnishing a striking instance of alternation of generation. The connection of the berberry with the prevalence of rust in wheat was noticed by Sir Joseph Banks as long ago as 1806. In the commune of Genlis (Department of Côte d'Or) a railway company not long since planted a berberry hedge on one of its embankments; immediately the crops of wheat, rye, and barley in the neighbourhood became infested with rust. The remonstrances of the farmers caused the appointment of a commissioner to inquire into the subject, who, after a full inquiry, reported that wherever the berberry is planted the cereals are more or less attacked by rust; where they are absent the crops are free from disease, and that the planting of a single berberry bush is sufficient to produce the rust where it has never appeared before.

PHYSICS

Products of Respiration

PETTENKOFER and VOIT have been making some observations, by the help of their famous dog, on the products of respiration during starvation and during a diet entirely composed of fat. They find the most notable effect of the fatty food to be *a diminution of the oxygen consumed*. Thus in one series, on the 6th and 10th days of a period during which nothing but water was taken, the oxygen consumed amounted to 358 and 302, the carbonic acid given off to 366, 289 grammes respectively. On the 2nd, 3rd, and 8th days of a similar series the quantities were of oxygen consumed 371, 358, 335; of carbonic acid given off 380, 358, 334 grammes. When, on the other hand, 100 grammes of fat were taken daily, the 8th and 10th days of the series gave respectively 262, 226 grammes of oxygen consumed, 302, 312 grammes of carbonic acid exhaled. (Zeitschrift für Biologie, bd. iii., p. 369.)

In these observations Voit finds support of his views on the nutritive influence of fat, and bases on them an explanation of Bantingism. To bring down a fatty body, he says, we must get it to take in a larger supply of oxygen. This can best be done by cutting off all the fat and carbo-hydrates and increasing the quantity of proteids. The effect of increasing the proteids is to augment the metamorphosis taking place in the blood and diminish the storing up of material in the tissues in the shape either of flesh or fat. The store of fat existing in the body is consequently more and more encroached upon, and in spite of the great metamorphosis taking place in the circulation, the body continues to get lean. A very long discussion on the *modus operandi* of fat as food (of which the above forms a small part only) will be found at p. 329.

The same observers have also examined the respiratory products in the case of a man suffering from Leukaemia, a disease in which there is an undue abundance of white corpuscles and scantiness of the oxygen-bearing red ones. They find, however, no marked difference in the respiratory products; in amount these closely approximate the standard of health. (Ibid. p. 319.)

G. Quincke on Specific Cohesion and Capillarity Constants

VARIOUS bodies were formed into drops, which were allowed to fall on a platinum or porcelain plane; after they had acquired the temperature of the surrounding air, certain measurements were made of their dimensions. The square of the vertical distance in millimeters of the upper surface of the drop from the vertical element of its meridian curve is the constant of specific cohesion, from which, by a method of calculation stated by the author, the capillarity constant can be inferred.

Fused substances of similar chemical composition, and at a temperature very near to their melting point, have the same specific cohesion.

Water, carbonates, and sulphates in the liquid state have double the specific cohesion of mercury; the same is true for the nitrates, metallic chlorides, sugars, and fats; metallic iodides and bromides have only half the mercurial value.

Lead, bismuth, and antimony have the same specific cohesion as mercury; platinum, gold, silver, cadmium, tin, and copper twice that amount; zinc, iron, and palladium thrice, and sodium six times the specific cohesion of mercury.

PHYSIOLOGY

Microcephalous Children

DR. BUCHNER has measured lately the *crania* of two *microcephalous* children, and by way of comparison adds the cranial measure of his own healthy infant son.

	Helena Becker.	Sophia S.	W. Büchner.
Age	6½ years.	3 years.	3 years.
Circumference of head.	13½ inches, Rh.	16¾ inches.	20½ inches.
From ear to ear .	6½ ,,	10 ,,	12½ ,,
From root of nose to occipital protuberance	8¾ ,,	10½ ,,	14 ,,

In both cases the size of head has apparently remained stationary from birth, and both children were born with closed *fontanelles*. One of the children is now exhibited at Darmstadt. Her height is 3½ feet; she cannot speak, walk, or stand, or seize hold of anything; in fact she is in a state of complete helplessness, with involuntary action of bladder and rectum. The upper portion of her skull, not larger than a man's fist, roof-like, flattened at the sides; the absence of forehead, a long aquiline nose terminating in a sharp point, an exceedingly diminutive receding chin, and a mouth with irregularly set teeth, with the orbital regions very prominent, give her quite an animal aspect. The mental phenomena are below zero; and the senses, though seemingly active, produce no ideas; her look is staring, vacant, devoid of expression; only bright shining objects and music attract her attention; she does not laugh but utters inarticulate animal sounds. Another characteristic feature is, that her limbs and head are subject to an involuntary unceasing agitation, like the reflex movements of a decapitated frog; she suffers also from a great want of sleep.

DR. OMANZA describes a method of registering photographically the beats of the pulse. The apparatus essentially consists of a small inverted funnel, having a long narrow stem and a caoutchouc base. This instrument is filled with mercury to a certain distance up the stem, and its base is applied to the heart or an artery; the oscillations of the mercurial column are then photographed by well-known processes. It is said that with this apparatus the apparently single stroke of the pulse is shown to consist of three, or even four, in succession.

SOCIETIES AND ACADEMIES

London

Royal Society, March 3.—The following papers were read: "Results of the monthly observations of dip and horizontal force made at the Kew Observatory from April 1863 to March 1866, inclusive," by Dr. B. Stewart. The author exhibited tabular statements of dip observations during six years, from which was deduced the existence of a semi-annual inequality, in virtue of which the dip is on an average 0'·27 lower in the six months from April to September, and 0'·27 higher in the six months from October to March than is due to its mean value. This result is in the same direction as that found by Sir E. Sabine for the six years ending March 1863, but is less in amount than the latter, that determined from the first six years exhibiting a range of 1'·31, while that determined from last six years only exhibits a range of 0'·54. From the first six years we deduce a mean dip equal to 68° 20'·07, corresponding to middle epoch April 1, 1860, and from the latter six, a mean dip equal to 68° 6'·62, corresponding to middle epoch April 1, 1866, while the secular change deduced from the first series is 2'·00, and that deduced from the last series is 1'·92, the mean of these two values being 1'·96. If we apply this mean value of the secular change to the mean result corresponding to the epoch April 1, 1860, in order to bring it to the epoch April 1, 1866, we obtain—

$$68° 20'·07 - 11'·76 = 68° 8'·31,$$

whereas that deduced from the second series corresponding to this epoch is 68° 6'·82. The former of these is 1'·69 higher than the latter. As regards the reason of this difference the author does not think it due to any personal equation in the observer. It would appear that the Kew observations present a peculiarity similar to those at Toronto, so that the difference of 1'·69 between the two sets of observations may probably be accounted for by this cause. The probable error of a single monthly determination of the dip, derived from the seventy-two monthly determinations, and after the application of the correction for secular change and annual variation, as derived from the results of these observations, has been made, is ±0'·96. There is, however, reason to believe that this probable error is increased to some extent by periods of disturbance, some of these of considerable duration. In the observations of horizontal force during the first six years, mean value of the horizontal force is equal to 3·8034, corresponding to the middle epoch April 1, 1860, and from the latter six years' observations we have a mean value of horizontal force equal to 3·8360, corresponding to epoch April 1, 1866; also the secular change deduced from the first six years is + ·0053, while that deduced for the second six is + ·0055, the mean of the two being + ·0054. If we apply this mean value of the secular change to the mean result corresponding to epoch April 1, 1860, in order to bring it to epoch April 1, 1866, we obtain 3·8034 + 0·0324 = 3·8358, a value which agrees as nearly as possible with that deduced from the second series, and corresponding to the same epoch which was 3·8360. The coincidence of these two values naturally leads us to imagine that the secular change of the horizontal force does not present the same peculiarity as that observed in the dip. In the observations of total force the mean of the April to September values of the horizontal component of the force in the last six years is 3·8346, corresponding in epoch to January 1, 1866; and the mean of the April to September values of the dip in the same six years is 68° 6'·83. The mean of the October to March values are for the horizontal force 3·8372, and for the dip 68° 6'·41, corresponding to epoch July 1, 1866. We may reduce these to a common epoch by applying to the former dip the correction —0'·96, this being the proportional secular change (as shown by these six years) necessary to reduce the former epoch to the latter. The former dip will therefore become 68°6'·83—0'·96 = 68° 5'·87. Reducing in the same way the horizontal force, we have

$$3·8346 + 0·00275 = 3·83735.$$

The values thus become as follows:—

	Hor. orce.	Dip.
From the April to September observations (reduced to epoch July 1, 1866)	3·83735	68° 5'·87
And from the October to March observations (corresponding to the same epoch)	3·83720	68° 6'·41

The total force derived from the first series will therefore be 10·28717, and that derived from the second series 10·29080, showing thus a difference of 0·00363 in British units as the measure of the greater intensity of the terrestrial magnetic force in the October to March period, than in the April to September period. This is in the same direction, and very nearly of the same amount, as that determined by Sir E. Sabine from the first six years, which exhibited a similar difference of 0·00317 in British units. Thus we find that the two series agree in showing nearly the same semi-annual variation for the total force, while the first period exhibits the greatest semi-annual variation of the dip. It ought, however, to be borne in mind that the two series bear a different relation to the disturbance period, the maximum of disturbances occurring about the middle of the first series, and the minimum near the middle of the second.

"Spectroscopic observations with the great Melbourne Telescope." By A. Le Sueur. The author stated that the spectroscopic observations of the nebulæ of Orion show distinctly that considerable nebulosity exists within and about the trapezium. The author's *telescopic* observations reveal a positive though comparatively faint nebulosity within and about the trapezium; the *spectroscope*, however, shows with much force that this nebulosity not only exists, but is comparable in brightness to that surrounding the trapezium at some distance,—the brightest part of the nebulæ in fact; that, in ordinary observations, therefore, the faintness or apparent complete absence of nebula is mainly due to the disturbing brightness of the four stars, not to any intrinsic extreme faintness or absolute vacuity.

In the examination of Jupiter, the large size of image is brought into prominent play; with the original Cassegrain image the light is barely sufficient, but with the image condensed (at pleasure within certain limits) fair work becomes possible, the spectrum being considerably bright. The lines G, F, *b*, C, D are seen without the slightest difficulty, and many other lines with attention. A marked feature is a nebulous band between C, D; from measures this turns out to be one of the bands examined by Mr. Huggins, 882 of his scale (C_6 of Brewster ?). With the slit perpendicular to Jupiter's equator and the advantage of a large image, an admirable opportunity is afforded of noting the behaviour of the lines as they cross the different parts of the surface, a spectroscopic picture of the planet, as it were, being presented beautifully to the eye. The nebulous line C_6 was specially and narrowly watched, but without any satisfactory evidence being elicited;

It is found that the spectrum of η Argo is crossed by bright lines. The mere fact of a bright line spectrum is not very difficult to ascertain on a good night; for although from faintness of the light the phenomenon is necessarily delicate, yet the bright lines occasionally flash out so sharply that the character of the spectrum cannot be mistaken. The most marked lines were made out to be, if not coincident with, very near to C, D, *b*, F, and the principal green nitrogen line. It cannot be determined whether the coincidence is with the magnesium group or the air-band; nothing more definite can be said than that the star line lies within the limits of the group. The comparison spectrum employed does not show F, but the position of the previously adjusted pointer, with reference to air lines in the neighbourhood, leaves little doubt as to the identity of the blue star line with F, due regard being had to the collateral evidence (when such close limits are reached) that C coincides with a red star line. The yellow (or orange?) line in the star has not yet received sufficient attention, it is, however, very near D. With the dispersion employed, D and the bright air line on the less refrangible side of D are well separated; so that, notwithstanding the delicacy of the star line, the author hopes if not to get satisfactory evidence of coincidence with a particular line, at least to eliminate one of the competitors; at present it cannot even be said whether the line may not be slightly more refrangible than D; the limits are, however, very small, placing the bright air group about 1,180 of Mr. Huggins's scale completely outside the possible range. The very faint nebulosity (if any) in the immediate neighbourhood of the star η is incompetent to give a trace of spectral lines with even a wide slit; for a considerable space S and *f* of η no lines at all are visible; the nearest nebula bright enough to show a line (the three usual lines are now easily seen on a good night over the brighter parts) is reached in the direction about 45° N p from η, and even then the distances from η, as judged by the appearance in the spectroscope with η threaded on the thus directed slit, is little less than one minute. This remark is of some importance in connection with the ordinary telescopic observations of the nebula, but is mentioned at this point to relieve any impression which might arise that the nitrogen line seen on the star spectrum is merely the chief nebula line crossing it. In the present state of the inquiry there is little doubt left as to the presence of hydrogen in the star; the other lines may perhaps be accounted for by nitrogen alone, or by nitrogen, magnesium, and sodium. On the whole the weight of collateral evidence will probably be considered to be in favour of the latter combination, with the possibility that for sodium may have to be substituted the substance which produces the line in sun-protuberance spectrum. For although there is no direct evidence as to identity of the line near D, if the coincidence were with the orange nitrogen line, it would be reasonable to expect a line in the star corresponding to the yellow line 1180±, yet none has been made out in that position; again, the second green line has probably less claims for visibility than the orange or yellow lines, yet in the star spectrum this line is not less well seen than that coinciding with the chief nitrogen line; these considerations, though perhaps not entitled to great weight, at least lead in the direction of the above inference. Owing to faintness of the general spectrum no dark lines are made out; one in the red is strongly suspected, and occasionally there is an appearance as if of a multitude over the spectrum generally, but they refuse to be seen separately and certainly. The spectroscope has decided that η in no way influences the configuration as now seen. Is not the presence of nitrogen and hydrogen in the star η a significant fact in connection with these changes, which appear to be nothing less than a destruction of nebula specially in its neighbourhood?

Orion has again been examined with an interesting result; the spectroscope proves that in and about the trapezium nebula exists comparable with the bright surrounding nebula. The stars, sharply focused to give a linear spectrum, being threaded on the slit singly or in pairs, or cautiously removed out of the field, it is seen that the bright lines cross the trapezium with little if at all diminished brilliancy. The ordinary telescopic view is therefore an erroneous one, produced by the disturbing effect of the bright group. Jupiter has been examined (generally on moonlight nights); with this object the original Cassegrain image is too faint for good work, but by interposition of a suitable lens the image is condensed at pleasure within certain limits; with the light thus increased the Fraunhofer lines G, F, *b*, E, D, are always easily seen, C also easily on a clear night; the lines to which special attention has been directed are the telluric lines 914 and 838 (for convenience of reference the numbers in Mr. Huggins's Jupiter and sky diagrams are used throughout.) These are the only lines seen with certainty between C and D. The identity of 914 and 838 rests partly on measures and partly on spark comparison, where, for the identification of 914, it is seen that this line is near to the air band 807 of Mr. Huggins's chemical scale. The line 914 is so easily seen, that having in mind Mr. Huggins's statement concerning the difficulty of discerning it at all, originally very imperfect measures on a bad night, and with the apparatus imperfectly adjusted, misleading in the same direction, this line was at first mistaken for 882, from which, however, it is separated far beyond the limit of error in a proper state of adjustment of apparatus. 882 is not seen at all with Jupiter at considerable altitude. On the night of December 29th, however, between the hours of 12.30 and 1, Jupiter being low, 882 was then seen almost as conspicuous as 914, which did not seem to have perceptibly increased in darkness by the additional absorption of the earth's atmosphere. On the night of December 14th (both objects being near the meridian) the spectroscope was turned on Jupiter and the moon alternately several times. On Jupiter 914 and 838 were easily visible, the former (as usual) the more conspicuous; on the moon no line could be certainly made out between C and D.

March 16.—The following papers were read:—"On the Contact of Conics with Surfaces." By William Spottiswoode, M.A., F.R.S. "Tables of the Numerical Values of the Sine-integral, Cosineintegral, and Exponential Integral." By J. W. L. Glaisher, Trinity College, Cambridge. Communicated by Prof. Cayley, LL.D. "Researches on Solar Physics.—No. II. The Positions and Areas of the Spots observed at Kew during the years 1865–66, also the Spotted Area of the Sun's visible disc from the commencement of 1832 up to May 1868." By Warren De la Rue, Ph.D., F.R.S., F.R.A.S., Balfour Stewart, LL.D., F.R.S., F.R.A.S., Superintendent of the Kew Observatory, and Benjamin Loewy, F.R.A.S. The paper commences with a continuation for the years 1864–66 of Tables II. and III. of a previous paper by the same authors; it then proceeds to a discussion of the value of the pictures of the sun made by Hofrath Schwabe, which had been placed at the disposal of the authors, and the result is that these pictures, when compared with simultaneous pictures taken by Carrington and by the Kew heliograph, are found to be of great trustworthiness. From 1832 to 1854 the pictures discussed are those of Schwabe, who was the only observer between these dates; then follows the series taken by Carrington, and lastly the Kew series, which began in 1862. A list is given of nine values of the sun's spotted area for every fortnight, from the beginning of 1832 up to May, 1868, and also a list of three-monthly values of the same, each three-monthly value being the mean of the three-fortnightly values which precede one of the three which follow it. These three-monthly values are also given for every fortnight. A plate is appended to the paper, in which a curve is laid down representing the progress of solar disturbance as derived from the three-monthly values; and another curve is derived from this by a single process of equalisation, representing the progress of the ten-yearly period. The values of the latter curve, corresponding to every fortnight, are also tabulated. From this Table are derived the following epochs of maxima and minima of the longer period:—

Minimum	Nov. 28, 1833.	
Maximum	....	Dec. 21, 1836.
Minimum	Sept. 21, 1843.	
Maximum	....	Nov. 1, 1847.
Minimum	April 21, 1856.	
Maximum	....	Sept. 7, 1859.
Minimum	July 14, 1867.	

This exhibits a variability in the length of the whole period.
Thus we have between 1st and 2nd minimum...... 9·81 years.
2nd and 3rd do. ......12·58 "
3rd and 4th do. ......10·81 "

Mean of all the periods ....11·07 years.

Another fact previously noted by Sir J. Herschel is brought to light, namely, that the time between a minimum and the next maximum is less than that from the maximum to the next minimum. Thus the times from the minimum to the maximum are for the three periods 3·06, 4·14, and 3·37, while those from the maximum to the minimum are 6·75, 8·44, and 7·44 years. In all the three periods there are times of secondary maxima after the first minimum; and in order to exhibit this peculiarity statistics are given of the light-curve of R Sagittæ and of β Lyræ, two variable stars which present peculiarities similar to the sun. Finally, the results are tested to see whether they exhibit any trace of planetary influence; and for this purpose 54 conjunctions of Jupiter and Venus, and 90 conjunctions of Venus and Mercury have been made use of with the following result, exhibiting the united effect of the sun's conjunctions, the unit of spotted area being one millionth of the sun's visible hemisphere.

Angular separation.	Excess or Deficiency. Jupiter and Venus.	Venus and Mercury.
0 to 30	+ 881	+ 1675
30 to 60	− 60	− 139
60 to 90	− 452	− 1665
90 to 120	− 579	− 2355
120 to 150	− 705	− 2318
150 to 180	− 759	− 1604
180 to 210	− 893	− 481
210 to 240	− 752	+ 547
240 to 270	− 263	+ 431
270 to 300	+ 70	+ 228
300 to 330	+ 480	+ 1318
330 to 0	+ 1134	+ 2283

Chemical Society, March 3.—Prof. Williamson in the chair. "On Refraction Equivalents." By Dr. Gladstone. Three distinct lines of research had led up to the discovery of these equivalents. The first was the influence of temperature on the refraction of light by liquids; the second, the refraction of mixtures or combinations as compared with that of their constituents; and the third, the refractive indices of different members of homologous series of organic compounds. As to the first of these it was found by the joint labours of Dr. Gladstone and the Rev. Pelham Dale, that the refraction and the dispersion decrease as the temperature rises. Further examination showed a close relation between the change of density and the change of the refractive index minus unity, which the investigators termed the "refractive energy," and which is expressed in the language of opticians as $u - 1$. This energy divided by the density, that is $\frac{u-1}{d}$ is called the "specific refractive energy," and is, in the case of liquids, a constant, not affected by temperature. This conclusion was subsequently confirmed by the experiments of Landolt, Wüllner, and Kühlman. As to the second line of research, that of the refraction of mixtures, solutions, and simple combinations, the conclusion was arrived at that here also the nearest approximation to the truth was given by $\frac{u-1}{d}$, and this conclusion has been fully confirmed by the careful experiments of Wüllner. The same general expression holds good also in the case of a gas or a solid in solution, and, indeed, it was expected to be so, for water, phosphorus, and sulphur have the same energies in the liquid and solid states. The question now presented itself, does an elementary substance retain its specific power of retarding rays when it is combined chemically with other elements? An affirmative reply was suggested by many considerations. It was, for instance, found that bromoform ($CH\,Br_3$) and bibromide of bromethylene ($C_2H_3Br_3$) have almost the same specific refractive energy as bromine itself. On the other hand, however, the investigators observed that isomeric liquids were not always identical in refractive energy, and that the replacement of hydrogen by oxygen in organic compounds effected a much greater optical change in some instances than in others. Hence the conclusion was drawn that the specific refractive energy of every liquid is composed of the specific refractive energies of its component elements, modified by the manner of combination. The third line of research was that of the refractions of different homologous compounds. The experiments of Delffs, of Landolt, and of Gladstone and Dale, have led to the view that in all the series containing the radicals, methyl and its congeners, the specific refractive energies increase as the series advances, and that the amount of optical change is less between the higher than between the members of the lower series. Landolt, adopting Gladstone and Dale's formula for the specific refractive energy, multiplied it by the atomic weight P; and this $P\frac{u-1}{d}$ he designated the "Refraction Equivalent." According to this representation, the refraction equivalent of a body is the sum of the refraction equivalents of its constituent elements. The great advantage of this kind of expression is, that it permits of the easy comparison of the optical properties of different substances. By making these comparisons, Landolt found that the refraction equivalent of carbon is 5·0; that of hydrogen, 1·3; and that of oxygen, 3·0. Direct experiments have given figures very close to these. The way of calculating the refraction equivalent of a compound from these data may be illustrated by ether. $C_4H_{10}O = 4\,(5{\cdot}0) + 10\,(1{\cdot}3) + 3{\cdot}0 = 36{\cdot}0$. The refraction equivalent deduced from observation is 36·26. A great variety of liquids have given the same equivalents by calculation as by direct investigation. Yet there are exceptions to this agreement with theory. The whole group of the aromatic hydrocarbons and their derivatives give refraction equivalents much above the calculated numbers. This anomaly must be due to an erroneous representation of the constitution of their nucleus, which cannot be greater than C_6H_3. However, the above method makes it possible to find the refraction equivalent of bodies, which could not otherwise be taken; for instance, of metals. The refraction equivalents of fifty elements have been determined in this way. It is to be remarked that the figures in the following list represent A of the solar system:—

Aluminium	8·4	Iodine	24·5—27·2
Barium	15·8	Iron	12·0—20·1
Bromine	15·3—16·9	Lead	24·8
Calcium	10·4	Magnesium	7·0
Carbon	5·0	Manganese	12·2—26·2
Chlorine	9·9—10·7	Mercury	21·3—29·0
Chromium	15·9—23·0	Nitrogen	4·1— 5·3
Copper	11·6	Oxygen	2·9
Hydrogen	1·3— 3·5	Phosphorus	18·3
Platinum	26·0	Sodium	4·8
Potassium	8·1	Sulphur	16·0
Silicon	7·5— 6·8	Tin	27·0—19·2
Silver	13·5	Zinc	10·2

It will be seen that some of the elements have a double value, and this peculiarity is in most cases coincident with a change of atomicity. Thus, iron in the ferrous salts has the equivalent 12·0, in the ferric salts 20·1, and since the refraction equivalent of iron in potassic ferridcyanide is 11·7, the view suggests itself that the metal is here in the same condition as in the ferrous salts. Great anomalies, however, present themselves in the case of oxygen. Its equivalent in many compounds is 2·9, but in others it comes down to 2·1, and in some cases, as in sulphates and phosphates, it would seem to be a negative quantity. This points to the conclusion that oxygen has the power of greatly modifying the action on light of those elements with which it is combined in a high proportion. On looking over the above list one is struck by the identity of the equivalents of those elements which have the same, or nearly the same, atomic weight. This property is still more prominent when the specific refractive energies of the elements instead of their refraction equivalents are considered. The following pairs may be noted in this respect:—

Iron,	0·214	Aluminium,	0·307	Bromine,	0·191
Manganese,	0·222	Chromium,	0·305	Iodine,	0·193

But the most suggestive comparison is that between the specific refractive energy and the combining proportions of those metals that form salts not decomposable by water. By combining proportion is meant the actual amount which will combine with a certain quantity of a salt radicle. A few of these metals may be quoted here:—

	Specif. refr. energy.	Combining proportion.
Hydrogen	1300	1
Albuminium	307	9·1
Calcium	260	20
Iron	214	28
Sodium	209	23
Potassium	207	39·1
Copper	183	31·7
Silver	125	108
Lead	120	103·5
&c.		

The regularity in the decrease of the numbers in the first column and the corresponding increase in the second column would suggest that the combining proportions of Silver, Lead, &c. ought to be halved in order to bring those elements to about their right places in the list. There is further a remarkable coincidence between the power of a metallic element to refract the rays of light, and its power to saturate the affinities of other bodies; of course, it must be borne in mind that a small combining proportion means a high saturating power.

The names of the officers proposed by the Council of the Chemical Society for election on the 30th March, are:—President: A. W. Williamson. New Vice-Presidents: E. Frankland, A. Matthiessen. New Members of Council: H. Bassett, F. Field, F.R.S.; M. Holzmann, Ph.D.; W. J. Russell, Ph.D.; R. Angus Smith, Ph.D., F.R.S.; John Tyndall, LL.D., F.R.S.

Entomological Society of London, March 7.—Mr. F. P. Pascoe, vice-president, in the chair. The Rev. R. P. Murray and M. J. C. Puls were elected members. Professor Westwood exhibited a number of old locusts, with a view to determine what is the true *Locusta migratoria* of Linnæus. The Rev. H. S. Gorham sent for exhibition specimens of *Sunius neglectus*, a beetle new to Britain, but probably confused in collections with *S. angustatus*. Mr. Albert Müller exhibited a curious acorn-like gall formed on the mid-rib on the underside of the leaf of a species of *Gnetum*. Mr. Janson exhibited a large number of butterflies collected by his son in Nicaragua in November and December last. Mr. Butler exhibited specimens of *Argynnis Adippe* and *Niobe*, in support of his previously expressed opinion that the two forms are but one species. Dr. Wallace exhibited dark varieties of *Melitæa Athalia*, and specimens of *Herminia derivalis*. Mr. Stainton exhibited *Cosmopteryx Sienigiella*, bred in this country from Russian larvæ which fed in reeds. Dr. Wallace exhibited cocoons and silk of several species of silk-producing moths, and addressed the meeting on the progress and science of Sericiculture in this country and in the colonies. The following paper was read: "Descriptions of twelve new exotic species of the Coleopterous family *Pselaphidæ*," by Professor Westwood.

Ethnological Society, March 8.—Professor Huxley, F.R.S., president, in the chair. Captain Campbell, R.E., was announced as a new member. Colonel Lane Fox read a paper "On the opening of two cairns near Bangor, in North Wales." One was situated on the summit of Moel Faban, and contained a cist in which an urn was found, together with s veral small dressed stones, probably arrow-heads and flakes, w rked not in flint, but in the trap and felspathic rocks of the neighbourhood. Other worked stones were found beneath the ci.. Professor Ramsay described the lithological characters of the materials. The second cairn examined by the author was called Carnedd Horvel, and contained fragments of an urn surrounded by particles of burnt human bone, but not protected by a cist. Among the speakers who took part in the discussion on this paper were Sir J. Lubbock, Bart., M.P., Professor Ramsay, Mr. J. Evans, Mr. J. W. Flower, Dr. Nicholas, and Mr. R. Hamilton. A paper was then read "On the earliest phases of civilisation," by Mr. H. M. Westropp. The author sought to show that every race passes through an invariable series of phases in definite sequence. These are the barbarous, the hunting, the pastoral, and the agricultural phases, which the author compared with the respective stages of infancy, childhood, youth, and manhood in the individual man. Numerous illustrations were adduced of different races exhibiting these several phases of civilisation in the successive stages of their development.

MANCHESTER

Literary and Philosophical Society, February 22.—Dr. J. P. Joule, president, in the chair, referred to the observations he had made in former years on the progressive rise of the freezing point of one of his thermometers. He had made a further observation, and found that a rise—unmistakeable, though very small—was still taking place after a lapse of twenty-six years since the bulb was blown.—Dr. F. Crace Calvert gave an account of the progress made during the last few months in the production of artificial alizarine, and expressed his opinion that many years must elapse before it can replace madder and its preparations in all their varied applications in calico printing; but ere long the purity of the substance artificially obtained may prove of great service to the calico printer, by enabling him to produce at a cheaper rate than now certain styles of prints as well as new styles and effects. Dr. Schunck remarked that practical success would in a great measure depend on the price of the raw material, anthracene, and on the amount of colouring matter to be obtained from it. The process of manufacture was, however, as far as he could judge, a very simple and easy one, requiring the use of no costly materials. He was convinced that the artificial product was identical with the natural alizarine of madder, the only difference being that the former was generally contaminated with some impurity which prevented its crystallising easily. Purpurine was not formed along with alizarine, as had been supposed. He also exhibited to the meeting some specimens of Turkey-red dyed with artificial alizarine, which had been sent to him by Mr. Perkin, and stated that the latter had already manufactured several tons of the new product. Dr. Schunck referred to a notice in the last number of the *Chemical News*, giving an account of a process for preparing pure alizarine from Turkey-red dyed cotton; and stated that almost the same process was described many years ago by himself. He also claimed to have been the first to point out that Turkey-red, madder pink, and all the finer madder colours are simply compounds of alizarine and fatty acids with bases.

"On the Organic Matter of Human Breath in Health and Disease," by Dr. Arthur Ransome. The vapour of the breath was condensed in a large glass flask surrounded by ice and salt, at a temperature of several degrees below zero. The fluid collected was then analysed for free ammonia, urea, and kindred substances; and for organic ammonia—the method employed being that invented by Messrs. Wanklyn and Chapman for water analysis. The breath of 11 healthy persons and of 17 affected by different disorders was thus examined, and the results were given in two tables. The persons examined were of different sexes and ages, and the time of the day at which the breath was condensed varied. In both health and disease the free ammonia varied considerably; the variation could not be connected with the time of the day, the fasting, or full condition. Urea was sought for in 15 instances—three healthy persons and 12 cases of disease—but it was only found in two cases of kidney disease, in one case of diphtheria, and a faint indication of its presence occurred in a female suffering from catarrh. The quantity of ammonia, arising from the destruction of organic matter, also varied, possibly from the oxidation of albuminous particles by the process of respiration; but in healthy persons there was a remarkable uniformity in the total quantity of ammonia obtained by the process. Amongst adults the maximum quantity per 100 minims of fluid was 0·45 of a millogramme, and the minimum was 0·35. A rough calculation was given of the total quantity of organic matter passing from the lungs in 24 hours—in adults about 3 grs. in 10 oz. of aqueous vapour, a quantity small in itself, but sufficient to make this fluid highly decomposable, and ready to foster the growth of the germs of disease. In disease there was much greater variation in the amount and kind of organic matter given off. In 3 cases of catarrh, 1 of measles, and 1 of diphtheria, the total ammonia obtained was much less than in health—less than 0·2 of a millogramme—a result probably due to the abundance of mucus in those complaints, by which the fine solid particles of the breath were entangled. In two cases of whooping-cough it was also deficient, but as they were both children, the lack of organic matter may have been due to their age. In cases of consumption also the total ammonia was less than in health; but in one case of this disease associated with Bright's Disease a larger amount of organic matter was given off, a portion of it due to urea. In kidney diseases the largest amount of organic matter of all kinds was found in the breath. The ammonia in one case of Bright's Disease was 1·8 millogrammes in 100 minims of fluid, and urea was largely present. Perhaps this fact might be taken as an indication of the need of measures directed to increase the activity of other excretory organs. In one case of ozona or offensive breath, the total quantity of ammonia obtained was greater than in any healthy subject, but the excess was chiefly due to organic matter. One convalescent case of fever was examined, and the total ammonia was found to be deficient. The air of a crowded railway carriage, after 15 minutes' occupation, was also tested by this method, and in about 2 cubic feet 0·3 millogrammes of ammonia and 3 millogrammes of organic matter were found. With reference to the presence of organic matter in the atmosphere, it was pointed out that the subject was in no way a novel one, and that it had, during the last thirty years, been very fully investigated by many observers, more especially by Schwann, Dusch, Schroeder, Helmholtz, Van den Broeck, Pasteur, and Pouchet, but it was shown that it is to Dr. Angus Smith that we owe the discovery of the readiness with which living organisms are formed in the condensed breath of crowded meetings, and the determination of the actual quantity of organic matter in the air of different localities. Mr. Dancer's calculation of the number of spores contained in the air was noticed, but a source of error was pointed out in the readiness with which organisms are developed in suitable fluids, even in the course of a few hours. Observations upon the organic particles of respired air had at different times been made by the author. 1. In 1857 glass plates covered with glycerine had been exposed in different places and examined microscopically. Amongst others in the dome of the Borough Gaol, to which all the respired air in the building is conducted, organised particles from the lungs and various fibres were found in this air. 2. During a crowded meeting at the Free Trade Hall, air from one of the boxes was drawn for two hours through distilled water, and the sediment examined after 36 hours. The following objects were noted:—Fibres, separate cellules, nucleated cells, surrounded by granular matter, numerous epithelial scales from the lungs and skin. 3. The dust from the top of one of the pillars was also examined, and in addition to other objects the same epithelial scales were detected. 4. Several of the specimens of fluid from the lungs were also searched with the microscope. In all of them epithelium in different stages of deterioration was abundantly present, but very few spores were found in any fresh specimen. On the other hand, after the fluid had been kept for a few hours, myriads of vibriones and many spores were found. In a case of diphtheria, confervoid filaments were noticed, and in two other cases, one of measles, and one of whooping cough, abundant specimens of a small-celled torula were found, and these were seen to increase in numbers for two days, after which they ceased to develope. These differences in the nature of the bodies met with probably show some difference in the nature of the fluid given off; but it was pointed out that they afford no proof as yet of the germ theory of disease. They simply show the readiness with which the aqueous vapour of the breath supports fermentation, and the dangers of bad ventilation, especially in hospitals. Dr. E. Lund and Dr. H. Browne stated that they had also made experiments, the results of which were, in general, confirmatory of those obtained by Dr. Ransome.

Microscopical and Natural History Section, January 31.—Mr. John Watson, president, in the chair. Mr. Charles Bailey read a paper "On the natural ropes used in packing cotton bales in Brazil."—Mr. J. Sidebotham exhibited some photographs of Pholas-bored Rocks, and said in reference to a paper by Mr. R. D. Darbishire on rocks bored by Pholas at the Little Orme's Head, that last spring he found many rocks so bored on both the Great and Little Ormes. The holes are most abundant near the tops of the mountains, and none whatever are met with very low down. At first sight the holes on the surface of the rocks having been weather-worn, and sometimes connected by channels with the natural fissures in the rock, it is difficult to say which or whether any of the holes have been caused by boring shells.—An interesting collection of Australian plants from Dr. Mueller, of Melbourne, was exhibited by Mr. H. A. Hurst.

EDINBURGH

Royal Society, March 7.—William Forbes Skene, vice-president, in the chair. The following communications were read: "On the Rate of Mortality of Assured Lives as experienced by Ten Assurance Companies in Scotland from 1815 to 1863," by James Meikle, actuary to the Scottish Provident Institution, communicated by Prof. Tait. In 1863 ten assurance companies contributed their mortality experience, embracing nearly 12,000 deaths, and published the result in May last. The present paper contained observations on the nature of that experience. After comparing the mortalities of the male population of England and Scotland, in which the mortality of males in Scotland was greater than in England up to about the age of thirty-five, the mortality of assured males was compared therewith, and also with the expectations of the Carlisle and the Actuaries' tables, the most general bases of life assurance computations. Assured life experience was shown to be greatly more favourable than the Carlisle up to about the age of fifty, thereafter less favourable; it was slightly more favourable than the Actuaries' at nearly all ages. Similar comparisons were made of the mortality of females. The whole observations on the lives were then thrown into various forms, so as to exhibit the effects of the selection exercised by the offices in assuring only healthy lives; and, after casting out the experience of the years when selection has its greatest force, comparing the remaining observations with the mortality of the population. The rate of mortality on policies effected "*with* participation in profits" was shown to be very much *higher* than on policies effected "*without* participation." The mortality on imperfect lives (those not assured at ordinary rates) was also discussed, and the amount of annual premium required to assure 100*l*. at death given for several classes of diseased lives. Interesting comparisons were also given of the causes of death of assured lives and of the population. It appears that assured lives have died in greater proportion from zymotic complaints, diseases of the brain, heart, and liver; while the populations have died in greater ratio from tubercular and lung diseases. The paper concluded with a description of the manner in which some of the results were adjusted and interpolated. The whole was very fully illustrated by diagrams, and called forth an interesting discussion.—"Brief Notes on Indian Society and Life in the Age when the Hymns of the Rigveda were composed," by John Muir. After stating that although the religious conceptions of the Indians of the Vedic era were in a comparatively simple and undeveloped stage, it would be a mistake to suppose that they had not made considerable advances in civilisation, the writer proceeded to give some account of the country which they occupied, and of the amount of opulence possessed by their kings, and to adduce a variety of particulars illustrative of their social and domestic relations and manners, their dress, food, drink, professions, relating to the tame and wild animals known to them, to their wars, armies, armour, and weapons, and to their poetry and incipient speculation. Some of the topics treated of were illustrated by metrical translations from the hymns of the Rigveda—(1) in praise of charity and liberality, (2) relating to the variety of men's tastes and pursuits, (3) satirically comparing the Brahmins with frogs reviving at the beginning of autumn, (4) descriptive of the miseries of gambling, (5) in celebration of warlike prowess and its instruments, and (6), containing a specimen of early speculation.

DUBLIN

February 28.—Sir Robert Kane, vice-president, in the chair, Professor J. P. O'Reilly exhibited a model and described a plan of a moveable barometer. The secretary read a note supplementary to their former paper "*Eozoon Canadense*, a mineral Pseudomorphite," by Professors W. King and T. H. Rowney, of Queen's College, Galway.

Natural History Society, March 2.—Rev. Professor Houghton, in the chair. Professor Houghton read a paper on the "Pathological lesions observed in the stomach of a lioness."—Professor Traquair exhibited fine specimens of *Calamosicthys* from Old Calabar, and Professor Macalister exhibited a specimen of *Ameida vulgaris*, from Mangerton, county Kerry.

PARIS

Academy of Sciences, March 7.—M. de Saint-Venant read a memoir on the establishment of the equations of the interior movements effected in solid ductile bodies beyond the limits at which elasticity can restore them to their former state. Papers were also communicated by M. Brioschi on the bissection of hyperelliptical functions, by M. Bourget on the algebraical development of the perturbative functions, and by M. Lucas on the calculation of the physical parameters and principal axes in a certain point of an atomic system.—M. Becquerel communicated a memoir on the electromotive forces of various substances, such as pure carbon, gold, platinum, &c., in the presence of water and of various liquids, in which he described the effects produced by plates of the substances above mentioned in contact with distilled water and various solutions.—M. Dubrunfaut presented some remarks upon the colours of rarefied gases submitted to spectrum analysis. He remarked upon the luminosity of hydrogen in Geissler's tubes at various degrees of tenuity, and stated that at its maximum attenuation a blood-red colour is characteristic of pure hydrogen, and that it communicates a similar coloration to the gas surrounding it. He also noticed the variation in the intensity of coloration of hydrogen, according to the calibre of the tubes through which the current passes. The purest nitrogen was stated to give a yellow tint, but usually the peculiarities of this gas are masked by the presence of water or mercury. The Torricellian vacuum was said to furnish the spectra of hydrogen, nitrogen, and mercury. The author also remarked upon some recent communications to the Academy on this subject.—A note by M. J. M. Gaugain on the electromotive force developed by platinum when in contact with various liquids was presented by M. E. Becquerel. The author stated that when one of two plates of platinum is taken out of acidulated water, washed in distilled water and restored to its place, a current is developed, which he ascribed to the presence of the water. He discussed the probable modes of action of this water, and inclined to the opinion that the plate still plunged into acidulated water forms a coat more positive than the platinum itself, which combination is destroyed by the water. He stated, however, that the electro-motive force is greatly increased by the exposure of the washed plate to a temperature of 150° C (=302° F.)—In a note on the illumination of transparent bodies, M. Soret adduced a further evidence in favour of his opinion that the presence of suspended particles takes a preponderant part in these phenomena.—M. Leroy de Boisbandeau communicated some remarks, illustrated by sketches, upon some curious icicles observed by him on the sides of a stone tank.—M. Aug. Houzeau noticed the absence of oxygenated water in the snow which fell at Rouen on several days during the past winter.—A note by Mr. Thudichum was presented, in which the author described an acid which, he stated, exists normally in the urine, and which he proposed to name *Kryptophanic acid*.—M. Jouglet communicated a note on the action of ozone upon nitro-glycerine and other explosive compounds. He found that nitro-glycerine, dynamite, iodide and chloride of nitrogen and some other bodies exploded when put into a vessel containing ozone, whilst picrate of potash was slowly decomposed, and gunpowder was sensibly altered in six weeks.—A note by M. Sacc on the distillation of tartaric acid was presented.—Notes on earthquake shocks observed at Lima and Ancona were communicated by the Minister of Public Instruction.—M. H. Sainte-Clair Deville presented a note by Father Denza, giving an account of a storm of sand accompanying rain and snow, which occurred in various parts of Italy on the 13th and 14th of February. The author remarked upon the periodicity of the phenomenon, and noticed its occurrence in various places; the sand, in all cases, appeared to be identical, and was considered by the author to come from the African deserts.—A note by M. H. Magnan on the cretaceous formation of the French slope of the Pyrenees and of Corbières, and especially on the Neocomian, Aptian, and Albian strata of that region, was presented by M. Daubrée.—MM. A. Roujou and P. A. Julien communicated a note on striæ observed on blocks of Fontainebleau sandstone and other rocks imbedded in the diluvium of the neighbourhood of Paris. These were stated to be the first striated blocks observed near Paris. M. Elie de Beaumont made some remarks upon these blocks. — M. A. Trécul communicated the fourth part of his remarks upon the position of the tracheæ in the ferns and on the ramification and radicular propagation of the rhizomes of some of those plants.—M. Duchartre presented a note by M. E. Prillieux on the influence of blue light upon the production of starch in chlorophyll. The author remarked that the production of starch was generally supposed to be due to the action of the yellow rays, and that blue light had no such effect. He considered that the results upon which this opinion was founded were due to the greater brilliancy of the yellow light, and by exposing a plant of *Spirogyra* deprived of starch to a more brilliant blue light, he found that formation of starch took place.—M. Duchartre also communicated a note by M. C. Cave on the free central placenta of the *Primulaceæ*, in which the author adduces as a further proof of the axial nature of that organ, that, on examination, the parts of recent formation are found to be outside the medullary sheath.—M. Auguste Duméril described some peculiar organs of the branchial apparatus in the Rays belonging to the genus *Cephaloptera*. These organs are the *prebranchial appendages* discovered by M. P. Panceri in *C. giorna*, which M. Duméril had detected in the large Indian *C. Kuhlii*. He stated that they occurred in no other fishes.—M. Pouchet noticed a transformation of the nests of the house martin (*Hirundo urbica*), and maintained that the nests of birds, instead of being, as generally supposed, constructed in the same way from century to century, really undergo certain progressive modifications of structure. In the case of the house martin, he stated that within the last forty years that bird has adopted a new form for its nests. The old nests are in the form of the quarter of a hemisphere, with a very small circular aperture for entrance. The improved nests, according to M. Pouchet, are in the form of the quarter of a hemi-ovoid with the poles much elongated, and the entrance is by a long transverse slit.—A second note on the tracheæ and differential characters of the lungs in birds, by M. Campana, was presented by M. C. Bernard. The author described the various modes of interbronchial communication, the mode of insertion of the pneumatic receptacles upon the lung, and the structure of the parenchyma of the organ.—M. Milne-Edwards communicated an extract from a letter from the Abbé David, giving the diagnosis of a new species of *Crossoptilon* (*C. cærulesceus*) discovered by him at Sse-tchuaro.—A note by M. Demarquay, on the reproduction and union of divided tendons was communicated by M. J. Cloquet. The author maintained that the regeneration of a divided tendon is effected by the proliferation of the elements on the inner surface of its sheath, in a manner analogous to the reproduction of bone by the periosteum. M. Dupuis presented some remarks on the confusion which has often occurred between the physicist, J. A. C. Charles, and the geometrician, J. Charles, and communicated some particulars relating to the biography of the two Academicians.

SYDNEY

Royal Society of New South Wales.—Mr. F. B. Miller, F.C.S., one of the assayers of the Sydney Royal Mint, described the practical results of his method for separating silver and gold directly by the use of chlorine gas, a process of which an account was given to the Chemical Society rather more than a year ago. At the Sydney Mint 6,820,198 ounces of gold have been received for coinage from the date of its establishment in May 1855 to December 31, 1868. The average composition of this gold would be about $94\frac{1}{3}$ per cent. of gold, 5 per cent. of silver, and $\frac{2}{3}$ per cent. of base metals; the gross amount of silver contained in the gold would be about 334,190 ounces, so that about 24,750 ounces of silver per annum have been lost to the colony for the want of a simple process of refinage. The gold now obtained in Queensland, as also that now brought from New Zealand, contains a much larger proportion of silver, so that the present loss to the colony is more nearly 42,000 ounces per year. The experience of the Sydney Mint proves that on the average there is a marked deterioration in the gold proceeding from Victoria, where the fineness is 96 per cent., northwards through New South Wales, where the average is $93\frac{1}{2}$ per cent., to Queensland, average $87\frac{1}{4}$ per cent. The silver can now be readily separated by passing a stream of chlorine gas into the melted gold for about an hour and a half, as it lies in a crucible heated in an ordinary melting furnace. The chlorine is at first rapidly absorbed, and the process is completed when a brownish yellow vapour appears. The chlorine is conveniently evolved from a self-acting generator, and 2,000 ounces of gold are readily refined in five hours, by three melting furnaces, 98 per cent. of the gold being delivered ready for coinage on the same day. The gold thus refined is perfectly tough, and contains only about one-half per cent. of alloy. The ultimate loss of gold is found to be only 19 parts in 100,000; the loss of silver is 240 in 100,000. The cost of refinage, including the above loss, but excluding rent of premises and expenditure, is five farthings per ounce. The silver is obtained in the form of fused chloride, and is reduced to the metallic state by plates of zinc combined with slabs of the chloride into a galvanic arrangement, devised by Dr. Leibriss. In twenty-four hours the chloride is completely reduced to the state of spongy silver, and 1,400 or 1,500 ounces could thus be readily treated in a day. No acid is required, and the zinc consumed is only 25 per cent. of the chloride reduced. The whole process, having been thoroughly tested at the time, is to be brought into active operation at once. It is already employed by some of the banks in Australia and New Zealand.

DIARY

THURSDAY, MARCH 17.

ROYAL SOCIETY, at 8.30.—On the Law which Regulates the Relative Magnitude of the Areas of the Four Orifices of the Heart: Dr. Herbert Davies. — On the Estimation of Ammonia in Atmospheric Air: H. T. Brown.

ROYAL INSTITUTION, at 3.—Chemistry of Vegetable Products: Prof. Odling.

LINNEAN SOCIETY, at 8. — The Flora and Fauna of Isle Ronde, near Mauritius: Sir Henry Barkly.—On Algæ found in the North Atlantic Ocean: Dr. Dickie.

ZOOLOGICAL SOCIETY, at 4.

CHEMICAL SOCIETY, at 8.—On Artificial Alizarine: W. H. Prekin, F.R.S.—On the Combination of Carbonic Anhydride with Ammonia and Water: Dr. Divers.

NUMISMATIC SOCIETY, at 7.

SOCIETY OF ANTIQUARIES, at 8.30.—On Ancient Round Barrows: Dr. Thurnam.

FRIDAY, MARCH 18.

PHILOLOGICAL SOCIETY, at 8.15.

ROYAL INSTITUTION, at 8.—On the Subway to France; J.F. Bateman, F.R.S.

SATURDAY, MARCH 19.

ROYAL INSTITUTION, at 3.—The Sun: J. Norman Lockyer, F.R.S.

MONDAY, MARCH 21.

LONDON INSTITUTION, at 4.

ROYAL ASIATIC SOCIETY, at 3.

ENTOMOLOGICAL SOCIETY, at 7.

TUESDAY, MARCH 22.

ROYAL INSTITUTION, at 3.—Nervous System: Prof. Rolleston, M.D., F.R.S.

ETHNOLOGICAL SOCIETY, at 8.—On Current British Mythology and Oral Tradition; Mr. Campbell of Islay.

INSTITUTION OF CIVIL ENGINEERS, at 8.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY, at 8.30.

WEDNESDAY, MARCH 23.

GEOLOGISTS' ASSOCIATION, at 8.

SOCIETY OF ARTS, at 8.—On Surface Decoration: W. Pitman.

GEOLOGICAL SOCIETY OF LONDON, at 8.—On the Discovery of Organic Remains in the Caribbean series of Trinidad: R. J. Lechmere Guppy, F.L.S., F.G.S. — On the Palæontology of the Junction-beds of the Lower and Middle Lias in Gloucestershire: Ralph Tate, F.G.S.—On the Geology of the district of Waipara River in New Zealand: T. H. C. Hood, F.G.S.

THURSDAY, MARCH 24.

ZOOLOGICAL SOCIETY, at 8.30.—On the Birds of Veragua: Osbert Salvin. —Exhibition of a metamorphosed Axolotl: W. B. Tegetmeier.—On two rare species of Pheasants recently added to the Society's Collection: Mr. Sclater.

CONTENTS

PAGE