# NATURE

A WEEKLY

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" *To the solid ground*
*Of Nature trusts the mind which builds for aye.*"—WORDSWORTH



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# INDEX

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THURSDAY, NOVEMBER 1, 1877


## *THE SUN'S DISTANCE*

A MOST interesting state paper has just been issued; we refer to the Report by the Astronomer-Royal on the Telescopic Observations of the Transit of Venus of 1874, made by the Expeditions sent out by the British Government and the results deduced from them. The Astronomer-Royal suggests that another report may be called for when the photographs of the transit have been completely measured and worked out, if possible in combination with the results of similar observations made in the expeditions organised by other governments.

It will be seen from the present Report that the plan of operations actually pursued has been very nearly that proposed by the Astronomer-Royal in his communication to the Royal Astronomical Society on December 11, 1868, when for the third time directing attention to the arrangements which it would be necessary to make for the efficient observation of the transits of 1874 and 1882. The method of absolute longitudes was to be applied for observations both of ingress and egress; it being therefore essential that the longitudes of the observing-stations should be determined with precision; and the longitudes recommended to be fixed by Great Britain were Alexandria, stations in New Zealand and in the Sandwich Islands, Kerguelen's Land, and Mauritius or the two islands of Rodriguez and Bourbon.

The stations eventually selected for observations by the British expedition were fixed upon "entirely by consideration of the influence which their positions would have in determining with accuracy the necessary alteration of parallax." They were: Egypt, the Sandwich Islands, the Island of Rodriguez, New Zealand, and Kerguelen's Land. It was intended to adopt in each of these districts one fundamental station, the longitude of which was to be independently determined, for conversion of local times into Greenwich times, and subordinate to this primary station, other stations were proposed to be selected at such distances that advantage might be taken of different states of weather that might possibly prevail.

In Egypt his Highness the Khedive rendered every possible assistance, tents being supplied with military guards for the protection of the observers and their instruments, and telegraph wires erected. The Astronomer-Royal acknowledges the obligations of the expedition to the liberality of the Eastern Telegraph Company, in affording the means of determining with extreme accuracy and great facility the longitude of the principal station Mokattam. Greenwich was easily connected with Porth Curno, in Cornwall, whence there is an uninterrupted line to Alexandria, the longest submarine line in the world; Alexandria was connected with Mokattam by aid of the special line constructed by the Khedive from Cairo to the station. It is further stated that time-communication was also made from Mokattam through Cairo to Thebes, and to Suez by the ordinary telegraph, Thebes and Suez being the other Egyptian stations where the transit was observed.

In the Sandwich Islands much assistance was received from King Kalakaua and members of the reigning family. The principal station was at Honolulu, the longitude of which was determined partly by meridian-transits of the moon and partly by transits of the moon observed with the Altazimuth instrument. Waimea, in the island Kauai, where observers were also placed, was connected with Honolulu by means of chronometers carried in H.M.S. *Teredos*. At the Island of Rodriguez the longitudes were determined in the same manner as for the Sandwich Islands stations, for three positions, viz., Point Venus, the Hermitage, and Point Coton; and communication was further made with the Mauritius and with Lord Lindsay's expedition with the aid of H.M.S *Shearwater*, the preliminary results being stated by Sir George Airy to agree closely with those given by the lunar observations. At Kerguelen's Land, again, the operations were similar; Supply Bay and Thumb Peak being the stations chosen.

In New Zealand unfavourable weather much interfered with the observations, and Sir George Airy had at first been led to suppose that all useful observation had been lost; it subsequently appeared, however, that this was not the case, one phase of the transit being well seen at Burnham, the longitude of which was fixed by meridian transits of the moon.

The Report is divided into three sections or tables.

In the first are given the descriptions of the various phenomena, in the words of the observers, with the Greenwich sidereal times of the different phases, obtained from accurate reduction of the observations for longitude here particularised; where such longitudes depend upon lunar observations the places of the *Nautical Almanac* were carefully corrected by observations on nearly the same days at Greenwich, Paris, Strasburg, and Königsberg. In studying these original descriptions, Sir George Airy was led to infer that it was "possible to fix upon three distinct phases for the *Ingress* and four for the *Egress*," though it might have been supposed that Egress and Ingress would exhibit the same number of distinct phases in inverse order; this was not the case in practice. The first phase, $\alpha$, utilised in the calculations is the appearance of the planet just within the sun's disc, but the light between the two limbs being very obscure. After an interval of about twenty seconds "the light begins to clear, and the observers generally think that the contact is passed;" this is phase $\beta$. About twenty seconds later, the light which at phase $\beta$ was not equal to that of the sun's limb, is free from all shadow, and the phase is called $\gamma$. Sir George Airy finds that of these phases $\beta$ is the most exact, observers, even in the presence of clouds of moderate density, agreeing within three or four seconds, though for other phases much greater discordances are exhibited. Similarly at the Egress, the first appearance of a fine line or faint shadow is called $\delta$, this becoming definite, or a "brown haze" appearing, is called $\epsilon$. When most observers record "contact," the shadow having reached a maximum intensity, the phase is called $\zeta$, and in this phase there is an agreement amongst observers, much closer than in other phases at Egress. The "circular" contact at Egress is called $\eta$.

In the second section of the Report, or Table II., these "adopted phases are massed for each district in which the parallax-factor is nearly identical," and several of the details of reduction are included. With the longitudes determined as above, the recorded times of the various phases of the transit were converted into Greenwich sidereal times. With the calculated apparent places of the sun and Venus in the *Nautical Almanac*, as deduced from Leverrier's Tables, an ephemeris was prepared exhibiting the predicted geocentric places for every tenth second of Greenwich sidereal time throughout the transit, and from these numbers the apparent positions of sun and planet at each station were computed. Calculations were further made, showing how the predicted places would be affected by alteration of the local longitude, by change in the tabular places of the sun and Venus, and by alteration of their tabular parallaxes; the first two alterations were not essential in these reductions, but the determination of alterations of the third class, as it is remarked, constituted "the special object of the expedition." The form of the reductions was "entirely determined by the consideration that such alterations must be made in the parallaxes as will render the observations of the same phenomena in different parts of the earth consistent with each other." In Table III. we have "the mean solar parallax deduced from every available combination." Thus Ingress accelerated at the Sandwich Islands is compared with Ingress retarded at Rodriguez and with Ingress retarded at Kerguelen's Land; Egress retarded at Mokattam and Suez with Egress retarded at Rodriguez, and likewise with Egress accelerated at the two stations in Kerguelen's; and again the retarded Egress at Thebes is compared with Egress retarded at Rodriguez and with Egress accelerated at Kerguelen's. The greatest separate value of the solar parallax resulting from these different comparisons is 8″·933 and the least 8″·407. Weights are given to the various determinations depending, firstly, upon the number of observations and the magnitude of the parallax-factor; and secondly, upon the particular phase $\alpha$, $\beta$, $\gamma$, $\delta$, $\epsilon$, and $\zeta$ being included. Thus it is found that all the combinations for *Ingress* give the mean solar parallax 8″·739, weight 10·46, and all the combinations for *Egress* give 8″·847, weight 2·53, whence the general result is 8″·760, from which Sir George Airy finds the mean distance of the sun equal to 93,300,000 miles. The New Zealand observations were not included in these calculations; their mean result is 8″·764, almost identical with the above. It is remarked that many persons may perhaps consider that the more closely-agreeing phases $\beta$ and $\zeta$ should be employed in deducing the value of the parallax to the exclusion of the others. If this be done we shall have from the *Ingress* 8″·748, and from the *Egress* 8″·905, or with their due weights a mean value 8″·773.

In this outline of the details contained in the Astronomer-Royal's first Report upon the observations of the transit of Venus, and the conclusions to be drawn from them we have adhered closely to his own words. Pending the appearance of the deductions to be made from the complete measuring of the photographs, the results before us are perhaps to be regarded as provisional ones only, or we have not yet learned all that may be done from the work of the British expeditions, so laboriously organised by Sir George Airy. Many astronomers we can imagine will regard with some suspicion so small a parallax as 8″·76, which is a tenth of a second less than has been given by the most reliable previous investigations, upon different principles. In illustration we may quote the separate results from which Prof. Newcomb obtained his value of the parallax, now adopted in most of our ephemerides:—

| | |
|---|---|
| From meridian observations of Mars, 1862 ... ... | 8″·855 |
| From micrometric observations of Mars, 1862 ... | 8·842 |
| From parallactic inequality of the moon .. ... ... | 8·838 |
| From the lunar equation of the earth ... ... ... | 8·809 |
| From the transit of Venus, 1769 (Powalky's reduction) ... ... ... ... ... ... ... ... ... ... | 8·860 |
| From Foucault's experiments on light ... ... ... | 8·860 |

To these may be added Leverrier's value subsequently deduced from the planetary theories, which is also 8″·86. Newcomb's mean figure, taking account of weights corresponding to the probable errors is 8″·848, which, with Capt. Clarke's measure of the earth's equator, implies that the mean distance of the sun is 92,393,000 miles. Sir George Airy's 8″·760 would similarly place the sun at a mean distance of 93,321,000 miles.

It is well known that some astronomers have not expected our knowledge of the sun's distance to be greatly improved from the observations of the transit of Venus, regarding such an opportunity as is presented by a close opposition of Mars as affording at least as favourable conditions, and the result of Mr. Gill's expedition to

Ascension to utilise the late opposition will be on this account awaited with much interest. Nevertheless, whatever degree of opinion might be entertained by competent authorities, it appears to have been felt by those immediately responsible for action, in different civilised nations where science is encouraged, that so rare a phenomenon as a transit of Venus could not be allowed to pass without every exertion being made to utilise it, and this country may lay claim to an honourable share in the great scientific effort, thanks mainly to the long-continued and admirably-directed endeavours of the Astronomer-Royal to secure this result.

Several of the stations occupied during the transit of 1874 will be available for the transit of 1882, Kerguelen's Land in particular, where at Ingress the sun will be at an elevation of 12°, the factor of parallax being 0·98. In that year there will also be the advantage of observations along the whole Atlantic sea-board of the United States and Canada, where, as pointed out by the Astronomer-Royal in 1868, the lowest factor is 0·95, and the smallest altitude of the sun 12° for observing the retarded Ingress; and for observing the Egress as accelerated by parallax, the factors are about 0·85, the sun's elevation varying from 4° at Halifax, to 32° at New Orleans, or Jamaica. Australian and New Zealand stations are important for retarded Egress.

As is well known, the transit of Venus on December 6, 1882, will be partly visible in this country.

## PARKER AND BETTANY'S "MORPHOLOGY OF THE SKULL"

*The Morphology of the Skull.* By W. K. Parker, F.R.S., and G. T. Bettany, M.A. (London: Macmillan and Co., 1877.)

IN the minds of most of those who have paid no special attention to the subject the skull is regarded as a bony case formed to contain the brain, together with the face. There is also a constancy in the number and position of these bones which lead to the apparently necessary conclusion that occipital, sphenoid, parietal, and other elements are fundamental cranial structures; so that an exhaustive study of their relationships and variations might be thought entirely to cover the subject of skull structure.

That such is not the case has dawned upon us since the elaborate researches of Rathke and other able embryologists, among the foremost of whom must be placed Profs. Huxley and Gegenbauer, who have been followed by Mr. Parker, the author of the work under consideration, who on account of his peculiar aptitude for manipulation, his untiring zeal and his immense experience, has placed the subject of cranial morphology upon a footing infinitely more satisfactory than it has previously been. His numerous memoirs in the *Transactions* of the Royal, Zoological, and Linnean Societies form a mine of biological facts, so beautifully supplemented by their accompanying illustrations. The perusal of them all, in their proper sequence, is however a task only to be undertaken by the specialist, and it is on this account that we have no small degree of pleasure in being able to give a notice of "The Morphology of the Skull," a work of less than four hundred pages, in which is collected, condensed, and digested the mass of information spread through the larger memoirs.

The work consists of a series of chapters on the skulls of carefully-selected types of the five classes of the Vertebrata. Those chosen are:—

1. The Dog-fish and Skate.
2. The Salmon.
3. The Axolotl.
4. The Frog.
5. The Common Snake.
6. The Fowl.
7. The Pig.

These are each described in all stages from their earliest appearance in the blastoderm to their adult condition. Following each chapter is a brief *résumé* of the peculiarities which have been observed in other members of each group, in such a manner that the student of any particular form can learn almost all he may require with reference to any special member of the sub-kingdom.

The primitive trabeculæ cranii, together with the parachordal cartilages and the branchial arches are traced from their earliest development until ossification in and around them has reached the limits of the different types. The insufficiency of our data for the determination of the cranial segments is prominently brought forward, although the moniliform constrictions of the anterior extremity of the notochord in the fowl and in the urodeles is stated, and thought to suggest a segmentation. On the subject of the vertebral theory of the bony skull, Mr. Parker tells us that "only one bony segment, the occipital, can be said to be clearly manifest in the skulls of fishes and amphibians. And in these forms there are no good grounds for assigning to the cranial bones special names indicating a correspondence to particular parts of vertebræ. From the study of adult structures in the mammalian groups skull-theories have been devised, lacking the basis of embryology; and granting that they express some of the truth respecting the highest forms of skull, there is only injury to knowledge in arbitrarily interpreting the lower forms by them. In reptiles the skull becomes much more perfect, but with wide variations in the different groups, such that they cannot be merely subordinated to and explained by the mammalian type. A careful study of the growth of the bird's skull, again, will show that it is impossible to express its composition on a simple formula derived from vertebral structures. But from the lower to the higher forms of vertebrates we can discern a growing away from the primordial type of skull towards and into a loftier development." This result of the extensive investigation upon which it is based is somewhat paradoxical. The "loftier development" of the highest types results in a skull some of whose components may be compared in detail with some expression of truth to vertebræ, whilst in the lower forms a similar comparison cannot be said to hold. And yet true vertebræ themselves, fully developed as far as their essential details are concerned, are found in forms far from high in the scale.

Mr. Parker's invaluable investigations besides their importance in a comparative anatomical point of view, have done much to demonstrate the degree of stress which must be laid on facts of cranial structure in problems relating to classification. His labours have led him to elaborate the instructive classification of birds promulgated by Prof. Huxley in 1867, and so to bring out many points of special interest in avian cranial osteology, demonstrating most clearly the principle which may be arrived at from the study of any special organ or single structure, that a fact which is of the greatest significance in determining the relationships of some one collection of species or genera, may be valueless in attempting to classify others. As an instance of this we may take the skull of the woodpeckers and wrynecks, the peculiarities of which have led Mr. Parker to place them in a division by themselves of primary importance, whereas there is nothing more certain than that their differences from the Toucans and Capitonidæ are only just sufficient to separate them as a family from either. And yet among almost all other orders of birds the cranial structure is invaluable in the determination of their affinities.

The uniformity of the nomenclature and the absence of any laxity in the expression of the mutual relations of parts, greatly increases the facility with which the great number of facts brought forward by the authors can be grasped, and no doubt it is Mr. Bettany whom we have in great measure to thank for the general selection and classification of those which have been chosen to form "The Morphology of the Skull."

In conclusion we feel certain that all who read the work under consideration, the very nature of which makes it almost impossible for us to discuss the details with reference to any of the points which it brings forward, will realise how important an addition it is to biological science, and no thinking student will lay it down without recognising how much scope there is for still further investigation in the same field, especially in that direction which leads to the explanation of the reason why cartilages grow and bones form in certain definite directions and situations and in them alone; in other words, the next book of the kind required is one on the dynamics of the development of the skull.

## *THOMSON'S "SIZING OF COTTON GOODS"*

*The Sizing of Cotton Goods.* By Wm. Thomson. (Manchester: Palmer and Howe.)

IN weaving cotton cloth it is necessary that the warp, which has to withstand a considerable strain in the process of manufacture, should be artificially strengthened by "sizing," that is, by dressing the thread with some adhesive material so as to enable it to resist the pulling and wearing action of the healds and shuttle. In the earlier days of cotton manufacture the weaver contented himself with the use of a mixture of flour-paste and tallow; the first ingredient gave the thread the desired extra strength, the second removed the harshness which the use of flour alone would have given. But the manufacturer soon discovered that by a judicious selection of the components of his "size," and by alterations in the mode of applying it, he could confer upon the cloth the appearance of being fuller and stouter than it actually was, judging from the amount of cotton contained in it. The great scarcity of the raw material during the cotton famine which sprung out of the American civil war had a powerful effect in developing the ingenuity of a certain set of manufacturers, and there is no doubt that their machinations have had a lasting influence upon the mode of manufacture of grey cloth. As the weight of a piece of calico is one of the chief elements in determining its value, attempts were quickly made to increase that weight by mixing such bodies as powdered heavy-spar, or, worse still, of deliquescent salts like the chlorides of magnesium and calcium, with the sizing material. Occasionally the manufacturer in thus attempting to palm off water or a worthless mineral in lieu of good cotton over-reached himself and a just retribution overtook him in the shape of heavy damages for mildewed or rotten goods.

The results of many of these attempts afford excellent illustrations of the proverbial danger of a little knowledge; the manufacturer somehow acquired the information that chloride of calcium, an almost worthless bye-product in many chemical operations, was an excellent absorbent of atmospheric moisture; its advantages as an ingredient of the sizing mixture were therefore obvious; unfortunately he knew nothing of *oidium oranteacum* or *puccinia graminis*, and had probably never heard of *pencilium glaucum*, or he might have known that he was preparing a mixture specially suited to the development of these fungi. Silicate of soda or water-glass doubtless appeared at first sight to be an excellent substance for dressing warp, but a painful experience was needed to teach some manufacturers that these alkaline silicates rapidly absorb carbonic acid, and that the resultant products, namely, free silica, and sodium carbonate, together occupying a larger volume than the original silicate, exerted a disruptive action upon the hollow cotton-fibre and made the cloth rotten and useless. Mr. Thomson does not altogether shirk the consideration of the moral aspects of the question of sizing; he makes no secret of the fact that the operation is often done with fraudulent intention. He expresses his opinion distinctly enough that the introduction of an undue amount of size into goods intended for the home trade can serve no useful purpose, but we think he will find it difficult to convince ordinary or unbiased people that a composition consisting, to the extent of half its weight, of a mixture of putrid flour, or British gum, China clay, barytes, or magnesium chloride, tallow, or palm-oil, with a sufficient amount of chloride of zinc or carbolic acid to prevent the whole from running into absolute nastiness, is a fit material to clothe even the patient Hindoo or the prudent Chinaman. Mr. Thomson, however, takes this business of sizing as a fact which, of course, cannot be ignored, and he tries to make the best of it. In the outset he shows that, as it now stands, the process is one of the clumsiest, most unscientific, and least understood of all the operations with which the manufacturer has to deal, and he points out, clearly and concisely, wherein it is faulty, and how it may be amended.

The book is, of course, designed primarily for the use of grey-cloth manufacturers, calico-printers, and generally of those whose business it is to buy and sell calico; and the subject is mainly treated from the point of view of a chemist perfectly familiar with the objects sought to be gained by legitimate sizing. In plain and albeit scientific language he describes the various pieces of apparatus employed in ascertaining the value of the different ingredients in size; he points out the qualities, good and bad, of the materials employed to give adhesive and softening qualities to the size; how the size is to be applied to the yarn; to what diseases or modes of decomposition it is liable; and how it may be preserved from mildew or mischievous changes. The book has every right to be regarded as the only important treatise on the subject which has yet appeared, and, as such, we would recommend it to all who are interested in the production of one of our chief staples. T.

## OUR BOOK SHELF

*Physiological Tables for the Use of Students.* Compiled by Edward B. Aveling, D.Sc., F.L.S. (London: Hamilton, Adams, and Co.)

WE are at a loss to find any excuse for the publication of these tables, which no one, we presume, would attempt to justify except on the plea that they may be useful in cramming students so as to pass the multifarious superficial examinations which are a blot upon our educational system.

They are unphilosophical in their plan, and altogether unreliable in their details. Some idea of the nature and value of the information which is here put up, as it were, into separate pigeon-holes for the use of the unwary, may be gathered from the following quotations. Nervous tissue, we are told, contains 15 per cent. of fats, thus classified:—

| | |
|---|---|
| Fats, 15 per cent. in white, 5 per cent. in gray. | Oleo-phosphoric acid.<br>Olein; margarin; palmitin.<br>Cholesterin. |

Would Dr. Aveling like to write a short essay upon oleo-phosphoric acid? Has he never heard of such bodies as glycerin-phosphoric acid and its derivative lecithin?

Or to quote from Table IV., where Dr. Aveling writes on the causes of the circulation:—

| | | |
|---|---|---|
| CAUSES OF CIRCULATION. | Impulse of heart. | |
| | Elasticity of arteries. | |
| | Capillary Force. Proofs. ... ... | 1. Alterations in diameter of capillaries.<br>2. Alterations of velocity of blood flowing through them<br>3. Movement of blood after excision of heart in cold-blooded amimals.<br>4. Emptying of arteries after death.<br>5. Secretion after death.<br>6. First movement of blood in embryo towards, not from, the heart.<br>7. Fœtus without heart has organs developed.<br>8. Degeneration of heart during life without much alteration in the circulation.<br>9. Heart working well, and yet circulation through some part ceases.<br>10. Asphyxia. |
| | Muscular pressure on veins. | |

Would it not be an admirable exercise to set the above lines to intending candidates in physiology and ask them to criticise them? Our readers will do so for themselves.

In the table referring to the sense organs we are confidently told that the nerve centres for the special sense of touch are the *thalami optici*, that the centres of the special sense of smell are the olfactory lobes, that the centres of sight are the corpora quadrigemina, the corpora geniculata, and the thalami optici.

But the above examples are more than sufficient to prove how dangerous a catalogue of mistakes Dr. Aveling has presented us with.

If science is to be used as a discipline in education, let it be fully and accurately taught; let us not imitate the old scholastic routine which forced unpalatable jargon in the form of "propria quæ maribus," &c., upon the unwilling student, and refuse to follow it in that which is its merit—its accuracy. A. G.

## 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.]*

### Indium in British Blendes

IT will be a matter of some interest to English mineralogists and chemists to know that certain blendes of Durham and, I believe, of Cumberland contain Indium in appreciable quantities. This fact has been made out by a very skilfully-conducted analysis by Dr. Flight in the laboratory attached to this department.

The work in the laboratory has, through the past two years, been almost exclusively devoted to the analysis of minerals selected from the division of the collection which is in process of being catalogued, and for which the crystallographic work has long been in progress.

When I gave the particular blendes in question to Dr. Flight for analysis, the grounds for their selection were that they were British, and that one of them in particular resembled certain foreign blendes which contain the rare metals found in association with this mineral.

The object of this letter is to secure a prompt announcement of Dr. Flight's having found Indium in the blende in question. He will in due time communicate further details of the analysis of the blende and of an elegant process by which he at once separates the Indium Sulphide from the blende.

NEVIL STORY MASKELYNE

Mineral Department, British Museum, October 30

### The Radiometer and its Lessons

WILL you allow me to make a few remarks in reply to to Dr. Carpenter's letter on "The Radiometer and its Lessons," published in the last number of NATURE, and to try to show that I had good grounds for the opinion I expressed at the late meeting of the British Association in reference to his article on the same subject in the *Nineteenth Century*?

Nearly the whole of the first three columns of Dr. Carpenter's letter is devoted to proving that he "was not influenced, when writing on the radiometer, by any *animus* arising from [his] personal antagonism to Mr. Crookes on another subject." As I never in any way charged him with being thus influenced, I do not think that this part of his letter calls for any further remark on my part than an expression of my sincere regret that it should have been possible for him to think that I intended to make such a charge.

Dr. Carpenter devotes the rest of his letter to showing that he had "adequate justification" for "making it appear that Mr. Crookes had put a wrong interpretation on his own results," and thus proves very conclusively that I had "adequate justification" for supposing it possible that he may have intended to make this appear in his article in the *Nineteenth Century*.

In order to make out his "justification," Dr. Carpenter sets himself to prove (1) that Mr. Crookes puts forward the "direct impact of the waves" as affording "a definite interpretation" of the motion of the radiometer, and (2) that he claimed "the discovery of a 'new force' or 'a new mode of force.'"

With regard to the first of these points, I think that few persons can have read or heard Mr. Crookes's accounts of his investigations without having observed how careful he was to reserve his judgment as to the cause of the remarkable effects he had discovered, and neither to give out as conclusive any explanation of his own, nor to adopt any of those suggested by others until, chiefly through his own further experiments, one of them had been shown to rest on sufficient evidence. It is true that on one occasion he uses the following words (quoted by Dr. Carpenter):—"My own impression is that the repulsion accompanying radiation is directly due to the impact of the waves on the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation," and that, in several places in his earlier papers, he shows a leaning towards the same hypothesis; but this is a very different thing from having adopted this view as a "definite interpretation" of the phenomena. Even Dr. Carpenter does not attempt to show that Mr. Crookes ever, in so many words, committed himself to this theory, but concludes that he held it from considerations which, for fear of misrepresentation, I must give in Dr. Carpenter's own words :—

"After pointing out that 'there is no real difference between heat and light, all we can take account of [I presume he means physically, not physiologically] being difference of wave-length,' he [Mr. Crookes] thus continues : 'Take, for instance, a ray of definite refrangibility in the red. Falling on a thermometer it shows the action of *heat;* on a thermopile it produces an *electric current;* to the eye it appears as *light* and *colour;* on a photographic plate it causes *chemical action;* and on the suspended pith *it causes motion.*' Now (1) this motion being elsewhere spoken of as due to *the impetus given by a ray of light,* (2) a set of experiments being made to determine *the mechanical values of the different colours of the spectrum,* (3) an observation being recorded on *the weight of sunlight* (without the least intimation that he was 'speaking figuratively' as Mr. Crookes says that he did to his audience at the Royal Institution), (4) the term *light-mill* being used by himself as a synonym for 'radiometer,' and (5) no hint whatever being given of the dependence of the result (as argued by Prof. Osborne Reynolds) on a 'heat-reaction' through the residual vapour, I still hold myself fully justified in attributing to Mr. Crookes the doctrine of the *direct mechanical action of light.*"

Taking these points in order and using Dr. Carpenter's numbers for reference, I may observe as to (1) that this seems to refer to Mr. Crookes's statement of an "impression" in a passage already quoted; with regard to (2) that Mr. Crookes having found that "every ray from the ultra-red to the ultra-violet" produced a mechanical effect under the circumstances of his experiments, it was very natural that he should hope to get some clue as to the nature of the action by finding what rays produced the greatest effect; of Dr. Carpenter's arguments (3), (4), and (5), it is difficult to speak with the seriousness befitting their author's many valuable services to the cause of science, and the "due consideration of . . . *his* and *my* relative positions." To conclude that Mr. Crookes must have held a particular theory from the fact that, when he had constructed an apparatus which spun round on exposure to light, he called it a "Light-mill;" from his having neglected to give warning that he was "speaking figuratively" when he talked of "weighing a beam of sun-light," or from his having given no hint that he had adopted a rival theory, is certainly not to exemplify the "strict reasoning based on exact observation" which Dr. Carpenter recommends in the paragraph with which he concludes both his article and his letter to this Journal.

A few sentences before the passage I have quoted, Dr. Carpenter refers to the "whole phraseology" of Mr. Crookes's papers of January 5 and February 5, 1876, as indicating "that he *then* considered [the motion of the radiometer] as directly due to the impact of the waves upon the surface of the moving mass." This again seems to me a very unsound conclusion. The effect to the elucidation of which these papers were devoted was unquestionably due to the incident radiation, but whether as a primary or as a secondary effect, was still a matter for discussion. In my opinion the phraseology used in them implies no more than this : it indicates a relation of cause and effect, but, for the most part, leaves the question as to *how* the latter follows from the former, entirely untouched. If, however, Dr. Carpenter will refer to § 195 of the paper of February 5, as it is printed in the *Phil. Trans.* for 1876, he will see that Mr. Crookes did not *then* attribute the motion to *direct impact of the rays upon the surface* of the moving body, but rather to an elevation of its temperature, and a consequently increased *radiation of heat from its surface.* At the same time he will see that this suggestion is put forward in a tentative and entirely undogmatic way.

Dr. Carpenter next undertakes to show that Mr. Crookes laid claim to the discovery of a "new force" or a "new mode of force," finding his proof of this in a passage included in the quotation from his letter that I have given above. Commenting on this passage in the *Nineteenth Century* (p. 248), he says : "To the *three* attributes of radiation universally recognised by physicists, Mr. Crookes proposes (in the passage already cited) to add a *fourth,* the power of producing an electric current in a thermopile; and a *fifth,* the power of producing mechanical motion when acting on light bodies freely suspended in a vacuum." Again, if Dr. Carpenter had consulted the *Philosophical Transactions* for 1876 (p. 361), he might have done Mr. Crookes more justice and might have given him credit for the discovery of a *sixth* attribute of radiation—(Mr. Crookes there mentions one more effect which the same ray can produce : "concentrate it on the hand by a lens, it raises a blister accompanied with pain"),—and, if he had read a few lines further, he might have spared himself the trouble of explaining to Mr. Crookes that the electric current of a thermopile is not directly excited by the incident radiation, for he would have found that this action, in common with the pain and the blister and the motion of the mercury in a thermometer, is there spoken of as being an effect of *heat.* I think it must be evident to any one who will read this passage attentively with its context (either in *Proc. Roy. Soc.* [February 10, 1876], from which apparently Dr. Carpenter quotes, or in the *Phil. Trans., loc. cit.*), that it has nothing at all to do with either one or more new forces, but that the whole gist of it is to assert that, whatever may be the mode in which radiation produces mechanical force, the result is to be attributed to it *as a whole* and not to a particular constituent assumed for the purpose.

As though with the object of covering a retreat, Dr. Carpenter says, near the end of his letter, that "Prof. G. Carey Foster will doubtless be able to pick out *points of detail* in my article, as to which faults may be found by a severe critic." I may therefore point out that I have so far carefully confined myself to what he himself singles out as the "main issues" of the question between us, and that, in my further remarks, I shall treat the matter from a still more general point of view.

In speaking (in my address at Plymouth) of the "tendency" of Dr. Carpenter's article, I meant to indicate that I referred in what I said about it to what seemed to me to be its general drift and tone, rather than to any particular passage or passages. And my judgment of the drift of the article was formed not only from what I found in it, but also from what I did not find there. For example, if Dr. Carpenter had thought as highly as I do of Mr. Crookes's work he would almost inevitably have pointed out more emphatically than he did the really astonishing number, variety, and laboriousness of his experiments ; he would also, I think, have pointed out that (with the important exception of Dr. Schuster) scarcely one of the numerous investigators, who, in consequence of his researches, have occupied themselves more or less with the radiometer, had obtained any significant experimental result which Mr. Crookes himself had not anticipated ; and he would have shown that the discovery of the radiometer, while affording a remarkable illustration of the importance of following up unexplained though apparently trivial phenomena, illustrates no less forcibly the truth that scientific discoveries are not chance revelations, coming now to one and now to another, but that they are made only by those who have eyes to see a clue when it is offered them, and patience and skill to follow where it leads.

Turning to what the article did contain, I think it is not incorrect to say that it tended to produce the impression that Mr. Crookes, more or less obstinately, and on insufficient grounds, rejected a satisfactory explanation of his results. I will therefore try to state, as shortly as I can, what seems to me to be the true state of the case in relation to this point.

Prof. Reynolds (in his paper read before the Royal Society on June 18, 1874) undoubtedly showed that a mechanical reaction, such as might account for the results obtained by Mr. Crookes, might arise when heat is communicated from a solid surface to a vapour or gas, but he did not (then at least) show that in Mr. Crookes's vacua there was enough residual gas to produce the results he ascribed to it. Mr. Crookes, without disputing the possibility of the action pointed out by Prof. Reynolds, made experiments from which he concluded that it was insufficient to explain the movements he had observed. (I must here remark that Mr. Crookes did not say, as Dr. Carpenter asserts that he did, that the explanation offered by Prof. Reynolds was one that "it is impossible to conceive." His words were : "It is impossible to conceive that in *these* experiments sufficient condensable gas or vapour was present to produce the effects Prof. Osborne Reynolds ascribes to it. After the repeated heating to redness at the highest attainable exhaustion, it is difficult to imagine that sufficient vapour or gas should condense on the movable index to be instantly driven off by a ray of light, or even the warmth of the finger, with recoil enough to drive backwards a heavy piece of metal."—*Phil. Trans.*, 1875, p. 547. But although Prof. Reynolds is unquestionably entitled to the credit of having originated the fundamental idea and worked out many of the details of the explanation that seems now to be generally adopted, his explanation not only rested on a somewhat slender experimental basis, but was theoretically incomplete, and in particular it did not show clearly why so high a degree of rarefaction should be needed for the production of the phenomena in question. An important step towards supplying this deficiency was taken by Profs. Tait and Dewar (July, 1875), who showed how the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action, but the explanation in the form which they gave to it required that the rarefaction should be carried far enough to make the mean length of path of a molecule of gas great as compared with the dimensions of the inclosing vessel. It has, however, been pointed out by Prof. Zöllner (*Pogg. Ann.*, February, 1877), and more recently by Mr. Tolver Preston (*Phil. Mag.*, August, 1877), that, in the majority of cases, this condition is far from being fulfilled. On the other hand, the residual-gas theory of the action of the radiometer received very important experimental support from Dr. Schuster's beautiful demonstration (February, 1876) that the force exerted on the discs was correlative with an equal opposite force exerted upon the glass envelope. The complete proof that the action was due in some way to the presence of residual gas was furnished by Mr. Crookes's own discovery (June, 1876) that it rapidly diminishes when the exhaustion is carried beyond a certain point depending on the nature of the gas. The outstanding defect in the theory was removed by Mr. Johnstone Stoney, who (*Phil. Mag.*, March and April, 1876) showed that the observed phenomena might arise at a degree of rarefaction at which the mean length of path of a molecule was still much below the distance from the discs to the envelope, it being sufficient that this distance should not be too great to allow the warming of the discs to cause a sensible increase in the velocity with which the molecules struck the glass. Mr. Stoney's form of the theory answers to all the facts of the case, so far as I am acquainted with them, and it has been confirmed and illustrated by Mr. Crookes with a numerous series of remarkably beautiful and ingenious experiments.

My object in thus tracing the chief stages in the growth of the accepted theoretical explanation of the radiometer has been to point out that the quality of mind which led Mr. Crookes to reject the various suggested explanations of the phenomena he had observed, so long as they were only approximate and did not account for *all* his facts, was merely a further exemplification of the quality which led him to the original discovery. If he had been content to disregard a seemingly trivial fact he would never have made this discovery at all, and if he had disregarded slight defects in the explanations that were offered he would have missed some of its most important consequences. I think that this also might have been suitably included among the "Lessons of the Radiometer." G. CAREY FOSTER

University College, London, October 27

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HAS Dr. Carpenter allowed himself to become possessed by a "dominant idea?" From his letter in NATURE (vol. xvi. p. 544), I infer that he *might* have taken the trouble to reply to my article in the July number of the *Nineteenth Century*, had he not thought that my assertions "were well known in the scientific world to be inconsistent with fact."

Some remarks, however, made by Prof. G. Carey Foster at the British Association seem to have forced upon Dr. Carpenter the conviction that he may have underrated my character for veracity, and that the "scientific world," at all events, is not unanimous in regarding my "assertions" as falsehoods. Dr. Carpenter therefore seeks in your columns to justify the statements contained in his article on "The Radiometer and its Lessons," in the *Nineteenth Century* for April last.

When Dr. Carpenter declares my "assertions (1) . . . (2) . . . (3)" to be false, I have a right to demand that Dr. Carpenter give my identical words, and not his own interpretation of my words—an interpretation which is "inconsistent with fact."

To show Dr. Carpenter's inaccuracies in small things as well as great, I may point out that he does not even quote correctly the title of my article in the *Nineteenth Century*. His carelessness in more important matters is of deeper consequence. In order to enforce one of his dominant ideas "yet more fully and emphatically," he tells us that he applied himself to a "careful reperusal of" my papers "with the most earnest desire to present a true history of the whole inquiry." A most laudable determination! And where, will it be believed, did Dr. Carpenter, a Fellow of the Royal Society, go for information? To the *Philosophical Transactions*, where my papers are printed at full length? No! He only referred to the "*Proceedings* of thd Royal Society," a record, as every one knows, that contains brief, and therefore imperfect abstracts of what is published in full in the *Transactions*.

In his "justification" Dr. Carpenter quotes a passage from a lecture I delivered in 1874, on The Repulsion Accompanying Radiation, commencing, "my own impression is," &c. Had Dr. Carpenter quoted the next paragraph, which is necessary to a correct interpretation of the sentence he did quote, your readers would have been enabled to judge how far I advanced theories of my own. My words were these: "I do not wish to insist upon any theory of my own. . . . The one I advance is, to my mind, the most reasonable, and, as such, is useful as a working hypothesis, if the mind must have a theory to rest upon. *Any* theory will account for *some* facts, but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed." My next paragraph concludes with the following quotation from Sir Humphry Davy:—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

With regard to my having "theorised on the subject," I have never denied having done so, although I have on five or six occasions specially stated that "I wished to keep free from theories," and "unfettered by the hasty adoption" of theories. But I do deny that I ever stated that my results were definitely explained by the direct mechanical action of light. Your readers will understand that an experimental research is necessarily and slowly progressive, and that the early provisional hypotheses have to be modified, and perhaps altogether abandoned, in deference to later observations. Until my experiments confirmed the explanation given by Mr. Johnstone Stoney, I adopted no definite theory, and I contend that a trained physicist would fail to gather from my published papers that I desired my first impressions to be regarded as final

Dr. Carpenter again attributes to me the terms "a new force," or a "new mode of force," as applied to the repulsion accompanying radiation. Unless Dr. Carpenter can point these words out in my published papers, he has no right to place them between inverted commas.

But the chief burden of Dr. Carpenter's song is that "Mr Crookes has another side to his mind, which makes Mr. Crookes the spiritualist almost a different person from Mr. Crookes the physicist." I fail to see how the investigation of certain phenomena called spiritual can make a man a spiritualist, even if he comes to the conclusion that some of the phenomena are not due to fraud. My position in this matter was clearly stated some years ago, and I ask your permission to quote the following passages from an article I published in 1871:—"I have desired to examine the phenomena from a point of view as strictly physical as their nature will permit. . . . I wish to be considered in the position of an electrician at Valentia examining, by means of appropriate testing instruments, certain electrical currents and pulsations passing through the Atlantic cable; independently of their causation, and ignoring whether these phenomena are produced by imperfections in the testing instruments themselves, whether by earth currents or by faults in the insulation, or whether they are produced by an intelligent operator at the other end of the line."

From this stand-point I have never deviated. Can Dr. Carpenter say that his position and mine, in respect to the investigation of the phenomena ascribed to spiritualism, are so very different? He asserts that he has shown beyond doubt that it is all imposture. But I would ask if this was proved to his satisfaction twenty years ago, why does he still waste valuable time in interviews and sittings with so-called mediums? If I am to be censured for having devoted time to this subject, such censure must be doubly applicable to a man who commenced the investigation when I was a child, and who cannot let the subject drop whenever a new "medium" comes in his way. Does he regard the subject as his own special preserve, and may his demonstrations against other explorers in this domain of mystery be looked upon as the conduct of a gamekeeper towards a suspected poacher?

To impress on the world that he has no "*animus*," Dr. Carpenter says he "cordially" and "personally congratulated" me. His words bring vividly to my mind the conversation, of which, by the by, he has omitted an important part. It was at the annual dinner of the Fellows of the Royal Society on November 30, 1875, when the royal medal was awarded to me. Dr. Carpenter accosted me with great apparent cordiality, and said, "Let us bury the hatchet! Why should scientific men quarrel?" I signified my full acceptance of the offered peace, and great was my surprise soon after to find that, unmindful of the understood compact, he had exhumed his hatchet and was dealing me unexpected and wanton strokes, tempered by a certain amount of half praise which reminds me of the sort of caressing remonstrance of Majendie in the pre-anæsthetic days, to the dog which he had on his operating table—"*Taisez vous, pauvre bête!*"

In all seriousness, however, I must again ask, what is the meaning of the "personal antagonism," and the persistent attacks which Dr. Carpenter, for the last six years, has directed against me? In his recently published book, in the *Nineteenth Century*, and in his last letter to you, the key-note struck in the *Quarterly Review* six years ago is sustained. We have the same personalities, the same somewhat stale remark about my double nature, and the same exuberance of that most dangerous and misleading class of averments, half truths. Dr. Carpenter, indeed, condescends to admit that I have pursued "with rare ability and acuteness a delicate physical investigation in which nothing is taken for granted without proof satisfactory to others as well as to himself," and that I have "carried out a beautiful inquiry in a manner and spirit worthy of all admiration;" but, after granting so much, he dissembles his love and proceeds to "kick me down stairs." I am damned with faint praise, and put to rights in such a school-masterly style, that I could almost fancy Dr. Carpenter carries a birch rod concealed in his coat-sleeve. He admits that in an humble and subordinate sphere I have done useful work, only I must not give myself airs on that account. Dr. Carpenter reminds me of Dr. Johnson defending Sir John Hawkins, when he was accused of meanness. "I really believe him," said Johnson, "to be an honest man at the bottom; but to be sure he is penurious, and he is mean, and it must be owned he has a degree of brutality, and a tendency to savageness, that cannot easily be defended." In the same magnanimous spirit Dr. Carpenter allows that I have contributed a trifle to science, but he does not forget to add that I am the victim of cerebral duplicity, and I am again held up to illustrate the sad result of neglecting to train and discipline "the *whole* mind during the period of its development," &c.

I have, it appears, two allotropic personalities, which I may designate, in chemical language, Ortho-Crookes and Pseudo-Crookes. The Ortho-Crookes, according to Dr. Carpenter, has acquired "deserved distinction as a chemist." He carries out a "beautiful inquiry in a manner and spirit worthy of all admiration." He has shown "ability, skill, perseverance, and freedom from prepossession." He pursues "with rare ability and astuteness a delicate physical investigation." He evinces the "spirit of the true philosopher," and he has "deservedly" received "from the Royal Society the award of one of its chief distinctions."

But Pseudo-Crookes, whose career Dr. Carpenter has evidently watched almost from his cradle—as he professes to know the details of his early education—unfortunately took a "thoroughly unscientific course," and developed into a "specialist of specialists." He had "very limited opportunities" and "never had the privilege of associating" with scientific men, although he displayed "*malus animus*" "towards those with whom he claims to be in fraternity." He is "totally destitute of any knowledge of chemical philosophy, and utterly untrustworthy as to any inquiry" not technical. His "assertions" are "well known in the scientific world to be inconsistent with fact." He enters on inquiries "with an avowed foregone conclusion of his own." He has "lent himself to the support of wicked frauds." He has "prepossessions upon which clever cheats play." His "scientific tests" are not "worthy of trust." He is a believer in "day dreams," and the supporter of a "seething mass of folly and imposture;" whilst, to crown all, he actually thinks that the radiometer is driven "by the direct impetus of light." In short, this Pseudo-Crookes is a compound of folly and knavery such as has rarely, if ever, previously been encountered.

WILLIAM CROOKES (The Ortho-Crookes?)

London, October 29

### Mr. Wallace and Reichenbach's Odyle

I AM amazed that Dr. Carpenter should think it necessary to make public, with such haste, Prof. Hoffmann's statement that Baron Reichenbach's facts and theories are not accepted by the body of scientific men in Germany. Of course they are not. But how this affects their intrinsic accuracy I fail to see. Less than twenty years ago the scientific men of all Europe utterly disbelieved in the co-existence of man with extinct animals; yet the facts adduced by Freere, Boué, McEnery, Godwin Austen, Vivian, and Boucher de Perthes, are now admitted to have been trustworthy and deserving of the most careful examination. The whole history of scientific discovery from Galvani and Harvey to Jenner and Franklin, teaches us, that every great advance in science has been rejected by the scientific men of the period, with an amount of scepticism and bitterness directly proportioned to the novelty and importance of the new ideas suggested and the extent to which they run counter to received and cherished theories. Rejection is one thing, disproof is another; and I have in vain searched for anything like disproof, or even rational explanation, of Reichenbach's facts: his theory, or "Odyle-doctrine," I have never "attempted to rehabilitate," as Dr. Carpenter, with his usual misconception, says I have done. In my review of Dr. Carpenter's lectures (*Quarterly Journal of Science*, July, 1877, p. 396), I adduce five tests employed by Reichenbach, and also the independent and simultaneous confirmation of Dr. Charpignon in France; and the only reply I get is: "All men of science disbelieve them." With the facts of history above alluded to in my mind, and believing that human nature is very much the same in the nineteenth century as it was in the eighteenth, I can only say, "so much the worse for the men of science."

Dr. Carpenter's reference to the believers in a flat earth, as a parallel case, is unfortunate, because the two cases are really of a totally different nature. Those who maintain the earth to be flat do not deny the main facts which we rely on as proving it to be round, but they attempt to give other explanations of them. The dispute is on a question of reason and inference; and every intelligent and fairly educated man is able to decide it for himself. But in Reichenbach's case it is the *facts* that are rejected without disproof or adequate explanation. The two cases are therefore quite distinct, and Dr. Carpenter's attempted parallel, as well as his setting up of scientific disbelief as a conclusive reply to evidence, is in conformity with his whole treatment of this subject.

I trust that such of the readers of NATURE as may feel any interest in the questions at issue between Dr. Carpenter and myself will read my article above referred to, and not allow themselves to be influenced by Dr. C.'s repeated appeals to authority and to prejudice.

ALFRED R. WALLACE

I HAVE to request your insertion of a post-card I have this morning received, for two reasons; *first*, because, as it is anonymous, and as the writer of it is obviously a reader of NATURE, no other way is open to me for replying to it except that which your columns may afford; and *secondly*, because it is a very curious example of the misconceptions into which men are apt to fall who allow themselves to become "possessed" by "dominant ideas."

"If Mr. A. R. Wallace has to choose between being either 'a fool or a knave,' there is *at all* events *no choice* left for *the man* who deliberately and maliciously makes incorrect assertions and suppresses the truth to further his own views. I dare say you know what most people would call such a man. Yours,

"ONE WHO WAS AT PLYMOUTH"

Now, in the first case, it must be perfectly obvious to any one who is capable of reasoning logically, that nothing which I said of Mr. Wallace in your last number can be twisted into the implication that he is either "a fool or a knave." John Hampden is continually saying this of Mr. Wallace and of everybody who upholds the rotundity of the earth. And I mildly suggested whether, in putting himself in opposition to the whole aggregate of scientific opinion on the value of Reichenbach's Odylism—not because he had himself repeated them, but because he believes in Reichenbach—Mr. Wallace is not assuming an attitude in some degree similar, that is, setting himself up as the one wise and honest man who duly appreciates Reichenbach, and therefore implying that everybody else is either stupidly or wilfully blind to the evidence he presented. If anyone thinks it worth while to read Mr. Wallace's review of my lectures on "Mesmerism, Spiritualism," &c., in the last number of the *Quarterly Journal of Science*, he will be able to judge whether I have or have not wronged Mr. Wallace in this matter.

The writer's appreciation of my own character, which has frequently been expressed to me before in the same manner and in the like terse and elegant language, is now enforced by what he deems to be Prof. Carey Foster's judicial opinion, delivered at the Plymouth meeting; and I find myself, therefore, fully justified in my opinion that by his introduction of the word "intentionally" Prof. Carey Foster made his judgment legitimately bear a meaning, which, as he has stated, he would consider insulting to my character. And I cannot but believe that Prof. G. Carey Foster will regret having thus given a new handle to a man who obviously *wishes* to insult me on account of my antagonism to spiritualism. As the writer of the post-card continues to use Prof. G. C. Foster's authority, *after* that gentleman's explicit disavowal of the offensive meaning here attached to it, and as I may, of course, expect that he will continue to avail himself of that authority, I should like him to know through your columns that it is scarcely worth while for him to trouble himself to repeat these attacks, since they have long since ceased to do anything else than amuse me, and will only furnish me with materials for amusing other people.

It seems much to be regretted that neither spiritualism nor attendance at the meetings of the British Association, nor even the reading of NATURE seems able to teach this person to behave like a gentleman. WILLIAM B. CARPENTER

October 29

### Potential Energy

YOUR correspondent "X." has described some of his troubles respecting potential energy. Many a learner could describe similar troubles respecting force and energy in general. They who earnestly contend for definiteness and accuracy do not always teach with definiteness and accuracy. For example: in his "Treatise on Heat," p. 137, Dr. Tyndall tells me that by raising a weight from the floor I have conferred upon the weight potential energy. Presently he tells me that this energy is derived (not from me, but) from the pull of gravity. He next tells me that we might call the energy with which the weight descends, moving force, *i.e.* he teaches me to confuse force and energy; and after all this he bids me remember that "exactness is here essential. We must not now tolerate vagueness in our conceptions."

Take another example. In his lecture on "Force" (NATURE, vol. xiv. p. 462), Prof. Tait teaches that force is a mere name, and that it has no objective existence; he also teaches that the product of this non-existence by its displacement has an objective existence. Few learners would say that is a very lucid statement. Again, in the same lecture he says "there is no such thing as centrifugal force, and accelerating force is not a physical idea at all;" but in his "Nat. Phil." he speaks of both these forces, and describes their effects (Nos. 185, 187, 598, 248).

When teachers deservedly eminent make statements like the foregoing, so likely to mystify and confuse a novice, it is no wonder that there is a good deal of smattering in popular science.

Prof. Tait says "the so-called accelerating force is really no force at all, but another name for the kinematical quantity acceleration." I venture to entirely disagree with this statement, and for the following reason:—$\frac{dv}{dt}$ is a *number*, and may be that number of units of *force*, or that number of units of *acceleration*. When it is called accelerating force it is the representative of $m\frac{dv}{dt}$, when $m = 1$, and $m$ does not appear in the expression; and it means $\frac{dv}{dt}$ units of *force*. When it is called acceleration it means $\frac{dv}{dt}$ units of *acceleration*. Accelerating force is just as real as moving force, for it is, in fact, the $m$th part of the moving force. In like manner $v$ may mean either $v$ units of velocity, or $v$ units of momentum; in the latter case it is the representative of $mv$, when $m = 1$, and means the momentum of a unit of mass which has $v$ units of velocity. In like manner $m$ may mean either $m$ units of mass, or $m$ units of momentum, or $m$ units of kinetic energy; in the two latter cases it is the representative of $mv$ or of $mv^2$ when $v = 1$, and means the momentum, or the *vis viva* of $m$ units of mass moving with unit of velocity.

A few simple definitions would remove the difficulties respecting force. Thus: If a mass of $m$ units of mass is at any instant receiving an acceleration of $a$ units of acceleration in any given direction, the force which is acting on it at the given instant in the given direction is $ma$ units of force. The force acting on the mass in the *direction of its motion* is called the moving force. The force in the normal to the direction of its motion and towards the centre of curvature is called the centripetal force. An equal and opposite force is called the centrifugal force. The $m$th part of the moving force is called the accelerating force, which is the moving force acting on a unit of mass.

In the case of a planet's orbit it is too common to give the name centrifugal force to two forces which generally differ both in magnitude and in direction, one of them being in the direction of the normal, the other in the direction of the radius-vector. This is the last instance which I shall give of sins against definiteness and accuracy. E. G.

Bardsea

### Hartlaub's "Birds of Madagascar"

THE excellent review, exhibiting traces of a master's hand, of the above-named useful work, which appeared in NATURE (vol. xvi. p. 498) prompts me to offer some remarks on the ornithology of Madagascar and its neighbouring islands, and to take exception on two points therein laid down.

The first of these is propounded by your reviewer and seems to me absolutely contrary to fact. He says:—"Compared with Madagascar itself the appendent island groups are poor in species, although in every case there are many interesting forms among their winged inhabitants. The Comoro Islands muster only some forty-four species of birds, Mauritius about sixty, of which fifteen or sixteen have been introduced by man's agency, and Bourbon about the same number, while Rodriguez appears to have only about twenty-five species now existing in it, of which four or five are certainly recent introductions."

Now twenty years ago my friend, Mr. Sclater, in that remarkable paper of his on the geographical distribution of birds (*Journ. Linn. Soc.* Zoology, ii. p. 130), which so happily laid the true foundation for our present researches into the subject, showed that the proper mode of comparing the wealth or poverty of one fauna with another was to state the proportion which the number of species composing it bears to the area over which they range. The same view was adopted very shortly after by Mr. Wallace, who took occasion (*Ibis*, 1859, p. 449) to question certain of Mr. Sclater's results, and its correctness seems to have been since generally admitted. Yet, applying this test to Madagascar and its neighbouring islands, we find a state of things to exist very different from that which your reviewer has alleged. The area of Madagascar is said[1] to be 10,751 German square miles, that of the Comoros collectively 38·57, of Mauritius 34·76, of Bourbon 42·05, and of Rodriguez 5. It will be sufficient for my purpose to compare the first and last of these. Your reviewer is willing to allow twenty indigenous species to Rodriguez; then—

| Area of Rodriguez. | | Area of Madagascar. | | Species in Rodriguez. | | Species in Madagascar. |
|---|---|---|---|---|---|---|
| 5 | : | 10,751 | : : | 20 | : | $x$ |

$$x = \frac{10,751 \times 20}{5} = 43,004.$$

But instead of an avifauna of 43,004 species, or about four times the number known to exist throughout the whole world, Dr. Hartlaub gives it 218, and your reviewer generously adds two more, making 220! Suppose (an extravagant supposition) that future explorations enable us to double the last number, it is Madagascar that will still be out of all proportion "poor in species" compared with "the appendent island groups," and not these with Madagascar.

The next point to which I must demur is that "the individuality of the fauna of Madagascar is so unique that even that of New Zealand can hardly be compared with it." I will leave to fitter hands than mine to show that this is not the case generally, and shall only remark here that it is not so with birds. Of the sub-class *Ratitæ* there have been until lately *five* strongly-marked groups, each of which is equivalent to an "order" among the *Carinatæ*. Now *two* of these groups were peculiar to New Zealand, and one (*Apterygidæ*) is so now, while the other (containing the families *Dinornithidæ* and *Palapterygidæ*) is but recently extinct. Willingly granting that *Æpyornis*, when we

[1] Behm und Wagner, "Areal und Bevölkerung der Erde" (Petermann's *Geogr. Mittheilungen*, Ergänzungsheft, November 20, 1876).

know more about it, may prove to form a sixth group, the balance of "individuality," if I understand the meaning of the word, will still be on the side of New Zealand. Turning to the Carinate birds, *Harpagornis* stands alone, while *Cnemiornis* will certainly count for as much as the *Dididæ*. The extraordinary Mascarene Rails (*Miserythrus* and *Aphanapteryx*) are well represented by *Ocydromus*, which so much resembles them, and *Strigops* is undoubtedly a more abnormal form than, so far as we can judge, either *Lophopsittacus* or *Necropsittacus*; just as *Nestor* is more aberrant than *Coracopsis*, and *Heterolocha* than either *Fregilupus* or *Necropsar*. But there is no need to continue the list, and in conclusion I will only declare that I think far too highly of the fauna of Madagascar and of the Mascarene Islands to wish that its extraordinary peculiarities should be undervalued, though I do not want them to be unduly magnified at the expense of those of the fauna of New Zealand.

ALFRED NEWTON

Magdalene College, Cambridge, October 27

### Eucalyptus

HAVING read with great interest the article in your journal (vol. xvi. p. 443) on the *Eucalyptus* I take the liberty of sending you a pamphlet on the same subject, in which I have endeavoured to unite all the arguments likely to persuade and convince the Italians of the immense utility of the above-named tree, the cultivation of which would be of the greatest importance for the *Agro Romano*.

As is well remarked in the article in NATURE, the *Eucalyptus* is extensively cultivated in France, Spain, and Portugal. But in Italy, where it prospers almost all over the country and might be cultivated with facility, in spite of the most earnest efforts on my part during my residence here for the last ten years, in spite of its being recommended in Parliament by one of the most influential members, it has not been adopted.

In my gardens on the Lake Maggiore, I cultivate forty different varieties of the *Eucalyptus*. Of these the *amygdalina* and the *globulus* have attained, in eight years, the height of 17 metres. It is to be remembered that the temperature has sometimes been as low as 7° C. below zero without injury to these plants.

If you consider it probable that these few words could be of interest to your readers I willing authorise you to publish them in your estimable journal. PRINCE PIERRE TROUBITZKOY

Villa Troubitzkoy, near Intra, Lago Maggiore, October 15

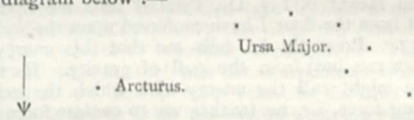
THESE trees are now attracting so much attention that even the small amount of experience I may be able to offer may not be unacceptable to your readers. Considerable stress is laid upon their influence in dissipating malaria; but I have not found this to be the case in Queensland, one of the head-quarters of the tree. I have personally suffered from malaria in the very heart of a forest extending for many miles in every direction, and composed mainly of all the varieties of *Eucalyptus*, and not by any means remarkable for the extent of swampy ground, and have known many instances of febrile attacks among shepherds and stockmen in the locality. Moreover I was told on inquiry that these attacks were not confined to any particular year, but that every year some cases might be expected. I was greatly surprised at reading in your "Notes" (NATURE, vol. xvi. p. 557) that the mosquitoes had disappeared with the introduction of the "gum" trees into Algeria. This would not be the experience of any one who has lived in Australia, I believe. I have found these pests so intolerable on high land, where almost the only tree to be found was one variety or other of *Eucalyptus*, and sometimes all, that sleep was impossible while camping out at night, and life a burden in the day by reason of these pests. The gums emit a most decided odour, especially in strong sunlight. When riding across the great Queensland plains and approaching wooded spurs I have (*Scotticè*) "felt" the characteristic smell of the gums at a considerable distance. These plains—ten miles in breadth—are not crossed in a short time, and the resinous odour of the gums, omnipresent in the forest and scarcely noticed there, strikes one forcibly when approaching the trees after the olfactory organs have been for some time deprived of it. Whether this odour has any effect or whether it is *the* preservative against malaria, I do not know. The growth of these trees in South America is very rapid. When in the Banda Oriental some years ago I examined a plantation of red and blue gums, then eight years old. The trees were at least forty feet high, and many of them measured thirty-six inches in circumference at three feet from the ground. They had a profusion of foliage such as I have never seen on the same trees in Australia. This was right out on "pampa" land, in deep alluvial soil. These trees had fought their way up, in spite of the black ants so destructive to foliage—the owner told me that they had at first stripped the young trees—and the tremendous gales which sweep over this open country. Those to the westward and south-westward of the plantation were far inferior in size to those on the east and north. This was the only grove of *Eucalypti* in the Banda, and it demonstrates the possibility of covering the naked pampas to any extent with forest. English settlers in the River Plate countries should note this fact, and I am sure the enlightened owner of the Estancia "Sherenden" would supply any of his countrymen with seed.

ARTHUR NICOLS

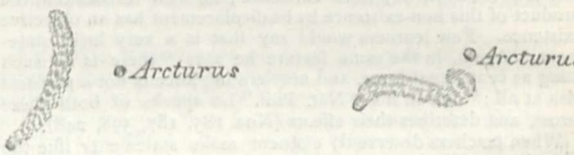
### Meteor of October 19, 6.15 P.M.

THE large meteor described by two correspondents (NATURE, vol. xvi. p. 551) was observed also by several persons in this district, but most of the accounts are so meagre and doubtful as to possess little scientific value. The meteor appears, however, to have been well seen by Mr. W. Watkins Old, of The Parade, Monmouth, and his notes are so interesting that I beg to transcribe them. He says:—

"The meteor fell at 6.15 exactly. It appeared to me to descend perpendicularly some degrees from and to the west of Arcturus (which was shining brightly), and it disappeared behind a bank of dark cloud above the horizon at a point in a line projected beyond Arcturus, half the distance between that star and the last of those in the tail of Ursa Major, as roughly shown in the diagram below:—




Thus it remained stationary, like a dazzling white wand, while I counted twenty, during which time I could perceive the vapour, of which the trail was composed, as it were in ebullition. It then gradually curved towards the north as depicted in the following sketch; and drifted slowly away during *eight* minutes,




until it lay almost horizontal though still brightly illuminated, while the clouds gradually rose and covered it from my view. Altogether I observed it over eight minutes by my watch. There was much twilight in the west and the moon was shining brightly from which one may judge the extreme brilliancy of the meteor. I should add that when it appeared there was simultaneously a sensible rent or flip, like one sometimes hears with a sharp flash of lightning, and which may possibly be due to the appulse of light, as it could scarcely be the sound of explosion if there was any. It was too simultaneous to be the report of the descent of the meteor through the air, but it was sufficiently loud to be pronounced and caused some people standing near me, with their backs to the west, to inquire what it was, though they evidently saw nothing of the meteor nor even turned towards its direction. I listened but heard no further sound."

Ashleydown, Bristol, October 26 W. F. DENNING

### Curious Phenomenon during the Late Gale

YOUR correspondent, "G. A. M." (vol. xvi. p. 551), may be interested to know that the "ball of fire" he saw descend on the evening of the 14th inst. was seen here by me, and by those who accompanied me, at precisely the same time (6.50 P.M.) that he mentions. We were walking in a south-easterly direction, and it seemed to fall from about half-way between that point of the compass and the moon, which was due south of us, and shining brightly. The ball itself appeared to us luminous white, while the "wake" left in its passage through the air, was bluish green. It was visible, I should say, for twenty seconds.

Occurring, as it did, at a time when thousands were wending their way to church, it must have been very generally observed.

Harrow, October 26 A. W. B. J.

## Singing Mice

WHEN at school a friend and I used to keep tame mice, and amongst our large stock was one of the so-called singing mice. The mouse in question was not one we bred ourselves, but was bought from a London dealer, so we had no opportunities of knowing whether it had ever been kept near a singing bird or not; but it was not at all averse to performing in broad daylight, and would chirp whilst a knot of boys were standing round it as freely as when the cupboard was closed.

As M. Brierre describes it (vol. xvi. p. 558), the mouse used to sit with its snout more or less elevated, but not at all to an uncomfortable height, and its throat used to throb like that of a bird whilst singing, the fur of the one being ruffled like the feathers of the other; and the song was something between that of a wren and that of a shrew mouse, and rather pleasing than otherwise.

At first we were inclined to attribute the noise to disease of the lungs or throat, but were unable to hold that opinion long, as there never seemed to be any pain or gasping connected with it, but the noise was always produced at periods of greatest rest, and chiefly when the mouse came out of its sleeping place to wash its face and paws, at which times it generally chirped at intervals. It never had the power of imparting the art to others, nor did any of its numerous progeny inherit its powers. Neither was it all short lived, but rather the contrary, and its death was caused by an accident. We were unable to consider the power of emitting the sounds at all the result of weakness or disease. HENRY H. SLATER

## Sound-Producing Arthropods

I HAVE read with much interest the brief abstract given in NATURE (vol. xvi. p. 567) of Mr. Wood Mason's announcement to the Entomological Society of the discovery of stridulating organs in association with scorpions; reference being made at the same time to his recognition of similar sound-producing structures among other Arthropoda, including certain Crustacea. In this latter case no mention is made of the particular types with which these sound-organs have been observed, and I therefore hazard the relation of an instance that has recently fallen under my own observation with the chance of its proving a newly-recorded example.

The crustacean in question, which I have ascertained to possess sound-producing properties to an eminent degree, is a species of *Spheroma*, belonging to the Isopodous order of the class. I have not as yet ascertained the exact method in which sound is produced nor whether the animal has organs specially adapted for the purpose; on numerous occasions, however, my attention has been attracted to the glass jar of which, with the exception of microscopic Copepods and Protozoa, a single specimen of the species is the sole animal occupant, by a little sharp tapping sound produced three or four times consecutively with intervals of about one second's duration, and which I can almost exactly imitate by gently striking the side of the jar with the pointed end of a pipette. On being approached the little creature always endeavours to elude notice by passing to the opposite side of the stalk of seaweed, upon which it usually reposes in the same way that a squirrel dodges round the branch of a tree, and on no occasion so far have I been able to catch the little fellow *flagrante delicto*, or in the act of producing the sound which it most undoubtedly emits. The character and intensity of the sound produced associated with the small size of the animal, scarcely one quarter of an inch in length, induces me to believe that it is caused by the sudden flexion and extension of the creature's body. A more prolonged observation will no doubt clear up this point, but Mr. Wood Mason may possibly be in a position to throw further light upon the subject by means of the evidence he has collected in reference to other crustacean types.

Among the higher Decapodous crustacean order one species, *Alpheus ruber*, frequently collected by me in Guernsey, produces a snapping noise beneath the water by the sudden extension of the terminal joint of its larger claw that can be heard at a considerable distance, and that at once betrays its lurking place to a practised ear. The large sea crayfish (*Palinurus quadricornis*) again, often emits when handled what may be fitly described as a shrill squeaking sound by the rubbing together of the spinous abdominal segments. It would seem indeed that a closer study of the life habits of the aquatic Arthropoda is likely to reveal among its members as infinite a variety of sound-producers as has hitherto been determined to exist among their more familiar terrestrial congeners. W. SAVILLE KENT

St. Heliers, Jersey, October 27

## Insects and Flowers

IN reference to the question whether insects are most attracted to flowers by scent or colour, may I mention that while staying at the hotel at Cettinge lately I was amused by the behaviour of some humming-bird sphinx moths. My room was roughly stencilled with a "spotty" pattern of purplish brown on the dull white plaster. Every morning these moths, with their probosces extended, used to attack the dabs of colour, hovering before them, just as though they were real flowers, but starting back with apparent amazement on finding that they were not. This seems the more remarkable because the wonderfully abundant aromatic herbs of that region, which must have supplied their usual food, have all, so far as I know, very inconspicuous flowers.

Notting Hill, October 27 A. J. H.

## *FRANCIS VON ROSTHORN*

FRANCIS VON ROSTHORN, who died June 17, 1877, was the son of Matthew Rosthorn, of Lancashire, who went to Vienna in 1765, at the invitation of the Empress Maria Theresa, to establish the manufacture of metal buttons. He constructed the first rolling-mills in Austria; one at Vienna, another (in 1792) at Fahrafeld, in Lower Austria. Matthew von Rosthorn was ennobled by the Emperor Joseph II. in 1790, and died at Vienna January 3, 1805, leaving five sons. The youngest of these, born April 18, 1796, at Vienna, is the subject of this notice. These five brothers joined in creating extensive metallurgic establishments; the first (1817) at Oed; and another (1823) in Carinthia, for smelting zinc (then high in price) out of the Raibl and Bleyberg ores, by means of brown coal. Having purchased (1826) the state demesne of Wolfsberg, in Carinthia, with extensive metallurgical works, they constructed there a large rolling-mill, together with a puddling furnace. Francis von Rosthorn, having prepared himself for his practical career by attending the Mining Academy of Schemnitz, in Hungary (1814 to 1818), soon became acquainted with several eminent geologists, and obtained the patronage of the late Archduke John. He made several scientific tours in Carinthia, Carniolia, Styria, Salzburg, and the Hungarian border; in 1827 with Prof. Keferstein, in 1828 with Archduke John, in 1829 with Escher and Schrötter, and in 1832 with Dr. Boué. His annual visits to Archduke John at Gastein (1829 to 1836) were always connected with Alpine exploration. His later travels (1842 to 1847) were chiefly southward. In 1832 he communicated the results thus obtained to the Meeting of German Naturalists at Vienna; and in 1836 to the meeting at Freiburg. In 1848 he was elected into the Legislative Assembly ("Landtag") of Carinthia; and from 1852 to 1870 held the office of President of the Commercial and Industrial Board of that province. Francis von Rosthorn's constitution was exceptionally robust, so that up to his seventy-sixth year he was able to undertake arduous Alpine ascents. His conversation with persons of any social station was unaffectedly amiable; but he could be sarcastic when he met with affectation or baseless pretensions.

[1] Obituary Notice by Prof. E. Suess ("Report of the Imperial Geological Institute, Vienna," August 31, 1877).

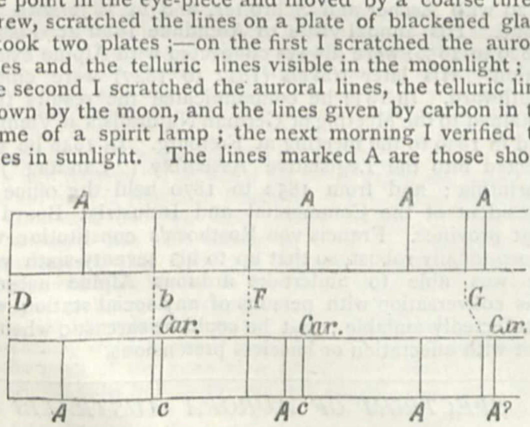
## *SPECTRUM OF AURORA AUSTRALIS*

AS I believe no account of spectroscopic observations of the Aurora Australis have as yet been published, I venture to send this description of two auroræ observed during the stay of H.M.S. *Challenger* in high southern latitudes. The opportunities of observing were not frequent, either from the rarity of the phenomena (which is very possible) or because the dense mass of cloud which is the prevailing feature of those regions prevented their being seen except when exceptionally bright.

Altogether four appearances were noted. The first was 1.30 on the morning of February 9, 1874, in lat. 57° S. and long. 75° E., bar. 29·0 in., ther. 35°. There were brilliant streaks to the westward; no spectroscopic observations were taken. The second was on February 21 at 9.30 P.M., lat. 64° S., long. 89° E., bar. 28·8, ther. 31°; one bright white curved streamer extended from Jupiter, which appeared to be near the focus, through Orion and about as far beyond. Under this was what appeared to be a black cloud, but the stars were visible through it. Real cumulus clouds hid great part of the remainder, but there were two vertical flashing rays that moved slowly to the right (west), generally the aurora was still and bright.

On examining the streamer with the spectroscope I found the usual three prominent lines, namely, one yellow-green, one green, the third blue or purple. I looked for the red line but could not find it.

The third aurora was seen on March 3, lat. 53° 30′ S., long. 109° E., bar. 29·1, ther. 36°, after some days wet and stormy weather. Soon after 8 P.M. the sky began to clear and the moon shone out. Noticing the light to the southward to be particularly bright I applied the spectroscope and found the distinguishing auroral line. About midnight I was called as there were very brilliant auroral clouds. The sky was almost clear, but south were two or three brilliant light clouds, colour very white yellow, shape cumulus stratus; from about west to near south extended a long feathery light of the same colour, parallel with the horizon, and between south and west there appeared occasionally brilliant small clouds, the upper edges seemed hairy, and gave one the idea of a bright light behind a cloud. The forms changed, but I did not notice any particular order, perhaps because my attention was particularly directed to examining the light with the spectroscope, and the great cold, for my fingers seemed almost frozen, and the motion of the ship made my task rather difficult. I could trace four lines, three bright, and one rather faint, and by reference to the moon, which was shining brightly, roughly determined their places. They must have been exceedingly bright to show so plainly in full moon. The spectroscope used was one of Grubb's single prism with long collimator. A needle point in the eye-piece marked the position of the lines, and a corresponding needle point carried on a frame with the point in the eye-piece and moved by a coarse thread screw, scratched the lines on a plate of blackened glass. I took two plates;—on the first I scratched the auroral lines and the telluric lines visible in the moonlight; on the second I scratched the auroral lines, the telluric lines shown by the moon, and the lines given by carbon in the flame of a spirit lamp; the next morning I verified the lines in sunlight. The lines marked A are those shown




by the aurora, those marked D, *b*, F, and G are the telluric lines, and those marked *car.* were given by the carbon in the spirit lamp.

The spectrum has been magnified five times from the plates. I cannot account for the different position of the auroral lines in the two plates, as the prism was not moved during the observations that I am aware of.

The fourth aurora was a slight one seen to the southward on March 6 at 8 P.M. It would be worth investigating whether the low barometer has anything to do with the absence of red in the spectrum, the normal state of the barometer is an inch lower in those regions than in more temperate latitudes.

I may as well add that on February 9 the aurora was preceded by a watery sunset, and the day broke afterwards with high cirrus clouds and clear horizon. On February 21 the aurora preceded a fine morning, cumulus stratus clouds. On March 3 there was a brilliant sunset followed by a fine morning; and on March 6, after the slight appearance of aurora, the clouds changed to high cirrus. J. P. MACLEAR

## *ABSOLUTE PITCH*

AT the present time the question of absolute pitch is attracting attention in consequence of the discrepancy between König's scale and the numbers determined by Appunn's tonometer. This instrument is founded upon the same idea as Scheibler's fork tonometer, and consists of a series of sixty-five harmonium reeds, bridging over an entire octave, and so tuned that each reed gives with its immediate neighbours four beats per second. The application to determine absolute pitch, however, does not require precision of tuning, all that is necessary being to count with sufficient accuracy the number of beats per second between each pair of consecutive reeds. The sum of all these numbers gives the difference of frequencies of vibration between the first reed and its octave, which is, of course, the same as the frequency of the first reed itself.

The whole question of musical pitch has recently been discussed with great care by Mr. Ellis, in a paper read before the Society of Arts (May 23, 1877). He finds by original observation with Appunn's instrument 258·4 as the actual frequency of a König's 256 fork, and Prof. Preyer, of Jena, has arrived at a similar result (258·2). On the other hand, Prof. Mayer in America, and Prof. Macleod in this country, using other methods, have obtained numbers not differing materially from König's. The discrepancy is so considerable that it cannot well be attributed to casual errors of experiment; it seems rather to point to some defect in principle in the method employed. Now it appears to me that there is such a theoretical defect in the reed tonometer, arising from a sensible mutual action of the reeds. The use of the instrument to determine absolute frequencies assumes that the pitch of each reed is the same, whether it be sounding with the reed above, or with the reed below; and the results arrived at would be vitiated by any mutual influence. In consequence of the ill-understood operation of the wind, it is difficult to predict the character of the mutual influence with certainty; but ("Theory of Sound," §§ 112-115) there is reason to think that the sounds would repel one another, so that the frequency of the beats heard when both reeds are sounding, *exceeds* the difference of the frequencies of the reeds when sounding singly. However this may be, in view of the proximity of consecutive reeds and of the near approach to unison,[1] the assumption of complete independence could only be justified by actual observation, and this would be a matter of some delicacy. If the mutual influence be uniform over the octave it would require a difference of one beat per minute only to reconcile König's and Appunn's numbers.

As to the amount of the influence I am not in a position to speak with confidence, but I may mention an obser-

[1] It must not be forgotten that the vibration of the tongue involves a transference of the centre of inertia, so that there is a direct tendency to set the sounding-board into motion.

vation which seems to prove that it cannot be left out of account. If two sounds of nearly the same pitch are going on together, slow beats are heard as the result of the superposition of vibrations. Suppose now that a third sound supervenes whose pitch is such that it gives rapid beats with the other two. It is evident that these rapid beats will be subject to a cycle of changes whose frequency is the same as that of the slow beat of the first two sounds. For example, in the case of equal intensities of two sounds there is a moment of silence due to the superposition, of equal and opposite vibrations, and at this moment a third sound would be heard alone and could not give rise to beats. The experiment may be made with tuning-forks, and the period of the cycle will be found to be sensibly the same whether it be determined from the slow beat of the two forks nearly in unison or from the rattle caused by the simultaneous sounding of a third fork giving from four to ten beats per second with the other two. In the case of forks there is no fear of sensible mutual action, but if it were possible for the third sound to affect the pitch of one of the others the equality of the periods would be disturbed. The observation on Appunn's instrument was as follows:— The reeds numbered 0 and 64 being adjusted to an exact octave, it was found that the beats arising from the simultaneous sounding of reeds 0, 63, and 64 were by no means steady, but passed through a cycle of changes in a period no greater than about five seconds. In order to work with greater certainty a resonator of pitch corresponding to reed 64 was connected with the ear by a flexible tube and adjusted to such a position that the beats between reeds 0 and 64 (when put slightly out of tune) were as distinct as possible, indicating that the gravest tone of reed 64 and the octave over-tone of reed 0 were of equal intensity. By *flattening* reed 64 (which can be done very readily by partially cutting off the wind) the beats of the three sounds could be made nearly steady, and then when reed 63 was put out of operation, beats having a 5 seconds' period were heard, indicating that reeds 0 and 64 were in tune no longer. It would appear, therefore, that when reed 63 sounds the pitch of reed 64 is raised, but in interpreting the experiment a difficulty arises from the amount of the disturbance being much in excess of what would be expected from the performance of the instrument when tested in other ways.[1]

I come now to an independent determination of absolute pitch, which it is the principal object of the present communication to describe. The method employed may be regarded as new, and it appears to be capable of giving excellent results.

The standard fork, whose frequency was to be measured, is one of König's, and is supposed to execute 128 complete vibrations in a second. When placed on its stand (which does not include a resonance box) and excited by a violin bow, it vibrates for a minute with intensity sufficient for the counting of beats. The problem is to compare the frequency of this fork with that of the pendulum of a clock keeping good time. In my experiments two clocks were employed, of which one had a pendulum making about $1\frac{1}{2}$ complete vibrations per second, and the other a so-called seconds' pendulum, making half a vibration per second. Contrary to expectation, the slower pendulum was found the more convenient in use, and the numerical results about to be given refer to it alone. The rate of the clock at the time of the experiments was determined by comparison with a watch that was keeping good time, but the difference was found to be too small to be worth considering. In what follows it will be supposed for the sake of simplicity of explanation that the vibrations of the pendulum really occupied two seconds of time exactly.

The remainder of the apparatus consists of an electrically maintained fork interrupter, with adjustable weights, making about $12\frac{1}{2}$ vibrations per second, and a dependent fork, whose frequency is about 125. The current from a Grove cell is rendered intermittent by the interrupter, and, as in Helmholtz's vowel experiments, excites the vibrations of the second fork, whose period is as nearly as possible an exact submultiple of its own. When the apparatus is in steady operation, the sound emitted from a resonator associated with the higher fork has a frequency which is determined by that of the interrupter, and not by that of the higher fork itself; nevertheless, an accurate tuning is necessary in order to obtain vibrations of sufficient *intensity*.[1] By counting the beats during a minute of time it is easy to compare the higher fork and the standard with the necessary accuracy, and all that remains is to compare the frequencies of the interrupter and of the pendulum. For this purpose the prongs of the interrupter are provided with small plates of tin so arranged as to afford an intermittent view of a small silvered bead carried by the pendulum and suitably illuminated. Under the actual circumstances of the experiment the bright point of light is visible in general in twenty-five positions, which would remain fixed, if the frequency of the interrupter were exactly twenty-five times that of the pendulum. In accordance, however, with a well-known principle, these twenty-five positions are not easily observed when the pendulum is simply looked at; for the motion then appears to be continuous. The difficulty thence arising is readily evaded by the interposition of a somewhat narrow vertical slit, through which only one of the twenty-five positions is visible. In practice it is not necessary to adjust the slit to any particular position, since a slight departure from exactness in the ratio of frequencies brings all the visible positions into the field of view in turn.

In making an experiment the interrupter is tuned, at first by sliding the weights and afterwards by soft wax, until the interval between successive appearances of the bright spot is sufficiently long to be conveniently observed. With a slow pendulum there is no difficulty in distinguishing in which direction the pendulum is vibrating at the moment when the spot appears on the slit, and it is best to attend only to those appearances which correspond to one direction of the pendulum's motion. This will be best understood by considering the case of a conical pendulum whose motion, really circular, appears to be rectilinear to an eye situated in the plane of motion. The restriction just spoken of then amounts to supposing the hinder half of the circular path to be invisible. On this understanding the interval between successive appearances is the time required by the fork to gain or lose one complete vibration as compared with the pendulum. Whether the difference is a loss or a gain is easily determined in any particular case by observing whether the apparent motion of the spot across the slit (which should have a visible breadth) is in the same or in the opposite direction to that of the pendulum's motion.

In my experiment the interrupter *gained* one vibration on the clock in about eighty seconds, so that the frequency of the fork was a thousandth part greater than 12·5 or 12·51. The dependent fork gave the ninth harmonic, with a frequency of 125·1. The beats between this fork and the standard (whose pitch was the higher) were 180 in sixty seconds, so that the frequency of the standard was as nearly as possible 128·1, agreeing very closely with König's

[1] The value of my instrument has been greatly enhanced by the valuable assistance of Mr. Ellis, who was good enough to count the entire series of beats, and to compare the pitch with that of the tuning-forks employed by him in previous investigations. Mr. Ellis, however, is not responsible for the facts and opinions here expressed. It may be worth mentioning that the steadiness or unsteadiness of the beats heard when three consecutive reeds are sounding simultaneously is a convenient test of the equality of the consecutive intervals. The frequency of the cycle of the four a second beats is equal to the difference of the frequencies of either of the actual extreme notes and that which, in conjunction with the other two, would make the intervals exactly equal.

[1] This tuning is effected by prolonging as much as possible the period of the beat heard when the dependent fork starts from rest. This beat may be regarded as due to an interference of the forced and natural notes.

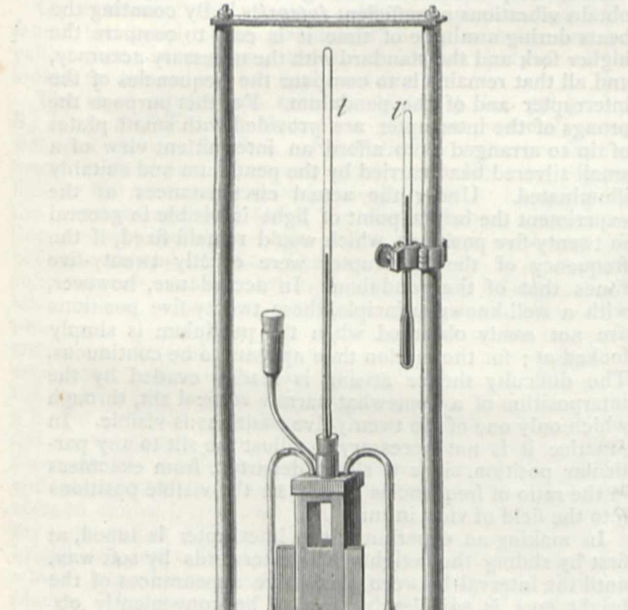
scale. The error of the determination may amount to ·1, but could not, I think, exceed ·2.

I ought to add that the *approximate* determination of the frequency of the interrupter must be made independently, as the observation on the pendulum does not decide *which* multiple of $\frac{1}{2}$ nearly coincides with the frequency of the fork. Also the relation between the two auxiliary forks was *assumed*, and not determined; but as to this there can be no doubt, unless it be supposed that König's scale may be in error to the extent of a whole tone.

RAYLEIGH

## *A NEW CONDENSING HYGROMETER*

A NEW apparatus of this kind, invented by M. Alluard, and described by him in *La Nature*, is distinguished from all those hitherto employed by the two



Alluard's Condensing Hygrometer.

following points:—(1) The part on which the deposit of dew is to be observed is a plane well-polished face A, of silver or gilt brass; (2) This plane face is set in a plate of silver or brass V, itself gilt and polished, which does not touch it, and which, never being cooled, always preserves its brightness. It results from this arrangement that the deposit of dew is observed with the greatest facility, in such a manner that there is scarcely any difference between the temperatures of the instants when the dew commences and ceases to appear on the instrument properly cooled by the evaporation of ether.

The form of the instrument is that of an upright prism with square base. Its height is eight centimetres and the side of its base eighteen millimetres. Three small copper tubes pass through the upper lid; the first reaches the bottom, and the two others, one surmounted by a funnel for introducing the ether, open only above. Two small windows enable us to judge of the agitation of the ether by the aspiration or driving back of the air intended to produce coolness by the evaporation of the volatile liquid; it is best to work with an aspirator, the aspiration of which we can regulate as we wish. A central tube permits the introduction of a thermometer, *t*, which, placed in the middle of the evaporating liquid, gives the temperature at which the deposit of dew occurs. A small sling thermometer, fixed on the side of a brass support, enables us to determine with precision the temperature of the air whose hygrometric condition we wish to ascertain.

Daniell's condensing hygrometer was formerly modified by M. V. Regnault. He made it an instrument of precision; but his apparatus has not been much used on account of its delicate construction. The deposit of dew, being made on a cylinder of polished silver, is difficult to observe. In the plane face hygrometer of M. Alluard this deposit is very easily seen by contrast, even at some metres distance, especially if care is taken to observe in such a manner as to avoid all reflection from the gilt faces, when they will appear a beautiful ebony black. Its employment being very simple, without losing anything of its precision, there is no reason why it should not come into general use.

Since meteorological observations have multiplied on all sides, the hygrometer has assumed an importance which it had not before. The psychrometer is at present almost exclusively employed. But all physicists know that below zero we cannot trust the results which it gives; it is the same when the air is much disturbed. And yet, almost everywhere, it continues to be employed on these conditions. We hope that the plane face hygrometer, furnished during the winter cold with an aspirator filled with glycerine, will be able to yield accurate results to all who do not fear to devote a few minutes to its working.

## *OUR ASTRONOMICAL COLUMN*

EARLY OBSERVATIONS OF THE SOLAR CORONA.—Referring to Mr. Dreyer's letter in NATURE (vol. xvi. p. 549), the note in this column relating to the solar eclipse of 1605 was by no means intended to imply that it afforded one of the earliest observations of the corona, nor can the eclipse of Stiklastad, as it has been usually called, on August 31, 1030, be so characterised. Prof. Julius Schmidt, of Athens, had called attention in 1870 to a record of the eclipse of December 22, 968, in Corfu, where he found a reference to the corona, but a much earlier date is assigned by Prof. Grant for the first mention of this phenomenon. It occurs in Philostratus' "Life of Apollonius of Tyana," Book VIII., chap. xxiii., in the Leipzic edition, and runs thus:—*Περὶ δὲ τὸν χρόνον, ὃν ἐν τῇ Ἑλλάδι ἐνεσπούδαζεν, ἐπεῖχε τὸν οὐρανὸν διοσημία τοιαύτη. τὸν τοῦ Ἡλίου κύκλον περιελθῶν στέφανος, ἐοικως Ἴριδι, τὴν ἀκτῖνα ἠμαυροῦ.* Prof. Grant considers that "the words here quoted refer beyond all doubt to a total eclipse of the sun, and that the phenomenon seen encompassing the sun's disc was really as well as verbally, identical with the modern corona." He also points out that Plutarch, who was contemporary with Apollonius, refers to a total eclipse of the sun which had *recently* occurred, and remarks of total solar eclipses in general that "a certain effulgence is seen round the circumference," so that although the sun may be wholly covered by the moon "still the eclipse is deficient in duration as well as in amplitude," this surrounding effulgence not allowing of a very intense shadow. These remarks of Philostratus and Plutarch Prof. Grant thinks may probably apply to the same eclipse, and afford "the earliest allusions to the corona recorded in history." Several attempts have been made to discover the date of the phenomenon, but so far as we know without success.

The earliest distinct and more accurate account of the corona is that given by MM. Plantade and Capiés, who observed at Montpellier on the occasion of the eclipse of May 12, 1706.

THE OUTER SATELLITE OF MARS.—Our ephemeris of the satellite of Mars is here continued ten days further from the elements employed last week, though much greater difficulty must now attend observations than when the discovery was first announced. In the middle of August the distance of the planet from the earth was less than 0·4; on November 12 it will have increased to 0·68. At the next opposition in 1879, the least distance of Mars will be 0·482, at a north declination of upwards of 18°, so that observations may be made at many observatories in this hemisphere, probably without greater difficulty than about the late opposition; at the following one in December, 1881, the planet will attain a declination of 27° N., but its distance from the earth will be at no time less than 0·602.

Prof. Asaph Hall's complete discussion of the observations of the satellites of Mars, made in the present year, will be looked for with much interest; it is only fitting that this investigation should be left in the hands of their discoverer, who has made the year 1877 a very notable epoch in the history of practical astronomy.

The following positions of the outer satellite are for 8h. G.M.T.

| | | | | | |
|---|---|---|---|---|---|
| Nov. 3 ... | Pos. 358° ... | Dist. 21″ | Nov. 8 ... | Pos. 27 ... | Dist. 25″ |
| ,, 4 ... | ,, 69 ... | ,, 52 | ,, 9 ... | ,, 75 ... | ,, 48 |
| ,, 5 ... | ,, 122 ... | ,, 25 | ,, 10 ... | ,, 150 ... | ,, 20 |
| ,, 6 ... | ,, 236 ... | ,, 42 | ,, 11 ... | ,, 243 ... | ,, 46 |
| ,, 7 ... | ,, 272 ... | ,, 37 | ,, 12 ... | ,, 285 ... | ,, 28 |

DE VICO'S COMET OF SHORT PERIOD.—The year to which we drew attention some time since (1876·9-1877·9) as one which might possibly witness the re-discovery of De Vico's comet of 1844 is drawing to a close without its having been remarked, and the chance of detecting it at this season if the perihelion passage be not already passed, is small. We must therefore probably place the comet in the class which, though undoubtedly moving in elliptical orbits of small dimensions when under observation, are now "lost." Whether in the case of De Vico's comet this arises from a larger error in the determination of the mean motion in 1844 than at present appears admissible, or whether the action of the planet Mars, to which allusion has been made in this column, may explain it, or again, whether the comet has encountered one of the minor planets, and thereby been deflected or disintegrated, cannot be at present ascertained. It was hardly to have been anticipated that the laborious investigations of Prof. Brünnow relating to the motion of this comet from 1844-55 would not have resulted in its re-observation.

## METEOROLOGICAL NOTES

MEAN ATMOSPHERIC PRESSURE OF EUROPE.—A great contribution to this very important subject has been made by Dr. Buys Ballot in the second volume of the "Nederlandsch Meteorologisch Jaarboek voor 1872," which has just been published. The first 130 pages of the volume are occupied with a very careful and in certain directions exhaustive discussion of the barometric observations made at about 110 places situated in different parts of Europe from 1774 to 1874. The method of discussion is identical with that adopted by Dr. Buys Ballot in his recently published paper on the Meteorology of Holland (NATURE, vol. xvi. p. 89). This method consists in accepting as the normal mean atmospheric pressure at Greenwich, Vienna, and Palermo, the arithmetic means of the observations made at these places which embrace periods of 100, 101, and 84 years respectively. The normal values for the other stations have been determined by the process of differentiation, that is, by a comparison of the means of all the observations made at the place with the corresponding means of one or more places at the nearest available stations whose normals have been already determined, and thereafter applying the necessary correction. Thus the normals which have been arrived at in this very laborious manner are substantially the averages which would have been obtained if the observations at each of the stations had been made during precisely the same terms of years. The thirty years' averages should probably have been accepted as the best normals for Stykkisholm in Iceland, instead of correcting these averages from the Greenwich and Christiania observations, seeing that a low average barometer at Stykkisholm is frequently coincident with a high barometer at either or both of these stations, and *vice versâ*. The resulting differences, however, are but slight. This work of Dr. Buys Ballot, particularly when looked at with reference to future discussions, may be said to take a place at once as a classic of meteorology. The next step to be taken in this field of European meteorology is the discussion of all good barometric observations made in Europe during the meteorological lustrum ending with 1875. To the results of this discussion corrections could be applied from Du Buys Ballot's normals, which are sufficiently numerous for the purpose, and thus a graphic representation could be made of the closest possible approximation to the true mean atmospheric pressure of Europe. In this way, by disclosing the striking, and in a large measure still unrecognised, influence of large masses of land and water on the barometric pressure, much light would be thrown on the origin and history of those great atmospheric currents which, flowing or sweeping over this continent, are mainly instrumental in determining the climates of its different regions.

METEOROLOGY OF NEW YORK, U.S.—The "Annual Report of the New York Meteorological Observatory for 1876" gives, in addition to the individual observations made daily, and their monthly and annual averages, a more than usually full statement of rain and wind observations. On pp. 39-88 are given the details of the amount of rain and snow-water which fell each hour from 1870 to 1876, together with the hourly averages of each month for these seven years. These hourly means show maximum amounts during winter, from 11 A.M. to 3 P.M.; during spring, from 9 P.M. to 1 A.M.; during summer, from 5 to 10 P.M.; and during autumn, from 3 to 8 A.M. The irregularity of these periods and the irregular occurrence of secondary maxima indicate that seven years is too short a time for the determination of the hourly curve of the rainfall at New York. There appears, however, a tendency to a double maximum varying considerably with season. Extended observation alone can give this curve. The influence of the daily fluctuation of temperature and of the sea breeze which sets in very decidedly from south-east during the hot months on the rain-curve, can then be studied. During the same seven years the duration of each shower has been noted in the number of minutes, the average result of which is that the minimum time of fall, a small fraction less than two days, occurred in June; from this time it slowly but steadily rose to 3 days

17 hours in January, fell a little in February, and rose to $4\frac{1}{3}$ days, the annual maximum in March, from which it rapidly declined to the minimum in June. On a mean of the past forty-one years the monthly averages are in excess from May to August inclusive, August and May being decidedly the months of maximum rainfall, whilst January and February are the months of least rainfall. From 1836 the annual amounts show with some interruptions a decided increase in the rainfall up to 1868, since which year there has been as decided a decrease. This result is generally corroborated by the rainfall at Washington, Philadelphia, and Providence, which Mr. Draper adds to his Report. A valuable table of the monthly amounts from 1836 to 1876 is printed at p. 6. In accordance with the suggestion thrown out by Mr. Hill (NATURE, vol. xvi. p. 505) the amounts for the winter months have been picked out, averaged for the eleven-years sun-spot period, and bloxamed. The results, thus worked out, are in inches these, beginning with the first year of the cycle :—22·57, 22·26, 22·92, 23·31, 22·24, 21·03, 21·98, 21·05, 21·14, 22·18, and 23·56.

METEOROLOGY IN RUSSIA.—The St. Petersburg Agronomical Society has appointed a special committee for the purpose of elaborating, in accord with other Russian scientific bodies, a scheme for establishing throughout Russia an extensive network of meteorological stations. Owing to the interest manifested in the subject by a great number of agriculturists, it is expected that the plan will soon be put into execution.

## NOTES

WE much regret to have to announce the death, on Sunday last, of Mr. Robert Swinhoe, F.R.S., a naturalist whose numerous contributions to our knowledge of the mammalia and birds of the Chinese Empire have proved invaluable to zoological science. We hope, next week, to give an account of Mr. Swinhoe's work.

THE International Committee for the erection of a monument to Liebig at Munich, having now at their disposal a sum of 120,000 marks, invite sculptors of all nations to send in models for their acceptance. A prize of 2,000 marks will be given to the model which takes the first place, and 1,500 to the second. The model of the statue should be forty centimetres, and of statue and pedestal about one metre in height. Models should be addressed to the "Castellan der königlichen Akademie der Künste, 38, Unter den Linden, Berlin," where they will be received from June 1 to 15, 1878, to be exhibited first at Berlin and then at Munich. The Committee bear all the expenses of transport.

IT has been noted in the French papers *à propos* of the recent colliery explosion, that M. Leverrier, when presiding at the meeting of the French learned societies at Easter, proposed to extend the telegraphic warnings of the International Meteorological System to the several French pits. The question of the illumination of mines by electricity has been revived by these terrible tragedies, and a number of interesting communications connected with that important topic will be presented and fully discussed at the next meeting of the French Academy of Sciences.

IT was stated by one of the speakers at the last quarterly meeting of the French Academies that M. Thiers had written a complete work on Spherical Trigonometry when quite a young man.

WE regret to record the death of M. Cazin, Professor of Physics at one of the Paris Lycées, and an active member of the Paris Physical Society. M. Cazin was sent to the Island of St. Paul by the Academy of Sciences under the command of Capt. Mouchez to make physical observations during the last transit of Venus ; he there contracted the germ of the illness which has proved fatal at the early age of forty years. He had been admitted to the Observatory by M. Leverrier to execute a series of delicate researches on magnetism, which have been left unfinished.

THE Harveian Oration at the Royal College of Physicians of London will be delivered in 1878 by Dr. J. Burdon Sanderson, F.R.S.

AN anthropological exhibition will be opened at Moscow in 1879, in connection with the society of Friends of Natural Science. Many objects of great scientific value, almost exclusively of Russian origin, are already in the hands of the organising committee.

MR. TUCKWELL, recently head-master of Taunton College School, has issued a circular addressed to head-masters, giving an account of his connection with the school whose reputation he did so much to raise, and which has treated him so ungratefully. Our readers are already familiar with the details of this unhappy matter, and we are sure will all wish with us that Mr. Tuckwell may soon find a field for the exercise of his powers as a successful teacher unfettered by the narrowness of uneducated and narrow-minded directors. Mr. Tuckwell gave Taunton School a status and a name ; the Council of the school have undone all his work, and left the school nowhere.

THE winter session of the Chester Society of Natural Science opened on October 25 with a lecture on "The Arctic Regions," by Mr. de Rance, of H.M. Geological Survey. The upper silurian, lower carboniferous sandstones, mountain limestone, and lias of the Parry Archipelago, as well as the oolites, cretaceous and miocene rocks of Greenland and Grinnel Land, were described as occupying hollows in the old Laurentian Mountains, and the existing cold climate was stated to have probably only prevailed since the last glacial epoch. The range of the northern mammals, and the discovery of remains of the Eskimo by Capt. Feilden, R.N., naturalist of the *Alert*, near Cape Beechey, far north of the present limit of human habitation, and further north than any previous discovery of man or his works, were commented on ; and a large collection of Arctic fossils were exhibited by Sir Phillip Egerton, collected in Grinnel Land by his nephew, Lieut. Egerton, R.N., of the late British Arctic Expedition.

AN unusually interesting scientific *soirée* was recently held at the Bristol Museum and Library, which has been characterised as "the headquarters of scientific research in the west of England." Many of the most recent scientific experiments were shown, the most attractive probably being Prof. Graham Bell's exhibition of the wonders of the telephone. During the winter a course of lectures has been arranged for at the museum, mostly scientific, as follows :—November 19—A. R. Wallace, F.R.G.S., F.L.S., the Distribution of Animals as indicating Geographical Changes ; November 29—Prof. Ball, F.R.S., a Night at Lord Rosse's Telescope, illustrated by the Oxy-hydrogen Lantern. December 10—Frederick Wedmore, Rembrandt ; his Life and Work. January 14—Prof. Marshall, M.A., Principal of University College, Bristol, The Economic Condition of America. January 28—Prof. W. C. Williamson, F.R.S., Coal and Coal Plants. February 11—C. T. Hudson, M.A., LL.D., The Larger and Rarer Rotifers ; illustrated with Transparent Diagrams. February 26—Prof. Rowley, M.A., of University College, Bristol, Francis Bacon : his Personal Character and Political Career. March 11—Dr. J. H. Gladstone, F.R.S., Fiery Meteors and Meteoric Stones. March 25—J. Norman Lockyer, F.R.S., Sun Spots in Relation to Indian Famines, with Spectroscopic Experiments and Oxy-hydrogen Lantern Illustrations.

THE Royal Society of New South Wales, originated in 1821 as the Philosophical Society of Australia; after an interval of repose it was revived in 1850 as the Australian Philosophical Society, by which designation it was known until 1856, when the name was again changed to that of the Philosophical Society of New South Wales, and finally, about ten years ago, by the sanction of her Majesty the Queen, it assumed its present title. Judging by its present list of members it would seem to be prospering, but judging from the volume of its *Proceedings* (vol. x. for 1876) its scientific life might be higher, and we would venture to express the hope that future volumes may give us a larger number of memoirs treating of that vast quadrilateral of which Sydney is the acknowledged capital. Of the articles in this volume we would notice the following: On the Deep Oceanic Depression off Moreton Bay, by the Rev. W. B. Clarke, F.R.S.; On some Tertiary Australian Polyzoa, by the Rev. J. E. T. Woods. The species were with one exception derived from the Mount Gambier polyzoan limestone, South Australia, and are all described as new; ten are described and figured as belonging to the genus Eschara, two species of the genus Pustulipora are described, and one Tubulipora. On the formation of Moss Gold and Silver, and on a Fossiliferous Siliceous Deposit from Richmond River, is the title of a paper by Prof. Liversidge. The composition of this deposit shows that it answers to the common siliceous sinters or geyser deposits. The weathered surfaces are usually marked with the remains of ferns which stand out in relief, and more rarely through the mass are to be found the remains of certain fruits and seeds. These latter have been described by Baron Müller as belonging to a plant (*Liversidgea oxyspora*) allied to Capparideæ and Bixaceæ, the fruits are from two-thirds to nearly an inch in diameter, divided into four turgid lobes, placentas parietal; seeds turgid; oval towards one extremity and attenuated at the other; both fern and fruits are figured. In the discussion following the reading of a paper by the Rev. W. B. Clarke, F.R.S., On the Effects of Forest Vegetation on Climate, many interesting statements were made as to the condition of the forests in the neighbourhood of Sydney at the present time, and so long back as forty years ago.

PETERMANN'S *Mittheilungen* for November contains a paper by Dr. G. Radde describing the journey of himself and Dr. Sievers from Erzeroum to the Bin-Gol-Dagh; the paper is full of details concerning the botany of the region traversed. Under the title of "Tekna and Nun," Dr. Rohlfs gives some valuable information on the part of the Sahara about the south-west of Morocco, showing that it is by no means so barren as is generally thought, and that even the most recent maps of the region are unsatisfactory.

ONE of the most interesting papers in the September number of the *Bulletin* of the French Geographical Society is an account by M. Brau de St.-Pol-Lias of his visit to the French Colonial stations recently established on the coast of Sumatra, in the province of Deli. The author gives many interesting observations on the people and the products of the part of the island which he visited, and speaks hopefully of the colony, which he considers an excellent basis for the exploration of the island. In the same number Dr. Harmand gives some account of recent journeys he made in Cambodia.

THE first map showing the whole of Stanley's route from Bogomayo to the mouth of the Congo has been published by *L'Exploration* (October 21). In this map the course of the Congo is roughly shown as indicated in Stanley's letter, and that also of the Ogové according to the explorations of de Brazza, Lenz, and Marche. The trend of the Ogové to the south-east is shown, and its probable junction with the Congo by two arms indicated.

THE Geographical Society of Paris has received letters from the French Consul at Zanzibar informing them that a road is being opened from Zanzibar to Tanganyika, for carting by oxen. It is expected that ere long explorers will be able to dispense with native porters.

A GEOGRAPHICAL paper has been started at Lyons by M. du Mazet, one of the staff of the *Courrier de Lyon*. It will record the transactions of all the provincial geographical societies of France. The Lyons Geographical Society will have the advantage of a number of communications from the Roman Catholic missionaries who have an old-established special seminary and college in that city.

IN the *Times* of Wednesday last week appeared a long story about the discovery of the remains of Columbus in St. Domingo. At Madrid, the *Times* Paris correspondent now states, the story is declared to be a hoax, inasmuch as "a Spanish squadron years ago escorted the remains to Havannah, where they lie in the Cathedral."

UNDER the title of "Pictorial Geography for Young People," Messrs. Griffith and Farran have published a neat little map intended to exhibit graphically the significance of the various terms used in geography—continent, island, river, lake, mountain, volcano, city, &c. It is necessarily exaggerated, but in the hands of a judicious teacher might be a valuable and attractive help to the teaching of the elements of geography.

TWO severe shocks of earthquake were experienced at Lisbon at 6.45 A.M. of October 25. No damage was reported.

UNDER date October 17, it is reported from Smyrna, in Asia Minor, that there had been, during a few days, several earthquake shocks doing no further harm but cracking some walls.

IT has been affirmed by P. Secchi of Rome, that iron heated red is transparent to light. This is denied by M. Govi of Turin, who, in a paper to the French Academy, describes some experiments on the subject, and shows how one may be deceived in studying the phenomena. If a mixture of borax and carbonate of soda be fused in a thin platinum crucible raised to a red heat, there will be seen on the exterior of the vessel the form of the liquid mass with all its accidents of rapidly varying form, indicated by a zone of less brightness than the upper portion of the metallic surface. At first sight it is natural to infer a transparence for light of the heated platinum, but (M. Govi points out) the case is really one of transparence for radiant heat; that is to say, a phenomenon connected with the good conductivity of platinum. The liquid, liberating carbonic acid, is less hot than the crucible, and is constantly borrowing heat from it. It is inevitable, then, that at every point where the liquid touches the metal, the latter relatively cooled, should appear less luminous than in the neighbouring region. M. Govi gives some other examples of the phenomenon.

"SHORTHAND FOR GENERAL USE" is the title of a little volume by Prof. Everett, of Belfast, published by Marcus Ward and Co. Prof. Everett's system claims several advantages over Pitman's, one being that the vowels can be written continuously with the consonants, and thus the word has not to be gone over a second time to insert the vowels. The system appears to us decidedly worth the attention of anyone wishing to learn shorthand.

WE have received the eighth edition of Prof. Atkinson's translation of Ganot's "Physics." About sixty pages of additional matter, with an equal number of illustrations, have been added to this edition. Messrs. Longmans and Co. are the publishers.

ANOTHER scientific play is now being performed at the Cluny Theatre, Paris, under the title of the "Les 6 Parties du Monde." It is written by M. Figuier, the well-known scientific story-teller. The sixth part of the world is supposed to be the Antarctic continent, where Dumont Durville is made to land. It is a masterly panorama of a number of climes and countries, enlivened by a well-constructed plot.

Dr. Hoek, of Leyden, sends us the following additions to the list of dealers in zoological specimens given by Prof. Ray Lankester in a recent number of Nature:—1. Hilmar Lührs, Fischer f. Zoologen und Aquarien, Helgoland (Unterland), for fish and invertebrates (alive and in spirits, specimens of all classes). 2. The Zoological Station of Dr. Anton Dohrn, Naples, for fish and invertebrates (spirit specimens).

The additions to the Zoological Society's Gardens during the past week include three Tigers (*Felis tigris*), born in the Gardens, but did not survive; a Common Genet (*Genetta vulgaris*) from North Africa, presented by Mr. P. V. Carletti; two Hyacinthine Porphyrios (*Porphyrio hyacinthinus*) from West Asia, presented by Mrs. Henry Cobb; two All-Green Parakeets (*Brotogerys tiriacula*) from South America, presented by Miss Rowe; two Yellow-bellied Liothrix (*Liothrix luteus*) from India, presented by Gen. Breton; two common Marmosets (*Hapale jacchus*) from South-east Brazil, presented by Mrs. Clayton; three Darwin's Pucras (*Pucrasia darwini*), a Chinese Blue Magpie (*Urocissa sinensis*) from China, a Sun Bittern (*Eurypyga helias*) from South America, deposited; a Moose (*Alces machlis*) from North America, purchased.

## AMERICAN SCIENCE

Prof. Henry's portion of the report of the Smithsonian Institution for the year 1876 has been printed in separate pamphlet form, in advance of the entire volume, and gives the usual record of operations for the period. It draws attention to the fact that it is the thirtieth of the annual series made by him, and that the policy advised at the first meeting of the board has been carried out with scarcely any modification. The original fund of 541,379 dols. has been increased to 714,000 dols., although a building costing nearly 500,000 dols. has been erected. There is a library of 70,000 volumes of the most valuable class of books, namely, the serial scientific publications of learned societies. The museum has grown until it now ranks among the best in existence. This embraces copious collections illustrating the ethnology and natural history of the world. The institution has published twenty-one quarto and forty-two octavo volumes of transactions and reports. It has carried on successfully a great system of meteorological observations (only intermitted on the successful operations of the Signal Service), the results of which have been issued by a number of stately volumes. It is now prosecuting a great system of international exchanges, for the benefit of the whole world. Its correspondence, both at home and abroad, requires a large number of clerks and specialists; and the name of Smithson is universally known in consequence.

Details have been recently published (*Proc.* Acad. Nat. Sci. Philadelphia, 1877, p. 255) of the exploration of a specially interesting mound at Coup's Creek, Macoupin County, Illinois. Four skeletons sat within it, considerably enveloped in a peculiar granulated but exceedingly tenacious earth. They were placed two and two, their arms crossed, the knees of one pair pressing sharply against the backs of the other, and the faces of all turned directly toward the east. Though the greatest care was taken, only one skull was removed comparatively perfect. The whole grave measured but six feet in length by three in width, and it contained in addition to the skeletons four large marine shells of *Pyrula* (*Busycon*) *perversa* (Luin.), each similarly placed in relation to the bodies. The smaller end of one shell was placed in the right hand of each individual, while the larger portion rested in the hollow above the left hip. But, still more remarkable, within each shell had been packed what appeared to be the bones of a child; the skull, crushed before burial, protruded beyond the aperture. The suggestion is made that these infants were sacrificial offerings in honour of the dead. The graves in these mounds are constructed of stone slabs from the locality, and hence they are known as stone graves. The builders give evidence of decided constructive ability, and of having been careful cultivators of the soil. The grave-mounds are found upon ridges, while others on which dwellings were supported are near streams. A systematic series of mounds of similar origin extends from the foot of Lake Michigan to the mouth of the Illinois river, a distance of two hundred and fifty miles. Unfortunately the remains are scarcely ever capable of being preserved, or even of being examined satisfactorily on exhumation.

The following are notes of papers in the October number of the *American Journal of Science and Arts*:—The nickel plates now largely used as anodes for nickel plating are prepared by fusing commercial nickel, generally with addition of charcoal, and casting in suitable form. From an analysis of several specimens of cast nickel by Mr. Gard, it appears that silica may be reduced and retained as silicon, and that a considerable amount of carbon may be present (*e.g.*, 1·9 and 1·8 per cent.). One experiment made with a view to ascertain how much carbon nickel may take up under conditions to which it is more or less exposed in the processes of manufacture and casting, was to pack half-a-pound of granular commercial nickel in layers with charcoal in a Hessian crucible, in which it was exposed to a full red-heat twelve hours. No fusion took place. The temperature was then raised till there was complete fusion. The resulting metal was strongly magnetic, quite soft, and to a considerable extent malleable. Its specific gravity was 8·04, and it had a fracture like that of fine-grained pig-iron, scales of graphite being plainly visible. It was found to contain of total carbon 2·105, 2·130; graphitic carbon, 2,030, 1·990; silicon, ·360. Mr. Gard also made some experiments on the deportment of nickel and cobalt towards hydrocarbon at a high temperature, the substances being placed in a platinum trough within a porcelain tube and treated with a slow current of pure dry marsh-gas at a full red heat. In one case thin plates of pure electroplate nickel (·8597 gr.) were found at the close to have gained 10·649 per cent.; in another 1·2697 gr. of cobalt gained 12·758 per cent.

Among other chemical contributions we note one on the iodates of cobalt and nickel, by Mr. Fullarton, who finds that the true normal iodates contain really six molecules of water of crystallisation, and that they are essentially different from the salts obtained by Rammelsberg. Several specific-gravity determinations follow (by students of Cincinnati University), including those of a series of chromates, by Miss Abbot. Pettersson has lately shown that selenates have molecular volumes exceeding those of the corresponding sulphates by six for each molecule of the acid radicle. On comparing the chromates with Pettersson's selenates, it is found that the two series of salts have approximately equal molecular volumes; the difference, if any exists, being very slightly plus for the selenates. If regularities of this kind can be thoroughly established, it will be easy (Prof. Clark suggests), having the density of a chromate, to calculate that of the corresponding sulphate or selenate, or *vice versâ*.

A preliminary catalogue of the reptiles, fishes, and Leptocardians of the Bermudas is furnished by Mr. Brown Goode, comprising 148 out of 163 known species. The Bermudan fauna shares with the West Indies 116 species (or 79 per cent.), of which 58 (or 40 per cent.) are peculiar to the West Indies, while many others have their centres of distribution in that region. With the Eastern United States Bermuda shares 47 species, and with the waters of the Pacific and Indian Ocean 32 species. Mr. Goode also gives a description of four species of fishes believed to be new.

Prof. Dana draws some lithological and orographic conclusions in his (continued) paper on the relations of the geology of Vermont to that of Berkshire, and the *Journal* also contains some information on the Archæan of Canada and the geology of New Hampshire, &c.

## THE EARTHWORM IN RELATION TO THE FERTILITY OF THE GROUND

From observations extending over a number of years, M. Hensen is led to the conclusion that infertile undersoil is rendered valuable by the action of worms in two ways, viz., by the opening of passages for the roots into the deeper parts, and by the lining of these passages with humus. This will be more fully understood from the following facts regarding the life-habits of the worm (*Lumbricus terrestris*) given in M. Hensen's paper in the *Zeitschrift für wissenschaftliche Zoologie*.

It is known that the adult animals in wet weather come up to the surface by night, and, with their hinder end in their tube, search the ground round about. They then draw whatever vegetable material they can find into their tubes—fallen stems and leaves and small branches. In the morning one then finds little heaps of plant-fragments projecting at various parts of the surface, and each of them penetrating the tube of a worm. On closer examination it is found that the leaves have each been rolled together by the worm, and then drawn into the tube in such a way that the leaf-stalk projects. The portion of the leaf in the tube is moist and softened, and only in this state are plants consumed by the worm. There are distinct indications that the worm gnaws them, and after some days the meal is ended. The food is never drawn deeper down into the ground. In digging the ground at various seasons it was only very rarely that plant remains were found in the subsoil, and probably they got there by accident.

With reference to the structure of the worm-tubes, some interesting facts were established in these researches. In humus their character is difficult to make out, owing to the looseness of the mass. In sand they proceed almost vertically downwards three, four, or even six feet, whereupon they often extend some distance horizontally; more frequently, however, they terminate without bending. At the end of the tube the worm is found with his head upwards, while round about him the tube is lined with small stones. On the sandy wall of the tube one observes more or less numerous black protuberances which make the sand fertile. These are the secretions of the worm, which, after being removed out of a tenanted tube, are found next morning replaced by fresh matter. They are observed after a few days, when a worm is put in a vessel with clean sand, and allowed to make a tube for itself. Older abandoned tubes are pretty regularly lined with the earth formed by the worm, and some passages are densely filled with black earth. This black substance appears to diffuse somewhat into the sand.

In about half of the tubes, not quite newly made, M. Hensen found roots of the plants growing at the surface, in the most vigorous development, running to the end of the tube and giving off fine root-hairs to the walls, especially beautiful in the case of leafy vegetables and corn. Indeed such tubes must be very favourable to the growth of the roots. Once a root-fibre has reached such a tube it can, following the direction of gravity, grow on in the moist air of the passage, without meeting with the least resistance, and it finds moist, loose, fertile earth in abundance.

The question whether all roots found in the under-soil have originally grown in the tubes of worms, cannot be answered with certainty. It is certain that the roots of some plants penetrate themselves in the sand, but not to great depths. M. Hensen is of opinion that the tap-roots, and in general such root-forms as grow with a thick point, can force a path for themselves, while the fine and flexible suction-roots have difficulty in obtaining a path into the depths other than what has been previously made for them. Roots of one year's growth especially can penetrate deep into the sub-soil, only where there are earth-worms.

A microscopical comparison of the earth deposited by the worm shows that it is like the two-year leaf-mould prepared by gardeners for the filling of flower-pots. Most of the plant-cells are destroyed; still there are present some cells and shreds of tissue, browned and friable, mixed with many sand grains and brown organic fragments. The chemical composition of the worm-earth shows much similarity to that of fertile humus ground. Its fertility, therefore, cannot be doubted, though direct experiments with it are wanting.

With regard to the numerical value of this action of the earthworm, the following observations by M. Hensen afford some information.

Two worms were put into a glass pot 1½ foot in diameter, which was filled with sand to the height of 1½ foot, and the surface covered with a layer of fallen leaves. The worms were quickly at work, and after 1½ month many leaves were down 3 inches deep into the tubes; the surface was completely covered with humus 1 cm. in height, and in the sand were numerous worm-tubes partly fresh, partly with a humus wall 3 mm. thick, partly quite filled with humus.

Counting, when an opportunity offered, the open worm-tubes in his garden, M. Hensen found at least nine in the square foot. In 0·15 square metres two or three worms were found in the deeper parts each weighing three grammes: thus in the hectare there would be 133,000 worms with 400 kilos. weight. The weight of the secretions of a worm in twenty-four hours was 0·5 grammes. While these numbers are valid only for the locality referred to, they yet give an idea of the action of this worm in all places where it occurs.

The assertion that the earthworms gnaw roots is not proved by any fact; roots gnawed by worms were never met with, and the contents of the intestine of the worms never included fresh pieces of plants. The experience of gardeners that the earthworm injures pot plants may be based on the uncovering or mechanical tearing of the roots.

"Let us take a retrospective glance," concludes the author, "over the action of the worm in relation to the fertility of the ground. It is clear that no new manure material can be produced by it, but it utilises that which is present in various ways. 1. It tends to effect a regular distribution of the natural manure material of fields, inasmuch as it removes leaves and loose plants from the force of the wind and fixes them. 2. It accelerates the transformation of this material. 3. It distributes it through the ground. 4. It opens up the undersoil for the plant roots. 5. It makes this fertile.

## *UNIVERSITY AND EDUCATIONAL INTELLIGENCE*

OXFORD.—The University Commissioners are at present occupied in taking evidence on the subject of University requirements. The Dean of Christ Church, the Master of Balliol, the Master of University, the Librarian of the Bodleian, Profs. Clifton, Bonamy Price, Bartholomew Price, Stubbs, and others have appeared, or are to appear during the present week, before the Commissioners.

Mr. Lazarus Fletcher, B.A., of Balliol, has been elected to the vacant Fellowship at University College. Mr. Fletcher obtained a first class in the School of Mathematics in 1875, a first class in that of Natural Science in 1876, and the senior mathematical scholarship in 1876.

It is proposed to found a high school for the City of Oxford, the mayor, aldermen, and citizens having long felt it a reproach that, being the site of one of the most ancient and famous of the Universities of Europe, it has been absolutely without any recognised grammar school available for the sons of the citizens.

LONDON.—Prof. W. K. Clifford, F.R.S., is at present delivering at University College a very interesting course of Lectures on Quaternions. The main object of the course is to bring the physical applications of quaternions as much as possible within the reach of mathematicians of moderate attainments.

A requisition is in course of signature to the chairman of Convocation of London University, Dr. Storrar, asking that an extraordinary meeting of that body may be convened for the purpose of considering and discussing the following resolutions, and for deciding with reference thereto in such manner as to Convocation may seem fit:—"That it being manifestly inexpedient that frequent application should be made to the Crown for new and additional charters, it is desirable that provision should be made in any such charter for all changes in the constitution of the University, either at the time urgent or likely to be soon required; and that it being probable that initiative measures will be shortly taken towards procuring such a new or additional charter, the following proposals require the serious consideration of Convocation and the Senate:—'(1) An enlargement of the powers directly exercised by Convocation; (2) An increase in the proportion of senators to be nominated or elected by Convocation, and the limitation of the tenure of office in the case of all senators to a term of years; (3) The encouragement of mature study and original research among the members of the University, by the establishment of University lectureships, of limited tenure, in different departments of learning and science; (4) The introduction into the constitution of the University of such modifications as may remove all reasonable ground of complaint, on the part of any of the affiliated colleges, with respect to the absence of means for expressing opinion and giving advice to the Senate on the examination regulations, and on the changes proposed to be made therein from time to time. And that a Special Committee of ten members of Convocation be appointed to consider the above-mentioned proposals, and to report thereon to Convocation as speedily as possible.'"

The Entrance Science Scholarships in St. Thomas's Hospital have been awarded this year as follows:—The Scholarship of 60*l*. to Mr. Wansborough Jones, B.A. Oxon., and B.Sc., London; and that of 40*l*. to Mr. A. E. Wells.

BRISTOL.—A well-printed and well-arranged Calendar of University College has been published. It extends to upwards of sixty pages, and contains all the information usually found in such publications, including full details as to the Medical School.

Dean Stanley's address on Education, at University College, on Saturday, attracted an audience of about 1,700 people, who listened with the closest attention.

## SCIENTIFIC SERIALS

*Kosmos*, Part 2 (May) opens with an article by L. Overzier, on "Heredity" (Part 1), aiming at the discovery of the real cause of inheritance.—Prof. Jäger, commencing a series of articles on "The Origin of Organs," deals with the development of the eye, showing how the laws of optics and the properties of living substance mutually influence one another.—Hermann Müller, treating on "The Origin of Flowers," considers the first metasperm (or angiosperm) to have been diclinous and fertilised by the wind, that is, supposing the metasperms to have originated from a single stock.—W. O. Focke deals with "The Conception of Species in the Vegetable Kingdom," especially in relation to the genus Rubus. He shows how far the different species are from being of equivalent value and that the term variety has no definite significance. He exposes the futility of much botanical "research," owing to imperfection of methods and lack of comparative study; Darwin has few imitators. Such work requires an entire devotion of time and complete botanical gardens, for the multiplication of which the author calls.—A. Lang, on Lamarck and Darwin (I.), expounds Lamarck's conceptions of natural history.

*Kosmos*, Part 3 (June).—L. Overzier continues his discussion of heredity, reviewing Darwin's theory of pangenesis, Haeckel's perigenesis, and Jäger's chemical theory; he considers the latter to be of great value.—Carl du Prel, on the needed remodelling of the nebula hypothesis.—Prof. Jäger treats of the origin of the organ of hearing, tracing it from the simplest condition where spicules diffused through the entire protoplasmic body of an animal serve to gather up and conduct vibrations of sound. He brings forward the remarkable theory that in animals possessing nerve fibres, the organs of hearing is but a specialisation from the general tactile sense.—W. von Reichenau, on the colours of bird's eggs, makes the generalisation that birds having open nests have coloured eggs, while those with covered or concealed nests have white ones; further, that in open and ground nests the colour of the eggs has a protective object.—A. Dodel-Port, on the lower limit of sexuality in plants, gives an account of the sexual processes in *Ulothrix zonata*, but appears not to have heard of the researches of Dallinger and Drysdale on the monads.—A. Lang, on Lamarck and Darwin, expounds Lamarck's "hydro-geology."

## SOCIETIES AND ACADEMIES

### PARIS

**Academy of Sciences**, October 22.—M. Peligot in the chair.—The following papers were read:—M. Leverrier's tables of Uranus and Neptune, by M. Tresca.—On some applications of elliptic functions (continued), by M. Hermite.—*Résumé* of a history of matter (first article), by M. Chevreul. This is an extract from a work commenced about the end of last year, and occupying 418 pages of the *Memoires de l'Académie*, t. xxxix. A sketch of the principles of alchemy is given.—On one of the causes of red coloration of the leaves of *Cissus quinquefolia*, by M. Chevreul. This cause is sunlight. The green colour is retained in the leaves that are shaded by others.—On the order of appearance of the first vessels in the shoots of some Leguminosæ, by M. Trécul.—Modifications in the conditions of maxima of electro-magnets by the state of more or less complete saturation of their magnetic core, by M. Du Moncel. The law of proportionality of the attractive forces to the squares of the intensities of the current is true only within certain limits, and under certain conditions; and electro-magnets through which the current is interrupted at very short intervals, are (more or less) not subject to it. When the forces are proportional to (say) the cubes of the electric intensities, the helices must always be less resistant than the exterior circuit. In the case of multiplied interruptions, the resistance of electro-magnets must always be less the shorter the duration of closures of the current; and for this reason (also because of defective insulation and extra currents) telegraph electricians reduce considerably the resistance of electro-magnets applied to long circuits. Reverting to the question in the title, the thickness of the magnetising spiral may be increased in case of defective saturation of the magnetic core; becoming double the diameter of this if the force increases as the cube of the intensities.—Preparations of sulphide of carbon brought to the solid state by means of gelatine, by M. Cassius. 100 grammes of gelatine are dissolved in 1,000 grammes of water, and sulphide of carbon (25,50, or 75 per cent.) is mixed at a temperature of 15° to 20°, and the mixture let cool. M. Cassius thinks the preparation might be useful in viticulture. The sulphide is liberated slowly, the time varying according to the proportion of sulphide absorbed.—Experiments on the formation of artificial ultramarine, by M. Plicque. He finds (in opposition to some German authors) that ultramarine does not contain nitrogen. Blue ultramarine, properly so called, is formed by an oxygenated compound of sulphur, and it is probable that this compound is fixed both by sodium and by aluminium.—On the catechines and their constitution, by M. Gautier.—On acid acetates, by M. Villiers. The increase of weight of some neutral acetates, dried and placed, in a summer month, under a bell jar with crystallisable acetic acid, was, in the case of acetate of soda, 404 per cent., or nearly six equivalents of acetic acid; acetate of potash, 264 per cent; of baryta, 179 per cent.; of lead, 134 per cent., &c. The solutions of neutral acetates in crystallisable acetic acid have much less tension of vapour than that of acetic acid.—Researches on butylene and its derivatives, by M. Puchot.—Note on the cause of anthrax, by M. Klebs.—On the structure of the blood corpuscle, and the resistance of its envelope to the action of water, by MM. J. Bechamp and Baltus. The demonstration of the membrane (by action of soluble fecula) is here given in the cases of the frog, the ox, the pig, and the sheep. Water does not destroy the globules; it merely renders them invisible, and they may always be discovered with the aid of picrocarminate, even in extremely dilute media, and after several weeks of contact. The blood of sheep (like that of the hen in M. A. Bechamp's experiments) contains globules of more delicate structure than those of the other bloods examined.—Researches on the functions of leaves of the vine, by M. Macagno. Glucose and tartaric acid are formed preferably in the upper leaves of the fruit-bearing vine-branch; this production of sugar progresses with that of the grape, and is much reduced (even to disappearance) after the vintage. The green branches are conductors of glucose. These facts explain the evil of "pinching" or removing the tops of the grape-bearing branches, with too great zeal. Where there is an abundant production of grapes, a sufficient quantity of leaves should be left for preparation of the necessary glucose.—Reply to a recent note of M. Buys Ballot, on the division into time and into squares of maps of nautical meteorology, by M. Brault.

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