THURSDAY, APRIL 25, 1878

THE COMING TOTAL SOLAR ECLIPSE [1]

II.

IN my former article I referred to the possible employment of slitless spectroscopes during the coming eclipse, the prism being replaced by a grating in some cases. It will be convenient here to give the results arrived at by the Siam Expedition with an instrument of this description, which, for shortness, was called a prismatic camera.

The plates secured present at first sight a very puzzling appearance; they are unlike anything ever obtained before, and a good deal of thought had to be spent upon them before all the knowledge they were afterwards found capable of furnishing to us was properly appreciated. One of the plates was exposed for one minute at the commencement of totality, the other for two minutes at the end. The differences between them are those due to the phases of the eclipse. In the first, two strong protuberances close together are photographed; these are partially covered up in the second, while another series is revealed on the following limb in consequence of the motion of the moon over the sun.

Now in both the photographs—that exposed for one minute and that exposed for two—the strongest of the prominences are repeated three times, that is to say, three spectral images of them are visible, each of these images being produced by light of different wave-lengths which the prominences emitted.

The question is what are these particular wave-lengths thus rendered visible? Unfortunately no photograph was taken of the cusps either before or after totality; a scale therefore was out of the question; and when the task of assigning wave-lengths to these spectral images fell upon Dr. Schuster and myself, while we were preparing the Report which was sent in to the Royal Society last year, the difficulties we encountered were very considerable.

Everybody I think will consider that we were justified in expecting the lines of hydrogen to be represented in such a photograph. Now the photographic hydrogen lines are those at F, near G and at *h*, and the silver salts usually employed are such that the action is most intense near G, less intense near *h*, and least at F; the running down from G to F being rapid, and that from G to *h* much more gradual, so that while at one end F may be said to be the limit of photographic activity, at the other it is continued long past *h*. We were therefore justified in assuming as the preliminary hypothesis, that the image of least refrangibility was produced by the F light of hydrogen, the more so as the continuous spectrum also photographed—which continuous spectrum, as we had independent means of determining, came from the base of the corona—gave us also an idea of the part of the spectrum in which each image was located.

Taking then F as a starting point and assuming the next line to be the one near G, we had a quite satisfactory method of checking the assumption, by comparing the real distance between the images with the calculated one.

[1] Continued from p. 483.

A goniometer was therefore brought into requisition, and the angular distance between F and the line near G carefully measured in order to determine the dispersion of the prism actually employed. This dispersion was one which should bring the images about as far apart as they were actually found to be; this therefore was so far in favour of our assumption, that is to say, it did look as if we had got hold, on the photographs, of images of the prominences built up by the F and G light of hydrogen.

It was next the turn of the third line, the one at *h*. On the assumption already made, it was easy to determine the distance from the G image, at which the one representing *h* should lie. In this place, however, we found no image whatever of any of the prominences.

Now this was a very extraordinary result, and there was only one way, so far as we could then see, of accounting for it. Dr. Frankland and myself, nearly ten years ago now, produced evidence which seemed to indicate that this line of hydrogen was only produced by a very high temperature. This being so, then, we should have to conclude that the prominences were of a relatively low temperature; this, however, I am far from saying, and here there is undoubted work of the greatest value to be done at the next eclipse, and I for one feel certain that our American cousins will do it.

I have not, however, yet referred to the strongest image of all shown in the photographs. This lies a little further from the central one than does the first on the other side of it. On the assumption before stated its wave-length lies somewhere near 3957. This number, of course, is only an approximate one, but the region occupied by the line was obviously so near the boundary of the visible spectrum, that a long series of experiments, in which we called in the aid of photography and fluorescence, was made in order to determine whether an unrecorded hydrogen line existed in that region. All I can say is that the point may be said to be yet undetermined. It is quite true that in several vacuum tubes which Dr. Schuster and myself employed, a strong line more refrangible than H was seen, but then these same tubes unfortunately showed us lines in the visible spectrum, which beyond all doubt did not belong to hydrogen. The elimination of impurities is such a delicate matter, and one requiring such a large expenditure of time, that our report was sent in leaving this point *sub judice*. We tried hydrogen at atmospheric pressure in order to get such a predominance of the hydrogen vibrations as to mask the impurities, but this did not serve us, for the continuous spectrum was so bright in the violet and ultra-violet as to render observations of lines next to impossible. Owing to many reasons, Dr. Schuster's absence from London being one of them, we have not been able to renew the search.

The near coincidence of this spectral image with the H-line leads us to ask the question whether Young's beautiful work in his mountain observatory might not help us on this point. Young found the calcium lines always reversed in the penumbra and near every large spot. This important statement shows us that calcium is one of the metallic vapours which is most frequently ejected from below into the prominences; it is possible, therefore, that the prominences, the spectral images of which were photographed, may have been due

to an eruption of calcium. This, of course, is only a suggestion, but the fact that it is a suggestion merely shows how important it is that this point should engage attention next July. If the prominences are then constituted as they were in '75, this violet line will doubtless turn up again, and that is why I have been most anxious to point out not only the conclusions to which we have been led, but the extreme difficulty of arriving at any conclusion whatever, unless by one method or another we have an absolute comparison of the spectrum of the prominences with that of the sun itself.

I have before referred to the fact of the registration on the plates of a continuous spectrum. If we were to suppose the whole light of the corona to be due to 1474 light, for instance, we should expect to get just as definite an image of the corona in the prismatic camera as in an ordinary one. And if everything outside the moon gave us nothing but a line spectrum, the moon's limb would have a perfectly defined edge. Now as a matter of fact, only one such edge is seen in the photographs. We have only one complete ring with a thoroughly defined hard outline, such as that to which reference has been made. This hard ring corresponds to the second spectral image of the prominences, and is a continuation of it. Supposing we were right about the prominences, the ring would be due to the high temperature h line of hydrogen (supposing us wrong it might be a companion line to 1474); as the observations of Respighi, Janssen, and others, in the Indian eclipse of '71 endorsed the American observations of '69 that the hydrogen lines are the strongest in the photographic parts of the corona, we may very possibly be really dealing with hydrogen.

Now the edge of the corona, or the upper part of it considering it as the sun's atmosphere, as seen on our photographs, is precisely such as would be given by homogeneous light; that is, there is a distinct image, and there is one image and not three or any other number. Have we any means of determining the wave-length of the light by which this image has been produced? Let me give an idea of one method which we employed:—A circle of the same size as the image of the moon on a photographic enlargement of the original negative was cut in paper and placed over the enlargement until the corona was symmetrical round it, as we know it to have been symmetrical round the moon's body, or nearly so, at that phase of the eclipse.

We found as a considerable endorsement of the assumption which we made regarding the hydrogenic origin of the chromospheric images, that the paper circle in this position had its circumference coincident with the hard ring to which I have referred as being a continuation of the middle spectral image of the prominences. Next, one of the ordinary photographs of the corona was enlarged to the same size as that of the one produced in the prismatic camera. When these were superposed so that the outlines of both coincided as much as possible, it was again found that the edge of the moon lay along the ring.

Now then for the continuous spectrum. The general woolliness of the photographs which at first sight gives rise to the idea that they were out of focus, and that there is nothing to be got out of them, is of course only in one direction, that at right angles to the edge of the prism employed. There is a well-defined structure running parallel to this direction, which of course is the line of dispersion; this structure is doubtless due to irregularities in the corona, drawn out by the prism into bands; it is easy to determine the limits of this continuous spectrum.

Examining the centre of the photographs we find that on one side the structure stops short at F, on the other it extends to a considerable distance beyond the prominence image in the ultra-violet, spaces of light being visible beyond 3530.

From these data we concluded that the continuous spectrum-giving region extends at least to a distance of 3′ of arc from the sun's limb. This continuous spectrum is well shown on photographs taken at the beginning and end of the eclipse. One of the plates of the prismatic camera was exposed, until the signal for the end of totality was given. Dr. Schuster states that all the observers agreed that the signal was given rather too late, and the fog on the plate indicates an intense illumination; nevertheless, the edge of the sun is not drawn out into a continuous band but rather into three distinct bands. It is probable, therefore, that when the plate was exposed, only the lower part of the chromosphere had appeared, and that it gave out light of such intensity that everybody imagined that the sun itself had come out of eclipse. I observed this myself in 1871, and a very striking fact it is.

So much then for the results obtained by the prismatic camera in '75. When the report is issued—and its issue cannot be much longer delayed—it will be seen that the hasty sketch I have now given can be followed in greater detail.

One of the most remarkable points about the expedition to Siam was the failure to obtain even spectra of the sun with the ordinary telespectroscopic cameras employed. No doubt the unforeseen delays which left very little time for the adjustment of instruments, have a great deal to answer for. I have little doubt that if the attempt is made next July, when any quantity of skilled help will be at hand, and any amount of rehearsal will be possible, that a full measure of success will be obtained, at all events for the most photographic part of the spectrum. An ordinary photograph of the corona was obtained by Dr. Schuster in two seconds; and my experience with photographic spectra enables me to say that this photograph was taken by means of an almost monochromatic light—that near G. Now as the coming eclipse will enable an exposure of almost 100 times longer than this to be employed, I do not think that the undoubted feebleness of the object need be feared. Besides, this method would enable us to pick up the light of those lower reaches of the chromosphere which, as has been already stated, are of such extreme brilliancy as to have been mistaken, on many occasions, for the sun itself.

Up to the present time no attempt has been made to obtain a photographic record of the polarisation of the corona. The difference of colours indicating radial polarisation observed by me when I used the biquartz in 1871, certainly have left the impression on my mind that it would be quite easy to obtain a permanent record of them. This would be a very valuable result, and one

which would set at rest a question which, though I consider it settled in my own mind, is yet, I believe, held to be still doubtful by many interested in these matters.

In what I have written I have touched only upon obvious work suggested by the previous observations. I have little doubt that the preparations of the skilled astronomers of the United States include many surprises and daring attempts among the solid work which we are quite certain of.

All here wish them the extremest measure of success, which I am sure their efforts will do more than command.

J. NORMAN LOCKYER

ATLANTIC SHELLS

Testacea Atlantica; or, the Land and Freshwater Shells of the Azores, Madeiras, Salvages, Canaries, Cape Verdes, and Saint Helena. By T. Vernon Wollaston, M.A., F.L.S. Royal 8vo, pp. 588. (London: L. Reeve and Co., 1878.)

IT is with a saddened feeling we take up our pen to notice this valuable contribution to malacology; for ere its pages had left the hands of the binder, its talented author had passed "into the shadowy land."

The name of Wollaston is connected ancestrally with more than one department of science, and the author of the present work has well maintained the honourable reputation of Dr. Wollaston, the discoverer of palladium and rhodium, and the founder of the Wollaston Medal and Award.

Compelled in 1847 to visit Madeira on account of his health, he commenced to collect the land-shells of the various outlying islands and rocks of the Madeiran Group; and although (as he tells us) insects, rather than mollusca, formed at that time the main object of his researches, he was able to add a considerable number of unmistakably new species to the careful and elaborate catalogue which had previously been compiled by his friend and companion, the late Rev. R. T. Lowe, then chaplain at Funchal, Madeira, and to whom the present work is dedicated.

So interested did he become in the insects and land snails of Madeira, that, although no longer compelled to submit to exile on account of his health, yet he returned again and again to Madeira and spent many weeks under canvas high up among the mountains collecting.

In 1858 he visited the Canaries in the yacht of his friend, Mr. John Gray, and again in 1859. On both these expeditions he was accompanied by Mr. Lowe. He was thus enabled thoroughly to explore the numerous and widely-scattered islands of the Canarian group under the most fortunate circumstances for collecting.

Under the same happy auspices he visited the Cape Verdes in 1866, Mr. Lowe again being his companion. In 1875 Mr. Wollaston sailed for St. Helena with Mr. Gray, where he spent six months in investigating the natural history of that remote little oceanic rock, being on this occasion accompanied by Mrs. Wollaston; the Rev. R. T. Lowe, his friend of many past years, having lost his life in 1874 on his outward voyage to Madeira.

Mr. Wollaston has felt it desirable to place these facts on record, in order to show that the several islands and archipelagos treated of in the volume before us—with the exception of the Azores—had all been visited personally by himself.

Although this book contains descriptions of no fewer than 558 species and varieties of land and freshwater mollusca, the author does not claim for it the position of a monograph, but rather a critical enumeration of all the forms hitherto recorded, with special reference to *habitat* in the several Atlantic archipelagos.

Out of the large number of species and varieties described in this work, there are only twenty-nine which are claimed by the author as *actual novelties;* sixteen of these are from the Canaries, nine from Madeira, two from St. Helena, one from the Salvages, and one from the Cape Verdes. Mr. Wollaston would have conferred a still greater service on his fellow-workers had he given short diagnostic characters of all the species enumerated. This would greatly have facilitated the identification of the various forms and saved the student much time and avoided the necessity of referring in many instances to other works. It is also much to be regretted that references are not given to the excellent published figures of most of the species which are to be found in Reeve's "Conchologia Iconica" and the second edition of Martini and Chemnitz's "Conchylien Cabinet" by Küster. Well-drawn and correctly-coloured figures are almost indispensable for the accurate determination of land-shells where form and colour are dominant characters. It is easy to *see* and identify the form, when carefully delineated, but almost an impossibility to convey it to the mind in words.

Mr. Wollaston has shown throughout the strongest preference for the limitation of species—at times becoming extremely hypercritical—and in some instances he seems to be altogether in doubt as to what constitutes specific rank. For example, under *Helix bicarinata* (*vide* p. 161), he states that he is far from certain that it is more than a phasis of *H. echinulata;* yet a few lines below he observes that he has never found a single example among thousands which could be strictly regarded as intermediate.

Again (p. 209) *Pupa fanalensis*, "this may be only a depauperated state of the var. *β. anconostoma* of the *Pupa umbilicata*, which the latter has gradually assumed through having found its way into the higher regions, nevertheless I believe it to be truly distinct."

It is strange to find a man with Wollaston's admitted talents and vast opportunities for observation struggling hard against the accumulated evidence of more than thirty years, and clinging tenaciously to the last to the doctrine of the immutability of species. Thus in his Summary (p. 561), when commenting on the difficulties which arise in defining what is a "species" and what a "variety," he adds, "these remarks are by no means intended to insinuate that the lines of demarcation between species, when correctly interpreted, are ever, in my opinion, *really* confused or doubtful, the exact opposite having always been my firm belief."

Eliminating what Wollaston calls "the European or more distinctly Mediterranean forms" from the catalogue, so that only "the Atlantic element" remains, "the actual species which range beyond the limits of a single archipelago are marvellously few—about four or five being common to the Madeiras and Azores, about

five or six to the Madeiras and Canaries, and about one to the Canaries and Cape Verdes; whilst between the Azores and Canaries there are only about five, and between the Madeiras and Cape Verdes about one. Moreover there are strong reasons for suspecting that some even of these (perhaps, indeed, most of them) may have been accidentally transported amongst the islands, through indirect human agencies, at a comparatively recent date; so that we are driven to conclude that, so far as the absolute *species* are concerned, of which their aboriginal faunas are respectively made up, the groups are practically almost independent of each other. And yet, in spite of this, I have had occasion to insist more than once upon the many characteristic *types* which, under the aspect of totally different but nevertheless allied species, permeate *to a greater or less extent* the entire 'province,' giving to it an amount of *unity*, through its several component parts, which it is scarcely possible not to recognise." (P. 563.)

The "*Atlantis* hypothesis" was clearly present to Wollaston when he wrote:—

"There may doubtless be many explanations, perhaps equally plausible, of these phenomena, but I must confess that none commends itself so thoroughly to my mind as the possible breaking up of a land which was once more or less continuous, and which had been intercolonised along ridges and tracts (now lost beneath the ocean) which brought into comparatively intimate connection many of its parts, even whilst others, though topographically near at hand, were separated by channels which served practically to keep them very decidedly asunder. It is on some such principle as this that I would account for the Canaries appearing to be not only as widely removed from Madeiras as perhaps even the Cape Verdes are, but (whilst further to the south) *to possess a fauna of which the 'Mediterranean' element is much more traceable.* This latter circumstance, which is shadowed forth likewise by the Coleopterous statistics, is by no means a fanciful one, whole groups which are indicative (more or less) of Mediterranean countries, but which have no single representative elsewhere in these Sub-African archipelagos, being quite at home at the Canaries." (P. 565.)

An evolutionary phase of mind must have prevailed with Wollaston when he penned the following sentence:—

"It is quite clear that the depression of certain tracts, and the upheaval of others, would produce an amount of disturbance in the fauna which could not fail to show itself *in some one way or other* which would afterwards become more or less decipherable; and I cannot conceive much difficulty in picturing the kind of change which might be brought about by the isolation of a cluster of individuals on a small rock, destined henceforth to become the habitat of a race which would, we may feel well nigh certain, rapidly mature for itself *some* slight distinguishing mark." (P. 566.)

But he quickly returns to his former state, and adds:—

"Considering how unmistakable the evidence is for the variability (in this particular sense) of many of the Atlantic types—a 'variability' so decided that a slightly different phasis has been assumed in certain of the Archipelagos, for nearly every separate island and isolated rock, it may sound, perhaps, somewhat paradoxical to speak, nevertheless, of their apparent freedom from further change; and yet if there is one fact more distinctly shadowed forth than another it is, without doubt, their *present stability*." (P. 566.)

Further on he continues:—

"After the most rigid and conscientious inquiry, I am bound to add that the 'developments,' so called, which might well be supposed to have been slowly elaborated, are (if any) simply *inappreciable*." (P. 567).

However widely we may disagree with Mr. Wollaston's conclusions on the *quæstio vexata* of species and varieties, his critical remarks are of great importance from the large series of specimens examined by him, and the fact that the types themselves were, in many cases, in his possession.

In an admirable lecture "On Insular Floras," by Sir Joseph Hooker, delivered before the British Association at Nottingham in 1868, he described the Madeiran flora as composed of two elements, the one clearly allied to that of the shores of the Mediterranean, the other totally different, and allied to none other but what is found in the Canaries and Azores, which he designated "the Atlantic Element."

That Sir Joseph Hooker's researches on the flora and Wollaston's observations on the insect and molluscan fauna of these Atlantic islands should bring out precisely similar results, will seem the less surprising when we remember the direct connection and interdependence existing between plants and insects, the latter acting as the great fertilising agents to the former; whilst the dependence of land snails upon plants is equally manifest.

All the difficulties raised by Wollaston as to the rare, peculiar, and isolated forms described by him were met and answered by Sir Joseph Hooker ten years ago. Assuming these minute islands to be relics of an older and once larger land-area which had been gradually reduced by subsidence, he pointed out that such a change, by contracting the area would intensify the struggle for existence. He showed that they were not new forms likely to increase and multiply, but rather old forms dying out. Also that in this exterminating process man was even a more destructive agent than the subsidences of land. For instance Madeira when discovered was so densely wooded that the settlers set fire to the forest and the fire raged for seven years, no doubt exterminating many species and reducing the number of others proportionately.

In Porto Santo rabbits had proved even more destructive than man; whilst in St. Helena, the introduction of goats in 1513, had almost exterminated the forests and the subsequent replanting of the island with exotic plants prevents the remaining indigenous vegetation from resuming its sway.

Whatever be the extent of area which we reclaim from ocean for our ancient "Atlantis," it is evident that formerly intercommunication existed between the Azores, Madeira, the Canaries, the Cape Verdes, and Southern Europe in Miocene times, for Prof. O. Heer considers some of the *Helices* of Porto Santo to agree with those of the Swiss molasse.

The poet's dream may therefore well be realised by the geologist:—

"Which tells, great pictured Continent, of thee
O blest ATLANTIS! can the legend be

Built on wild fancies which thy name surround?
Or doth the story of thy classic ground
With the stern facts of Nature's face agree?
What if no tongue may tell!—thy halo fair
Still lingers round the isles which slumber there."

("Lyra Devoniensis," p. 135).

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall

As Dr. Hunter has been good enough to mention my name in his letter to NATURE (vol. xvii. p. 59) in connection with a comparison of the rainfall of Northern India and the sun-spot period, I may, I trust, be allowed to express my opinion regarding the validity of some of the conclusions he draws therefrom. In the first place I would remark that Dr. Hunter's idea of the *winter* rainfall of Northern India being due to the *immediate rebound of the summer monsoon* from the Himalayan barrier is at variance with facts in the meteorology of the country. The rebound ought to take place directly the monsoon vapour-current impinges upon the Himalaya, *i.e.*, in the *summer*. In fact, it is by a succession of oblique rebounds from this impassable barrier that the monsoon is gradually reflected towards the N.W.P. and the Punjab.

The *winter* rains, on the contrary, have nothing to do with the monsoon, being, as is well known, due to a branch of the anti-trade, which, descending in the Punjab, is deflected by the Himalayas towards Behar and Bengal, occasionally reaching Calcutta, lat. 22° 35′ N.

Now between the rains of the summer monsoon and those of the anti-trade (or anti-monsoon as it is occasionally called), there is a well-marked interval of bright, clear, settled weather for two or three months throughout Northern India. After this interval the skies again become clouded, and about Christmas, or shortly after, the gentle but soaking rain of the cold weather sets in, and is repeated at intervals up to the end of March. It is evident, therefore, that the two currents, monsoon, and anti-trade, are totally unconnected with each other; and hence arises the desirability, especially in a question like the present, in which its secular variations are being discussed, of completely separating the rain of the former from that of the latter vapour current. I cannot but think that it is his omission to allow for these vapour currents that has led Dr. Hunter to offer such an erroneous explanation of the results obtained. According to him, copious precipitation should take place in the interval (October to December) between the two seasonal falls, during which clear weather is invariably present. It may be added that the period (December to April) which I took to comprise the winter fall, commenced after this interval.

The real explanation of the direct variation of the summer, and the inverse variation of the winter rainfall of North India, with the sun-spot period, is the hypothesis which first led to its verification coincidently and independently, by Mr. Hill and myself.

To enter upon a complete exposition of this hypothesis would occupy too much of your valuable space, but as it has been found to explain most of the anomalies which have hitherto proved such powerful obstacles (especially in extra-tropical regions) to the universal extension of the theory of sun-spot influence (I use the term advisedly) on the different elements of terrestrial meteorology, I will here briefly indicate its general outlines for the benefit of other workers in the same field of investigation.

The hypothesis, to start with, assumes *the solar radiation to vary inversely with the sun-spot frequency*.

It then takes account of the probable effects of such a variation upon the vapour-bearing currents throughout the globe with respect to velocity, direction, season, and latitude. According as trade, anti-trade, monsoon, or anti-monsoon, prevail (1) at different places at the same season, (2) at the same place at different seasons, so will specifically distinct effects arise both from the amount of vapour brought and its conditions of precipitation, to determine which, not only the general conditions introduced by latitude and season, but the local and peculiar meteorological functions of the region must be carefully studied.

Now as the principal effect of a secular change in solar radiated heat must be to cause a similar direct secular change in the normal convection currents of the atmosphere, we may expect the tropical trade-wind and monsoon regions to furnish us with some evidence, whether direct or indirect, in favour of the above hypothesis.

Little direct evidence has at present been adduced besides that given by Mr. Hill from a comparison of wind velocity in the N.W.P. (NATURE, vol. xvii. p. 505). A good deal of indirect evidence, however, is furnished in the monsoon regions by the occurrence of abnormal droughts and floods in contiguous districts (the drought in the N.W.P. and floods in Assam and Burmah last year were good examples of this kind) at the time of minimum sun-spot, when the velocity of the current being increased it travels in a more contracted channel, and, by a more equable distribution of rain at the time of maximum sun-spot, when the velocity of the current being decreased, it is more liable to extend laterally. In the trade-wind regions similar evidence is furnished by the fact of a deficiency of rain and cyclones at the time of minimum sun-spot, with a corresponding excess of both at the time of maximum sun-spot. The augmented velocity of the wind currents at the former epoch, preventing the formation of local areas of condensation and precipitation, and therefore (according to Messrs. Blanford and Eliot's theory of cyclone generation) of cyclones and their accompanying downpours; while the diminished velocity at the latter epoch favours the same.

Finally, the anti-trade which in its seasonal shifts north and south traverses the entire temperate zone, in the winter bringing rain to North India, Palestine, Madeira, California, &c., and in the summer to Northern Europe and Siberia, should give signs of a secular change in intensity and humidity, corresponding according to the hypothesis inversely with the sun-spots. In the summer, when large continental areas like Europe are more immediately under the direct influence of solar heat, local convection currents being set up will tend to disturb and complicate the effect of any general change in the strength of the anti-trade. In the winter, on the other hand, the obliquity of the incidental solar rays leaves the anti-trade in undisputed possession of the field. At this season, therefore, there should be a marked variation in the rainfall of the temperate zone, more particularly in those regions between 25° and 40° N. and S. lat., where the rainfall of *this season* is the chief rainfall of the year, corresponding *inversely* with the sun-spots. Even in those regions where the rain falls at all seasons, if we pick out the winter from the total annual falls, as was done by Mr. Draper, for New York (NATURE, vol. xvii. p. 15) in accordance with Mr. Hill's admirable suggestion (vol. xvi. p. 505), the results favour the hypothesis. But they do this in a far more marked manner where the rain of the entire year falls during the winter months, as in the Mediterranean and at Jerusalem, which have consequently hitherto been considered by Dr. Jelinek and Mr. Meldrum to afford strong evidence against the theory of a direct connection between rainfall and sun-spots. The inverse variation of the winter rainfall of Northern India is only another example of the same law, and shows how extremely important it is to analyse the seasonal variations separately before deciding the question by a mere cursory glance at the *total annual* falls. The apparent anomalies which Dr. Hunter finds presented in the North American rainfalls are, I think, due to his having compared the total annual falls. If he and other investigators will only take the hint dropped by Mr. Hill, and which I cordially endorse, of comparing the seasonal falls separately, they will find, I think, that while the summer rainfalls of the temperate zone show either a non-periodic variation, or symptoms of one coinciding directly with the sun-spots, the *winter* falls will in general show unmistakable signs of a variation coinciding *inversely* with that of sun-spot frequency and area.

E. D. ARCHIBALD

Sun-spots and Rainfall

I HAVE read with much interest Dr. Meldrum's paper on Sun-spots and Rainfall in NATURE (vol. xvii. p. 448), particularly that part of it in which Dr. Hunter's method of discussing the rainfall of Madras is criticised, and a method of inquiry in sun-spot researches is proposed. This method is, so far as I am aware, a new one, and as such, is deserving of careful examina-

tion as to how far it is applicable to the data submitted for discussion.

Dr. Hunter published the data for discussing the rainfall at Madras during the six sun-spot cycles, ending 1876, these being all the available data for Madras. As regards the sun-spots, we certainly have no *positive* data earlier, at least, than these cycles, whatever value may be attached to the approximate earlier figures supplied by Dr. Wolf. As regards, therefore, both the elements under discussion, viz., the sun-spots and the rainfall, the period discussed by Dr. Hunter represents the whole of the cycles for which material is available.

In dealing with this period, Dr. Hunter divides it into six equal cycles of eleven years each, this being substantially the average duration of the sun-spot cycles. I have arranged the relative numbers published in Wolf's last list (*Wolf. Astronomische Mittheilungen*, pp. 35-37), according to the cycles adopted by Dr. Hunter, with the result that all the six minimum years of sun-spots occurred either in the first year of the cycle, or in one of the immediately adjoining ones on either side of it, viz., in the second or in the eleventh years. As regards the years of maximum sun-spot, five out of the six occurred in the fifth or sixth years of the cycle, and the remaining year of maximum sun-spots occurred in the eighth year.

In his paper Dr. Meldrum states that as the sun-spot cycles are not all of the same length, it is evident that, by starting from any one year and going backwards over a long period, always using the same fixed number, a maximum and a minimum year might fall into the same group, and it was to obviate the occurrence of this contingency which the above analysis of Dr. Hunter's method shows did not occur during the period discussed by him, that Dr. Meldrum has proposed his new method as a more accurate mode of discussing the data.

To test the value of this new method of inquiry, I have arranged Wolf's relative numbers of sun-spots in accordance therewith, the maximum year of sun-spots of each cycle being placed in the sixth year, the minimum years being marked with an asterisk, and the "mean cycle" of eleven years being calculated from the thirteen years in the manner described by Dr. Meldrum :—

Year.	1811-23	1824-36	1832-44	1843-55	1855-67	1865-77	Means	Mean cycle.	Year of cycle.
1	1'6	8'1	26'3	*13'1	7'7	31'4	14'7		
2	4'9	16'2	*9'4	19'3	*5'1	14'7	11'6	14'9	1
3	12'6	35'0	13'3	38'3	22'9	*8'8	21'8	25'4	2
4	16'2	51'2	59'0	59'6	56'2	36'8	46'5	48'8	3
5	35'2	62'1	119'3	97'4	90'3	78'6	80'5	77'0	4
6	46'9	67'2	136'9	124'9	94'8	131'8	100'4	91'1	5
7	39'9	67'0	104'1	95'4	77'7	113'8	83'0	83'0	6
8	29'7	50'4	83'4	69'8	61'0	99'7	65'7	65'6	7
9	23'5	26'3	61'8	63'2	45'4	67'7	48'0	49'0	8
10	16'2	*9'4	38'5	52'7	45'2	43'1	34'2	34'6	9
11	6'1	13'3	23'0	38'5	31'4	18'9	21'9	24'6	10
12	3'9	59'0	*13'1	21'0	14'7	11'3	20'5	22'5	11
13	*2'6	119'3	19'3	7'7	*8'8	*7'0	27'5		

It will be seen from this table that with this arrangement the year of minimum sun-spots has occurred on the tenth, twelfth, thirteenth, first, second, and third years. By Dr. Hunter's arrangement the minimum years fell within a compact group of three consecutive years out of a cycle of eleven, whereas by Dr. Meldrum's arrangement they are scattered over seven years out of a cycle of thirteen. Further, I find that in the second cycle what is virtually a maximum year (viz., 1836 with 119'3 of sun-spots) fell within his minimum group, or in the thirteenth year. This is precisely the result which the method was designed to avoid, but as to the occurrence of which there was not an approach under Dr. Hunter's arrangement.

Again, if the same relative numbers of Wolf be arranged as Dr. Meldrum proposes, so that the year of minimum sun-spots of each cycle be placed in the ninth year of the thirteen years, it will be found that the maximum years are scattered over the twelfth, thirteenth, first, second, third, and fifth years of the series. By Dr. Hunter's method of arrangement five out of the six maximum years fell in the fifth and sixth years of the series, while the remaining one fell in the eighth year, thus again presenting a compact group, whereas Dr. Meldrum's method scatters them over more than half of his series of thirteen years.

An objectionable feature of this new method is the necessary repetition of figures which it involves. Thus, in the table given above, embracing six cycles, nine minimum years occur; and in the table in which all the minimum years are so arranged as to stand in the ninth year of the cycle, nine maximum years also occur, so that if the Madras rainfall were discussed by this method, the averages would be computed from tables in which the maximum and minimum years occur eighteen instead of twelve times.

Mr. Meldrum's method might be improved if he entirely struck out the first and thirteenth years of the thirteen years series, and simply "bloxamed" the remaining eleven years for the years of his "Mean Cycle;" that is, made the first of these years the mean of the eleventh, first and second; the second year the mean of the first, second and third. Even, however, with this change the method is inferior to that employed by Dr. Hunter, and the force of this statement will be the more readily recognised if it be kept in mind that we have no *positive* data from which the relative numbers of the sun-spots can be calculated prior to the time when Schwabe began his great work of sun-spot observation.

Edinburgh, April 22 ALEXANDER BUCHAN

Trajectories of Shot

MR. NIVEN was perfectly welcome to make use of my experiments and tables, as he has done, in trying to devise new methods of calculating trajectories of shot. And when he had satisfied himself that his methods possessed some advantages over others, he required no excuse whatever for their publication. But I altogether object to Mr. Niven's rule for finding v_β being connected in any way with the mode of calculation adopted by me. I beg, therefore, to place side by side Mr. Niven's rule, to which I object, and my rule, which I make use of, and so leave the matter. Mr. Niven says respecting v_β :—

"The first steps in our work must be to guess at it. The *practised calculator* can, from his experience, make a very good estimate. Having made his estimate he determines k. He uses the value of k in equation (a), and if he gets the velocity he guessed at, he concludes that he guessed rightly, and that he has got the velocity at the end of the arc. If equation (a) does not agree with him he makes another guess, and so on till he comes right."

The following is the course I pursue to find v_β. Refer to the table of coefficients and take out the value of k_α corresponding to the initial velocity v_α. Substitute in equation (a) and find a *first* approximate value of v_β. Now determine the mean value of k between v_α and v_β just found, substitute in equation (a), and thus find a *second* approximate value of v_β, which will generally be found sufficient. Otherwise adjust by proportional parts.

In this way the value of v_β is found accurately on the supposition that k has remained constantly at its mean value between v_α and v_β. Here the operations are of the simplest kind, and no guessing or practised calculator is required. And with a view to diminish the tedium of making these calculations, tables of $\Sigma(k)$, $\Sigma(k \div g)$, $(1000 \div v)^3{}_\alpha$, &c., have been calculated and printed, but their publication has been delayed on account of the experiments proposed to be made with low velocities.

Since Mr. Niven described the process of guessing as "*extremely* dangerous," there can be no doubt that the epithet was "extreme." As I supposed, he is not prepared to supply me with a single practical case where his condition of danger is satisfied. And if a case cannot be found then the objection falls to the ground. Whether we consider the range of values of k for spherical or ogival-headed shot, for velocities above 1,200 f.s., we shall find that $\frac{dk}{dv}$ lies between the limits o and $-$ 0'09, or, where k is a mean over an arc, between o and $-$ 0'05 about. And it is the smallness of this tabular value which renders it difficult, if not impossible, to satisfy Mr. Niven's condition of danger. But if this quantity had not been small, then the cubic law could not have been used even approximately. Mr. Niven is at liberty to take shot of any size used in practice, moving at any attainable velocity beyond 1,200 f.s., and the coefficients of resistance for either spherical or ogival-headed projectiles. The objection is Mr. Niven's, and he must take the onus of supporting it if he still thinks it of value.

I regret to have to write anything in opposition to Mr. Niven's paper, because in all other respects it appears to me a valuable contribution to the science of ballistics. F. BASHFORTH

Minting Vicarage, April 17

"Mimicry" in Birds

IF Mr. Young will look at the fourth edition of Yarrell's "British Birds," he will find that the fact he mentions (*ante*, p. 486) has already attracted notice, for he will there read (vol. i. p. 616) :—

"In places near Thetford, where the ringed plover is common, skylarks often imitate the note of that bird, making it part of their own song." ALFRED NEWTON

Magdalene College, Cambridge, April 19

OUR starlings here, which are a numerous and accomplished colony, have acquired the following notes of other birds :—Curlew, red-shank, blackbird, thrush, magpie, swallow, swift, chaffinch, house sparrow, hedge sparrow. The most successful imitations are those of the curlew, red-shank (the note uttered by the latter on taking wing), and the swallow. I have several times this year been certain that I heard a swallow twittering on the house-top, and found that the note proceeded from a starling.

The jays also in this neighbourhood, which are very plentiful, are very able mimics; the note of the carrion crow is about their most successful effort. H. H. S.

Riding Mill-on-Tyne, April 22

The Westinghouse Brake

UNDER the heading, "Notes," in NATURE, vol. xvii. p. 140, there is a paragraph describing the automatic brake of the Westinghouse Brake Company, St. Stephen's Palace Chambers, Westminster, the latter part of which refers to a ball which performs certain functions under different circumstances. In a previous account in the *Times*, three balls are mentioned as being used in the experiment; it also states that several gentlemen were investigating the mathematical principles under which these functions fell. I have not seen any results of their work, neither is there any comment upon it in NATURE. I therefore take occasion to mention it, in order that if any account of it has passed me, I may be informed of it, or that, if no results have appeared, this may lead to the subject being investigated by some of the mathematical correspondents of your esteemed paper. G. O. K.

Sound and Density

SINCE velocity of sound does not vary with *density* (Balfour Stewart, Chap. IV., "Elementary Physics"), would you kindly state the answer that should be given to the question, *Why* does sound travel quicker in *water* and *wood* than in *air*, and what is the relation between *density* and *velocity of sound* in *water*, *wood*, *air?* J. CAMERON

The Academy, Montrose, April 18

[The velocity of sound depends on the ratio between the mass and the elasticity, and in air (to which Prof. Stewart refers) it does not vary with the density of the air if its temperature only remain constant. In this case the denser the air the greater the mass, but the greater the elasticity in the same proportion. The ratio between mass and elasticity is thus unaltered, and therefore the velocity remains under these conditions the same.—ED.]

OUR ASTRONOMICAL COLUMN

THE TRANSIT OF VENUS IN 1882.—In addition to independent calculations of the circumstances of this phenomenon, founded upon Le Verrier's tables of the sun and planet, to which reference has already been made in NATURE, we have to record the publication of two memoirs upon the same subject, the first by Herr Bruno Peter, who is attached to the Observatory at Leipsic, the second by Dr. Karl Friesach, of Graz, which has been received within the last week. As was to be expected where practised calculators are working upon the same data, the direct results from the tables are in very close accordance with those previously published; indeed the advantage of so many repetitions of such work is not very evident. The differences which the calculated times of the geocentric contacts exhibit are almost wholly due to the employment of different semi-diameters of sun and planet. Le Verrier suggested (*Annales*, vol. vi. p. 40) that for the present the values to be employed should be respectively 958″·424 and 8″·305 for the mean distance. Herr Peter has used 961″·21 and 8″·472, and Dr. Friesach, 960″·0 and 8″·305. Their results for Paris mean times of contacts and least distance of centres are subjoined :—

Transit of Venus, 1882, *December* 6.

	PETER.	FRIESACH.
	h. m. s.	h. m. s.
First external contact ...	2 4 21·4	2 4 52·8
,, internal ,, ...	2 25 3·9	2 25 11·6
Last internal ,, ...	8 1 56·5	8 1 42·6
,, external ,, ...	8 22 39·0	8 22 1·6
Least distance of centres ...	5 13 29·9 } 641″·7 }	5 13 27·3 } 641″·5 }

ENCKE'S COMET IN 1878.—Observers in the southern hemisphere may be reminded that this comet is likely to be a pretty conspicuous telescopic object in their evening sky, in the first days of August. According to Dr. von Asten's latest researches on the motion of this comet, the period of revolution at the last perihelion passage was 1200·8 days, which, without taking any account of perturbations (not likely to be very material during the present revolution), would bring it again to perihelion on July 27. Mr. Tebbutt, of Windsor, N.S.W., has once found Encke's comet without assistance beyond his own calculations, but it will probably be Dr. von Asten's intention to furnish southern observers with a reliable ephemeris commencing with August next. Observations will not be practicable before the perihelion passage, the comet being too near to the sun's place.

THE "BERLINER ASTRONOMISCHES JAHRBUCH" AND THE MINOR PLANETS.—The volume of this ephemeris for 1880 has just appeared under the joint editorship of Professors Förster and Tietjen. The general contents are similar to those of preceding volumes. The ephemeris of the moon is again transferred, with full acknowledgment from the *Nautical Almanac*, and a great amount of labour of computation is thereby saved, which is made to tell upon the specialty of the work, the preparation of ephemerides of the small planets as far as their orbits are sufficiently determined. The reader who may be in search of the elements of these bodies will find in this new volume of the *Berliner Jahrbuch* the most complete and reliable table yet in the hands of astronomers. It includes orbits of all the minor planets to No. 172, with the exception of No. 155, *Scylla*, for which the necessary materials for calculation are wanting; and while referring to *Scylla*, it may be remarked that the four observations on November 8, 9, 22, and 23, 1875, cannot be represented by an elliptical orbit, which raises a suspicion that those of November 8 and 9 may belong to one planet, and those of November 22 and 23 to another, not, so far, recognised in the list. On examining the table of elements it is seen that No. 153, *Hilda*, has by far the longest period, while No. 149, *Medusa*, is credited with the shortest, according to the calculations of Prof. Tietjen. The observations of *Medusa*, however, extend over a period of eight days only, but they appear very exact, and it has happened that from a similar short interval of accurate observation, very close approximation to the true elements of an elliptical orbit has been attained; we may especially note the case of the short-period comet of De Vico in 1844: from eight days' very precise observations, M. Faye deduced an orbit which, as was pointed out by Prof. Brünnow, was almost identical with the result of his own elaborate investigation of the elements from the whole extent of observation. It is unfortunate that *Hilda* has escaped observation at the last opposition, since of all the small planets it is most desirable to keep this one in view, from the fact of its orbit allowing of a very much closer approach to the planet Jupiter than is possible in the case of any other.

The best orbit is by Kuhnert, but it is probable that the want of observations in 1877-78 is not wholly attributable to errors of elements, but in a certain degree to the position of the planet at a considerable distance from the ecliptical belt of the sky for which charts of small stars are as yet published, and, in addition, to its faintness. *Medusa*, which appears to have a very small inclination, may possibly be recovered in the ensuing summer.

GEOGRAPHICAL NOTES

TASMANIA.—The prospects of Tasmania are reported to be improving, owing to the development of its mineral resources. Very large quantities of tin, as is well known, have been discovered at Mount Bischoff, and quite recently the vast tract of country to the north-west, which has always been looked upon as valueless, has been explored with more care, and though it is probably of little use for agricultural purposes it has been found to contain enormous quantities of iron and other minerals. Traces of gold have been met with in the beds of some of the rivers, and copper has also been found. In the dense forests which are common in this region specimens of the *Eucalyptus* have been seen which are said to be more than 300 feet in height. Further exploration is still being carried on with a view to the accurate determination of the capabilities of this part of Tasmania.

AFRICAN EXPLORATION.—Journalistic enterprise is again contributing to the exploration of Africa, and this time Paris is vying with London and New York. M. P. Soleillet, who has been equipped under the auspices of the *Moniteur Universel*, departs soon for a tour of exploration in Equatorial Africa, to follow in the footsteps of his fellow-journalist Stanley. The development of openings for French commerce is to form a prominent feature in the undertaking.

PARIS.—The Paris *Société de Géographie* has elected Baron de la Roncière Le Nourry its president for the ensuing year.

FRENCH GUAYANA.—Dr. Crevaux, who was sent out by the French government to explore the interior of French Guayana, has returned to Paris after completing one of the most arduous journeys in the annals of South American discovery. After having fulfilled his instructions to penetrate to the Tumuc-Humac range, he determined to make the passage of these mountains, and descend into the valley of the Amazon, an attempt which has several times been tried in vain during the past three centuries. Although deserted by all his attendants, with the exception of a negro, he succeeded, after overcoming numerous obstacles, and battling with famine during a march of sixteen days across an uninhabited tract, in reaching the head waters of the Yary, from whence a canoe-voyage brought him to the Amazon. Of the 500 leagues traversed in this journey, 225 were hitherto completely unknown.

SURVEY OF NEW YORK.—The Second Annual Report of the State Geographical and Topographical Survey of New York, in charge of Mr. James T. Gardner, gives an account of the labours of the commission during the year. The principal work of the year has been the precise determination by primary triangulation of points in eleven counties, embracing an area of about 3,000 square miles; 167 points were located in an area of 1,700 miles in seven counties alone; the average has been one to every ten square miles. Fifty-one monuments have been placed in defining the boundaries of counties, this being a very important part of the work of the survey. The report is accompanied by several maps showing the progress of the work, the position of the stations, &c.

BIOLOGICAL NOTES

A NEW FRUIT.—Mr. Hollister has introduced from Japan to San Francisco a fruit, which is said in its native country to have as many varieties as are grown in this country of our apple, and the sweetness of the fruit is more or less retained by all of them. It is known as the Japanese Persimmon and, according to Mr. Hollister, is the most beautiful of all the fruits he had ever seen and the most delicious to the taste—four of the fruits which ripened with him weighed three quarters of a pound each, they were of a rich yellow colour, and looked like balls of wax; these were pronounced equal to a good pear or peach. The tree is a highly ornamental one, a prolific bearer, and as hardy as a pear. Its fruit season is from October to March. It seems quite adapted to the soil and climate of California. The grafted trees bear in four years. The seedlings require double that time, and are not reliable (*Proceedings*, Acad. of Science, California, in *American Naturalist* for March, 1878). This is the well-known and beautiful fruit of *Diospyros kaki*, Linn., fil., a near ally of the Persimmon of the Southern United States of America. Mr. Hiern tells us in his Monograph of the Ebenaceæ that the Chinese preserve this fruit with sugar, and that it has for a long time been in cultivation with them and the Japanese. The fruit has a thin skin, with a sweet orange-scarlet coloured flesh, with six or eight dark smooth seeds. It was beautifully figured in the *Gardeners' Chronicle* for 1872.

FOSSIL INSECTS.—Messrs. S. H. Scudder, of Cambridge, and F. C. Bowditch, of Boston, attached to Hayden's United States Geological and Geographical Survey, spent two months in Colorado, Wyoming, and Utah, in explorations for fossil insects and in collecting recent coleoptera and orthoptera, especially in the higher regions. They made large collections of recent insects at different points along the railways from Pueblo to Cheyenne, and from Cheyenne to Salt Lake, as well as at Lakin (Kansas), Garland and Georgetown (Colorado), and in various parts of the South Park and surrounding region. Ten days were spent at Green River and vicinity in examining the tertiary strata for fossil insects, with poor results. The tertiary beds of the South Park yielded only a single determinable insect; but near Florissant the tertiary basin described by Mr. Peale in one of the annual reports of the Survey was found to be exceedingly rich in insects and plants. In company with Rev. Mr. Lakes, of Golden, Mr. Scudder spent several days in a careful survey of this basin, and estimates the insect-bearing shales to have an extent at least fifty times as great as those of the famous locality at Œningen in Southern Bavaria. From six to seven thousand insects and two or three thousand plants have already been received from Florissant, the specimens from this locality being remarkable for their beauty. There is every reason to believe that the tertiary strata of the Rocky Mountain region are richer in remains of fossil insects than any other country in the world, and that within a few months the material at hand for the elaboration of the work on the fossil insects of the American tertiaries which Mr. Scudder has in preparation, will be much larger than was ever before subject to the investigation of a single naturalist. Mr. Scudder has in all now more than 12,000 specimens of fossil insects.

THE CLIMBING OF THE VIRGINIA CREEPER.—Mr. B. D. Halsted has studied the mechanism of climbing in the Japanese Ampelopsis, and finds that the clinging discs terminate tendrils which are homologous with main stems. While approaching a support, these discs flatten themselves on the inner side. The surface of the disc is papillose, and excretes a sticky substance; and the irregular contraction of the tendril draws the vine to its support (*Proc.* Boston Soc. Nat. Hist., January, 1878).

THE EARLIEST CHANGES IN ANIMAL EGGS.—The patient researches of van Beneden, Grieff, and Oscar Hertwig have discovered many interesting facts in the structure of simple ova when laid, the mode of fertilisation, and the first segmentation. Oscar Hertwig's last observations are on the starfish Asteracanthion (*Morphologisches Jahrbuch*, vol. iv. Part I.), and he describes the changes as follows :—The germinal spot of the unfertilised ovum first shows a separation into two portions, while part of the germinal vesicle contributes material out of which first one and then a second "directive corpuscle" is formed. By this time the germinal vesicle is undistinguishable, having left a small portion as the ovinucleus (*eikern*). When fertilisation takes place, the spermatozoon gives rise to a small body, the sperm-nucleus (*spermakern*); this body approaches the ovinucleus, and they fuse to form the segmental nucleus (*furchungskern*); this precedes the division of the whole egg into two cells. If such observations are extended to many species and confirmed by other observers, we shall have an important gain in our knowledge of the results of fertilisation.

GLACIAL AND POST-GLACIAL FISHES OF NORWAY.—We learn from the Danish *Naturen* the appearance in the third part of the *Nyt Magazin for Naturvidenskaberne*, of a paper, by M. Robert Collett, on the glacial and post-glacial fishes of Norway. These fishes, which are most perfectly preserved in chalk-lumps, the outer shapes of which more or less perfectly exhibit the outer shapes of the included fishes, are found in clay deposits some 360 feet above the sea; the fishes belong all to the existing fauna, displaying at the same time their Arctic and North Atlantic origin. Out of twelve species, described by the authors, the most common is the *Mallotus villosus*, which is found everywhere; one species, the *Clupea sprattus*, is worthy of notice, because it is now a native of more southern waters.

POACHING BIRDS.—Mr. N. B. Moore has made observations at the Bahamas on the *Certhiola flaveola*, which obtains nectar from the flower of *Verea crenata* by thrusting its bill at once through the petals into the nectary. It is only after the bird has made an opening that small black ants and other small insects are found in the nectary. But these birds also poach on the woodpecker's preserves. One day Mr. Moore observed a *Picus varius* extracting sap from a logwood sapling, and as the woodpecker flew away, two Certhiolæ appeared, perched near the sap-pits from which the juice was oozing, and by cunningly thrusting in their penicillate or bristle-tipped tongues, commenced to lap or suck the fluid into their mouths. This practice was constantly observed afterwards. Mr. Moore fixed the bowl of a teaspoon in a fork of the same tree, and placed some strained honey in it. In three days the Certhiolæ found this, and commenced to feed on it. They were followed by another bristle-tongued bird, *Dendræca tigrina*, and other species, who also attacked the woodpecker's sap-pits. These are interesting instances of apparent intelligence on the part of birds (*Proc.* Boston Soc. Nat. Hist., January, 1878).

GEOLOGICAL TIME[1]

IF a rigid body be in rotation about an axis of symmetry it will continue to rotate about that axis, but if it be set spinning about an axis inclined to that of symmetry the axis about which it spins will be continuously displaced relatively to the body; in other words, it will wabble.[2] This wabbling is well illustrated by the motion of a top whilst it is "going to sleep."

As the rotating body approaches more and more nearly the spherical shape, so does the wabbling become slower and slower. If the earth, which is nearly spherical, were to wabble in its diurnal rotation it would do so in about 305 or 306 days.

Dr. Haughton has lately published[1] an ingenious speculation, founded on the possibility of the wabbling of the earth, in which he seeks to determine limits to the duration of geological time from the observed absence of any motion of this kind.

The object of the short paper, of which I am here giving an account, was to combat the applicability to the case of the earth of Dr. Haughton's results.

The method pursued by him may be shortly described as follows :—If a continent were to be suddenly upheaved the earth's axis of figure (or strictly speaking, the principal axis of greatest moment of inertia) would be displaced from its previous position; immediately after the earthquake, the axis of rotation being where it was just before the earthquake, is no longer coincident with the axis of figure, and therefore a wabble is set up in the earth's motion. If it were not for frictional resistances that wabble would continue for ever after. But it is easy to see that, as the ocean is not rigidly connected with the earth, a tide of 306 days period would be set up. This tide would then rub on the sea-bottom, and would gradually reduce the wabble and bring the earth "to sleep" again like a top.

By reference to the estimate of Adams and Delaunay of the effects of tidal friction in retarding the earth's rotation, Dr. Haughton endeavours to find a numerical value for the frictional effect of such a 306-day tide as above explained. He then finds how long it would take to reduce a wabble of given amount to one of any smaller amount.

In a previous paper he had already shown that the elevation of the continents of Europe and Asia must have shifted the earth's axis of figure by 69 miles at the earth's surface. If, therefore, such an elevation took place suddenly, it must have started a wabble, in which the axis of rotation described a circle of 69 miles radius round the axis of figure.

But Dr. Haughton is of opinion that astronomical instruments are now so perfect, that a wabble of 5 feet in radius would be detected, and that it is not, therefore, permissible to suppose that the present actual wabble has a radius of even 5 feet. His numerical calculations, then, show that it would take 641,000 years to reduce the radius from 69 miles to 5 feet by means of the tidal friction, and he, therefore, concludes that, if Europe-Asia were manufactured *per saltum*, that event cannot have taken place less than 641,000 years ago, and that it may have been at a much more remote epoch.

The improbability of this supposition induces him to consider the case of elevation by 69 geological convulsions, each of which displaced the axis through one mile, and where the radius of the wabble is reduced to five feet between two successive convulsions. He here finds that the elevation of Europe-Asia must have occupied 27½ million years, and that no geological change altering the position of the earth's axis through one mile can have taken place within the past 400,000 years.

He lastly supposes that the wabble has a radius of 5 feet, and that the geological changes take place at such a rate that the increase of the radius is exactly destroyed by friction during each wabble, so that the radius of 5 feet remains constant. On this supposition he finds that the time required was 4,170 million years.

Now it appears to me, from this method of treatment, that Dr. Haughton is of opinion that a second earthquake of elevation following a first would necessarily increase the radius of the wabble. For if not, why does he postulate a lapse of time between successive earthquakes, and in the last case make the supposition of the increase of radius be exactly destroyed? It is on this point

[1] Abstract of a paper read before the Royal Society on March 14.
[2] I follow Dr. Haughton in the use of this very expressive word.

[1] Notes on Physical Geology, No. III., *Proc. Roy. Soc.*, vol. xxvi. p. 534.

that I venture to differ from him, for it seems to me, from dynamical considerations, that a second equal impulse following a first, at some time within 306 days, might either double the wabble, alter its amount, or annihilate it, according to how it was timed to take place.

If I am correct in this view, I cannot but think that the estimate of geological time falls to the ground. For even if the elevation of continents took place impulsively, we can have no possible data for judging of how the earthquakes were timed with reference to the position of the axis of rotation, and unless they were properly timed the radius of the wabble could not increase; and the increase of the radius is, I imagine, essential to Dr. Haughton's method.

But if we set aside the impulsive theory of elevation, the work contained in my paper, "On the Influence of Geological Changes on the Earth's Axis of Rotation,"[1] will be applicable; for I there considered the effects of a slow continuous elevation of continents. In that paper I show that such a mode of elevation would set up a wabble of 306 days' period in the earth's motion. But this wabble is of quite a different character from that contemplated by Dr. Haughton, for it is unsymmetrical, so that the axis of rotation coincides with the axis of figure every 306th day.

By a very simple application of a formula given in that paper, it will be found that, supposing the continuous elevation to take place at such a rate that the axis of rotation is 5 feet distant from that of figure when at its greatest distance, then the axis of figure must be travelling with reference to the solid earth at the rate of $\frac{3}{10}$ of a second of arc per annum. Thus, in 19,200 years it will have travelled over 1° or 69 miles. That is to say, Europe-Asia might have been elevated in 19,200 years without the axis of rotation ever having described a circle of more than 5 feet in *diameter*. If the elevation were then to stop suddenly a symmetrical wabble would be set up (such as that considered by Dr. Haughton), and the *radius* of this wabble could not be greater than 5 feet, and might be zero, according to the exact time of the stoppage.

This investigation makes no reference whatever to the effects of tidal friction, and there are certain considerations which lead me to believe that even the above estimate of time might be largely reduced.

The conclusion at which I arrive therefore is that the elevation of Europe and Asia might have taken place in very much less than 20,000 years without leaving behind any wabbling in the earth's motion traceable by astronomical observations. Dr. Haughton's views, if generally accepted, are of the very greatest interest to geologists, and they therefore merit the strictest examination; as I have devoted a good deal of time to this subject I thought it might perhaps be useful to write this note. Should my present criticism be incorrect, there is little doubt but that it will meet its just fate of refutation.[2]

G. H. DARWIN

EARLY ELECTRIC TELEPHONY

IN 1861 the first successful attempt at the construction of an electric telephone was made by Philip Reis, a teacher in a school at Friedrichsdorf, near Homburg. On October 26, 1861, Reis showed his instrument, which he termed a "telephone," to the Physical Society of Frankfort-on-the-Main; and on that occasion he succeeded in electrically transmitting various melodies, which were distinctly heard throughout the room. In the paper he read before this Physical Society, published in the annual report of the Society for 1861, Reis states:—"Melodies were sung, not loudly, into the transmitting apparatus placed in a hospital some 300 feet away from the audience, care being taken that no sound could be heard, by direct transmission, or by conduction along the wires. The sounds of various musical instruments were clearly reproduced, as the clarionet, horn, organ-pipe, and even harmonium and pianoforte

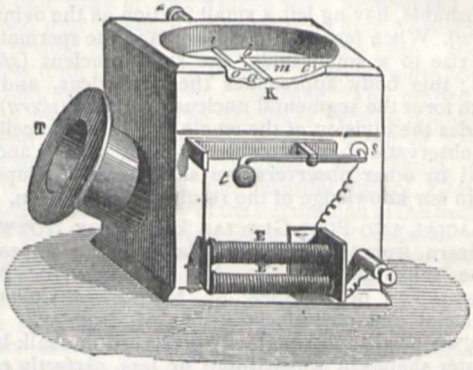

FIG. 1.—This and Fig. 2 show the usual but imperfect form of Reis's telephone. Fig. 1 is the transmitting apparatus. T represents the mouthpiece, *m* the membrane closing the upper portion of the box K, a portion of which is cut away to show the movable lever, *a b c*, resting by a projecting point *b*, on the platinum disc *o*, fixed to the centre of the membrane and connected with the binding screw 2. The arm *a b c* is metallicly connected with the binding screw 1. The key *t* closes the circuit when the instrument is in use, and the electro-magnet E E is for the purpose of receiving communications.

when the transmitter was placed on their sound-boards, provided the tones were within the compass of *f* to F'''. Articulation was not reproduced equally well. Consonants, however, were in general pretty clearly heard, but not the vowels." In this report, which is entitled "Telephony by Means of Electric Currents," Reis shows how he was led to the construction of his instrument by a study of the mechanism of the organ of hearing, and of the manner whereby sounds are perceived by the human ear, and he gives a series of diagrams representing the resultant curves that would be produced by the combination of various concords and discords. Thus, he was led to perceive that "if it were possible to create, in any manner, a mode of vibration whose curve resembles that of any tone or chord, then a sensation would be produced similar to that given by the tone or chord itself." This principle, he affirms, guided him onwards.

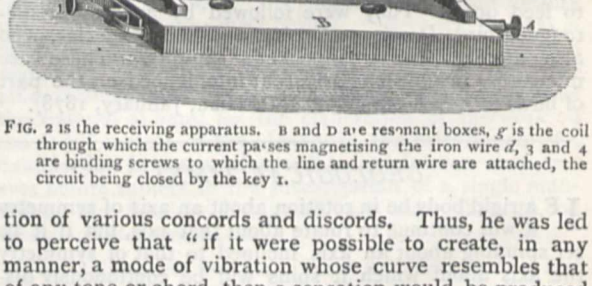

FIG. 2 is the receiving apparatus. B and D are resonant boxes, *g* is the coil through which the current passes magnetising the iron wire *d*, 3 and 4 are binding screws to which the line and return wire are attached, the circuit being closed by the key 1.

The first instrument he made was constructed of very homely materials. The bung of a beer-barrel was pierced through with a conical hole, the smaller end was then covered by a membrane, the skin of a German sausage being used for this purpose; to this was fixed, with a drop of sealing-wax, a little strip of platinum joined up to one

[1] *Phil. Trans.*, vol. 167, Pt. 1.

[2] Since this has been in type Dr. Haughton has read another paper before the Royal Society, in which he concludes, from purely geological evidence, that "the hypothesis of a shifting pole (even if permitted by mechanical considerations) is inadmissible to account for changes in geological climates." Therefore whether he agrees or not in the justice of my mechanical criticism, he seems to be of opinion that the wabbling of the earth will not give geologists much light as to the duration of geological time.

end of a small battery; a wire was adjusted near to, but not touching, the platinum strip; this wire led to the receiving instrument, and thence back to the other pole of the battery. On speaking into the conical orifice in the bung the membrane was thrown into vibration, and the attached metal strip coming into contact with the adjoining wire, momentarily completed the electric circuit. The vibrations of the membrane thus sent a corresponding series of intermittent currents into the receiver, which, in the first instance consisted simply of

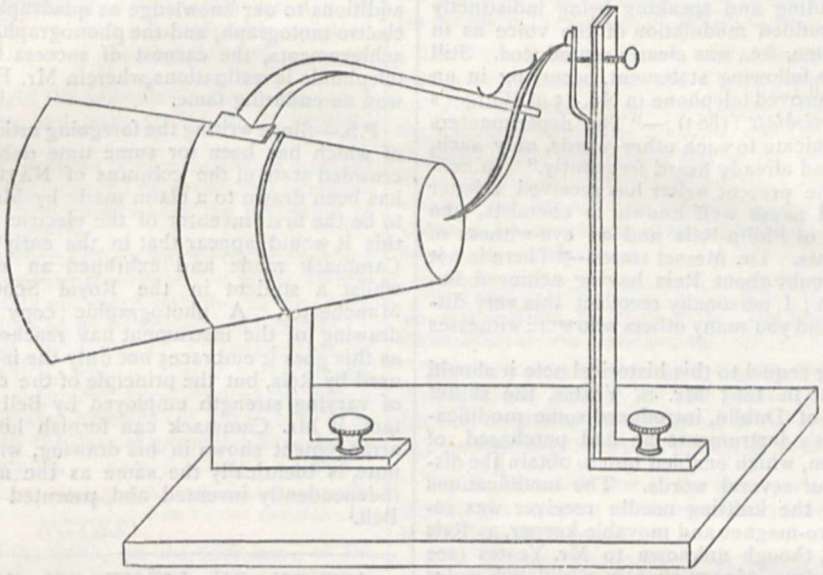

FIG. 3.—Sketch of improved form of transmitter made by Reis in his telephonic experiment (1862).

a knitting needle surrounded by a coil of wire, and placed on a violin to serve as a sound-board. Though Reis afterwards considerably improved upon his earlier instruments, the improvements do not seem generally known, and the arrangement just described is substantially that usually constructed and figured as Reis's telephone (see (Figs. 1 and 2.)

In Dingler's *Polytechnisches Journal*, vol. 169 (1863), p. 29, is a report on Reis's improved telephone by Legat, inspector of telegraphs in Cassel, &c. This report was originally printed in the *Journal* of the East German Telegraph Company for 1862. Considerable modifications are here shown in both transmitter and receiver. The membrane is formed of a collodion film and is not loaded with any metal

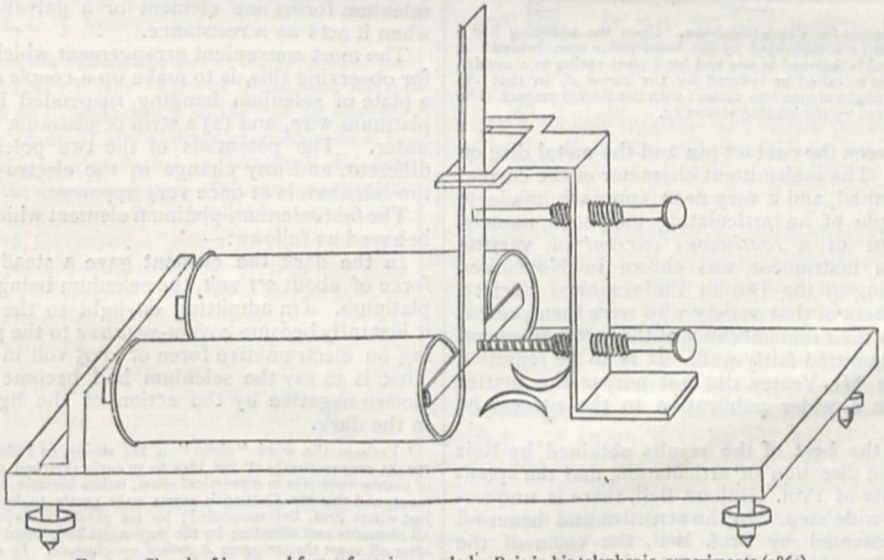

FIG. 4.—Sketch of improved form of receiver made by Reis in his telephonic experiments (1862).

contact-breaker. A light S-shaped arm, supported a little above its centre, so as to move freely in a vertical plane, abuts at the lower end against the membrane, and at the upper against the contact pin (Fig. 3). The circuit is completed through the cross-piece which supports the S-shaped lever; the least outward motion of the membrane would thus break the contact, and in this way very feeble vibrations were able to be transmitted. The receiver consisted of, practically, a horse-shoe magnet fixed horizontally on a sound board; the movements of a light iron keeper, adjustible by a spring before the poles of the magnet, reproduced the original sounds (Fig. 4). Here it will be noticed a molar motion of the iron has replaced the molecular motion first employed. A much louder sound is thus

obtained, and by bringing the iron keeper near to, or even into gentle contact with the magnet, every grade and rate of simple vibration could be reproduced, as the present writer is able to testify.

With this instrument Reis obtained better results and even transmitted imperfect articulation. Legat speaks of single words in reading and speaking being indistinctly heard; but any sudden modulation of the voice as in surprise, interrogation, &c., was clearly reproduced. Still more definite is the following statement, occurring in an article on Reis's improved telephone in No. 15 of Böttger's *Polytechnisches Notizblatt* (1863):—"The experimenters could even communicate to each other words, only such, however, as they had already heard frequently." In confirmation of this the present writer has received a letter from Dr. Messel, a name well known to chemists, who was a former pupil of Philip Reis and an eye-witness of his early experiments. Dr. Messel states—"There is not the shadow of a doubt about Reis having achieved imperfect articulation; I personally recollect this very distinctly and could find you many others who were witnesses of the same fact."[1]

As an interesting sequel to this historical note it should be mentioned that in 1865 Mr. S. Yeates, the skilful instrument maker of Dublin, introduced some modifications in one of Reis's instruments he had purchased, of the usual early form, which enabled him to obtain the distinct articulation of several words. The modifications were twofold: (1) the knitting needle receiver was replaced by an electro-magnet and movable keeper, as Reis had already done, though unknown to Mr. Yeates (see Fig. 5); and (2) a drop of very slightly acidulated water

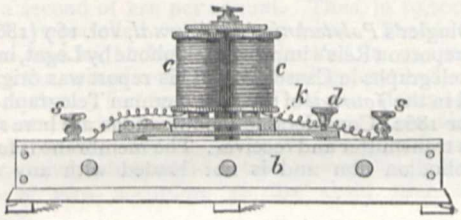

FIG. 5.—Yeates's receiver for Reis's telephone. Upon the sounding box *b* an electro-magnet *cc* is supported by the brass pillar seen behind. A light iron keeper *k* is fastened at one end by a steel spring to a wooden bridge, which can be raised or lowered by the screw *d*, so that the keeper can be brought almost into contact with the electro-magnet. The circuit is completed by the binding screws *s s*.

was placed between the contact pin and the metal disc on the membrane. The intermittent character of the current was thus abolished, and a very near approach made to the true principle of an articulating telephone, namely, the employment of a *continuous current* of varying strength. This instrument was shown in November, 1865, at a meeting of the Dublin Philosophical Society, and some members of that society who were then present have testified to their remembrance of the fact that several words were transmitted fairly well. It is to be regretted that at the time Mr. Yeates did not pursue the matter further, nor give a wider publication to the success he obtained.

But between the best of the results obtained by Reis and others in the direction of articulation, and the splendid achievements of Prof. Graham Bell, there is unquestionably a very wide step. In the sensitive and beautiful instrument discovered by Prof. Bell, the voice of the speaker generates thrills of magneto-electricity, which, being strictly proportional to the sonorous vibrations, reproduces the voice and its expression in the receiver in a fairy-like far-away whisper. Nevertheless it must be borne in mind that it is unlikely the telephone of the future will employ the voice to generate the driving power, but only to modulate the flow of a current obtained by coarser means. It is in this direction that Reis worked, and though his method was faulty in the employment of an intermittent current, the same cannot be said of the arrangements adopted by Mr. Edison, of New Jersey. And inasmuch as Mr. Edison has already discovered and brought to a practical issue such remarkable additions to our knowledge as quadruplex telegraphy, the electro-motograph, and the phonograph, we have, in these achievements, the earnest of success to those excellent telephonic investigations wherein Mr. Edison has already won an enduring fame.

W. F. BARRETT

[1] My best thanks are due to Dr. Messel for much information concerning Reis and for a reference to his papers in the journals alluded to.

P.S.—Since writing the foregoing article, the publication of which has been for some time delayed owing to the crowded state of the columns of NATURE, my attention has been drawn to a claim made by Mr. John Cammack, to be the first inventor of the electric telephone. From this it would appear that in the early part of 1860 Mr. Cammack made and exhibited an electric telephone, whilst a student in the Royal School of Medicine, Manchester. A photographic copy of the original drawing of the instrument has reached me, and so far as this goes it embraces not only the intermittent current used by Reis, but the principle of the continuous current of varying strength employed by Bell and Edison. In fact, if Mr. Cammack can furnish historical proof, the arrangement shown in his drawing, with its explanatory note, is identically the same as the method, long after independently invented and patented by Prof. Graham Bell.[1]

W. F. B.

ACTION OF LIGHT ON A SELENIUM (GALVANIC) ELEMENT

IN the course of a series of experiments on the electrical behaviour of selenium, undertaken with a view to remove, if possible, the difficulties in the way of constructing constant resistances of this material, I have had occasion recently to investigate the effects of surface tension due to light.

I find that the action of light on crystalline selenium (annealed at 200° C.) is much more striking when the selenium forms one element of a galvanic couple than when it acts as a resistance.

The most convenient arrangement which I have found for observing this, is to make up a couple consisting of (1) a plate of selenium hanging suspended by means of a platinum wire, and (2) a strip of platinum foil, in distilled water. The potentials of the two poles are not very different, and any change in the electro-positiveness of the selenium is at once very apparent.

The first selenium-platinum element which I constructed behaved as follows:—

In the dark the element gave a steady electromotive force of about 0·1 volt, the selenium being *positive* to the platinum. On admitting daylight to the selenium plate it instantly became *electro-negative* to the platinum, showing an electromotive force of 0·05 volt in that direction. That is to say the selenium had become 0·15 volt more electro-negative by the action of the light than it was in the dark.

[1] Perhaps the word "claim" is too strong, as I observe Mr. Cammack speaks very modestly of the idea he so early sketched out. Such ideas are of course valueless in a practical sense, unless brought to the test of experiment, and this Mr. Cammack seems only partly to have done; this too is just where Prof. Bell succeeded; by his persistent experiments overcoming all obstacles and affording by the way a striking illustration that facts may after all upset the strongest *à priori* conclusions. In connection with this remark the following passage from the last edition of a well-known work on Mental Physiology (p. 632), is not without interest:—"Everyone who accepts as facts, merely on the evidence of his senses, or on the testimony of others who partake of his own beliefs, what Common Sense [with capitals] tells him to be much more probably the fiction of his own imagination—even though confirmed by the testimony of hundreds affected with the same epidemic delusion—must be regarded as the subject of a 'diluted insanity.'" Yet Baron Münchausen's trumpet has been outdone by the phonograph: the "fiction of imagination" by a fact "confirmed by the testimony of hundreds." However as these latter have "merely the evidence of their senses to offer," we presume they are all the victims of "a diluted insanity," if the reasoning of the eminent author be accepted.

After the first impulse this extreme electro-negativeness of the selenium, due partly to polarisation, gave way and it gradually passed again to the electro-positive side, where, after a few minutes, it settled to a constant value, but still electro-negative to its condition in the dark.

I found that the slightest shadow or other variation in the intensity of the light caused a considerable variation in the electromotive force of the couple and a consequent indication.

On excluding the light the selenium instantly increased in electro-positiveness, and soon settled to its original position.

A couple in which two plates of selenium were opposed to each other, light being excluded from one and admitted to the other, gave identical results, only the resistance of the element was much greater.

The effect of light, therefore, in modifying the surface tension of selenium is evidently to render it more electro-negative and presumably not more metallic, as has been suggested in explanation of its increased conductivity.

I am endeavouring to construct a combination of selenium elements which, with a mirror galvanometer and photographic arrangement may be used to give a trustworthy record of the intensity of daylight. The practical difficulties in the way at present I have hopes will not be insurmountable. ROBERT SABINE

NOTES

WE regret to notice the death, on the 18th inst., of Dr. Thomas Thomson, F.R.S., for some years Superintendent of the Botanic Gardens at Calcutta, and joint author of Hooker and Thomson's "Flora Indica." He was also a contributor to Sir J. D. Hooker's "Flora of British India," now in progress.

THE following are the names of those whom the Council of the Royal Society have recommended for election on June 6 next as appointed:—J. G. Baker, F. M. Balfour, Rev. T. G. Bonney, Prof. J. H. Cotterill, Sir W. Elliot, Canon W. Greenwell, T. Hawksley, C.E., J. Hopkinson, D.Sc., J. Hughlings Jackson, M.D., Lord Lindsay, P.R.A.S., S. Roberts, E. A. Schäfer, H. Sprengel, G. J. Symons, and C. S. Tomes.

THE scientific world has sustained a loss by the decease of the Rev. James Booth, LL.D., F.R.S., Vicar of Stone, near Aylesbury, which occurred on the 15th inst., at the age of seventy-one. He was educated at Trinity College, Dublin, where he obtained several prizes and graduated in honours. He was elected a Fellow of the Royal Society in 1846, to a very great extent in recognition of his earliest publication, "A New Method of Tangential Co-ordinates," and also as the inventor of a new system of parabolic trigonometry. In 1852 and 1853 he contributed to the *Philosophical Transactions* two memoirs on "The Geometrical Properties of Elliptic Integrals." He was also known as the contributor of several papers on mathematical subjects to the *Philosophical Magazine*, and not a few of these, we believe, have found their way into other languages. In 1859 he was presented to the living of Stone by the Royal Astronomical Society, to whom the advowson belongs.

DR. FREDERICK KAMPF, who has been the astronomer of Lieut. Wheeler's U.S. exploring expedition, died in Washington, on March 30, at the age of thirty-six. Dr. Kampf was educated at Bonn, and emigrated to the United States in 1870, securing a position in connection with the United States Coast Survey until 1873, when he joined the party of Lieut. Wheeler as already mentioned. He promised to attain to much distinction as an astronomer and observer, and his untimely death is much to be lamented.

THE collection of shells of the late Dr. P. P. Carpenter, of Montreal, is for sale. The opportunities of Dr. Carpenter for making this collection of desirable specimens were very great, especially from the great Reigan collection of Mazatlan shells, which he purchased many years ago, and after investigation deposited duplicate series in several museums in Europe and America. The collection embraces about 4,000 species and varieties, for the most part original types. The collection is deposited for the present in the museum of M'Gill College, Montreal.

DR. RUD. FALB, of Vienna, who is engaged in studying the earthquake region in South and Central America, has left Chile and announces his arrival at Arequipa. He intends to ascend the volcano of Misti, near Arequipa, which is some 17,600 feet in height. He also reports that at the southern part of the Peruvian coast the shocks of earthquake continue with unabated violence, and that a violent eruption occurred recently from the Cotopaxi Volcano in the Andes of Quito, without, however, doing much damage.

AN Ethnographical Congress will assemble in Paris on June 24, and continue for three days. The head-quarters of the Commission are rue Monsieur, 19.

The Chair of Surgery at the Collége de France, occupied by the late Claude Bernard, has been offered to Prof. Charrot. It has been decided to erect the statue of the distinguished physiologist immediately before the Collége de France.

DR. E. BAUMANN, one of the most promising of the young physiological chemists of Germany, has received a Professorship in the Berlin University.

WE notice the death of Prof. H. Girard at Halle, on April 12. He was, until a recent date, director of the Mineralogical Museum of the University of Halle, and his name is associated with several mineralogical researches, more especially in connection with the Stassfurt deposits.

AMATEURS of spectrum analysis will thank Mr. Browning for a little pocket case he is now selling which permits a study of absorption phenomena in a very satisfactory manner. Various substances, which give very characteristic band absorption, have been mixed with gelatine so as to form a thin transparent coloured film. In that we have received, roseine, eosine, cochineal, indigo, aniline blue, Hofmann's violet, and other colouring matters have been treated in this way. There are twelve differently coloured films in all, and the variations in the spectra are very striking. On holding the films horizontally close to the slit so that one film falls on the upper and the next on the lower part of it, a capital idea of the use of comparison spectra can be gained.

M. DABRY DE THIERSANT, a French *Chargé d'Affaires*, who has been instrumental in introducing a number of Chinese plants and animals into his native country, is now making arrangements for importation in quantities of the *setz*, one of the most valued fish found in Chinese waters. The fish belongs to the carp family, and when fed on sea-plants in ponds, attains with great rapidity a weight of about forty pounds. During the past three years experiments made on the fish in the Jardin d'Acclimatation have shown it to be well adapted to a European climate, and as it increases rapidly, it is hoped that within a few years it can be introduced extensively throughout Europe.

A LITTLE village in the neighbourhood of Draguignan, France, has lately been the scene of a remarkable subsidence which has attracted the curious from all directions. An elliptical tract of ground, containing over 10,000 square feet, sank gradually one day, accompanied by loud noises, until it left an orifice of over 100 feet in depth, with water at the bottom. Numerous trees and vines disappeared completely in the depths of the new lake. A similar depression on a smaller scale occurred in the same vicinity a century ago, and both the phenomena are attributed to the action of subterranean streams.

THE Central Society of Agriculture of France took possession, on March 13, of a splendid hotel which has been built for its use and presented to it by M. Behague, one of its most active members. This society is one of the oldest in Europe, having been established more than a century ago, under the reign of Louis XV. It has never been suppressed or interrupted in spite of the several changes and revolutions the French Government has undergone during that eventful period.

THE International Congress of Medical Sciences, which was to be held at Amsterdam in September, 1878, has been postponed to 1879, in order to avoid a coincidence with the Paris International Exhibition.

M. BARDOUX, the French Minister of Public Instruction, having established an observatory for astronomical and meteorological purposes at Besançon, the general council of the Doubs department have voted a sum of 5,000 francs for meteorological observations to be conducted in the building.

THE private view of Winkler's Lunar Landscape, which we recently described, will be on Saturday and Monday next, at the new premises of the Belgian Gallery, 112, New Bond Street. The picture will be lighted by artificial light, this having been found to be most suitable to the nature of the subject.

KEW GARDENS were opened to the Public on Easter Monday at 10 A.M., and will be opened at the same hour on future bank holidays. Of the necessity of this step some idea may be formed from the following statement of the number of visitors on Monday :—From 10 to 1, 3,352; from 1 to 7, 42,833; total, 46,185.

MACMILLAN AND CO. are about to publish a little manual of "Practical Chemistry, for the Use of Medical Students," &c., by Mr. M. M. Pattison Muir, of Caius College, Cambridge.

A TERRIBLE hurricane is reported to have occurred at Tahiti, on February 7, in which 120 persons were killed, and much property destroyed.

HITHERTO we have had no occasion to say anything concerning the disturbances in the East, but during the past week there was a commotion at that now historical place St. Stefano, which we cannot let pass without reference. Happily the commotion was only seismic and did no damage. It occurred on Friday evening last and was strongly felt at Pera, while at Ismid and Broussa damage is said to have been done. The sea in the neighbourhood of the British fleet was so agitated that the commander of a gunboat sent a request to Admiral Hornby to give him previous notice on the next occasion of torpedo practice.

IN connection with our recent note on chemical dictionaries we notice the completion of the second volume of the German "Handwörterbuch der Chemie," ending with the article on Electricity from the pen of Prof. Wiedemann, of Leipzig.

AT a recent meeting of the French Academy, M. Gaiffe presented an apparatus with which one may determine immediately, and by a simple reading, the electromotive force of any electro-generator. It is based on Fechner's method of measuring such forces, and the measures are read in volts. M. Gaiffe employs a very resistant multiplier, and a small rheostat, by means of which introducing resistances, greater or less, into the circuit, the instrument may be adapted for very different measures of electromotive force, the same divided scale, however, being always employed. On introducing such and such a resistance you divide or multiply, in a proportion marked on the contacts of the rheostat, the electromotive force indicated by the galvanometric needle. Forces may thus be measured by the instrument from $\frac{1}{10}$ volt up to 150 volts.

J. E. PEIJSMANN has just published, in Batavia, an account of a scientific tour through the island of Amboina, one of the most important spice islands in Malaysia. In addition to a variety of valuable and novel observations, M. Peijsmann collected over 1,000 varieties of plants and 300 specimens of the fauna, which are to be transmitted to the museums of the University of Leyden.

A NEW bridge across the Rhine is now in course of construction at Basel; it will rest on five pillars.

THE Dutch Government intends to construct a canal from Amsterdam through the so-called Geldern Valley to the Upper Waal (the southern branch of the Rhine), and also additional canals in the provinces of Drenthe and Overijssel. Both projects are of great commercial importance with regard to navigation on the Rhine and the communication by water between Germany and Holland.

DR. SCHOMBURGK, the director of the Botanic Garden, Adelaide, South Australia, has issued a very brief report relative to the economical value of the various species of South Australian "Eucalyptus." He shows that out of the large number of species of Eucalyptus spread over Australia and Tasmania, only thirty appear in the extra-tropical part of South Australia. The South Australian species do not reach so great a height as those of the east, north, and south, and only about ten species yield timber that is much valued and used, though Dr. Schomburgk thinks many more might be utilised. The most valuable timber in the colony is stated to be the red gum (*Eucalyptus rostrata*, Schlecht). It is the most durable of all the South Australian woods, and is mostly used for underground work, bridges, jetties, railway-sleepers, and for shipbuilding; moreover, it has the reputation of being proof against the attacks of white ants. This tree grows to a height of from 100 feet to 130 feet. The next most important species is the white gum (*Eucalyptus stuartiana*, F. Muell.), the blue gum (*E. vincinalis*, Las.), and the stringy bark (*E. obliqua*, L'Herit.). Dr. Schomburgk points out that the woods are not the only useful products of the Eucalypti. From *E. obliqua*, *E. leucoxylon*, and *E. rostrata*, acetic acid is obtained; wood-spirit is also procured from the first two, essential oils are produced from the leaves of *E. vincinalis*, *E. stuartiana*, and *E. citriodora*; tar from woods of *E. rostrata*, *E. leucoxylon*, and *E. obliqua*; and from the barks of several other species paper has been made.

IT is announced by M. Toselli, that through successive improvements of his refrigerating apparatus, he can now produce one kilogramme of ice in the space of two minutes.

THE announcement of the intended publication, in a short time, by the Smithsonian Institution, of a complete catalogue of the plants of North America, will be hailed with great satisfaction by botanists. The region covered extends from Greenland and the Arctic Ocean, on the north, to the borders of Mexico, and from the Atlantic to the Pacific. The species are enumerated in their systematic sequence, with their synonyma. The work will be published in two parts, the first, on the polyptalæ, constituting a volume of about 480 pages. It covers the ground of volume i. of Torrey and Gray's "Flora of North America." The title of the work will be "Bibliographical Index of North American Botany," by Sereno Watson.

THE increase of volume of liquids through absorption of gases has lately been investigated by Messrs. Mackenzie and Nichols, in the Physical Laboratory of Berlin University. Experimenting in the first instance with carbonic acid and water only, and employing two different methods, they reach the same result, viz., that the expansion is directly proportional to the quantity of gas absorbed. They further examined the expansion of water saturated with carbonic acid by heat, and got a curve having

about the same course as that for pure water, except that the maximum of density was reached, not at 4°, as in the case of pure water, but under 3°, as is the case with salt substances.

RECENT observations by M. Ebermayer demonstrate (1) that the air in a large forest is in summer nearly twice as rich in carbonic acid as free open air ; (2) that forest ground in summer contains much less CO_2 than unwooded ground (the CO_2 formed by slow decomposition of humus in the close forest seems mostly to pass into the air, and is probably utilised by the leaves for assimilation) ; (3) that, with rise of temperature, the increase of CO_2 in arable ground is very much greater than in forest ground ; and (4) that the spread and motion CO_2 in the ground seems to take place very slowly, for in two places quite near together the amount of CO_2 may be very different. Among other bearings of these facts, the ground covering of a forest can have no important influence on the amount of CO_2 and lime in spring water, and unwooded ground may have a greater action in this respect. Again, animals living underground, *e.g.* foxes, naturally prefer the ground air of the forest, with its little CO_2, to to the ground air of the open field, which has much more.

THE influence of concentration of liquids on their electromotive force has lately been investigated by M. Moser (*Monatsb. der Berliner Acad. der Wiss.*) who connected two glasses of differently concentrated solutions of the same salt by a siphon, and completed the circuit by wires with electrodes, which were always of the same metal. In all such cases a current arises, passing in the liquid from the dilute to the more concentrated solution. M. Moser used zinc sulphate, nitrate, chloride, and acetate, copper sulphate and nitrate, iron chloride, silver acetate and nitrate, and other salts. The highest electromotive force was $\frac{1}{5}$ Daniell, and was got with very dilute and concentrated zinc chloride solution. The various effects are arranged in tension-series. By the currents referred to, metal is dissolved in the dilute solution, separated out in the concentrated one. The equivalent of the work done by the current, M. Moser considers, is the work of attraction force between the salt and the water. The current is to be regarded as a reaction current against passage of the ions, as the polarisation current is the reaction current against the decomposition current.

THE subject of acoustic repulsion continues to be studied by M. Dvorak (*Wied. Ann.*, No. 3). Among other things he constructs an acoustic reaction wheel and an acoustic torsion balance. The former consists of four light paper or glass resonators placed tangentially at the four ends of two thin cross-bars of wood, pivoted at their intersecting point by means of a glass cap. The mouths of the resonators are all in the same relative position. The wheel is placed before the open end of a tuning-fork resonator, and enters into rotation when the fork is sounded. In another case the sound from the large resonator is transmitted through a conical tube beyond whose thin end is a wheel with square pieces at the end of the cross-arms. In the acoustic torsion balance a wooden bar furnished with a resonator is hung by a wire (as in Coulomb's balance) within a case, which has on the resonator side an opening for admission of sound. By repulsion of the resonator the strength of tones of the same number of vibrations may be compared.

LECTURING at the Sorbonne lately on atmospheric electricity, M. Mascart sought to reproduce the phenomena of thunderstorms. The dull explosions of thunder and the fulgurations in the heart of clouds preceding fulminant discharges, as also the latter, were imitated by means of a powerful Holtz machine, charging batteries, and condensers suitably arranged. The singular movements of thunder-clouds, which, obeying electric attractions and repulsions, are often observed to move in the atmospheric ocean in counter-currents, were illustrated with the aid of a balloon of hydrogen gas, to which was suspended a piece of metallic wire. The weight of the wire was such that the small aerostat, rendered slightly heavier than the displaced air, would descend ; but when it was electrified, it rose again, as if freed from its burden. M. Mascart did not attempt an explanation of this curious phenomenon, which has not been repeated since the time of van Marum.

IN a recently-published report by M. Kellner to the Naturforscher Versammlung at Munich, he describes experiments made along with some others on an eleven-year old Wallachian horse of 434 kilo. weight, with regard to the relation of work done and decomposition of albumen. In five successive periods of thirteen to fourteen days the animal was fed with 5 k. meadow-hay, 5 k. oats, and 1·5 k. chopped wheat straw, and did work to the extent of 500,000, 1,000,000, 1,500,000, 1,000,000, and 500,000 kilogrammetres in the five periods respectively. In periods I. and V. the work done was the same, in II. and IV. doubled, and in III. tripled ; in II. and III. the course was doubled and tripled, and in IV. the weight doubled. Of the dry substance of the fodder were digested in period I. 56·53 per cent., II. 56·45 per cent., III. 56·49 per cent., IV. 54·01 per cent., V. 53·07 per cent. The horse's weight varied as follows :—I. 534·1, II. 529·1, III. 522·3, IV. 508·8, V. 518 kilo. The excretion of nitrogen was on an average of the last six to nine days of each experimental series, I. 98·81 k., II. 109·16, III. 119·82, IV. 107·53, V. 101·88. These numbers show strikingly, in opposition to Voit's and Pettenkofer's results, that with increase of work done, is associated a not inconsiderable increase of decomposition of albumen.

THE additions to the Zoological Society's Gardens during the past week include an Indian Leopard (*Felis pardus*) from India, presented by Major Tubbs ; a Red Deer (*Cervus elaphus*), a Common Fox (*Canis vulpes*), European, presented by Mr. Carroll W. Ansdell ; two Spotted Ichneumons (*Herpestes auropunctatus*) from Nepal, presented by Mr. J. McIntosh ; a Suricate (*Suricata zenik*) from South Africa, presented by Mr. Percy Howard ; an Azara's Fox (*Canis azaræ*) from Brazil, presented by Dr. A. Stradling ; a Stanley Crane (*Tetrapteryx paradisea*) from South Africa, presented by Capt. A. F. Lendy ; a Lead-beater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mr. W. Ruston ; a Collared Fruit Bat (*Cynonycteris collaris*), four Common Foxes (*Canis vulpes*), born in the Gardens.

THE DETERIORATION OF OIL PAINTINGS[1]

II.

IF we compare the pictures of the Italian and Dutch schools of the fifteenth, sixteenth, and seventeenth centuries, with those of the French and English schools of the last hundred years, we are struck by the great difference in the nature of their diseases. We may divide those diseases into *constitutional* ones—that is to say, such as are based on the method and the material used for painting, and into those produced by external influences.

The Dutch pictures of the fifteenth, sixteenth, and seventeenth centuries, and the Italian pictures of the fifteenth and sixteenth centuries, seem to me perfectly free from constitutional diseases. It is only in the seventeenth century that the Italian pictures show a special constitutional alteration, caused by the practice of the Bologna school.

The pictures of the last hundred years of the French school, of a part of the English school, and some painters of other schools, have been attacked by a constitutional disease perfectly defined and characteristic of this period.

Among external influences injurious to oil painting, we have to consider dampness, heat, bad air, dust, smoke, mechanical injuries, and last, not least, the destructive or "altering" hand of the picture-restorer.

Pettenkofer's scientific researches first clearly defined the influence of humidity on oil paintings, showing that it produced a discontinuity of the molecules of the vehicle and the resinous substances. As glass, when pulverised and thereby mixed with air, loses its transparency, and water, when mixed with oil,

[1] Paper read at the Royal Institution, Friday, March 1, by R. Liebreich, M.D., M.R.C.S., M.R.I. Continued from p. 495.

becomes of a milky aspect, so the oily and resinous substances contained in paintings will become dim as soon as air penetrates between their particles. The picture thus assumes a greyish, dim appearance, and the pigments seem to have been fading. That this is not really the case has been proved by the influence of a process invented by Pettenkofer, which he calls regeneration. In a flat box the picture is exposed to air impregnated with alcohol. Of this latter the resinous elements of the picture absorb a certain quantity, swell and fill up the interstices between the separated particles so as to reunite them into an optically homogeneous transparent substance.

The alcohol does not affect in the same way the hardened oil. If the interstices between its particles are not filled up by the swelling resin, it becomes necessary to introduce a new substance into the picture, and this is called nourishing a picture.

Pettenkofer has the great merit of having clearly proved that the nourishing of a picture with oils, as the custom was formerly, and still is to some degree, is a very objectionable proceeding, as it has the effect of darkening the colours for ever. He recommends, instead of oil, balsam of copaiva, which has become since an invaluable means for preserving and restoring oil paintings, and will be more and more extensively used.

I have frequently applied Pettenkofer's method, and with very beneficial effect; but whenever I mentioned it to professional picture-restorers, here as well as on the Continent, I always found them to reject it, either *à priori*, or after experiments incorrectly made.

In Munich, it seems, the pictures of all periods and of all schools have had to suffer under local influences and through the changes in the humidity of the air. This accounts for Pettenkofer having principally described this, so to say, endemical disease. In other galleries this affection does not appear so frequently, and Pettenkofer's method, therefore, will not find everywhere the same extensive application as at Munich. I think, however, that with some modifications it may be employed against some other alterations. I have, for instance, found it efficacious with paintings which had been injured by exposure to great heat. I shall show you a small picture which had been hanging for a long time so near a gas flame that it was almost completely scaling off, and so entirely faded that it scarcely looked like an oil painting at all. In that state it was exposed to alcoholised air, then nourished with balsam, and its back slightly varnished; and the scales starting from the canvas were refixed by pressure. And now it appears fresh in colour, firm in substance, and perfectly smooth on its surface. The old, cracked varnish, melted together by the alcohol, looks as if fresh laid on.

Humidity sometimes favours the development of fungus. The round, black, small spots which pass through the canvas and the painting of these two pictures are produced by the same little plant which Prof. Tyndall showed you when he spoke on the highly interesting subject of spontaneous generation.

Oil and water, so injurious to oil paintings, enter both into the material used for lining. Anxious to exclude these sources of danger, and to simplify the whole process, I have endeavoured to replace it by a new method which I shall submit to you this evening.

How paintings may be disfigured by restorers you see in this picture, which was renovated with oil colours according to the practice only abandoned about thirty years ago, when it was advantageously replaced by the use of varnish colours.

The amount of external injury oil paintings sometimes endure and stand is perfectly amazing. Pictures in the course of centuries, during the destructive fury of wars and revolutions, may have been torn out of their frames, rescued from below the ruins of burned monasteries, may subsequently have passed from one *bric-à-brac* shop to another, where they have been piled up, to be pulled about at each new inspection, and literally trodden under foot, whereby they have finally been reduced to a state of colourless, greyish, or black rags. Still such pictures may not unfrequently be awakened, as it were, to new life, to their original brilliancy of colour, if, with all necessary care, their injured limbs are put together again, their wounds are healed, and fresh nourishment, air, and thorough cleansing, are administered to their lacerated bodies.

A sound constitution is, of course, a necessary condition for obtaining any such result, without it we can only obtain a partial cure. We see this with reference to the Bolognal school of the seventeenth century. The pictures which you see here are instances of this. From the state of rags to which they were reduced they have passed, by appropriate treatment, into the state of firm, even, well-conditioned, and clean pictures. The constitutional alteration characteristic of their time and school, however, could not be cured. You will, therefore, perceive that the contrast is too great between light and shade, that the half tones are too weak and that the glazings spread on dark ground, which certainly existed formerly, have been destroyed by the growing of bolus and umber of the priming. That this is not the fault of the method of restoration is clearly proved by the state in which you will find all the pictures of this school, even those best preserved in the best galleries of all countries.

The constitutional diseases of pictures belonging to the French and to the English school of the last hundred years are of still more serious nature, and much more difficult to cure. Many of them, though they were never exposed to any injury whatever, nor are likely ever to be so in our present state of civilisation, cannot be guarded from premature decay in spite of all possible care with which they are kept.

The principal symptoms of their bad constitution are :—

1. Darkening of the opaque bright colours.
2. Fading of the transparent brilliant colours.
3. Darkening, and above all, cracking of the transparent dark colours.

The best opportunity to study these several appearances is given us in the Museum of the Louvre, which contains a great number of such pictures in the section occupied by the French school. I have paid particular attention to the cracks in these pictures, as I find that in shape, in size, in position, as well as in relation to the various colours, they differ distinctly from the cracks in older pictures and in those of other schools. This, of course, is of importance, not only for the explanation of the reasons which produced them, but as a symptom which, in a given case, might determine the diagnosis, whether a picture be an original or only a copy. The special characteristics of these cracks are the following :—

They are all but exclusively found in the thickly laid on transparent dark colours, and they are the deeper and the more gaping in proportion to the thickness of the layer of the colour and the extent of the dark surface. The chief cracks run parallel to the outlines of surfaces painted with bright opaque colours, such, for instance, as are used for the flesh tints, and which are more or less thickly laid on. But there is generally a slight distance between the bright colours and the cracks.

Lateral branches of these cracks pass into the white, but they do not gape, provided the white colours had been laid on directly upon the priming, and not upon a layer of dark transparent and not sufficiently dried colour.

This examination of the cracks of pictures has sometimes afforded me a peculiar insight into the practice used for the picture. In the well-known picture, for instance, by Guèricault, of "The Wreck of the *Medusa*," in the Gallery of the Louvre, the cracks follow exactly the outlines of the bright flesh-tints. The arm of one of the dead bodies hanging in the water is so covered by planks and water that nothing of the forearm is to be seen. It is, however, very easy to prove that originally that arm was painted in all its length, for the cracks do not only follow the outline of the visible upper arm, but also the no longer visible forearm, and all the five fingers. This proves that the fore part of the arm and the hand were originally painted in flesh-tints before they were covered over by the planks, and the water painted afterwards. In Ingres' portrait of Cherubini, the face of the latter is beautifully preserved, while that of the Muse, as well as her drapery, is covered with cracks. In the depth of the cracks of the white drapery an intense blue tint is to be seen. Mr. Henry Lehmann, of Paris, the favourite pupil of Ingres, who knows the history of this picture as an eye-witness, and whom I consulted about this very striking appearance, gave me the following information :—Ingres painted the head of Cherubini in Paris, and then took it with him to Rome. There it was pieced into a new canvas and lined. Then the Muse was painted, and before the colours were perfectly dry, another model was chosen, and a new Muse painted over the old one. The colour of the drapery was likewise altered, and this explains the cracks in the white colour, and explains also why the blue appears in the depth of the cracks of the drapery.

Among the English artists of the last hundred years, some have painted with the same material and by the same process as their French contemporaries, and consequently with the same unfortunate results. Others avoided these by using the same

material with more precautions. Others, again, and among them Sir Joshua Reynolds, have in their different works followed various practices, and consequently had varied results. Thus, some of Sir Joshua's pictures have kept perfectly sound. Others are cracked in the characteristic way just mentioned. Others, again, are cracked in an absolutely irregular way. We can easily form an idea of it if we read in his "Diary Notes," for instance, the way in which he painted the portrait of Miss Kirkman, which he began with whiting and gum tragacanth, then covered it successively with wax, then white of eggs, and then varnished it.

The study of the alterations already fully developed in pictures painted within the last hundred years only, and their comparison with the works of the old masters, would suggest the following rules for the process of painting:—

1. The oil should in all colours be reduced to a minimum, and under no form should more of it than absolutely necessary be introduced into a picture.

2. All transparent colours which dry very slowly should be ground, not with oil at all, but with a resinous vehicle.

3. No colour should be put on any part of a picture which is not yet perfectly dry; and, above all, never a quick-drying colour upon a slow-drying one, which is not yet perfectly dry.

4. White and other quick-drying opaque colours may be put on thickly. On the contrary, transparent and slow-drying colours should always be put on in thin layers.

If the effect of a thick layer of these latter is required, it must be produced by laying one thin layer over another, taking care to have one completely dry before the next is laid on. If transparent colours are mixed with sufficient quantity of white-lead, they may be treated like opaque ones.

We come now to the last layer of the picture, to that one which is spread over its surface in order to equalise optical irregularities, and to protect it at the same time from the air. I mean the varnish.

The varnish may crack or get dim; then it should be treated with Pettenkofer's method; but it may become dark yellow, brown and dirty, and so hide the picture that it becomes necessary to take it off and to replace it by a thin layer of new varnish. It is here that picture-restorers, or we may say picture-cleaners, display their beneficial skill, and also their very destructive activity.

If a picture is throughout painted in oil, if its substance has remained sound and even, and varnished with an easily soluble mastich or dammar varnish, then there will be neither difficulty nor danger in removing the varnish. This can, in such a case, be done either by a dry process, that is, by rubbing the surface with the tips of the fingers, and thus reducing the varnish by degrees to a fine dust, or by dissolving the varnish by application of liquids, which, when brought only for a short time into contact with the oil painting, will not endanger it. We have, however, seen that the works of the old masters are not painted with oil colours like those used by modern painters, but, on the contrary, that certain pigments, and especially the transparent colours used for glazing, were ground only with resinous substances. These latter have, in the course of time, been so thoroughly united with the layer of varnish spread over the surface of the picture, that there no longer exists any decided limit between the picture and the varnish. It is in such pictures that a great amount of experience, and knowledge of the process used for the picture, as well as precaution, are required in order to take away from the varnish as much only as is indispensable, and without interfering with the picture itself. Numberless works of art have been irreparably injured by restorers, who, in their eagerness to remove dirt and varnish, attacked the painting itself. They then destroyed just that last finishing touch of the painting, without which it is no longer a masterpiece.

The difficulty and danger are much greater in cleaning those pictures which have not been varnished with the ordinary easily-dissolved mastich or dammar varnish, but have been painted over with oil, oil-varnish, or oleo-resinous varnish. It seems incredible that these substances should ever be used for such purposes; it is, however, a fact that there are still people who fancy that it will contribute to the good preservation of their pictures to brush from time to time a little of those liquids over their surface. They recognise too late that the varnish becomes more and more dark, of a brownish colour, and opaque. If such varnish has afterwards to be removed, then we meet with the great difficulty, that this can be done only with substances which would just as easily dissolve the whole picture as the hardened layers spread over it.

This shows what can be the value of those universal remedies which from time to time appear, and are praised for the innocuous way in which pictures by their means may be cleaned.

There is at this moment a great discussion going on in Italy about Luporini's method. Luporini is a painter and picture-restorer in Pisa, who believes himself to have invented a new means of cleaning pictures without any danger. Some months ago, in Florence, I examined a large number of pictures cleaned by him. Those of the Gallery of St. Donato, belonging to Prince Demidoff, mostly Flemish and Dutch landscapes, are cleaned very well and without any injury to the painting. On the contrary, the St. John, by Andrea del Sarto, one of the finest pictures of the Palazzo Pitti, I found very much altered by the restoration of Luporini. I had studied that picture very closely the year before, and should now sooner believe it to be a modern copy than the cleaned original. It has lost all softness of outline and the characteristic expression of the face. The change in the flesh tints can scarcely be explained otherwise but by an entire removal of the glazing.

I think it is taking a heavy responsibility to allow a new experiment to be tried upon such an invaluable work of art. Even private persons, who are fortunate enough to be in possession of such treasures, ought to feel responsible for the good preservation of masterpieces, which are, it is true, their material property, but which intellectually belong to the whole civilised world of the present and of the future.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Messrs. Mackren, Robbs, and Hichens, have been appointed to Scholarships in Natural Science at Gonville and Caius College.

EDINBURGH.—At the Graduation Ceremonial on Tuesday the degree of Doctor of Science in the Department of Mental Science, was conferred on Jacob Gould Schurman, B.A.; in the Department of Mathematics on Alexander Macfarlane, M.A., B.Sc.; in the Department of Chemistry on William Inglis Clark, B.Sc. The degree of Bachelor of Science was conferred on William Thomson in the Department of the Mathematical Sciences; on John Adrian Blaikie and James Johnstone Dobbie in the Department of the Physical Experimental Sciences; on William A. Haswell in the Department of the Natural Sciences; on James Alfred Ewing and John Gray in the Department of Engineering; and on John Brown, M.D., John Berry Haycraft, M.B., C.M., and John Trehame, M.B., C.M., in the Department of Public Health. The Hope Prize Scholarship in Chemistry was awarded to Mr. Lewis Johnstone, and the Falconer Memorial Fellowship for the encouragement of the study of Palæontology and Geology, of the annual value of 100*l*., tenable for two years, and conditionally for four years, was awarded to R. A. Lundie, M.A., B.Sc.

BALTIMORE.—We recently referred to the system of fellowships at the Johns Hopkins University, Baltimore. From a statement on the subject which has come to hand, we learn that twenty fellowships, each yielding 500 dols. a year, are annually open in the University. They are awarded by the trustees on the nomination of the Faculty, as nearly on the first of June as may be found practicable. Candidates are invited from any part of the country. The object of this foundation is to give to a few scholars of promise the opportunity to prosecute further studies, under favourable circumstances, and likewise to open a career for those who propose to follow scientific and literary callings. The University expects to be benefited by the presence and influence of the Fellows, and by their occasional services; from among the number it hopes to secure from time to time some of its teachers. Three of the twenty fellowships are allotted this year to each of the five departments, Greek, mathematics, chemistry, physics, and biology; and the remaining five will be allotted either in these departments or in others, at the discretion of the Faculty. Appointments are made by a careful consideration of all the evidence submitted to the Faculty. Every candidate in presenting his name is expected to address a letter to the president indicating the course of his previous reading and study, and his general purposes with reference to future work. It is desirable for him to present in printing or manuscript an essay or thesis which may have been written either

for this occasion or for any other purpose. If he has been engaged in any scientific or literary research he should indicate its character, and generally give evidence as to his previous career and *bonâ fides*. The holders of the fellowships are required to reside in Baltimore during the entire academic session, and they are not permitted to engage in teaching, out of the walls of the University, unless for exceptional reasons in other colleges which may ask for some temporary service. They are expected to devote all their time to study under the guidance of one of the professors, or if there be no professor in the chosen department, under the general approbation of the Faculty. Toward the close of the Academic year a report of his work is expected from each Fellow. As opportunities offer, the Fellows are encouraged to prepare and read lectures or essays on subjects to which they have given special attention. They are also required to render occasional services as examiners or as assistants in the laboratories; but those services are not burdensome, unless they are compensated by additional stipends. Those who become distinguished by their attainments may be assured of the constant encouragement of the Faculty. With all these precautions there seems little chance of the Johns Hopkins University being eaten up by idle Fellows.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 11.—"The Acceleration of Oxidation caused by the Least Refrangible End of the Spectrum," by Capt. Abney, R.E., F.R.S.

In a paper contributed to the *Philosophical Magazine* in January last, the author expressed an opinion that Chastaing's idea regarding an acceleration of oxidation being caused by red light might prove true in regard to the oxidation of the photographic image, and elsewhere[1] that Becquerel's coloured spectra might be explained on the same principles, and this he finds to be true as regards oxidation of the photographic image.

A silver bromide film was exposed to diffused light. It was then submitted to the action of the solar spectrum, whilst immersed in a solution of potassium permanganate, hydroxyl, potassium bichromate, or nitric acid, or in ozone. When the strength of these was correct, a reversed image of the least refrangible end of the spectrum was obtained, an increase in oxidation taking place where the red rays acted, the reversal commencing somewhere near D, and extending into the ultra-red.

The accelerating effect of the red rays is most marked when the solutions are weak; but there is a limit to the dilution caused by the fact that in the films employed the silver salt is sensitive as far as the wave length 10,000, and there must be sufficient strength to oxidise the invisible image as it is formed, besides gradually destroying the effect of the preliminary exposure. With silver iodide, as there is no reduction by the red rays, the reversed action is much more readily obtained.

A reversed image of the least refrangible end of the spectrum can thus be produced by using solutions of a certain strength, whilst if made more dilute an unreversed image is obtained. This throws a light on Draper's photographs of this region of the spectrum.

Geological Society, March 6.—Henry Clifton Sorby, F.R.S., president, in the chair.—Henry Edward Richard Bright, George James Cotton Broom, William James Farrer, George Scamell, and Joseph Fletcher White were elected Fellows of the Society.—The following communications were read:—On the geology of Gibraltar, by Prof. A. C. Ramsay, F.R.S., and James Geikie, F.R.S. In this paper the authors, after giving some account of the physical features of Gibraltar, described in detail the various rock-masses of which the peninsula is composed. The chief rock is a pale grey, bedded limestone, overlain by shales containing beds and bands of grit, mudstone, and limestone. Fossils are very rarely met with in the limestone, and have never as yet been found in the shales. The only recognisable fossil they obtained from the limestone was a *Rhynchonella*, which Messrs. Etheridge and Davidson think is most likely *Rh. concinna*. This would make the beds of Jurassic age. The limestone forms the great eastern escarpment, and dips west under the shales, which form the lower slopes upon which the town is built. The dips vary from 12° or 20° up to vertical. The connection of these strata with the rocks of the adjoining districts in Spain and the opposite coast of Africa was traced, and it was shown that the Gibraltar limestone reappears in Ape's Hill in Barbary, while the overlying shales and the sandstones of Queen of Spain's Chair form all the ground to the west of Ape's Hill up to Cape Spartel. The Jurassic strata of Gibraltar are overlain by various superficial accumulations, the oldest of which is a great mass of limestone agglomerate, which is unfossiliferous, and shows as a rule no trace of stratification. It is made up of angular blocks of limestone of all shapes and sizes, and rests upon an uneven surface of limestone: it also covers wide areas underneath which only shales are present. It is excessively denuded, being worn into ravines and gullies, and presents generally a highly honeycombed surface. Terraces of marine erosion have also been excavated in it. It is not now accreting, and could not have been formed under present conditions of climate and surface. The authors gave at length their reasons for believing it to have been the result of a severe climate. The blocks were wedged out by the action of frost, and the heaps of angular *débris* thus formed were saturated by water derived from melting snows, and so were caused to flow *en masse* down the mountain slopes and over the gently inclined ground at their base. The caves and fissures of Gibraltar were then described. It was shown that the true bone-breccias were confined to these. Many of these fossiliferous breccias are of later date than the great agglomerate, since they are met with in fissures and caves that intersect the limestone and limestone agglomerate alike. When the mammalia tenanted Gibraltar, Africa and Europe were united, and the climate was genial. All round the rock occur platforms, ledges, and plateaus, which are evidently the work of the sea. These erosion-terraces are covered in many places with calcareous sandstones containing recent species of Mediterranean shells. Such marine deposits occur up to a height of 700 feet. The movement of depression was interrupted by pauses of longer or shorter duration, and the climatic conditions were probably much the same as at present. After the rock had been re-elevated, the subaërial forces modified the surface of the marine sands that covered the limestone platforms, so that they came to form long sand slopes. The land at this period was of greater extent than it is now, and some grounds exist for believing Europe to have been again united to Africa, for mammalian remains occur here and there in the deposits that overlie the limestone platforms. These relics, however, it is just possible may be derivative. The climate was probably still genial like the present. Overlying the marine and subaërial deposits just referred to occurs an upper and younger accumulation of massive unfossiliferous limestone agglomerate. This deposit the authors believe to owe its origin to severe climatic conditions. After the marine deposits that cloak so much of the eastern side of the rock had been weathered into subaërial sand-slopes, large blocks were detached from the cliffs and steep slopes, and these dropped down upon the sand and were soon drifted over. By and by the blocks fell in such quantities that the sand-slopes in many places were completely buried under a talus of limestone *débris*. This was subsequently consolidated by infiltration into a solid agglomerate, in the same way as the underlying sands were hardened into sandstone. These sandstones contain a few blocks of limestone only in their upper portions. In their horizontally-bedded and lower-lying portions no limestone blocks occur. This later agglomerate bears every stamp of great antiquity, and could not have been formed under present geographical and climatic conditions. The surface is honeycombed and worn, just like that of the solid limestone and the older limestone agglomerate. Since its accumulation the climate has greatly changed, the present being characterised by the absence of frost. In concluding, the authors discussed at length the cause of the cold conditions that gave rise to the great limestone agglomerates, and argued that this cause could not have been *elevation of the land*. They also pointed out that a *submergence of the Sahara* would be equally incompetent to bring about the desiderated climatic conditions, and that even a former much greater elevation of the land, combined with the appearance of a Sahara sea, would fail to supply us with the severe winter climate that was necessary to produce the great agglomerates. They thought that the most probable explanation of the phenomena described is that the cold conditions referred to were contemporaneous with that general refrigeration of climate which took place over so vast an area in our hemisphere during pleistocene times. The limestone agglomerates they look upon as the equivalents of those glacial deposits that occur so plentifully in our own and other countries, and the bone breccias, which are intermediate in date between the lower and upper limestone agglomerates, are paralleled by the interglacial beds of the British Islands, Sweden, Switzerland,

[1] "Treatise on Photography," p. 225. Longmans.

&c.—Notes on the geology of Japan, by J. G. H. Godfrey, F.G.S.

Physical Society, March 16.—Prof. W. G. Adams, president, in the chair.—A special general meeting was held for the election, as an *ex officio* honorary member of the Society, of the President of the Physical Society of Paris.—The following candidates were then elected Members of the Society:—J. S. Bergheim, W. M. Hicks, M.A., Dr. J. Hopkinson, M.A., D.Sc., Miss E. Prance, and T. Wills.—The Secretary read a paper by Mr. W. J. Millar, C.E. on the transmission of vocal and other sounds by wires. The author was led, mainly by a consideration of the manner in which sounds are conveyed through walls and partitions, to make an extensive series of experiments on this subject, from which he concludes that conversation can be carried on at considerable distances by simply employing stretched wires provided with suitable vibrating discs. In one experiment two copper wires were attached to points on a telegraph wire 150 yards apart, and breathing, singing, &c., were distinctly audible; by stretched wires extending through a house and provided with mouth- and ear-pieces in the several rooms, conversation could be carried on without difficulty. The materials employed for terminals were very varied, and the vibrating disc, whether metal, wood, or india-rubber, &c., was generally formed as a drum-head, the wire being fastened at its centre. The volume of sound appears to be greater with a heavy wire, but in all cases it requires to be stretched.—The President referred to the experiments of Wheatstone on the conduction of sound by vibrating bodies, especially long wooden rods. He mentioned that in 1856 a performance was given at the Polytechnic at which numerous experiments connected with such conduction were exhibited. Some years ago M. Cornu, in conjunction with M. Mercadier, made experiments which showed that vibrations can be transmitted along a copper wire and rendered visible at the distant end on a rotating blackened drum. The free end of the wire was attached to a piece of copperfoil fixed at its base and provided with a point which left a clear trace on the drum when the distant end was attached to, say, a vibrating tuning-fork. By connecting such an arrangement with different instruments and varying the players also, M. Cornu has ascertained the form and extent of vibration corresponding to each. The arrangement adopted by him was exhibited by Prof. Adams, and in conclusion he referred to a passage in Hooke's "Micrographia," which clearly showed that he was aware of the facility with which sounds can be transmitted by solid bodies.—Mr. W. H. Preece described some experiments made in September of last year, by Mr. A. W. Heaviside and Mr. Nixon at Newcastle-on-Tyne on this subject, from which they conclude that the method might certainly be applied with success to the transmission of speech within a building. They find that a No. 4 wire gives the best results. The terminals were wooden discs about $\frac{1}{8}$ in. thick, and to these the wire was attached "end on," but speech could be distinctly heard by laying such a disc on any intermediate point of the wire. When the wire was particularly still speech was audible up to 200 yards.—Mr. G. W. von Tunzelmann then read a paper on the production of thermo-electric currents in wires subjected to mechanical strain. The wire, of iron, steel, or copper, was stretched vertically between two cans which could be maintained at different temperatures. It was fixed in the base of the lower can and held in the upper one by a clamp attached to the shorter arm of a lever, to the longer arm of which the stretching weight was applied. The free ends of the wire were joined to copper wires which led to the Thomson galvanometer, these junctions being covered with cotton wool. He has succeeded in reconciling the contradictory conclusions arrived at by Sir W. Thomson and M. Le Roux; whereas the former only used moderate strains, the latter worked near the breaking limit, and the author finds that if the weight be gradually increased the direction of the current changes, and hence these two authorities found the currents to flow in opposite directions. A great number of experiments were made, and from them it is evident that on applying a strain the deflection does not immediately attain a maximum, but it gradually rises for about eight minutes, and then gradually falls, attaining a stationary point at the end of about twelve minutes.—Prof. Adams then exhibited a simple arrangement for projecting Lissajous' figures on to the screen which has been made by his assistant, Mr. Furze. It consists of two strong straight steel springs, fixed in separate heavy iron frames, the one horizontally and the other vertically. The latter carries at its end a double convex lens and the former carries a black disc perforated with a small hole and is so mounted that its length may be varied as required. If now the disc be placed before the lamp and the point of light be focussed on the screen by means of the lens on the vertical spring, the two springs may be caused to vibrate and the spot will describe a figure corresponding to their relative rates.—Dr. Guthrie exhibited an experiment to show the behaviour of colloids and crystalloids in relation to electrolysis. A solution of gelatine was coloured with litmus, made acid and mixed with sulphate of soda; two platinum poles of a 6-cell Groves' battery were then immersed in it and the gelatine was allowed to set. The mass became comparatively clear round the positive pole and red and blue clouds were formed which met across a space of about $1\frac{1}{2}$ in. in three-quarters of an hour. The relative advance of the ions was indicated by the brightening of the litmus round one pole and by the blue coloration produced at the other.

Chemical Society, April 4.—Dr. Gladstone, president, in the chair.—A lecture "On the Application of the Microscope to some Special Branches of Chemistry" was delivered by Mr. H. C. Sorby, F.R.S. The lecturer confined his discourse to the application of the microscope for determining the refractive indices of liquids and solids. An object is placed on the stage of a microscope and the focus adjusted accurately; on covering the object with a plate of some refracting substance, the object will be invisible; to bring it again into focus the body of the microscope must be moved further out. If this distance be "d" and the thickness of the plate be T, then the index of refraction $= \frac{T}{T-d}$. This distance can be measured either by a scale and vernier attached to the body of the microscope or by graduating the head of the screw which works the fine adjustment. The lecturer then described the various methods by which the two quantities T and d could be practically measured to $\frac{1}{1000}$th of an inch; the curious and diversified images seen by observing with a microscope a circle or a grating through transparent plates of various substances were then explained. Minerals having no double refraction are unifocal, *i.e.*, both systems of lines in a grating can be seen at the same focus. Minerals having double refraction are bifocal, *i.e.*, only one system of lines can be seen at one focus, a new focus having to be found in order to see the lines at right angles to the first set. This method has enabled the author to identify various minerals in sections $\frac{1}{600}$th inch thick and $\frac{1}{100}$th inch in diameter. Thus in a dolerite $\frac{1}{100}$th inch thick, a zeolite, labradorite, calcite, and augite were identified with almost absolute certainty. In sections of shells $\frac{1}{1000}$th of an inch thick calcite can be easily distinguished from arragonite. In conclusion the lecturer referred to the connection between the indices of refraction and chemical composition; the data are defective at present, but several points have already been made out; thus of two minerals having similar compositions, but one containing calcium and the other one of the alkalies, the first has a higher index of refraction; a lime garnet on the other hand has a lower index than a precious garnet which contains iron instead of calcium.

Linnean Society, April 4.—W. Carruthers, F.R.S., vice-president, in the chair.—There was exhibited by Dr. H. Trimen the base of the stem of the Water Hemlock (*Cicuta virosa*, Linn.) in its floating winter state, obtained near Yarmouth. This was well figured in the *Phil. Trans.* last century, but since has seldom been referred to by botanists.—Mr. G. Murray showed under the microscope specimens of growing Saprolegnia, exhibiting terminal and interstitial oogonia.—A paper on some minute hymenopterous insects, by Prof. J. O. Westwood was, in his absence read by Mr. McLachlan. This contains descriptions of the following new forms: *Mymar taprobanicus*, *M. wolastonii*, *Alaptus excisus*, *Oligosita subfasciata*, *O. stanforthii*, *O. nodicornis*, and *Trichogramma erosicornis*. All singular insects alike interesting structurally and as regards habits, &c.—A short notice was made by Mr. M. C. Cooke on a collection of fungi from Texas, made by Mr. Ravenel. Adding all other recorded species the series shows that much yet remains unknown in the mycologic flora of what probably is one of the richest States of the Union.—The Secretary read some remarks on the peculiar properties ascribed to a fungus by the Samoans, by the Rev. Thos. Powell. The natives name it "Limamea"; specimens of which have been forwarded to the Rev. M. Berkeley for identification. It destroys their bread-fruit trees and the Chestnut (*Inocarpus edulis*). An antidote to its ravages is said to exist in the liliaceous plant *Crinum asiaticum*, which the natives grow

between the trees liable to be affected.—The following gentlemen were elected Fellows of the Society :—Frederick Manson Bailey, Dr. Archibald Hewan, George Payne, jun., and James R. Reid.

Zoological Society, April 2.—Prof. Newton, F.R.S., vice-president, in the chair.—A communication was read from the Marquis of Tweeddale, F.R.S., containing the seventh of his contributions to the ornithology of the Philippines. The present paper gave an account of the collection made by Mr. A. H. Everett in the Island of Panaon.—Mr. A. G. Butler, read descriptions of new Lepidoptera of the group Bombycites in the collection of the British Museum.—A communication was read from M. E. Oustelet, containing the description of a new species of cassowary, from New Guinea, proposed to be called *Casuarius edwardsi*.—A communication was read from Mr. F. Nicholson, F.Z.S., containing the description of an apparently new species of American pipit from Peru, which he proposed to call *Anthus peruvianus*.—Prof. A. H. Garrod, F.R.S., read some notes on the placentation of *Hyomoschus aquaticus* as observed in the pregnant uterus of a fresh specimen of this animal recently examined.

Victoria (Philosophical) Institute, April 1.—A paper on modern geology and its bearing on the antiquity of man, was read by Prof. Birks, of Cambridge.

Institution of Civil Engineers, April 9.—Mr. Bateman, president, in the chair.—The paper read was on the embankments of the River Thames, by Mr. Edward. Bazalgette, Assoc. Inst. C.E.

EDINBURGH

University Chemical Society, March 13.—Mr. W. Inglis Clarke, B.Sc., vice-president, in the chair.—A paper was read by Mr. Adrian Blaikie on the salts of trimethylsulphine, containing the results of a joint investigation carried on by Prof. Crum-Brown and himself. They find that the oxalate of trimethylsulphine crystallises in clear hydroscopic plates with one molecule of water of crystallisation, $\{(CH_3)_3S\}_2C_2O_4 + H_2O$. On heating, the salt at 110° C. gives off its water of crystallisation, and at 140° gives off sulphide of methyl, leaving pure oxalate of methyl, $\{(CH_3)_3S\}_2C_2O_4 = (CH_3)_2C_2O_4 + 2(CH_3)_2S$. The sulphide of trimethylsulphine, obtained by mixing equal quantities of sulphydrate and oxyhydrate of trimethylsulphine, can only be obtained in a solution which when evaporated over phosphoric anhydride in an atmosphere of coal gas, decomposes, after a certain strength of solution has been reached, into sulphide of methyl, $\{(CH_3)_3S\}_2S = 3(CH_3)_2S$. The hyposulphite of trimethylsulphine is obtained either by oxidation of the sulphide or polysulphide of trimethylsulphine. It crystallises in clear hydroscopic four-sided prisms with one molecule of water of crystallisation, $\{(CH_3)_3S\}_2S_2O_3 + H_2O$. This salt has all the characteristics of an alkaline hyposulphite. On drying over phosphoric anhydride it gives off its water of crystallisation, and on heating the anhydrous salt at 137° C. it gives off 23·5 per cent. sulphide of methyl, leaving a white crystalline substance, soluble in water, alcohol, and ether, which is at present under investigation.—A paper was also read by Mr. John Treharne, M.B., C.M., on some phenomena observed in the cooling of fats.

PARIS

Academy of Sciences, April 15.—M. Fizeau in the chair.—The following among other papers were read :—Sun-spots and magnetism, by M. Faye. Replying to Prof. Piazzi Smyth's question (NATURE, vol. xvii. p. 220), M. Faye says :—1. The periods 10·45 years for the needle, 11·11 for the spots, have been well determined by Mr. Broun and M. Wolf respectively. 2. The two phenomena are not related. 3. A combination of favourable circumstances, reproduced every 176 years, has led to belief in their connection. 4. These temporary concomitances are not absolutely rare in the history of sciences.—On a new compound of palladium, by MM. Sainte-Claire Deville and Debray. This relates to an ammoniacal sesquichloride of palladium, obtained by causing chlorine solution to act in the cold state on yellow chloride of palladamine. One analysis of it gave : palladium, 42·6 ; chlorine, 43·5 ; ammonia, 12·9.—Experiments tending to imitate various forms of foldings, contortions, and ruptures met with in the earth's crust (continued), by M. Daubrée. A thin layer of an adhesive colouring matter is applied to the surface of a distended balloon of vulcanised caoutchouc. On letting some of the air escape the coated portion forms a protuberance with regular and parallel wrinkles in certain directions ; and M. Daubrée finds like phenomena in the earth's crust.—On the annual temperature of the air, the earth, and the water, in the Jardin des Plantes of Montpellier, according to twenty-six years of observations, by M. Martini. The mean annual temperature (of the air) is 13·42° ; at Paris and Montsouris Observatories it is 10·67° for the same twenty-six years. The mean annual temperature at 0·10m. depth in unsodded ground is inferior to that of the air (about 2°) if only morning observations are taken ; but from observations morning and evening they are nearly the same (ground, 14·65°, and air 14·11°, in the year 1863). The mean temperature of the subterranean sheet of water is 12·77°.—Report on a memoir by M. Jobert relating to aerial respiration of some Brazilian fishes. M. Jobert has found several fishes in the Upper Amazon, having two modes of respiration, one by the gills, the other by the alimentary canal, swallowing air and evacuating by the anus a gas which has more CO_2 and less O than air has. The intestine has a number of filiform appendices composed of blood vessels, which doubtless absorb some of the swallowed oxygen. In other fishes the gas returns by the mouth instead of the anus. In others the swimming-bladder (which has numerous blood vessels in its walls) takes the place of the lungs.—On the equivalent of gallium, by M. Lecoq de Boisbaudran. From calcination of the alum and calcination of the nitrate the mean obtained for the equivalent is 69·865. This agrees closely with a theoretical number got for a body between aluminium and indium.—On the mode of formation of the meteoritic breccia of Santa Catharina, Brazil, by M. Meunier. Four phenomena are traced:—1. Shattering of the metallic iron, and accumulation of the fragments with spaces between. 2. Penetration of sulphuretted hydrogen into these spaces, producing sulphur, and a mixture of pyrrhotine and graphite. 3. Mechanical crushing of the mass. 4. Production of new graphitous matter filling the fissures of the second formation.—On the dissociation of hydrate of chloral, by MM. Moitessier and Engel. From experiment they find that the tension of the vapour of the substance, when boiling, is superior to atmospheric pressure, hence they infer dissociation of the hydrate between 78° and 100° as affirmed by M. Wurtz.—On a rare form of the hepatic organ in worms, by M. Chatin. In a nematoid of the group of *Agamonema*, Dies, an exterior glandular mass is developed round the middle intestine.—Experiments proving that pure urea never causes convulsive disorders, by MM. Feltz and Ritter.—On two rainbows with opposite curvature, by M. Faraguet. This was observed at Agen, on April 8. The bows formed a figure like *x*.—M. Tommasi presented a new system of relays for long submarine cables.

CONTENTS