THURSDAY, NOVEMBER 8, 1877

EXPLOSIONS IN MINES

AFTER the occurrence of great colliery explosions such as those which took place recently in Pemberton and Blantyre collieries, one very general and pertinent question presents itself to most minds, namely, What has been done or attempted with the view of preventing these disasters? It would be impossible to condense into an article like the present all that could be said in reply to this question, but I shall endeavour to give a brief outline of the subject, and point out, as well as I can, what appear to be its most prominent features.

Before the invention of the safety-lamp, the only means of guarding against the ignition of firedamp consisted in the employment of an apparatus called the "steel mill." The light obtained by its aid was feeble and uncertain, and Mr. Buddle informs us that explosions were known to have been caused by the sparks emitted by it. When Davy made his brilliant invention in 1815–16, the steel mill was laid aside for ever, and it was then imagined that colliery explosions had also become phenomena belonging to a past order of things. So confident, indeed, was Davy in the efficacy of his lamp, that he believed it could be safely employed for carrying on work in an explosive atmosphere; and he even went so far as to propose to make use of the firedamp itself as the light-giving combustible. These fond expectations were soon roughly dispelled, as one explosion followed another in an apparently unaccountable manner; and at length they were succeeded by a feeling of positive distrust, which found expression in the report of a select committee appointed, in 1835, to inquire into the nature of accidents in mines.

In 1850 Mr. Nicholas Wood made a series of experiments, which proved that when a Davy lamp is subjected to an explosive current travelling at the rate of eight or nine feet per second, the flame soon passes through the wire gauze. This was corroborated about 1867 by experiments conducted by a committee of the North of England Institute of Mining Engineers.

Lastly, in 1872–73, the writer demonstrated, also by experiment, that when a lamp burning in explosive gas is traversed by a violent sound-wave, such as that produced by a blasting shot, the same result follows, that is, ignition is communicated to the outside atmosphere. These are weak points inseparable from the construction of the ordinary Davy and Clanny lamps; but as it is now a thoroughly-recognised maxim that work must never, under any circumstances, be continued in an explosive atmosphere, they are seldom put to the test.

The atmosphere of part of a mine may, however, become explosive before the men can escape, either by the sudden influx of a quantity of firedamp from some natural cavity in which it had existed in a state of tension, or by a partial or total cessation of the ventilating current; and I propose in the next place to consider how such an event could produce an explosion supposing all the men to be provided with safety lamps.

This will happen, firstly, if the inflammable gas passes over a furnace at the bottom of the upcast; secondly, if it is carried against a Davy or Clanny lamp at a greater velocity than seven feet per second, or if the lamp is traversed by a sound-wave; thirdly, if a blasting shot is fired directly into it; and lastly, if it reaches a safety lamp that has been opened by one of the men.

The means that have been provided for guarding against these contingencies are as follow:—1. Furnaces have to a large extent been replaced by ventilating fans in fiery collieries. 2. Davy and Clanny lamps are still almost universally employed, and little importance seems to be attached to their known imperfections by those who are supposed to be capable of deciding the question. 3. Shot-firing having been found to originate many explosions, although probably in a manner not yet understood by most people, is now carried on under certain restrictions which it could easily be shown are still insufficient. 4. Much nonsense has been talked and written about miners opening their lamps. That they sometimes do so is beyond a doubt; but why should this state of matters be allowed to continue when it can be easily put an end to? The present flimsy pretence for a lock is not a necessity but a cheap convenience; and who is responsible if say a hundred men are killed through its being opened by one? Is there no responsibility attaching to the owners or the legislature for placing the lives of ninety-nine innocent men in danger? I think surely there is.

The influence of changes of weather on the internal condition of mines has been remarked since the remotest times, and for the last fifty or sixty years at least many have asserted that firedamp is more prevalent when the barometer is low than in the opposite case. The explanation of these phenomena is easily found by anyone who has an elementary knowledge of the physical properties of gases. On the other hand, when vigorous artificial means of ventilation are employed, and ordinary skill practised in distributing the air, the effects of changes of weather become much less perceptible.

Hence if a large proportion of explosions can be shown to occur simultaneously with, and therefore, presumably, in consequence of, those atmospheric changes that would tend to augment the amount of firedamp in the workings, there is a strong argument in favour of the supposition that they are preventible, and cannot therefore be considered as accidents in the true sense of the term. With this object in view diagrams have been made from time to time by Mr. R. H. Scott and myself, and also by one or two others, showing the connection that exists between the two classes of phenomena, and an examination of these is sufficient to convince unbiased persons that there is a striking coincidence between the explosions and the favourable atmospherical conditions. As might, perhaps, be expected, some persons engaged in mining either fail to see the connection, or possibly they do not understand it. Nevertheless a general rule was inserted in the Coal Mines' Regulation Act (1872) making it compulsory for mine-owners to place a barometer and thermometer at the entrance to every mine in the coal-measures.

It has always been difficult, and sometimes impossible, for mining men to give an adequate reason for the extent of great explosions, and more especially when it is known that, immediately beforehand, little or no inflammable gas has been present in the workings. The reports of the Inspectors of Mines bear ample testimony to the correctness of this statement. It has therefore been customary in the absence of any other tenable hypothesis to assume that a large volume of firedamp had been suddenly poured into the workings. But these so-called "outbursts of gas" are entirely unknown in some localities in which great explosions have occurred; and therefore it is much to be marvelled at that some other explanation was not at least sought for.

In September, 1844, before the appointment of inspectors of mines, Lyell and Faraday were sent to Haswell Colliery by the Home Secretary to report on an explosion that had just taken place there. I am unable to quote from their official report, but I am firmly convinced that the following sentences taken from their article on the subject in the *Phil. Mag.* 1845, is the true key to a solution of the problem as regards both the mode of occurrence and means to be used for the purpose of avoiding great explosions in future; and, moreover, I believe that it has been highly unfortunate, both for the cause of the miner and his employer, that these two philosophers were not induced to prosecute their investigations further than they did.

The sentences referred to are these:—"In considering the extent of the fire for the moment of explosion, it is not to be supposed that firedamp is its only fuel; the coal-dust swept by the rush of wind and flame from the floor, roof, and walls of the works, would instantly take fire and burn, if there were oxygen enough in the air to support its combustion; and we found the dust adhering to the face of the pillars, props, and walls in the direction of, and on the side towards, the explosion, increasing gradually to a certain distance as we neared the place of ignition. This deposit was in some parts half an inch, and in others almost an inch thick;[1] it adhered together in a friable coked state; when examined with the glass it presented the fused round form of burnt coal-dust, and when examined chemically, and compared with the coal itself reduced to powder, was found deprived of the greater portion of the bitumen, and in some cases entirely destitute of it."

About three years ago M. Vital, Ingénieur des Mines in France, showed that a flame resembling that produced by a blasting shot which blows out the tamping is greatly lengthened in an atmosphere containing a cloud of coal-dust; and soon afterwards the writer ascertained that air containing a small proportion of fire-damp (less than one per cent. by volume) becomes highly inflammable when coal-dust is mixed with it.

These discoveries complete what Lyell and Faraday began, and show how explosions of any conceivable magnitude may occur in mines containing dry coal-dust. A blasting shot or a small local explosion of firedamp, or a naked light exposed when a cloud of coal-dust is raised up by a fall of roof in air already containing a little fire-damp is sufficient to initiate them, and, when once they are begun, they become self-sustaining.

These remarkable facts are either not yet sufficiently well known or their true significance is not yet fully appreciated. In conclusion I may state that out of many hundred collieries known to me there is not, to my knowledge, a single damp one in which a great explosion has happened; while, on the other hand, there is a considerable number of very dry ones in which explosions causing the deaths of from 12 to 178 men at a time have occurred.

W. GALLOWAY

[1] In the reports of the Inspectors of Mines, human bodies, timber, and coal, are described as being *charred* or *burnt* where they are covered with this deposit.—W. G.

THE SUN'S PHOTOSPHERE

DR. JANSSEN has just made a communication to the French Academy of Sciences, which will be received with interest, not only by students of solar physics, but by all who follow the various triumphs achieved by modern scientific methods. It seems a paradox that discoveries can be made depending on the appearance of the sun's surface by observations in which the eye applied to the telescope is powerless; but this is the statement made by Dr. Janssen himself, and there is little doubt that he has proved his point.

Before we come to the discovery itself let us say a little concerning Dr. Janssen's recent endeavours. Among the six large telescopes which now form a part of the equipment of the new physical observatory recently established by the French government at Meudon, in the grounds of the princely Château, there is one to which Dr. Janssen has recently almost exclusively confined his attention. It is a photoheliograph giving images of the sun on an enormous scale—compared with which the pictures obtained by the Kew photoheliograph are, so to speak, pigmies, while the perfection of the image and the photographic processes employed are so exquisite, that the finest mottling on the sun's surface cannot be overlooked by those even who are profoundly ignorant of the interest which attaches to it.

This perfection and size of image have been obtained by Dr. Janssen by combining all that is best in the principles utilised in one direction by Mr. De la Rue, and in the other by Mr. Rutherfurd. In the Kew photoheliograph, which has done such noble work in its day that it will be regarded with the utmost veneration in the future, we have first a small object-glass corrected after the manner of photographic lenses, so as to make the so-called actinic and the visual rays coincide, and then the image formed by this lens is enlarged by a secondary magnifier constructed, though perhaps not too accurately, so as to make the actinic and visual rays unite in a second image on a prepared plate. Mr. Rutherfurd's beautiful photographs of the sun were obtained in a somewhat different manner. In his object-glass he discarded the visual rays altogether and brought only the blue rays to a focus, but when enlargements were made an ordinary photographic lens—that is, one in which the blue and yellow rays are made to coincide—was used.

Dr. Janssen uses a secondary magnifier, but with the assistance of M. Pragmowski he has taken care that both it and the object-glass are effective only for those rays which are most strongly photographic. Nor is this all; he has not feared largely to increase the apertures and focal length, so that the total length of the Kew instrument is less than one-third of that in operation in Paris.

The largely-increased aperture which Dr. Janssen has given to his instrument is a point of great importance. In the early days of solar photography the aperture used was small, in order to prevent over-exposure. It was soon found that this small aperture, as was to be expected, produced poor images in consequence of the diffraction effects brought about by it. It then became a question of increasing the aperture while the exposure was reduced, and many forms of instantaneous shutters have been suggested with this end in view. With these, if a spring be used, the narrow slit[1] which flashes across the beam to pay the light out into the plate changes its velocity during its passage as the tension of the spring changes. Of this again Dr. Janssen has not been unmindful, and he has invented a contrivance in which the velocity is constant during the whole length of run of the shutter.

By these various arrangements the plates have now been produced at Meudon of fifteen inches diameter, showing details on the sun's surface of less than one second of arc.

So much for the *modus operandi*. Now for the branch of solar work which has been advanced.

It is more than fifteen years ago since the question of the minute structure of the solar photosphere was one of the questions of the day. The so-called "mottling" had long been observed. The keen-eyed Dawes had pointed out the thatch-like formation of the penumbra of spots, when one day Mr. Nasmyth announced the discovery that the whole sun was covered with objects resembling willow leaves, most strangely and effectively interlaced. I here quote from Sir John Herschel.[2]

"According to his observations, made with a very fine telescope of his own making, the bright surface of the sun consists of separate, insulated, individual objects or *things*, all nearly or exactly of one certain definite size and shape, which is more like that of a willow leaf, as he describes them, than anything else. These leaves or scales are not arranged in any order (as those on a butterfly's wing are), but lie crossing one another in all directions, like what are called spills in the game of spilikins; except at the borders of a spot, where they point for the most part inwards, towards the middle of the spot, presenting much the sort of appearance that the small leaves of some water-plants or sea-weeds do at the edge of a deep hole of clear water. The exceedingly definite shape of these objects; their exact similarity one to another; and the way in which they lie across and athwart each other (except where they form a sort of bridge across a spot, in which case they seem to affect a common direction, that, namely, of the bridge itself), all these characters seem quite repugnant to the notion of their being of a vaporous, a cloudy, or of a fluid nature. Nothing remains but to consider them as separate and independent sheets, flakes, or scales, having some sort of solidity. And these flakes, be they what they may, and whatever may be said about the dashing of meteoric stones into the sun's atmosphere, &c., are evidently the *immediate sources of the solar light and heat*, by whatever mechanism or whatever processes they may be enabled to develop, and as it were elaborate these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view, we cannot refuse to regard them as *organisms* of some peculiar and amazing kind . . . ."

Here, then, was a discovery with a vengeance! and absolute endorsement from the man above all others who had a right to express an opinion. Nevertheless, the organisms have since disappeared, and the work of many careful observers has established that the mottling on the sun's surface is due to dome-like masses, and that the "thatch" of the penumbra is due to these dome-like masses being drawn, either directly or in the manner of a cyclone, towards the centre of the spot. In fact the "pores" in the interval between the domes are so many small spots, while the faculæ are the higher levels of the cloudy surface. The fact that faculæ are so much better seen near the limb proves that the absorption of the solar atmosphere rapidly changes between the levels reached by the upper faculæ and the pores.

These masses are in all probability due to a rapid increase of pressure in the portion of the solar atmosphere occupied by the photosphere; we know, or think we know, that they are not due to reduction of temperature.

Thus much presumed we now come to Dr. Janssen's discovery.

An attentive examination of his photographs shows that the surface of the photosphere has not a constitution uniform in all its parts, *but that it is divided into a series of figures more or less distant from each other, and presenting a peculiar constitution*. These figures have contours more or less rounded, often very rectilinear, and generally resembling polygons. The dimensions of these figures are very variable; they attain sometimes a minute and more in diameter.

While in the interval of the figures of which we speak the grains are clear, distinctly terminated, although of very variable size, in the interior the grains are as if half effaced, stretched, strained; for the most part, indeed, they have disappeared to make way for trains of matter which have replaced the granulation. Everything indicates that in these spaces, as in the penumbræ of spots, the photospheric matter is submitted to violent movements which have confused the granular elements.

In an article recently contributed by Dr. Hunter and myself to the *Nineteenth Century*,[1] the following passage occurs:—

"The spots may be taken as a rough index of solar energy, just as the rainfall may be taken as a convenient indication of terrestrial climate. *They are an index but not a measure* of solar activity; and their absence indicates a reduction, not the cessation, of the sun's energy. *Whether this reduction means one in a hundred or one in a thousand we do not know.*"

With the same idea in his mind Dr. Janssen points out that this fact throws light upon the forms of solar activity, and shows that that activity, in the photosphere, is always very great, although no spot appears on the surface.

We have already referred to the paradox that the sun's appearance can now be best studied without the eye applied to the telescope. This is what Dr. Janssen says on that point.

The photospheric network cannot be discovered by optical methods applied directly to the sun. In fact, to ascertain it from the proofs, it is necessary to employ glasses which enable us to embrace a certain extent of the photographic image. Then if the magnifying power is quite suitable, if the proof is quite pure, and especially it

[1] I have recently been making some experiments with a view of getting rid of the narrow aperture in general use, as it has appeared to me that the diffraction effects produced by it must be as injurious to definition as those due to a small object-glass. I have found that a circular aperture, allowing the whole beam to be flashed on the plate in conjunction with a plate of optically pure yellow glass nearly in contact with the photographic plate can be used without over-exposure.

[2] "Familiar Lectures," p. 87.

[1] "Sun-spots and Famines," *Nineteenth Century*, November, 1877, p. 584.

it has received rigorously the proper exposure, it will be seen that the granulation has not everywhere the same distinctness, that the parts consisting of well-formed grains appear as currents which circulate so as to circumscribe spaces where the phenomena present the aspect we have described. But to establish this fact, it is necessary to embrace a considerable portion of the solar disc, and it is this which it is impossible to realise when we look at the sun in a very powerful instrument the field of which is, by the very fact of its power, very small. In these conditions we may very easily conclude that there exist portions where the granulation ceases to be distinct or even visible ; but it is impossible to suppose that this fact is connected with a general system.

We have written enough to show that when the daily history of the sun comes to be recorded another method and another point of view have now been added as the first fruits of Dr. Janssen's labours in his new observatory.

J. NORMAN LOCKYER

FOWNES' "MANUAL OF CHEMISTRY"

Fownes' Manual of Chemistry. Vol. II. Chemistry of Carbon Compounds, or Organic Chemistry. Twelfth Edition. By H. Watts, B.A., F.R.S. (London: Churchill, 1877.)

ORGANIC chemistry is now progressing with such rapid strides, that a work on this subject becomes antiquated, at least in some parts, in the course of a few years. A new edition of a well known and favourite book must therefore be most welcome to students of this branch of chemical science, and more so when edited by a man whom we may justly call "the English Gmelin."

The old familiar, bulky Fownes has now been divided into two handy volumes, enabling the editor to devote the same space to the carbon compounds as to inorganic chemistry.

The arrangement of the subject is in principle almost the same as in the last edition ; organic compounds being divided into hydrocarbons, alcohols, ethers, amido-compounds, organo-metallic bodies, acids, &c., the compounds of each group being arranged in homologous series.

Physiological chemistry is omitted, and this must be considered as an improvement, as that branch of chemical science now requires special treatment in a separate work.

The name of the author is a sufficient guarantee for the soundness of the knowledge which this book imparts, and we hope to see it soon in the hands of numerous students who will find it a most useful and trustworthy guide, embracing as it does the most important recent researches. The book is singularly free from misprints, and the few which we have found can be easily corrected by a student who is accustomed to think for himself.

As a reviewer is expected to point out any faults, we will do so, but "sine iræ et studia," and only for the benefit of the students who will largely use this work.

Thus we miss an account of the normal sulphuric ethers, which are found by the action of sulphuryl chloride, or oxychloride on the alcohols and phenols. Perhaps these parts were written before the researches we allude to were published, and the same may be the case with phenyl-sulphuric acid, and its homologues, compounds which possess such interest both for the chemist and physiologist. To lactide, the author still assigns the old formula $C_3H_4O_2$, although Henry has proved, by determining its vapour density, that its molecular formula is $C_6H_8O_4$. On page 285 we find a statement which might lead a beginner in practical work to disappointment, it is there said that "crude acetyl chloride is purified by heating it with water and dilute soda solution." "Quandoque bonus dormitat Homerus."

We were much pleased to find that Mr. Watts has given particular attention to the study of isomerism, especially among the derivatives of benzene, and he justly says in the preface: "This part of the subject is here presented in a form in which it has not yet appeared in any English publication, except in the *Journal* of the Chemical Society."

Speaking of the disubstitution products of benzene, the following definition is given : "A di-derivative of benzene is para-, ortho-, or meta-, according as it can give rise to, or be formed from, one, two, or three tri-derivatives. This definition is, however, incomplete, and only holds good if in the di-derivative the substituting elements or radicals are the same. For it is easily seen that, to take the most simple case, a para-compound containing two different groups such as paranitrobromobenzene can give rise to or be formed from two different amidonitrobromobenzenes. The oversight is, however, a matter of small importance, and an attentive student will not be led astray by it.

The theory of structure or position which Mr. Watts treats so fully has been lately attacked by eminent chemists who seem to overlook or forget the great impulse which this theory has given to the progress of organic chemistry. The "modern chemists," as they sneeringly have been called, know well enough that the structural formulæ which they use do not pretend to give a picture of the real position of atoms in space, and do not mean more than the parallelogram of forces in mechanics, *i.e.*, they only express the manner in which the different forces of the atoms attract each other. They fully understand that their present theory, with the progress of science will have to undergo many modifications, and it is not a dogma, but will stand or fall on its own merits.

The opponents of the modern school remind us of the last followers of the phlogistic theory who got hold of any fact which the antiphlogistonists were not able to explain as a proof that the latter were in the wrong. We can easily imagine how pleased Priestley was when it was found that when heating certain metallic calces with charcoal an inflammable air was formed, whereas, according to Lavoisier's school, only carbonic acid could be produced. Just in the same way the opponents of the structural theory point out that the existence of four lactic acids is incompatible with it ; and Mr. Watts himself, although a strong adherent of the theory of structure, shirks the discussion of this point, and rusticates one of the four in a foot-note, in which he expresses his doubts as to its existence.

The recent researches of Wislicenus, however, hardly leave any doubt that four such acids exist. We must confess that we are not able to explain the difference between hydracrylic acid and ethenelactic acid, and quite agree with Mr. Watts that Wislicenus' explanation of the cause of their isomerism is improvable and far-fetched. But there exist other isomeric compounds which, like these two acids, have apparently the same chemical constitution, and in some of these cases it has lately been shown that the bodies are not chemical isomerides but physical isomorphides, or differ from each other in exactly the same way as calcite differs from arragonite. We have not the least doubt that the cause of the isomerism of the lactic acids will, at no distant time, also find a satisfactory explanation, because we are convinced that organic chemistry is working in the right direction. Time will show whether we prophesy truly or not.

OUR BOOK SHELF

Transcaucasia and Ararat; being Notes of a Vacation Tour in the Autumn of 1876. By James Bryce. (London: Macmillan and Co., 1877.)

ALTHOUGH in this narrative Prof. Bryce takes the reader over pretty well-known ground, about parts of which, at least, much has been written, still even the best-informed readers will read his book with pleasure and profit. Prof. Bryce used his own eyes, and as he is a good and independent observer, there is an unusual freshness about his narrative. He journeyed down the Volga, crossed the southern steppe and the Caucasus to Ararat, which he ascended, thence to the shore of the Black Sea, sailing along the coast to Constantinople. Nijni Novgorod Fair, he thinks, has been much over-estimated in some respects, and he has a good word to say of the recently much-abused Cossack. Prof. Bryce is a good geologist, and his work abounds with interesting notes on the geology as well as the flora of the regions which he traversed. Perhaps the most interesting chapter in his book is that in which he describes his ascent of Mount Ararat. In a previous chapter he has collected much valuable information concerning the mountain, the legends connected with it, its geology, volcanic phenomena, meteorology, vegetation, and animals. Prof. Bryce, with a companion, six Cossack soldiers, and an interpreter, set out from Aralyk, a little to the north of the mountain, at 8 A.M., on September 11 last year, to attempt the ascent. About noon they were fairly on the side of Ararat, and at about 6,000 feet came upon a small Kurd encampment, some of the Kurds, with their oxen, being induced to act as baggage-bearers. At the well of Sardarbulakh they camped late in the afternoon, about 7,500 feet above the sea. About one A.M. they started again, thirteen in all, but as they proceeded, with many vexatious halts, the Cossacks dropped off one by one, and at last, at about 12,000 feet, Prof. Bryce resolved to take what he wanted in the way of food, and start at his own pace. Two Cossacks and a Kurd accompanied him to the height of about 13,600 feet, when they too dropped off, and Prof. Bryce resolved to accomplish the remainder of the 17,000 feet alone, a hazardous undertaking even for a trained Alpinist. Partly up a rocky slope which seems to extend considerably beyond the snow-line, and partly over the soft snow itself, and enveloped much of the time in cloud, Prof. Bryce continued his solitary and fatiguing climb, until about half-past two P.M., he became convinced that he was really on the top of Ararat, at least one of the tops, for there are two, one about thirty feet higher than the other, and he did not descend until he had set his feet on both. There were difficulties and dangers both in the ascent and descent, though they do not seem to be nearly so great, judging from Prof. Bryce's description, as those which attend the ascent of a moderate Alpine summit. Prof. Bryce reached his companions again in safety. Notwithstanding he had to make all haste to reach the summit, he had time to make several interesting notes of what he saw by the way, the evidences of volcanic action particularly attracting his attention. To show the superstitious awe with which the sacred summit is regarded in the region around, Prof. Bryce tells that when the Archimandrite of Etchmiadzin was told that the Englishman had ascended to the top of "Massis," the venerable man replied, smiling sweetly, "No, that cannot be. No one has ever been there. It is impossible." Prof. Bryce's is the sixth known ascent of Ararat, the first having been made in 1829 by Dr. Frederick Parrot, a Russo-German professor in Dorpat University.

Thermodynamics. By R. Wormell. (The London Science Class-books. Elementary Series. Longmans, 1877.)

THIS work is one of the earliest published of a series "adapted for school purposes," and "composed with special reference to use in school teaching," as we are told in the general preface.

We feel very strongly that no good can come of the introduction of such subjects as the dynamical theory of heat into school-teaching. That an average school-boy can be taught the elements of such subjects as astronomy, botany, and natural history, and that he will to a certain extent profit by such teaching, may probably be true; but only in so far as his powers of observation are concerned. We believe that it is a complete mistake in practical education to try to carry the process farther than the elements, even in the case of the comparatively easy subjects just named.

Some elementary experimental facts connected with heat might, no doubt, be added to the list. But it is simply the work of the crammer to stuff a school-boy's head with such utterly unassimilable materials as reversible engines, absolute temperature, and the kinetic theory of gases. This is education run mad.

This obvious consideration decides at once our opinion as to the value of the work before us. It is beyond the intelligence of schoolboys, and in the hopeless endeavour to sink it to *their* level it has been deprived of much that might have made it a serviceable work for more mature minds.

After what we have said, it would be superfluous to criticise the book minutely, for nearly all our objections would be mere repetitions in part of the first and general one. We note, however, a want of strictness, or at least of completeness, in some of the mathematical proofs. The first example we meet with may serve as a type. Thus (p. 4) it is assumed, without any attempt at explanation, in fact without a word to warn the reader that a distinct step has been taken, that in uniformly accelerated motion the mean velocity during any period is half the sum of the initial and final velocities—a truth, and a very important one, but most certainly not self-evident to the average schoolboy.

Simple Lessons for Home Use. (London: E. Stanford, 1877.)

THESE simple lessons are intended for younger children than those for whom the primers published by Messrs. Macmillan have been written, and they appear admirably adapted for the purpose they have in view. Mr. W. E. Forster, in his recent speech at Huddersfield, referred to the importance of teaching the elements of science in primary schools by means of appropriate reading books. The little books before us, so far as they go, meet the wish expressed by Mr. Forster. The print is clear, the language on the whole simple, and the price (threepence) places them within the reach of the humblest. Perhaps there is a little too great a tendency to moralise in parts of the otherwise capital little lessons on birds and money. The author of the last-named—the Rev. T. E. Crallan—tells in a simple and interesting way how money grows, and writes for younger minds than does the Rev. G. Henslow, who contributes lessons on flowers, where too many technical terms are, we think, introduced, especially in the first chapter. Miss Fenwick Miller's lessons on the human body, and on ventilation, are excellent, and so are Mr. Philip Bevan's on food, and Dr. Mann's on the weather. Altogether, we congratulate the publisher on the subjects selected, and the authors he has chosen: no doubt the remainder of the lessons that are to be issued will confirm the high opinion we have formed of those already before us. W. F. B.

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.]

Appunn and Koenig.—Beats in Confined Air

IN my letter published in NATURE (vol. xvi. p. 227), I stated that I should re-examine the question of the discrepancy between Appunn and Koenig, and inform you of the result. During the whole month of September I was engaged in very carefully counting and recounting Appunn's tonometer in the South Kensington Museum, the reeds of which had got a little out of order, a circumstance which did not interfere with the ascertainment of pitch, but disposed at once of any errors in Appunn's pendulum. I employed one of Webster's ship chronometers, which was rated to lose one second daily, and counted each set of beats repeatedly through one or two minutes. I ascertained by this means that the objections made by Koenig on the score of false pendulums and false counting were entirely groundless, and that the former determinations of the relative pitch of Koenig's forks and Appunn's reeds, made by Dr. Preyer and myself, were practically correct.

But as Lord Rayleigh pointed out in NATURE (vol. xvii. p. 12) the practical agreement of the results obtained by Professors Mayer and MacLeod, and by his own new method there described, with Koenig's, serves to show that there is a physical phenomenon to be accounted for. Mr. Bosanquet had drawn my attention to the subject several months ago, and my own experiments on the beating of disturbed consonances had led me to the same conclusion. Accordingly I had devised a series of experiments for ascertaining the fact, the nature of which I lately communicated to Lord Rayleigh; but as they required the use of two tonometers excited by separate bellows, there were difficulties in the way of making them, which I did not overcome till this week. To-day I made the first of these experiments, lasting four hours or more, and ascertained—

1. That the beats of the harmonium reeds in Appunn's tonometer *are* affected by taking place in a confined space of air.
2. That they are *accelerated*, and
3. That the acceleration, being roughly about one per cent., will probably, when completely ascertained, account for the discrepancy observed.

Details have been sent privately to Lord Rayleigh; they are too incomplete for publication. The experiments will require many weeks to complete with the necessary accuracy. But in the meantime I hasten to communicate an important acoustical fact which may bear upon many other phenomena besides the ascertainment of absolute pitch. ALEXANDER J. ELLIS

25, Argyll Road, Kensington, November 3

The Radiometer and its Lessons

As I now learn for the first time what are the grounds on which Prof G. C. Foster based his inculpation of me, I may ask for a *very* few last words. I fully admit that in giving a sketch of the history of the Radiometer, I *intended* to attribute to Mr. Crookes that he had in the first instance put a wrong interpretation upon his own results; because I believed that this was a simple fact, well known to everybody who had followed the history of the inquiry. And Prof. Carey Foster has not called in question the correctness of my statement of the general impression which prevailed among scientific men, alike when Mr. Crookes first exhibited his radiometer at the *soirée* of the Royal Society, and when its phenomena were discussed at the subsequent meeting. Having followed that discussion with the greatest interest, I cannot now recall one word that was not in harmony with the "direct impact" doctrine, or that suggested the idea of "heat reaction" through residual gas. If the question had been then asked, whether the rotation would continue to take place in an *open* vacuum (were such possible), or in a *perfect* vacuum,—so as to eliminate all "reaction," through residual gas, between the vanes and the containing flask,—I believe that the general, if not the unanimous, verdict would have been in the affirmative. Certainly I heard nothing from Mr. Crookes on the other side, he having previously spoken of the dependence of the "Repulsion resulting from Radiation on the presence of residual gas as 'impossible to conceive.'"

It is clear, then, that in referring to this then prevalent view, I no more wished to put Mr. Crookes in the wrong, than I wished to put in the wrong my very excellent friends among the other eminent Physicists who shared it; the special purpose of this part of my paper being to bring out, as strongly as I could, *the thoroughly scientific and philosophical method in which Mr. Crookes afterwards worked himself right.* If this is not expressed in as much detail as Prof. G. C. Foster would have approved, it surely afforded no adequate ground for his going out of his way to charge me with having "depreciated Mr. Crookes's merits." Yet this is the *only* ground that I can find in the whole of Prof. Carey Foster's statement, for what I could not but regard as a very grave imputation.

On Mr. Crookes's reply I shall make but a single remark, with reference to his perfectly correct citation of the latter part of my conversation with him, on the occasion of his receiving the Royal Medal. If I had not found, after the publication of my Lectures (in which I said nothing but what was respectful to Mr. Crookes), that he had himself been "digging up the hatchet" which I was quite disposed to keep buried, by giving his public attestation to the "spiritualistic" genuineness of what had been proved to be a most barefaced imposture, I should not have again brought his name into the controversy. But I felt that his greatly increased reputation as a Scientific man would do an increasing injury to what I honestly believed to be the cause of reason and common sense, not only in this country but still more in the United States.

Since the death of Prof. Hare, not a single scientific man of note (so far as I am aware) has there joined the Spiritualistic ranks; but the names of the "eminent British scientists," Messrs. Crookes and Wallace, are a "tower of strength" to the various orders of "mediums"—rapping mediums, writing mediums, drawing mediums, materialising mediums, test mediums, photographic mediums, trance mediums, healing mediums, and the like—*whose names form many columns* of the "Boston Trades' Directory." And the now notorious impostor, Eva Fay, has been able to appeal to the "endorsement" given to her by the "scientific tests" applied to her by "Prof. Crookes and other Fellows of the Royal Society," which had been published (I now find) by Mr. Crookes himself in the *Spiritualist* in March, 1875. Within two months of that date, as Mr. Maskelyne has publicly stated, an offer was made him (I have myself seen copies of the letters) by Eva Fay's manager, that for an adequate sum of money the "medium" should expose the whole affair, scientific tests and all, "*complicating at least six big guns, the F.R.S. people,*" as she was not properly supported by the Spiritualists.

I have therefore felt it incumbent on me to show that in dealing with this subject Messrs. Crookes and Wallace have followed methods which are thoroughly *un*-scientific; and have been led by their "prepossession" to accept with implicit faith a number of statements which ought to be rejected as completely untrustworthy.

My call to take such a part—which I would most gladly lay aside for the scientific investigations which afford me the purest and most undisturbed enjoyment—seems to me the same as is made upon every member of the Profession to which I have the honour to belong, that he should do his utmost to cure or to mitigate *bodily* disease. The training I originally received, and the theoretical and experimental studies of forty years, have given me what I honestly believe (whether rightly or wrongly) to be a rather unusual power of dealing with this subject. Since the appearance of my Lectures I have received a large number of public assurances that they are doing good service in preventing the spread of a noxious *mental* epidemic in this country; and I have been privately informed of several instances, in which persons who had been "bitten" by this malady, have owed their recovery to my treatment. Looking to the danger which threatens us from the United States, of an importation of a real spiritualistic *mania*, far more injurious to our *mental* welfare, than that of the Colorado beetle will be to our *material* interests, I should be untrue to my own convictions of duty if I did not do what in me lies to prevent it. That I do not take an exaggerated view of the danger, will be obvious to any reader of Mr. Home's book. I know too well that I thus expose myself to severe obloquy, which (as I am not peculiarly thick-skinned) will be very unpleasant to myself, and unfortunately still more so to some who are nearly connected with me. But I am content to brave all, if I can believe that my *exposé* will be of the least service either to individuals or to society at large. W. B. CARPENTER

THE high scientific position which Prof. Foster holds, as well as the decided manner in which his letter was written, must lead the otherwise unbiassed reader to the conclusion that not only has a satisfactory explanation of the action in question been found and generally adopted, but that this explanation turns upon certain considerations, and particularly on the mean length of the path of the gaseous molecules as influenced by the degree of rarefaction.

I feel my position, therefore, particularly unfortunate in having, for the sake of truth, to show that the explanation which Prof. Foster has adopted, and supposes others to have adopted, is, if judged by the statements in his letter, inconsistent with well-established laws.

Prof. Foster gives me credit for having originated the fundamental idea of the explanation, but states that my "explanation was theoretically incomplete; in particular it did not show clearly why so high a degree of rarefaction should be necessary for the production of the phenomenon in question;" and then he proceeds to explain how this asserted deficiency was supplied by other thinkers, who showed that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules, would favour the action."

It is this supposed completion of my explanation that is erroneous. It is contrary to the law of the diffusion of heat in gases that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action," and so far from supplying any deficiency in my explanation it is incompatible with it. The only result from such an increase is to diminish the action—a result which rises into importance only when the rarefaction is carried so far that the mean length of the path of a molecule becomes comparable with the dimensions of the inclosing vessel.

In my first paper I gave a definite proof, which has nowhere been questioned, that according to the kinetic theory the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional. If therefore the increased rarefaction increased the force it must increase the rate at which heat is communicated, but according to the law established by Prof. Maxwell the rate at which heat is communicated is independent of the density of the gas, whence it follows that the increase in the mean length of the path of the gaseous molecules, resulting from rarefaction, cannot favour the action which remains approximately constant until the gas becomes so rare that the law of diffusion no longer holds, after which it may easily be shown the communication of heat, and hence the action in question, diminishes but never increases.

The fact that in the radiometer the force caused by the communication of heat only causes motion when the surrounding gas becomes extremely rare is, as I pointed out in my first papers, fully explained by the action of what I have called convection currents, which action depends on the weight and density of the gas. The gas adjacent to the hot surface is hotter than that which is more remote, and hence the former rises forming an ascending column, to supply which the gas is drawn in laterally on all sides, and tends to carry the surface forward with it. With the same difference of temperature and surrounding circumstances the speed of these convection currents is the same whatever may be the density of the gas, and hence the force which they exert on the surface is proportional to the density of the gas.

This force is opposite in direction to that arising from the communication of heat to the gas, and since the former diminishes with the density while the latter is constant, there must be some density for which they balance, and below which the constant force will predominate, while above this point the convection currents will carry the surface with them. The fact that, starting from low densities, the motion of the vanes in the radiometer does not only diminish as the density increases, but is actually reversed at higher densities, requires explanation, and no other than this has yet been offered.

I have gone into the subject at considerable length, as I felt bound, when venturing to differ from so high an authority as Prof. Foster, to state my reasons. There is, however, nothing in what I have said here which I have not said elsewhere, in the same or other words; and however incomplete in theory the explanation given in my first papers may be, I can only say that it included all the facts known to me at the time these were written; it has led me to predict many of the experimental results which have since been obtained, and I have not been able to find one fact with which it is not in accordance, nor has it been, so far as I am aware, controverted in any particular.

OSBORNE REYNOLDS

Potential Energy

I HAVE reason to believe that the "grievous error" with which I charged "John O'Toole" in his reference to the clock is not meant by him to be his own view of the matter at all, but merely a legitimate deduction from the confused and inconsistent language of "the doctors." Such an erroneous view on his part is, indeed, obviously out of harmony with the extensive knowledge of the subject of energy displayed by him in letters which, without doubt, will convince "the doctors" of the necessity of adopting consistent and strictly logical phraseology.

G. M. MINCHIN

Royal Indian Engineering College, Cooper's Hill

Effects of Urticating Organs of Millepora on the Tongue

AN article by Mr. Moseley, in NATURE (vol. xvi. p. 475), reminds me of an experiment I made some years ago in Florida. In collecting corals on the reefs, I had of course become familiar with the disagreeable, though not very painful, effects of contact of the hands with Millepora. But the vulgar names of Pepper-coral or Sea ginger induced me to try the effect on the tongue, to find out how far the taste resembled those condiments. I accordingly broke off a fresh piece and applied it to the tongue. Instantly a most severe pain shot, not only through that organ, but also through the jaws and teeth. The whole course of the dental nerves and their ramifications into every single tooth could be distinctly and painfully felt. I can compare the sensation to nothing better than to the application of the poles of a pretty strong galvanic battery. The pain remained severe for about half an hour, then became duller, leaving a sensation still perceptible five or six hours later. The whole impression was much too violent to allow the distinction of any particular taste.

Such an experiment made with Physalia might be positively dangerous, considering the much more powerful urticating effects of its polyps. Indeed, a friend of mine once related to me that when a boy he had come in contact with one of the long tentacles of a Physalia, when bathing, and had to be carried out of the water almost fainting. L. F. POURTALES

Cambridge, Mass., October 22

Drowned by a Devil Fish

THE following account of the destruction of a human being by a cuttle fish at Victoria, in Vancouver Island, has all the appearance of authenticity about it. It occurs in the *Weekly Oregonian* of October 6, 1877. The *Oregonian* is the principal paper of Oregon, and is published at Portland.

The insertion of the account in NATURE may lead to further information on the subject. I know of no other authentic instance of the kind.

An account of the habits of the huge octopus of the Vancouver Island Sounds and also of the Indian method of hunting and killing the beasts for food is to be found in John Keast Lord's "Naturalist in Vancouver Island and British Columbia," vol. i. p. 192. Mr. Lord measured specimens which had arms five feet in length, with a thickness at their base as great as his wrist, and he once collected a detached sucker of one of these cephalopods as large as an egg cup in mistake for a huge actinia.

"BRITISH COLUMBIA

"*Drowned by a Devil Fish*

"VICTORIA, September 27.—An Indian woman while bathing was pulled beneath the surface of the water by an octopus or devil fish and drowned. The body was discovered the following day in the bottom of the bay in the embrace of the monster. Indians dived down and with their knives severed the tentacles of the octopus and rescued the body. This is the first recorded instance of death from such a cause in this locality, but there have been several narrow escapes."

Exeter College, Oxford H. N. MOSELEY

The Earthworm in Relation to the Fertility of the Soil

IN NATURE, vol. xvii., p. 18, there is an account under the above heading of M. Hensen's investigations of this subject, to which I wish to add a note. He says the assertion that the earth-worms gnaw roots is not proved by any fact; roots gnawed by worms were never met with by him, and the contents of the intestines of the worms never included fresh pieces of plants. The experience of gardeners that the earth-worm injures pot plants may be based on the uncovering or mechanical tearing of the roots.

I should have thought that the universal experience of gardeners is that earth-worms never eat vegetable matter until it has decayed, and that their instinct leads them to draw the points of leaves as far as they can into their tubes for the purpose of setting up the decaying process, and likewise to sever the roots of pot plants with the same object. I can hardly understand how earth-worms have any mechanical means of severing the roots of plants except by gnawing.

But there is an omission in M. Hensen's account of the fertilisation of the subsoil by earth-worms which surprises me. He mentions but two ways in which this is effected, viz., by the opening of passages for the roots into deeper parts, and by the lining of these passages with humus.

I thought it was a well-known fact that worms, by means of their "casts," effect a complete *renversement* of the soil of meadow land down to a certain depth in the course of a few years. But whether well-known or not I met with a demonstration of this important fact in 1857. When putting down a considerable extent of iron fencing in the alluvial meadows near my house (consequent upon an exchange of land) I had occasion to cut a ditch two or three feet deep, and when the workmen had finished the ditch—a quarter of a mile long in all—I was astonished to see in one portion, of about sixty yards in length, a distinct and very even narrow line of coal-ashes mixed with small coal in the clean cut surface of the fine loam of the ditch face, perfectly parallel with the top sward. It immediately occurred to me that this was the work of the earth-worms, and upon inquiry I found that the farmer, who had occupied this land for many years, remembered having once, and only once, carted out some coal-ashes and spread it at this spot not many years before. I forget the exact number of years, but I believe it was about eighteen. I have a distinct recollection, however, that the depth of the line of coal-ashes below the surface was at least seven inches, and that this seemed to confirm the general belief that the depth to which the earth-worm usually burrows is about that amount. I may add that the colour of the loam above the line of coal-ashes was decidedly darker than of that below.

HENRY COOPER KEY

Stretton Rectory, Hereford, November 2

IN NATURE, vol. xvii. p. 18, some details are given of observations made by M. Hensen on the relation of the earthworm to the fertility of the ground. He has observed, as everyone must have observed, that the earthworm during night draws into its tube or hole the loose leaves and fibres which may be lying about. But this operation of the earthworm has a significance in relation to the vegetable world of even a profounder kind than that of the fertilisation of the soil. Some months ago, in searching for young ash plants with three cotyledons, I found that in a great many cases the samara or seed of the ash had been drawn into a worm's hole, and had there found moisture and other essential conditions of growth; while the same seeds lying dry upon the surface had not germinated. There can thus be no doubt that many seeds of all kinds are drawn under the surface of the ground, or covered by the earth thrown up by worms. They are thus preserved from birds and various enemies, and are placed in the proper position for germination. The dead plant is perpetuated from its fallen panicle by the earthworm. An ash tree, or a whole forest of ash trees, may have been planted by earthworms.

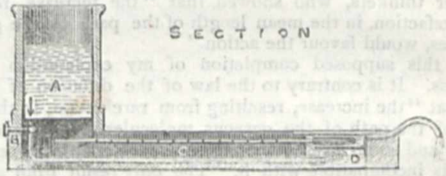
North Kinmundy, November 5 A. STEPHEN WILSON

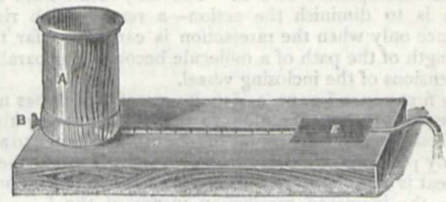
M. Alluard's Condensing Hygrometer

THE notice of the above instrument in last week's NATURE (p. 14) is an excellent illustration of the necessity for increased communication between the scientific men of all countries. The labour which is at present wasted by repeating what has been done before is enormous, and until international intercommunication is improved it must be so.

I quite agree with you in your appreciation of M. Alluard's hygrometer, but I think it is desirable to state that it is not the first in which "the part on which the deposit of dew is to be observed is a plane well-polished face A, of silver or gilt brass." The annexed engravings represent the form of plane-faced hygrometer invented by Mr. G. Dines, F.M.S., described by him in the *Meteorological Magazine* for October, 1871, and exhibited at the Brighton Meeting of the British Association, 1872.

The action is extremely simple; no ether is required nor any aspirator. Water colder than the dew point is the only requisite—it is poured into the reservoir A, passes through the regulating-tap B into the chamber D; it is, by the black diaphragm, thrown past the bulb of the thermometer C, and then allowed to escape. The cooled plane surface E of silver or black glass, is excessively thin, and the space between it and the thermometer-bulb is wholly occupied by the effluent water, so that the great essential





of all hygrometers, a true indication of the temperature of the cooled surface, seems to be reached. The plate E can be kept within $0^\circ\cdot2$ or $0^\circ\cdot3$ for a length of time by adjusting the screw B, and as the condensation usually takes an elliptical form over the thermometer-bulb, and in the middle of E, the advantage of an adjacent bright surface is usually attained. I am, however, not sure that M. Alluard's surrounding plate might not be a convenience, although for the reason above given I have not found it necessary. G. J. SYMONS

62, Camden Square, N.W., November 2

Optical Spectroscopy of the Red End of the Solar Spectrum

NATURE, dated August 2 (vol. xvi. p. 264), containing Prof. Piazzi Smyth's communication on "Optical Spectroscopy of the Red End of the Solar Spectrum," reached me on the 21st ult., when I had no leisure to avail myself of the outgoing mail and reply immediately to the subject of his last paragraph. Inquiry is there made of "anyone" (besides the Royal Society), in association more or less with my name, whether *more recent particulars* have been published, of the spectrum in question, than "those (*i.e.* my) Indian observations," "printed in the *Philosophical Transactions* so long ago as 1874" (*i.e.* 1875).

2. The Astronomer-Royal for Scotland is presumably in a better position to reply for "any one," than myself, located in latitude N. 30°, longitude E. 78°; and so far as the inquiry relates to the Royal Society, his penultimate paragraph in itself furnishes the information sought, because the Society's publica-

tion prominently alluded to by himself *is the last* publication. As respects myself, I have printed no further particulars in addition to those which the Professor dismisses, briefly for the present, with the announcement of having discovered, "total contradictions" to certain "conspicuous features."

3. It is necessary to point out, that the designation for my observations adopted by the Professor of "the Royal Society's and Mr. Hennessey's high-sun series" suggests existence of the *divided* responsibility which is plainly disavowed in the "Advertisement" to the *Philosophical Transactions*, 1875, Part I., and elsewhere; for the professor can hardly intend that two separate and independent high-sun series taken on the Himalaya Mountains, one by the Royal Society, and the other by myself, have appeared in the *Transactions*.

4. I shall look forward with interest to the perusal of Prof. Piazzi Smyth's promised *complete* account of his sun-high observations at Lisbon; meanwhile I may be pardoned for my inability to follow his prompt and brief announcement of "total contradictions," written while yet on his return voyage.

J. B. N. HENNESSEY

N.W. Provinces, India, Dehra Doon, October 3

Singing Mice

PERHAPS the following account of a singing mouse may be of interest to your readers :—

Last winter we occupied the rooms we now do at Menton. Early in February we heard as we thought the song of a canary, and fancied it was outside our balcony; however we soon discovered that the singing was in our *salon*, and that the songster was a mouse; at that time the weather was rather cold, and we had a little fire, and the mouse spent most of the day under the fender, where we kept it supplied with bits of biscuit; in a few days it became quite tame, and would come on the hearth in an evening and sing for several hours, sometimes it would climb up the chiffonier and ascend a vase of flowers to drink at the water, and then sit and sing on the edge of the table and allow us to go quite near to it without ceasing its warble; one of its favourite haunts was the wood basket, and it would often sit and sing on the edge of it. On February 12, the last night of the carnival, we had a number of friends in our *salon*, and the little mouse sang most vigorously much to their delight and astonishment and was not in the least disturbed by the talking. In the evening the mouse would often run about the room and under the door into the corridor and adjoining rooms, and then return to its own hearth; after amusing us for nearly a month it disappeared, and we suspect it was caught in a trap set in one of the rooms beyond. The mouse was small and had very large ears, which it moved about much whilst singing; the song was not unlike that of the canary in many of its trills, and it sang quite as beautifully as any canary, but it had more variety, and some of its notes were much lower, more like those of the bullfinch. One great peculiarity was a sort of double song, which we had now and then—an air with an accompaniment; the air was loud and full, the notes being low and the accompaniment quite subdued. Some of our party were sure that there was more than one mouse until we had the performance from the edge of the wood basket, and were within a yard or two of it. My son has suggested that many or all mice may have the same power, but that the notes are usually so much higher in the scale that, like the cry of the dormouse and the bat, they are at the verge of the pitch to which the human ear is sensitive; this may be so, but the notes of our mouse were so low and even the highest so far within the limits of the human ear, that I am inclined to think the gift of singing in mice is but of very rare occurrence.

JOSEPH SIDEBOTHAM

Hotel de Menton, Menton, S. France, October 31

SEVERAL years ago I received some of these animals from a friend, and kept them in confinement for one or two months. The description which your correspondent gives of their performance leaves very little to be added by me, as in all respects this description agrees perfectly with my own observations. I write, however, to remark one curious fact about the singing of these mice, namely, that it seemed to be evoked by two very opposite sets of conditions. When undisturbed, the little animals used for the most part to remain quiet during the day, and begin to sing at night; but if at any time they were alarmed, by handling them or otherwise, whether during the day or night, they were sure to sing vigorously. Thus the action seemed to be occasioned either by contentment or by fear. The character of the song, however, was slightly different in the two cases.

That these mice did not learn this art from singing birds there can be no doubt, for they were captured in a house where no such birds were kept. It may be worth while to add that this house (a London one) seemed to have been suddenly invaded, so to speak, by a number of these animals, for although my friend has lived in this house since the year 1862, it was only during a few months that singing mice were heard in it, and during these few months they were heard in considerable numbers.

Regent's Park, November 1 GEORGE J. ROMANES

Meteor

THE following account of a meteor seen here may perhaps interest some of your readers :—

On October 29, at 8h. 1m. 30s. Greenwich mean time, a brilliant meteor exploded in right ascension 268°, declination + 60° (equator of 1855); it left a bright crooked train scarcely half a degree long, which remained visible for about ten seconds, and pointed towards ξ Draconis. The course of the meteor must have been directed downwards, almost exactly towards this observatory. The flash of the explosion was seen by the assistant-astronomer, Mr. Lohse, although he was sitting in such a position as to be unable to see the meteor directly.

Lord Lindsay's Observatory, RALPH COPELAND
Dunecht, Aberdeen, November 3

INTERNATIONAL POLAR EXPEDITION

IN February, 1875, when the Arctic Expedition was being prepared, I asked the First Lord of the Admiralty, in Parliament, whether, in view of the small value for scientific purposes of isolated observations in the Arctic regions, in comparison with simultaneous observations at different places, and in view, also, of the interest now taken in Arctic science by foreign Governments, he would postpone for one season the departure of the proposed Arctic Expedition, and in the interval communicate with foreign Governments with a view to the organisation of other expeditions to make observations simultaneously with our own at fixed times? The First Lord said that he considered the preparations for an expedition too far advanced to admit of this, and added: "I should regard the project of combination with other powers to attain the objects in view as one beset with difficulties"—in which, I think, he was in error. In the following month, when the Supplementary Estimate for the Arctic Vote was under discussion, I again drew the attention of the Government and Parliament to the advantages of simultaneous Arctic expeditions (see *Hansard*, vol. ccxxii. p. 1354), and in *Naval Science* for April of the same year, in an article on "Foreign Polar Expeditions," I drew still further attention to the matter, concluding with an extract from a paper by Capt. Weyprecht (who so greatly distinguished himself in the Austro-Hungarian polar expeditions of 1871 and 1872–74), in which he pointed out in the clearest manner the desirability of extending future Arctic researches far beyond mere geographical exploration, and pressing forward with our studies of magnetism, electricity, the best of meteorology, &c. "The solution of these questions cannot," he said, "be expected until all nations which claim to come up to the present high standard of civilisation unite to go hand in hand, setting aside all national rivalries. To bring about decisive scientific results it will be necessary to make a number of simultaneous observations, so conducted that they will furnish a yearly *résumé* of observations made in different parts of the Arctic regions with exactly similar instruments, and from exactly similar instructions."

Upwards of a year ago NATURE gave details of Weyprecht's project for the scientific exploration of the Polar regions. It was referred to on several occasions, and pointed out that Weyprecht's plan was the only satisfactory method of obtaining results of real and permanent value.

The programme has now been extended and completed, and was prepared for submission to the International Meteorological Congress which was to have met at Rome in September, but which has been adjourned to next year. I have just received from my friend Weyprecht a copy, and may summarise its contents as follows :—

The enterprise proposed by Count Wilczek and Capt. Weyprecht has for its aim strictly scientific exploration, purely geographical discovery being a secondary matter. It will be the first step towards a systematic scientific investigation of the regions around the poles of the earth and the minute observation of phenomena peculiar to these regions—phenomena the earnest investigation of which is of the highest importance in connection with a great number of problems with regard to the physics of the globe. The international expedition will have for its aim to make in the Arctic and Antarctic regions, or in the neighbourhood of these regions, and at as many stations as it is possible to establish, synchronous observations according to a programme mutually agreed upon ; for the purpose, on the one hand, of deducing by comparison from observations collected at different points, independently of the peculiarities which characterise the years of different observations, the general laws of the phenomena investigated ; and, on the other hand, of arriving by probable inductions at a knowledge of the chances of penetrating further into the interior of the unknown regions. For this purpose each of the states participating in the work will undertake to equip at its own expense, and send out an expedition to one of the points designated. Each state will of course be at liberty to authorise its expedition to carry on work outside of that mutually agreed on.

The investigations to be made in common bear only on meteorological phenomena, those of terrestrial magnetism, aurora borealis, and on ice phenomena. At each station the observations must be continued one year, from September 1 to August 31. The meteorological observations will be made in conformity with the resolutions of the permanent International Committee, and will relate to atmospheric pressure, the temperature and humidity of the air, the direction and force of the wind, the state of the sky and its degree of clearness, and also to phenomena of condensation. The programme then gives detailed instructions as to methods and instruments of observation, all being arranged to secure accuracy, fulness, and uniformity.

It is probable that each station will be near a coast, and one of the chief objects of the expedition will be to observe the connection between the movements of the ice and the winds and currents, and if these are observed regularly, important results will no doubt be obtained as to the movements of the ice in the Arctic regions, and therefore as to the routes most favourable for reaching the pole. The best ice-observations will of course be at those stations where local conditions have the least influence.

The magnetic observations are divided into absolute determinations and determinations of the three elements. Minute directions are given in the programme as to the method to be followed in taking these observations, the fixing of the positions of the various instruments, the kinds of instruments to be used, the methods of verification and testing, the construction of observatories, &c. These directions, if faithfully carried out, would give the observer plenty of work to do, but the result would be of unprecedented value. In consequence of the persistent perturbations which prevail in these regions, isolated readings made only from hour to hour, even when carried on for long periods, are not sufficient to give with precision the hourly, daily, and monthly magnetic character of the place of observation. It is necessary, consequently, to multiply these observations. Ten observations per hour for each of the three elements will be sufficient, and to insure a rigorous synchronism it is stipulated that the three instruments of variation be read during ten minutes, from minute to minute, viz., at the full minute (– h. 56m. os.) the declination, ten seconds after (– h. 56m. 10s.) the horizontal intensity, and ten seconds after that (– h. 56m. 20s.) the inclination. Before and after each observation, viz., – h. 52m. os., and at – h. 69m. os. the form and position of the auroræ should be noted. Immediately after the meteorological observations should be proceeded with in the following order :—Temperature, humidity, winds, clouds, atmospheric pressure. (For magnetic observations it is proposed to use Göttingen mean time.) Besides observations of the regular magnetic variations, it will be of great importance to have made, by three observers, rigidly synchronous readings of the three elements in order to obtain precise data of the total intensity. For this purpose there will be made, during one hour each day, by these observers, from minute to minute, from – h – m. os., readings of the three instruments. The hours of these observations should be advanced an hour each day, so as to return to the point of departure at the end of every twenty-four days.

The auroræ should be observed as to their form, their intensity, and their position. The programme then names and describes the various forms assumed by auroræ—arches, streamers, beams, corona borealis, haze, waves, flashes—for the adequate and scientific observation of which the programme gives directions.

The most favourable time for this joint expedition will be October and November, when the temperature is not so low as to necessitate special preparations.

As the absolute simultaneity of the observations is of the utmost importance, each station must be furnished with the means of obtaining the exact longitude ; good chronometers will also be necessary. To carry out the above observations to their fullest extent, four observers will suffice for each station, if among the subordinates there are men who can perform the purely mechanical duty of reading the instruments.

The programme concludes with three propositions, the purpose of which is to insure the possibility of the exact comparison of the magnetic observations.

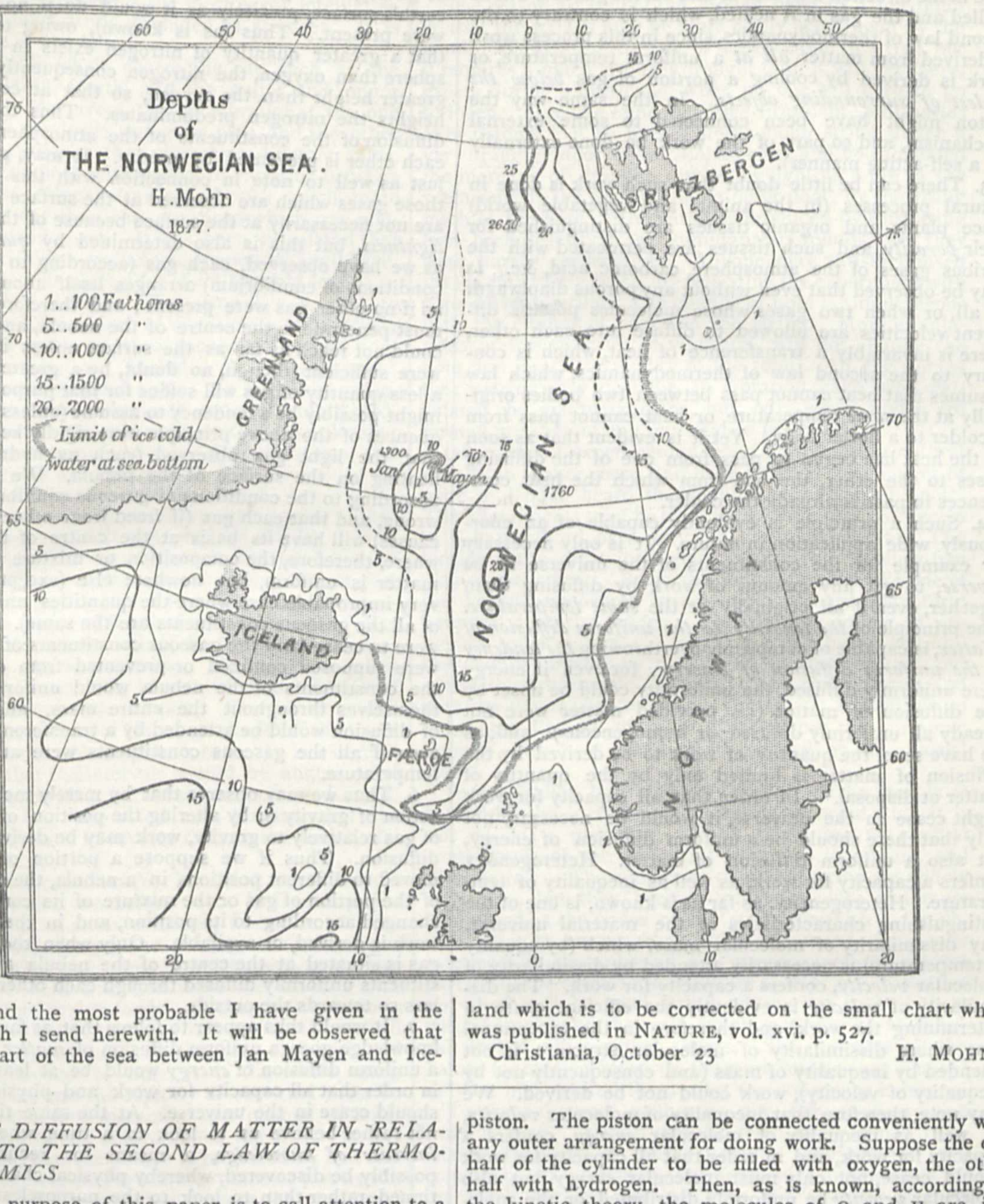
The following are the points proposed as most favourable for the various observations referred to above :—In the northern hemisphere—The north coast of Spitzbergen ; north coast of Novaya Zemlya ; Finmark, near the North Cape ; the mouth of the Lena, on the north coast of Siberia ; New Siberia ; Point Barrow, on the north-east of Behring Strait ; the west coast of Greenland ; the east coast of Greenland, about 75° N. lat. In the southern hemisphere—The neighbourhood of Cape Horn ; the Kerguelen or Macdonald Islands ; one of the groups south of the Auckland Islands.

I wish that in the influential pages of NATURE this great international scientific subject could be again urged. I cannot help thinking that in the present Hydrographer of the Navy we have an officer who would be at once most able and willing to take part in giving, in the way suggested, true scientific direction and scope to future Arctic research. My confidence in the great value of simultaneous observations in comparison with the meagre results of isolated expeditions must be my apology for thus writing.

E. J. REED

THE NORWEGIAN DEEP-SEA EXPEDITION

FROM soundings taken by the second German Polar Expedition, and kindly communicated by Capt. Koldewey, of Hamburgh, I have been induced to alter my views about the configuration of the sea-bottom around Jan Mayen. The figure of the bottom which I at



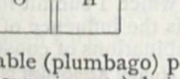
present find the most probable I have given in the chart which I send herewith. It will be observed that it is the part of the sea between Jan Mayen and Iceland which is to be corrected on the small chart which was published in NATURE, vol. xvi. p. 527.

Christiania, October 23 H. MOHN

ON THE DIFFUSION OF MATTER IN RELATION TO THE SECOND LAW OF THERMODYNAMICS

1. THE purpose of this paper is to call attention to a natural process that appears to constitute an exception to the second law of thermodynamics, and which, if noticed by others, would at least appear from its importance to merit a more general recognition. The subject may be best dealt with by means of a simple illustration, the principles involved in the action of which are already perfectly well known.

2. Let the annexed figure represent a cylinder, contain-



ing a piston, P ; a suitable (plumbago) porous diaphragm (as used for diffusion experiments) being fitted into the piston. The piston can be connected conveniently with any outer arrangement for doing work. Suppose the one half of the cylinder to be filled with oxygen, the other half with hydrogen. Then, as is known, according to the kinetic theory, the molecules of O and H are impinging continually against the porous partition or diaphragm, P, and the molecules in their impacts thus occasionally encounter vacant spaces or pores, and so continue their motion on across the diaphragm into the opposite compartment. Owing, however, to the fact that the molecules of hydrogen are moving four times as fast as the molecules of oxygen, they strike the diaphragm correspondingly more frequently, and thus four times as many hydrogen molecules pass through into division O, as oxygen molecules pass through into division H. [The piston is supposed fixed at present, so that no work being done, there is consequently no heating or cooling of the gas.] But on account of the excess of molecules passing into division O, the pressure there will rise. If, then, after the pressure has risen to a certain degree, the piston be suddenly released, it will be driven by the excess of pressure in the direction O H, and in that act the gas in O will be chilled and the gas in H heated, which is contrary to the second law of thermodynamics, since in this process work is derived from matter *all at* a uniform temperature, or work is derived by cooling a portion of gas *below the coldest of surrounding objects.* In the same way the piston might have been connected to some external mechanism, and so part of the work be done externally (in a self-acting manner).

3. There can be little doubt that such work is done in natural processes (in the animal and vegetable world) since plants and organic tissues are distinguished for their *porosity*, and such tissues are permeated with the various gases of the atmosphere, carbonic acid, &c. It may be observed that even without any porous diaphragm at all, or when two gases whose molecules possess different velocities are allowed to diffuse into each other, there is invariably a transference of heat, which is contrary to the second law of thermodynamics, which law assumes that heat cannot pass between two bodies originally at the *same* temperature, or heat cannot pass from a colder to a hotter body. Yet it is evident that as soon as the heat has begun to pass from one of the diffusing gases to the other, the one from which the heat commences to pass is already the colder.

4. Such a principle is evidently capable of an enormously wide application in nature. It is only necessary for example for the constituents of the universe to be *diverse*, to get any amount of work by diffusing them together, even if all originally at the *same temperature.* The principle of *the tendency to the uniform diffusion of Matter*, is capable of completely overthrowing *the tendency to the uniform diffusion of Energy;* for even if energy were uniformly diffused, the uniformity could be upset by the diffusion of matter (*i.e.* provided matter were not already all uniformly diffused or homogeneous): and, as we have seen, the quantity of work to be derived by the diffusion of matter is limited only by the quantity of matter at disposal.[1] In order that all capacity for work might cease in the universe, it would be necessary not only that there should be a uniform diffusion of energy, but also a uniform diffusion of matter. Heterogeneity confers a capacity for work, as well as inequality of temperature. Heterogeneity, as far as is known, is one of the distinguishing characteristics of the material universe. Any dissimilarity of molecular mass, which (by equality of temperature) is necessarily attended by dissimilarity of molecular *velocity*, confers a capacity for work. The dissimilarity of *velocity* is evidently the efficient cause in determining the work, and therefore in the exceptional case where dissimilarity of molecular structure is not attended by inequality of mass (and consequently not by inequality of velocity), work could not be derived. We may note, therefore, that inequality of molecular *velocity*, as well as inequality of molecular *energy*, confers a capacity for work, and in order that all capacity for work should cease, not only must molecular *energy*, but also molecular *velocity* be uniformly distributed, or the molecules of matter which (by equality of temperature) possess unequal velocities, must be uniformly diffused.

5. We may observe that gravity which does not interfere with the uniform diffusion of *energy*, does interfere with the uniform diffusion of *matter*. Thus, for example, the energy (heat) of the atmosphere tends to be uniformly diffused throughout a vertical column of the atmosphere, in spite of the action of gravity. But the uniform diffusion of matter (*i.e.*, the uniform mixture of the gases of the atmosphere through each other) is prevented by gravity. For by the well-known law of Dalton (which accords with the result of the kinetic theory of gases), each gas arranges itself as a layer upon the earth's surface, precisely as it would do if no other gas were present. Thus (as is known), owing to the fact that a greater quantity of nitrogen exists in the atmosphere than oxygen, the nitrogen consequently rises to a greater height than the oxygen, so that at considerable heights the nitrogen predominates. Thus the uniform diffusion of the constituents of the atmosphere through each other is prevented by gravity. It may, perhaps, be just as well to note in connection with this point that those gases which are observed at the surface of nebulæ are not necessarily at the surface because of their greater *lightness*, but this is also determined by *quantity;* for as we have observed, each gas (according to the known conditions of equilibrium) arranges itself about a centre as if no other gas were present; and therefore each gas must penetrate to the centre of the nebula, and therefore could not reach as far as the surface unless its *quantity* were sufficient (though, no doubt, by a greater lightness a less quantity of gas will suffice for that purpose). There might possibly be a tendency to assume (unless the consequences of the above principle were rigidly kept in view) that the light gas observed (such as hydrogen) was floating on the surface of the nebula. We know that according to the conditions of gaseous equilibrium this is wrong, and that each gas (if freed from other disturbing causes) will have its basis at the centre of the nebula, where, therefore, the composition or mixture of gaseous matter is uniform, but nowhere else (excepting in the very improbable case where the quantities and densities of all the gaseous constituents are the same). If gravity were to cease (and the gaseous constituents of the nebula were supposed confined or prevented from expanding), the constituents of the nebula would uniformly diffuse themselves throughout the entire mass, and this act of diffusion would be attended by a transference of heat, even if all the gaseous constituents were at the same temperature.

6. Thus we may observe that by merely modifying the action of gravity or by altering the position of a portion of gas relatively to gravity, work may be derived through diffusion. Thus if we suppose a portion of gas to be moved to different positions in a nebula, the constitution of the portion of gas or the mixture of its constituents is changed according to its position, and in these changes work is derived, or available. Only when the portion of gas is situated at the centre of the nebula are its constituents uniformly diffused through each other; less and less so towards the outside.

7. It would thus appear to follow that, as far as present knowledge goes, a uniform diffusion of *matter* as well as a uniform diffusion of *energy* would be at least required, in order that all capacity for work and physical change should cease in the universe. At the same time does it not rather behove us to look to a time when, through *increase of knowledge*, a means for recurrence may possibly be discovered, whereby physical change is continued, rather than to look to the purposeless end of a chaos of uniform temperature and uniform distribution of matter? Humboldt says relatively to this point (Preface to "Cosmos"): "I would therefore venture to hope that an attempt to delineate nature in all its vivid animation and exalted grandeur, and to trace the *stable* amid the vacillating ever-recurring alternation of physical metamorphoses, will not be wholly disregarded at a future age."

S. TOLVER PRESTON

[1] Since the first draft of this paper was written, I have been informed that the question of the quantity of work to be derived by diffusing gases has been treated of by Lord Rayleigh (*Phil. Mag.*, April, 1875), but he does not apparently mention the bearing of the case on the second law of thermodynamics.

MUSIC A SCIENCE OF NUMBERS[1]

THE subject which I submit for your consideration this afternoon is the influence of numbers in music, as in the various combinations of consonances and dissonances

[1] Read before the Musical Association of London, November 5, 1877, by W. Chappell, F.S.A.

which we hear every day, and to show how these are explained by the fundamental laws of the science.

Although music has appeared to many persons a difficult subject, it is really one of the most easily intelligible and one of the most firmly grounded of sciences. It is purely a science of numbers.

The consonances which charm the ear, such as the octave, twelfth, fifth, fourth, and the major and minor thirds, have two concurrent sets of vibrations; the one set produced by the lower string or pipe, and the other by the upper. Although they vibrate at different rates, yet there are periodical coincidences of vibration between them, and these coincidences sound with much more power upon the ear than the vibrations which are non-coincident, or sound apart. It has been calculated that two hammers striking simultaneously upon an anvil have, through the greater displacement of air, fourfold loudness, instead of merely double. The same law applies to musical sounds. Coincidence of vibration is more briefly expressed by its synonym, "consonance;" and all non-coincident vibrations are included in "dissonances," meaning only that they sound apart. In a musical sense, dissonance is the medium between concord and discord, running from one into the other; for, in the most pleasing intervals, there are some non-coincident vibrations, and when these become very numerous, they overpower all concord. This will be shown in the sequel.

Suppose we take one long pianoforte string or an organ-pipe. The lowest sound it can produce will be that of its whole length, and this may be made the foundation of an entire scale of consonant notes, for every aliquot part of the length, being such as will measure without any remainder, will be also a multiple of the vibrations of No. 1. Thus No. 2, the octave, is half the length and vibrates twice as fast as the whole string. No. 3, the so-called twelfth, or octave and fifth, is a third of the length of No. 1, and it vibrates thrice as fast. Then, if we sound No. 3 with No. 2 instead of No. 1, we throw off the lower octave and have the fifth only, or 3 to 2. It is essential for consonance that the intervals should be aliquot parts of No. 1, for if otherwise, we should only create discord. The musical law is expressed very simply, that the number of vibrations is in inverse ratio to the length of a string.

The scale of all consonances is called the harmonic scale, copies of which are before you. It is exemplified by string or pipe. Let us consider, first, the Æolian harp, on which the winds alone produce the consecutive sounds. The strings are tuned in unison, except the two outmost, one on each side, and those are covered with wire, and tuned an octave lower. When the wind blows quickly enough to sound the bass strings, which we will suppose to have tuned to C on the bass clef, with 128 vibrations in a second of time, it is the whole string which sounds first, and the rapidity of the wind must be doubled before the harp will sound any change of note, and that note will be the octave above the first. It has already been said that the octave is produced by half the length of a string, and that it vibrates twice as fast as the whole—but mark the coincidence between the music and consecutive numbers; 1 and 2 have no note between them, although the sound jumps from the whole length to that of the half! When the bass strings sound the half length they have divided themselves into equal halves by a node, and that node creates tension in opposite directions, the one ventral segment pulling, as it were, against the other. These self-forming nodes may be easily seen by daylight, and at night by throwing a light upon the string. They were shown at our first *conversazione* in these rooms by Mr. Spiller, and at the Edinburgh meeting of the British Association by Mr. Ladd. The gust of wind which sounds the octave, or half length of the bass strings of the Æolian harp, sounds at the same time the whole length of the gut strings, because they are tuned to that pitch. Then, as the wind rises, subdivision goes on in both with every multiple of 128 vibrations for the bass, and of 256 vibrations for the tenor strings.

The reason for tuning the Æolian harp to a low pitch is, that the strings may be more easily acted upon by the wind. We read, poetically, of hanging one in a tree, but it requires a much stronger draught than it will get there, except during a hurricane, when no one will care to go to listen. Our late lamented Vice-President, Sir Charles Wheatstone, F.R.S., fixed a single violin string under a very draughty door, as an Æolian harp, and he calculated the increase of draught caused by lighting a fire in the room, and by the opening of an outer door, by the rising pitch of the note. The varieties produced by this string have been described as "simultaneous sounds," but they were purely consecutive. Anyone may satisfy himself that it could only be so, by repeating the experiment with a good violin string. The change of note is simultaneous with the change of nodes in the string. Mere undulations, or irregularities of vibration, will not change the note, but injure the quality of the tone. All the curves that a string may describe in vibration have been calculated by mathematicians, but only when nodes are formed are they of any importance in music.

Often have I experimented upon harmonics or natural sounds, in former years, and have watched the changes of node, and have heard the simultaneous change of note. The experiments may be tried by any one who has access to a harpsichord, or a very old grand pianoforte. The tension is too great in modern instruments to allow free play to the string. Raise the damper and strike one of the longest uncovered strings with a hard pianoforte hammer near the bridge. The changes follow in numerical order, 1, 2, 3, 4, 5, as in the paper before you, and the sounds ascend by octave, fifth, fourth, major and minor third, harmonic seventh, to the third octave, and then to the major and minor tones. It is difficult to attain the highest of these numbers, but the harmonic seventh, No. 7, is readily distinguished by its unusual sound.

In the Æolian harp the rising pitch of the sounds is caused by the increasing rapidity of the wind; but it is not so on a pianoforte. It is there due to gradual contractions of the string till it ceases to vibrate, and sinks to rest. The vibrations of a long string are widely discursive, but they become gradually more and more contracted as the nodes of the string diminish in length. The point to be remarked is that the sounds jump over intermediate discords—all are consonances—all aliquot parts: all the sounds are multiples of No. 1. It matters not whether it be wind, string, or pipe; in each of them nature teaches us the scale which is to resolve all musical doubts, all disputed chords. She indicates all the basses for musical intervals, the more remote ones adapted only for melody, and the nearest for consonant harmony.

To prove the case further we may take an illustration from a pipe. It must not be from those which have lateral openings, or keys, because they shorten the column of air artificially, but from such instruments as the coach horn, or hunting horn, the so-called French horn, or the trumpet without valves.

The fundamental tone, No. 1, or lowest sound it can produce, is derived from the whole column of air within the tube. To produce No. 2 the rapidity of the breathing must be doubled, and then the column of air within the horn divides itself into two equal halves, and the sound is an octave above; so that, if the first note be tenor C with 256 vibrations in a second of time, this treble C requires to be blown at the rate of 256 vibrations to produce it. Here, again, we arrive at the identification of sounds with numbers; for, just as there is no intermediate number between 1 and 2, so is there no intermediate sound between 1 and 2, its double in vibrations, produced by half its length, upon the horn. The numbers run both ways. They are fractions as to length of tube, and multiples as to vibrations. Again, just as there *is* an intermediate number between 2 and 4 (the second octave), so is there one intermediate sound, and one only; it is No. 3, which is produced by a third of the length of the tube, and is the fifth above No. 2. The fifth and fourth divide the vibrations of the octave equally between them, so that the fifth is three times No. 1, and the fourth immediately above it is four times;—this, notwithstanding the diminution of the musical interval. The names which we have adopted for musical intervals are usually calculated from the keynote, as from C to E a third, from C to F a fourth, and from C to G a fifth, but these names are not real quantities, and are rather confusing than an assistance. The octave is not an eighth, but half, and the double octave is not a fifteenth, but a quarter of the length of No. 1, and vibrates four times as fast. Octaves are powers of 2, thus 2, 4, 8, 16, and 32 are successive octaves. But the octave 4 to 8 has only four sounds, and these are our major and minor third, and two others, divided by the harmonic seventh, which we do not use. From 8 to 16 are eight sounds, of which we use three, the major and minor tones, and the so-called diatonic semitone, as from B to C. It is really the smallest of the eight tones, and not a semitone. The next octave is from 16 to 32, and that is all of semitones, while 32 to 64 is all of quarter-tones. After that, the octave is divided into eighths, sixteenths, and thirty-second parts of tones, among which it is only useful to note (and that only among musicians and mathematicians, that the so-called "comma," having the ratio of 80 to 81, is the eighth of a tone above the third of any key—as it is above E in the key of C. We have lately had mathematicians among us who are not *μουσικοί*, and who have, therefore, proposed to divide an octave into "twelve *equal* semitones." This is pure geometry, and not music. In music there cannot be even two equal semitones within an octave. If our friends will only change their theme from twelve equal semitones into twelve *equally tempered* semitones, and give us their experience of the proposed sounds when heard *with the bass* (which seems not to have yet been taken into account), we shall gladly avail ourselves of their research, on the grounds of modern expediency. In the meantime we must be content to leave the tempering of a scale in the hands of experienced practical men, who, judging only by their ears, as they always will, have hitherto satisfied our immediate requirements.

The interval of a fifth is 2 to 3 in ascending and 3 to 2 in descending, but, as the figures are usually placed over the upper note in scales, the 3 is written above the 2 as in the scale in your hands (the third of them), where it appears over G, referring to C as 2.

And now for the practical use of these figures, for although the harmonic scale may be referred to, they are most easily remembered. All young pupils are taught the difference between an octave, a fifth, a fourth, and a third, upon the pianoforte, and it is only to associate the numbers with those intervals, to find out the best bass, and *every admissible bass*. All octaves are in the ratio of 2 to 1, whether it be 4 to 2, 8 to 4, or 16 to 8. All fifths are in the ratio of 3 to 2, all fourths in that of 4 to 3, all major thirds 5 to 4, and minor thirds 6 to 5.

For instance, in the key of C, C to the F above it is a fourth, and F is No. 4, therefore, the F, two octaves below, is the consonant bass; whereas, if we strike G with the C above, C becomes the natural bass to that interval. The most consonant basses are always found in the lowest numbers, because the proportion of consonant vibrations is there greatest. Thus, from D to G is also a fourth, in the key of C, but the numbers are 9 to 12, with a remote bass in C, and there will be 21 vibrations, of which only two will coincide in every cycle—1 of the 8, with 1 of the 9. Then, the proportion of non-coincidence will be so great as to make the sound unpleasing to the ear. But as 9 to 12 is in the *ratio* of 3 to 4, we have the best bass in these lowest numbers, and take G. By the various basses to intervals we modulate into other keys.

At the International Exhibition, held at South Kensington in 1862, Mr. Saxe, the eminent inventor of Saxe horns, exhibited an immense horn with an exceedingly long coil of tube, and perhaps standing six feet in height. When asked by the jury the object of this excesssive size and length, he answered, "C'est pour jouer dans le cinquième étage"—"It is for playing in the fifth octave," and he produced with facility any of the sixteen tones and semitones of that octave from it. Half the length of any open conical tube is expended upon its second note, the octave. No human power could have blown the low notes of that horn. Supposing it to have been tuned to the lowest C upon the pianoforte, with thirty-three vibrations in a second, as the usual French pitch, it would have had 66, 132, 264, and 528 for its first, second, third, and fourth octaves, while its fifth octave would commence on treble C, with 528, and extend to C above the lines with 1056 vibrations in a second of time. It would thus be within the power of the lungs. He utilized only from the 16th to the 32nd part of his enormous tube, but it gave him the command of the semitones.

This great incumbrance of length is not necessary in a cylindrical stopped tube. It will take up its own octave according to the ratio of its length to its diameter. We have here an example in a resonating tube invented by Charles Wheatstone just fifty years ago. The lecture for which he invented it was afterwards reported in the twenty-fifth volume of the *Quarterly Journal of Science, Literature, and Art*, January to March, 1828. Both he and I knew Eulenstein, an accomplished musician, whose admirable skill in playing upon the Jew's harp was the inducing cause of that particular lecture. Eulenstein had a peculiar facility for contracting and expanding the cavity of his mouth, through the pliability of his very thin cheeks and by the management of his tongue, so that he could fit them for any harmonic note within a certain compass. Wheatstone then gave the law, that a perfect harmonic scale might be drawn from a single tuning-fork, or from the vibrating tongue of a Jew's harp, by resonators adapted, or adapting themselves, to *multiples* of the original number of vibrations. "I took," said Sir Charles, "a tube, closed at one end by a movable piston, and placed before its end the branch [or prong] of a vibrating tuning-fork of the ordinary pitch—C. The length of the column of air [within the tube] was six inches. On diminishing the length of the column of air to three inches [by moving up the piston], the sound of the tuning-fork was no longer reciprocated [in unison], but its octave was produced." "It is therefore evident from experiments," says he "that a column of air may vibrate by reciprocation, not only with another body whose vibrations are isochronous [or in unison] with its own, but also when the number of its own vibrations is any multiple of the sounding body." Again, he says: "No other sounds can be produced by reciprocation from a column of air, but those which are perfectly *identical with the multiplications* of the original vibrations of the tuning-fork or the tongue of the Jew's harp." I produced the original tube in this room about two years ago, to check a recent theory—that resonators strengthened the ear, and answered only in unison, and Sir Charles ordered this one for me, made by Mr. Groves, under his own superintendence. The improvement in this is, that the piston now works in a groove and is not liable to stick. Two octaves are produced from the tongue of one Jew's harp as rapidly as the piston can be moved up and down. There is no slurring between one sound and another, but clear jumps from one multiple to another, and every one of them may be arrested and heard by itself by checking the piston. But, although I am glad to produce this tube before those who were not present on the last occasion, and to do honour to the memory of our eminent vice-president, who declined to refer in any way to himself, I have another motive also. This is a principle which has never been utilised. We have had pipes stopped at the top, like the usual pitch-pipe, but they have been found too slow in action to be suitable for any other purpose. This is rapidity itself, and might surely be utilised for some such purpose as pedal-pipes for an organ. The piston can be balanced outside to the greatest nicety, and one such pipe will take the scale of C, and another that of F. All that is required is to blow across the top in the manner of the Pandean pipes, or, as it appears, better still, to set free a fan or cogged wheel at the mouth tuned to each of the two fundamental notes. The wheel might be set free by the action of the foot upon the pedal. It is now well known that the length of a 32 or a 16 foot pipe may be greatly reduced by breadth of scale. We Europeans have made little, if any, use of resonators, and yet they have been long in use in Java. The drawing on the wall is of an instrument brought from Java by Sir Stamford Raffles more than half a century ago. There is one of the same kind in the British Museum. But this is perhaps of greater interest, as it may have suggested to Wheatstone the *principle* of the resonating tube. The natives of Java cast metal plates which they suspend in a row upon strings, and strike them with drum-sticks, which are fitted into circular heads. As all cast metal is more or less false in tone, owing to inequalities and lack of homogeneity, they place some of the largest bamboos, cut to short lengths, and placed upright, under the metal to make the true sounds of these resonators to overcome the false harmonics of the metal plates.

Resonators were used in the theatres of ancient Greece—we here find them used in Java; but these powerful auxiliaries to tone still await their development in modern Europe.

And now, in conclusion, permit me to draw your attento a harmonium with two keyboards, the upper one having four octaves of our scale tuned without tempering, and the lower with the five octaves of the harmonic scale, and the sixteen notes in the fifth octave. Much has been said of the harmonic scale, and this is perhaps the only instrument on which the harmonics can be fully heard and sustained for experimental use.

ROBERT SWINHOE, F.R.S.

WITHIN the last thirty years or so their respective vocations happen to have called two able lovers of natural history in the direction of the Celestial Empire—Mr. Robert Swinhoe, from England, and the Père Armand David, a Frenchman. The simultaneous investigations of these two biologists have added immensely to our knowledge of a country whose fauna not long ago was thought to be in no way interesting, because the huge population had succeeded in extirpating all the indigenous species. How far from the truth such an assumption is, has been demonstrated by the researches of the two naturalists above mentioned, the lamented death of the former of whom, at the early age of forty-one years, we recorded last week.

Mr. Swinhoe was born at Calcutta on September 1, 1836, and was educated at King's College, London, whence he matriculated at the University of London, in 1853. The next year he went, as supernumerary interpreter, to Hong Kong, being transferred to Amoy in 1855, and to Shanghai in 1858. In the same year he was attached to the Earl of Elgin's special mission to China, and afterwards to H.M.S. *Inflexible* as interpreter in a circumnavigating expedition round Formosa, in search of certain Europeans said to have been held in captivity at the sulphur mines on the island.

In 1860 Mr. Swinhoe attended Gen. Napier, and afterwards Sir Hope Grant, the Commander-in-Chief, as interpreter, and received a medal for war service. At the end of the same year he was appointed Vice-Consul at Taiwan, Formosa, and in 1865 to the full Consulship. In 1866 he was Consul, temporarily, at Amoy, and in 1868 went to explore the Island of Hainan. From May, 1871, to February, 1873, he was acting Consul at Ningpo, and at Chefoo until October of the latter year, when he had to retire from the service, on account of increasing paraplegia, from which he died on October 28 last.

Mr. Swinhoe was a Fellow of the Asiatic Societies of China and of Bengal, as well as of many other societies, having been elected into the Royal Society in 1876.

By far the majority of Mr. Swinhoe's scientific communications—fifty-two in number—mostly on the mammalia and birds of China, are to be found in the *Proceedings* of the Zoological Society of London between 1861 and 1874. Other papers appeared in the *Ibis* and the *Annals and Magazine of Natural History* within the same period. Among the most important of these are the "Catalogues" of the mammals and birds of China and its islands, in which are to be found descriptions of many new species of both classes, among which are St. John's Macaque (*Macacus sancti-johannis*), the Water Deer of Shanghai (*Hydropotes incruris*), the Mantchurian Deer (*Cervus mantchuricus*), the Orange-bellied Helictis (*Helictis subaurantiaca*), the Superb Flying Squirrel (*Pteromys grandis*), Boyce's Stork (*Ciconia boyciana*), together with a great number of other birds, for a complete account of which we cannot do better than refer our readers to a work upon the birds of China, by M. l'Abbé David and M. E. Oustalet, published at Paris a week ago.

Michie's Deer (*Lophotragus michianus*) is the name given by Mr. Swinhoe to a small deer from Ningpo, with antlers more diminutive than many other species. This, or a very closely-allied species, was previously sent to Paris by Père David, and described by M. A. Milne-Edwards under the name *Elaphodus cephalophus.*

Mr. Swinhoe, besides the collections which he made, was indefatigable and particularly successful in his endeavours to send living animals from China to this country, and there are many species, including *Cervus swinhoii*, *Hydropotes incruris*, and *Ciconia boyciana*, which were first procured by him.

It will be some time, we fear, before so enterprising a naturalist as Mr. Swinhoe takes up his residence in China, and employs every available opportunity for the prosecution of his favourite line of research.

DOUGLAS A. SPALDING

OUR readers must be familiar with this name as that of an occasional contributor to NATURE of thoughtful and acute articles in the department of mental science; they will be sorry to hear—but those who knew him will not be surprised—that Mr. Spalding died on October 31, at Dunkirk, just as he was preparing to go to the Mediterranean coast to spend the winter. Not much is known of Mr. Spalding's early life, but we are told by one who ought to know that his parents, belonging to Aberdeenshire, were in very humble circumstances, and that he was born in London about the year 1840. He himself spent his early years in Aberdeen as a working slater, doing his best to educate himself. By the kindness of Prof. Bain Mr. Spalding was allowed to attend the classes of Literature and Philosophy in Aberdeen University free of charge, in the year 1862. After that he got some teaching about London, and worked very hard to support himself, and even managed to keep his terms as barrister, though he never practised. It was during this period of privation that he contracted disease of the lungs, from which he suffered greatly up to the time of his premature death. The first thing that brought him to the notice of the scientific world was his experiments on the instinctive movements of birds, which were first described at the Brighton meeting of the British Association in 1872, and published in *Macmillan's Magazine* for February, 1873. From a series of interesting experiments on chickens he showed that the only theory in explanation of the phenomena of instinct that has an air of science about it is the doctrine of inherited association. Instinct, he maintained, in the present generation of animals, is the product of the accumulated experiences of past generations. In another paper at the Bristol meeting of 1875 he communicated the results of further experiments, some described in NATURE, vol. viii. p. 289, bearing out still more strongly the conclusions he had already reached, and which he summed up in the statement that "animals and men are conscious automata." The Brighton paper brought Mr. Spalding into deserved repute. While travelling in France he became acquainted with John Stuart Mill, and through him afterwards with many other distinguished men, who all treated Spalding with great respect. Through Mill also, we believe, he became acquainted with Lord and Lady Amberley, with whom he lived as companion and tutor to their children from 1873 until the death of Lord Amberley. Mr. Spalding was appointed guardian to the children, but was ultimately compelled to withdraw from this office owing to his religious opinions, Earl Russell, however, allowing him to retain for life the salary settled upon him by Lord Amberley. For the last two years Mr. Spalding has lived mostly in the south of France, bearing his fatal and protracting illness with the greatest equanimity, regretting only his powerlessness to work and his enforced absence from London.

As to the value of his scientific work our readers having the material before them are able to judge. By his experiments on animals he did much not only to clear up the nature of what is called instinct, but also to shed a new light on certain mental phenomena in man himself. His papers in NATURE, mostly reviews of works connected with psychology, on the metaphysics of instinct and evolution—of the latter doctrine he was a warm advocate—were good specimens of clear and close reasoning. That he had a tender side to his character is evident from even his Association papers, and still more so from the interesting letters written by him to NATURE, last April, on the swallows and cuckoos at Menton. All who knew him felt that had his health permitted he would have added largely to scientific knowledge in the special department to which he had devoted himself—physiological psychology.

OUR ASTRONOMICAL COLUMN

THE SOLAR ECLIPSE OF 1878, FEBRUARY 2.—The eclipse of the sun in February next will be annular, but the central line passes at such high southern latitudes that the annular phase is not likely to be observed unless it be in the western parts of Tasmania near sun-set. Thus the central eclipse will commence in longitude 103° 0′ west of Greenwich, latitude 73° 8′ south, and will end in longitude 149° 25′ east, latitude 40° 58′, and the eclipse is central at noon in longitude 112° 27′ west, and latitude 84° 3′ south. Another point upon the central line is in longitude 145° 25′ east, and latitude 42° 25′, where the sun's altitude, however, will be less than 4°; this point lies on the west coast of Tasmania. Launceston is near the central line, but at the middle of the eclipse the sun at that place is almost in the horizon.

A large partial eclipse will be visible over the southern parts of Australia. At Melbourne it will commence at 6h. 1m. P.M. local mean time, at 120° from the sun's north point towards the west, and will attain its greatest magnitude 0·91, just before sunset, or at 7h. 4. At Adelaide the eclipse will begin at 5h. 44m. local time and will be greatest about 6h. 45m., when the magnitude will be 0·85, with the sun at an altitude of between 5° and 6°. At Perth, in Western Australia, the whole eclipse will be visible; greatest about 5h. 25m. local time, magnitude 0·66, with the sun at an elevation of 23°.

The next *total* eclipse of the sun visible in those parts of the earth will take place on the morning of September 9, 1885. At Wellington, New Zealand, the eclipse begins about a quarter of an hour after sunrise; totality commences at 7h. 42m. A.M., but continues only about forty seconds; in 175° 3′ east, and 40° 34′ south, on the central line, the duration of totality is 1m. 54s. It should be stated that these figures are founded upon the tables of Damoiseau and Carlini.

THE MINOR PLANET EUPHROSYNE.—It does not frequently happen that we have to look for a planet at 60° of north declination; such, however, will be the case at the end of the present year, and in the first days of 1878 as regards Euphrosyne, No. 31 of the group, which was discovered by Ferguson at Washington, on September 1, 1854. The planet will be in opposition on December 18, with the brightness of a star of the tenth magnitude. The following are its calculated positions when passing its greatest northern declination.

12h. Berlin M.T.	Right Ascension. h. m. s.	Declination. ° ′ ″	Distance from the Earth.
1877, December 31 ...	5 20 17·1 ...	60 2 56 ...	1·613
1878, January 1 ...	5 18 49·5 ...	60 2 59 ...	1·614
,, ,, 2 ...	5 17 24·1 ...	60 2 38 ...	1·618

The star L. 10067 in Camelopardus, which Lalande calls an eighth, and Argelander a seventh, will be a good guide for identifying the planet in this position. At midnight at Greenwich on January 1, by calculation, Euphrosyne will precede the star seven seconds in R.A., seven minutes to the south of it.

The latest elements of this body which, it will be seen, approaches much nearer to the pole of the equator than the generality of the small planets, are as follows, according to the computations of Mr. S. W. Hill:—

Epoch 1877, December 18 0 M.T. at Berlin.

	° ′ ″
Mean Longitude	90 10 23
Longitude of Perihelion	93 17 30
,, Ascending Node	31 33 23
Inclination	26 28 34
Eccentricity	0·222786
Semi-axis major	3·14902

COMETS OF SHORT PERIOD IN 1878.—Of the comets known to be performing their revolutions in periods of less than ten years, two are due in perihelion again in the ensuing year, probably within a few days of each other. According to Dr. von Asten's elements of Encke's comet at its appearance in 1875, the next perihelion passage, neglecting perturbation, would fall about July 27·0, which involves an apparent track in the heavens unfavourable for observation. In 1845, when the conditions were more nearly the same than at any of the comet's returns since its periodicity was first ascertained, only four observations were secured between July 4 and 14—at Rome, Philadelphia, and Washington. If the comet is not observed before the perihelion in 1878, while at a considerable distance from the earth, it may be found at the observatories of the southern hemisphere, after perihelion, or in the latter part of August, when it makes its nearest approach to us, although its distance at that time will not be less than the mean distance of the earth from the sun. The second comet, which is due in perihelion in 1878, is that discovered by Dr. Tempel on July 1, 1873. The period of revolution assigned by Mr. W. E. Plummer from observations extending to October 20, is 1,850 days; and the comet, neglecting perturbations which are not likely to be material, would be in perihelion again about July 20; this date, however, will be uncertain, as thus far no definite discussion of the observations in 1873 has been published. Some time since it was stated that Herr Schulhof, of the Vienna Observatory, was at work upon this comet. With the above date for perihelion passage, the apparent path would be favourable for observations, and the comet would approach the earth almost as closely as is possible with the actual form of orbit.

NOTES

THE session of the Royal Society opens next Thursday with the Bakerian Lecture On the Organisation of the Fossil Plants of the Coal Measures, Part ix., which will be delivered by Prof. W. C. Williamson, of Manchester, F.R.S.

WE learn from the *Times* that the following is the list of the new Council which will be submitted to the Royal Society for election at their anniversary meeting on St. Andrew's Day next, the 30th instant:—President, Sir Joseph Dalton Hooker, C.B., K.C.S.I., M.D., D.C.L., LL.D.; Treasurer, William Spottiswoode, M.A., LL.D.; Secretaries, Prof. George Gabriel Stokes, M.A. D.C.L., LL.D., Prof. Thomas Henry Huxley, LL D.; Foreign Secretary, Prof. Alexander William Williamson, Ph.D.; other members of the Council—Frederick A. Abel, C.B., V.P.C.S., William Bowman, F.R.C.S., Frederick J. Bramwell, M.I.C.E., William B. Carpenter, C.B., M.D., LL.D., William Carruthers, F.L.S., William Crookes, V.P.C.S., Prof. P. Martin Duncan, M.B., P.G.S., William Farr, M.D., D.C.L., Prof. William H. Flower, F.R.C.S., Prof. G. Carey Foster, B.A., F.C.S., John Russell Hind, F.R.A.S., Lord Rayleigh, M.A., Vice-Admiral Sir G. H. Richards, C.B., Prof. Henry J. Stephen Smith, M.A., Prof. Balfour Stewart, M.A., and Prof. Allen Thomson, M.D., F.R.S.E.

MR. F. M. BALFOUR, Fellow and Lecturer of Trinity College, Cambridge, has joined the editorial staff of the *Quarterly Journal of Microscopic Science.* The journal will in future be conducted by Prof. Ray Lankester as responsible editor, with the co-operation of Mr. Archer in Dublin, Mr. Balfour in Cambridge, and Dr. Klein in London. The volume for the year just concluded shows an increase in the number and efficiency of the lithographic plates. Instead of sixteen octavo plates as usual four years ago, there are twenty-five, many of which are double sized, and some coloured.

MADAME LEVERRIER, the widow of the astronomer, died on November 1, at the age of fifty-eight years. This lady was suffering from a protracted illness, when the loss of her husband produced a shock from which she was not able to recover. She was a daughter of M. Choquet, an eminent professor of mathematics in Paris. Her father, about eighty years old, was present at the funeral. On the very day that Madame Leverrier died, the *Journal Officiel* published a decree, signed by M. Brunet, the Minister of Public Instruction, ordering the bust of Leverrier to be placed in the Palace at Versailles, where are to be collected the memorials of the great Frenchmen of the nineteenth century. This honour has been decreed to a number of other men who have ranked foremost amongst *littérateurs*, artists, or politicians. M. Leverrier, it is strange to say, has been chosen as the only representative of science.

THE French Academy of Medicine has been authorised by the ministry to accept a legacy of 4,000*l*. bequeathed by Dr. Demorquay, to help them to build a hall of meeting.

M. FAYE, Inspector-General in Science of Secondary Education in France, has been appointed to a similar office for superior education in succession to the late M. Leverrier. M. Fernet has succeeded to M. Faye's post.

M. WATTEVILLE, director of Arts and Sciences in the French Ministry of Public Instruction, has issued a circular notifying that a special exhibition will be held at the Champ de Mars, for collecting the results of the scientific missions granted by the French Government in 1867. Almost every country, civilised and barbarian, near or remote, has been visited.

M. BERTRAND, the perpetual secretary of the French Academy of Sciences, has been appointed by M. Bonnet member of the International Metric Commission.

COMMANDER GUISEPPE TELFENER has announced his intention of placing at the disposal of the Italian Geographical Society a sum of 40,000 francs to found a section of commercial geography and organise at Rome a museum to contain specimens of all the products which Italy exports and imports.

AT a meeting held at the London Library on October 26 (Mr. Robert Harrison in the chair), it was determined to form an Index Society, with the immediate object of compiling subject indexes and indexes of standard books of facts, to be printed and circulated among the members; and with the ultimate object of building up a general index of universal literature, which can be referred to at the office of the society during compilation. The great aim of the society will be the gradual accumulation of aids towards the preparation of a key to all knowledge, and with this object a library of indexes will be commenced. The subscription will be one guinea. Subscribers' names and suggestions on the subject of the proposed society will be received by Henry B. Wheatley, hon. sec. *pro tem.*, 5, Minford Gardens, West Kensington Park, W. The utility of such a society and such an index to scientific men of all classes and grades will be obvious, and the effort now being made deserves their hearty support.

THE system under which the official addresses are made at the annual meeting of the American Association for the Advancement of Science seems curiously complicated, and sometimes is a puzzle even to the old members of that body. The retiring president, who has been the presiding officer in the preceding year, makes the opening address, which is the presidential address for that year. The presidents of the sections, on the other hand, who have just entered on their duties, open their sections respectively with an address. There are only two sections, A and B; other divisions are parts of these, and are characterised as sub-sections. Section A has charge of mathematics, astronomy, physics, chemistry, and microscopy; Section B of zoology, botany, geology, palæontology, ethnology, and archæology. There is a further complication in the circumstance that the presidents of the sections are also the two vice-presidents of the Association. To illustrate this arrangement, we may cite proceedings at the meeting of last August at Nashville. Prof. W. B. Rogers, who was the president of the Association last year, and president at the Buffalo meeting, was expected to open the Nashville meeting with the presidential address, but was prevented by illness. Professors E. C. Pickering and O. C. Marsh are respectively presidents for the present year of Sections A and B, and also vice-presidents of the Association. The address on "The Introduction and Succession of Vertebrate Life in America," by Prof. Marsh, which we recently published in full, was his official address as the president of Section B, delivered at the opening of the Section. To carry the illustration further, it may be added that Prof. Marsh, who was elected at this year's meeting, president of the Association, will not preside till next year at St. Louis, and will not be expected to deliver his presidential address until the meeting of the following year, 1879.

THE death is announced of Dr. Henry Lawson, until recently editor of the *Popular Science Review.*

MR. JAMES FLOWER, for many years the articulator of the skeletons at the Royal College of Surgeons, has just died from carcinoma of the rectum, from which he had been suffering for some time past. Mr. James Flower was seventy-seven years of age, and had served in the army in his younger days.

FROM statements made at a meeting of the California Academy of Sciences, the eucalyptus tree may be enumerated among the means for checking fire. Eucalyptus shingles are said to be fire-proof. A tree of this species was exposed to the San Francisco fire of 1876, and is still flourishing. The notion is urged that the spread of fires in cities could be checked by setting out such trees for shade and ornament. All varieties of the eucalyptus are said to possess this valuable property.

THE first examination of Surveyors and Inspectors of Nuisances by the Sanitary Institute of Great Britain, took place on Monday, October 29. Eight candidates presented themselves, five of whom were successful in obtaining certificates of competence, namely, Mr. H. M. Robinson, Surveyor, Ulverston; Mr. J. Parker, ditto, Bridgwater; Mr. F. Booker, Inspector of Nuisances, Bradford; Mr. W. S. Prebbles, ditto, Blackburn; Mr. Thomas Blanchard, ditto, Evesham. Fifteen candidates have already entered their names for the next examination.

NEWS has been received, the *Geographical Magazine* states, from M. Kelsief, who has been making researches during the past summer along the Murmanian coast and in Lapland, for the Moscow Anthropological Exhibition of 1879. M. Kelsief had been travelling with M. Singer, secretary of the Natural History Society; and the two had, up to the time of their parting company on the borders of the White Sea, made a good collection of stone implements and other prehistoric remains, M. Kelsief then took a cruise in a small vessel, and traversede with considerable difficulty, about 800 versts in all in the White and Polar Seas, and passed the whole of the summer within the Arctic circle. Along the Murmanian coast he visited the Lapps, who inhabit there subterranean dwellings, grouped at intervals of between 70 and 100 versts. He was accompanied by only one servant, and after enduring considerable hardships through exposure and insufficiency of food, he started on August 29 for the north of Finland, where he proposes to visit the Lapps of Lake Enara, and to return to St. Petersburg by way of Tornea.

THE portion of the Indus River where it emerges from Kashmir territory and flows through the mountainous region of Gilgit and Chilas to rejoin our frontier near Darband—a strip in all of about 120 miles in length—has just received, we learn from the *Geographical Magazine*, detailed exploration at the hands of a Punjab surveyor. This piece of work will complete our geographical knowledge of this river, and will contribute useful topographical information to our future maps, though it must be remembered that the general course of the river had been pretty accurately determined in 1870 by Capt. Carter's careful triangulation of the peaks flanking its eastern and western banks.

THE London papers contain frequent announcements of expected high tides, which are no doubt useful to many as forewarnings of danger. But we cannot understand why the burden of such predictions should fall solely upon Capt. Saxby. Is he the only one qualified and concerned to make such predictions?

WE have received a reprint of four important papers which originally appeared in the *New York Tribune*, and which are now sold separately by that paper at the insignificant price of 10 cents. The papers are on the Evolution of Life, by Dr. Draper; Ancient Life in America, by Prof. Marsh; Catastrophism and Environment, by Mr. Clarence King; and the Peabody Museum (illustrated), by Mr. Wyckoff. This is No. 37 of these science numbers of the *Tribune*; evidently, then, it is the interest of the management to find space for so much science.

A COMMITTEE has been formed in Holland under the patronage of Prince Henry of the Netherlands, and 24,000 florins have been collected, to send out in May of next year a small but strong sailing vessel to the west coast of Spitzbergen, with the view of reaching the mouth of the Yenissei. The objects of the expedition are to explore the new commercial route to the Siberian rivers, to train sailors who might ultimately be intrusted with the formation of a scientific station, and to erect a few monuments to the memory of the early Dutch arctic navigators.

THE celebrated mammalian and reptilian remains obtained by Mr. Beckles from the base of the middle Purbecks at Durdlestone Bay, and described by Prof. Owen in the Palæontographical Society's *Memoirs* were acquired last year by the British Museum. Under the care of Mr. Davis they have been carefully cleaned, mounted, and labelled, and are now being placed in cases. The total number of specimens acquired was about 2,000, but only some of the best are exhibited.

THE tank prepared at the Westminster Aquarium for the whale is now used as a seal pond. Its large size gives ample scope for the gambols of the young seals, which can now be seen under circumstances more favourable than have before been offered in London.

MR. O. H. A. MOGGS writing to the *Times* from Bullpits, Bourton, Dorset, states that that place was visited on Friday last by what seemed to be two shocks of an earthquake. The first occurred at about 8.10 A.M., and was accompanied by a rumbling sound, which lasted about ten or twelve seconds. The vibration of the ground was very slight, although it could be distinctly felt. The second shock was felt at 11.20 A.M. The vibration of the ground was very violent, causing houses to shake and the windows to rattle. This lasted about six seconds, and was accompanied by a rumble like the former, only heavier, which lasted about eight or ten seconds.

A SLIGHT shock of earthquake was felt on Sunday afternoon at New York. It was also felt in New Brunswick and Quebec.

MESSRS. J. AND A. CHURCHILL have published in a separate form, for the use of students, the valuable "Notes on Embryology and Classification" by Prof. Lankester, from the current number of the *Quarterly Journal of Microscopic Science*.

UNDER the title of "The Lazy Lays and Prose Imaginings, written, printed, published, and reviewed by William H. Harrison," of Great Russell Street, the author has published a collection of verse interspersed with short prose pieces partly sentimental but mostly intended apparently to be funny. Scientific men and matters are in one or two cases alluded to, and the imprint bears that the work is published "A.D. 1877 (popular chronology); A.M. 5877 (Torquemada); A.M. 50,800,077 (Huxley)." We believe our readers may derive a little amusement from a perusal of the volume.

THE additions to the Zoological Society's Gardens during the past week include an Anubis Baboon (*Cynocephalus anubis*) from West Africa, presented by Mr. Ward; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Dr. W. B. Stirling; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mrs. Henry Jephson Mello; a Central American Agouti (*Dasyprocta isthmica*) from Central America, presented by Mr. A. Stradling; three Sclater's Curassows (*Crax sclateri*) from Paraguay, presented by Mr. Alex. F. Baillie; a Puff Adder (*Vipera arietans*) from South Africa, presented by Mr. A. Biden; a Pike (*Esox lucius*) from British Fresh Waters, presented by Mr. A. D. Bartlett; an Axis Deer (*Cervus axis*) from India, a Three-banded Armadillo (*Tolypeutes conurus*) from La Plata, deposited; a Cape Buffalo (*Bubalus caffer*), two Coatis (*Nasua nasica*), born in the Gardens.

AMERICAN SCIENCE

THE chief signal officer of the U.S. army has been urging that physical observations of the sun be made, as of sun-spots, faculæ, protuberances, &c., in reference to their supposed influence upon terrestrial meteorology, and has offered to publish the results monthly, or such of them as may be considered desirable by the observer, in the *Monthly Weather Review*. The United States Naval Observatory at Washington has already accepted this proposition, and it is considered very desirable that some other observatories in the east, and at least one on the western coast, co-operate in this undertaking.

Dr. C. A. White, palæontologist to the United States Geological and Geographical Survey of the Territories, has spent the past season making a critical study of the mesozoic and cainozoic strata of the great Rocky Mountain Region, and the results have tended to confirm in a remarkably clear manner the statement so often expressed by Dr. Hayden in his annual reports, that the entire series of deposits are consecutive from the Dakota group of cretaceous age below, to the Bridger group of tertiary age above. The sedimentation was evidently continuous through all the changes, from marine to brackish, and from brackish to fresh waters, that successively took place in that great region, although those changes in aqueous conditions produced corresponding changes in the then prevailing forms of invertebrate life.

The annual report of the Board of Regents of the Smithsonian Institution for 1876 has been published, and, as usual, contains a great deal of matter interesting to men of science. The portions of the volume detailing the operations of the institution for 1876 is more especially occupied with an account of what was done in connection with the International Exhibition of 1876, at Philadelphia, and especially of the very extensive and valuable presents made to the United States by the various foreign commissions, and taken charge of by the institution, in accordance with the law of Congress. Reference is made to an application for an appropriation to erect an additional building to accommodate these objects, for which it is estimated that a floor space of 80,000 square feet will be required. Until this is done the collections in question must remain in their original packages, more than 4,000 in number, which are stored on four floors of a separate building, 50 by 100 feet, and filling them completely from floor to ceiling. As usual, the funds of the institution are reported as being in a favourable condition, the income not being exceeded by the expenditure, and an available balance even remaining in hand at the end of the fiscal year. The second part of the volume embraces biographical notices of Dom Pedro II., and also of Gay-Lussac, articles on the kinetic theories of gravitation, the revolutions of the crust of the earth, the asteroids between Mars and Jupiter, and a number of papers on ethnology and archæology. Of these the most important is by Prof. Mason on the Latimer collection of antiquities from Porto Rico, in which the more interesting objects of this unique series are figured. Other papers on ancient mines and mounds, implements of various kinds, &c., are also contained in the volume.

We have to record the death of Mr. Timothy Abbott Conrad, one of the oldest and most accomplished palæontologists of the United States. Mr. Conrad was born in 1803, and commenced his investigations early in the century, beginning with the tertiary and cretaceous formations of the United States. In 1832 he commenced an illustrated work on the "Fossil Shells of the Tertiary Formations of the United States," which was, however, preceded in 1831 by his "American Marine Conchology." Most of his papers appeared in the *American Journal of Science and Arts*, and in the *Proceedings* and *Memoirs* of the Academy of Natural Sciences, Philadelphia. He also contributed largely to the reports of the various government exploring expeditions.

The Nation announces the death of Mr. John G. Anthony, for many years a devoted coadjutor of Agassiz in the Museum of Comparative Zoology at Cambridge, where he had charge of the conchological department. Long residence and extensive travel in the Ohio Valley had made him the first authority in the United States on fresh-water shells. He accompanied the Thayer expedition to Brazil, but sickness prevented him from taking part in it after its arrival. In addition to his special work Mr. Anthony always maintained an interest in Botany and horticulture. He was a native of Rhode Island, and was in the seventy-fourth year of his age.

Prof. Marsh makes the announcement of the interesting discovery of the remains of two species of fossil bison in the lower pliocene of Nebraska and Kansas. They were much larger than the existing bison, with more powerful horns.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Vice-Chancellor, Dr. Atkinson, on resigning his office on November 3 (he has been re-elected) spoke of the progress of scientific teaching in the University. The efficiency of the University as a school of natural science has been greatly promoted, Dr. Atkinson stated, during the past year by the erection of the new buildings for the department of comparative anatomy and physiology. Although the whole building is not yet completed, many of the rooms are already in use, and the accommodation which is thus provided for both teachers and students will be of the greatest advantage. In connection with this subject Dr. Atkinson referred to Prof. Clerk Maxwell's announcement that His Grace the Chancellor has now completely equipped the Cavendish Laboratory with all the apparatus and instruments which the professor considers that a first-class institution of this kind ought to possess. This singular munificence, continued so steadily and ungrudgingly for such a number of years, is but one of the many proofs which His Grace is constantly giving of his unwearied care and concern for the welfare of the University.

The following gentlemen have been elected to fellowships at St. John's College:—Arthur Milnes Marshall, B.A., Senior in Natural Science Tripos, 1874, and Donald M'Alister, B.A., Senior Wrangler and First Smith's Prizeman, 1877.

OXFORD.—At a special meeting of the Town Council held at Oxford on Monday it was resolved to establish a first-class grammar school, the Corporation granting a site in the centre of the city of nearly an acre in extent, 4,000*l*. towards the building, and 100*l*. per annum towards its maintenance. There are to be fifty free scholarships tenable for three years, thirty of which are to be filled up from the public elementary schools.

LONDON.—The Council of University, College, London, have appointed the Rev. T. G. Bonney, B.D., of St. John's College, Cambridge, Professor of Geology and Mineralogy for five years.

ST. ANDREWS.—Mr. George Chrystal, B.A., Fellow and Lecturer of Corpus Christi College, Cambridge, has been appointed to succeed Prof. Fischer in the chair of mathematics.

Among the names likely to be brought forward by the students for the honorary and honourable post of rector of the University, that of Prof. Tyndall is mentioned.

SCIENTIFIC SERIALS

Morphologisches Jahrbuch, vol. iii. Part 3.—R. Bonnet, on the structure of, and circulation in, the gills of Acephala, pp. 45, three plates.—C. Hasse, fossil vertebræ (the Squatinæ), two plates.—R. Wiedersheim, the skull of Urodeles, pp. 97, five plates; a most valuable memoir on Menobranchus, Siren, Proteus, Amphiuma, Cryptobranchus, Menopoma, Salamandrina, Triton, Axolotl, Plethodon, Spelerpes, Ellipsoglossa, Amblystoma.—M. Fürbringer, on the cephalic skeleton of Cephalopods.

Annalen der Physik und Chemie, No. 9.—On discontinuous liquid motions, by M. Oberbeck.—Explanation of Dufour's and Merget's experiments on the diffusion of vapours, by M. Kundt.—On the diffusion of liquids, by M. Johannisgänz.—On the internal friction of solid bodies, by M. Schmidt.—On the photo-electricity of fluorspar, by M. Hankel.—On the resistance of flames to the galvanic current, by M. Hoppe.—On the electro-chemical process at an aluminium anode, by M. Beetz.—Further experiments on galvanic expansion, by M. Exner.—Reply to Zöllner's objections against my electro-dynamic views, by M. Clausius.—On a mode of inference employed by Prof. Tait in the mechanical theory of heat, by M. Clausius.—On the sounding of air in pipes, by M. Ciamician.—The spectrum of nitrous and hyponitric acid, by M. Moser.—On optical illusion, by M. Trappe.

Beiblätter zu den Annalen, &c., No. 8.—On the equilibrium of a drop between two horizontal plates, by M. Bosscha.—On cylindrical sound-waves, by M. Grinwis.—Application of the galvanic current to investigation of the spheroidal state of some liquids, by M. Hesehus.—On the tenacity of copper and steel, by MM. Pisati and Saporita Ricca.—On the polymorphism of crystals, by M. Moutier.—The heat of solution of chlorine, bromine, and iodine compounds, by M. Thomsen.—New saccharimeter, by M. Laurent.—Lecture experiment on the colour-change of certain double iodides, by M. Boettger.

No. 9.—On physical isomerism, by M. Lehmann.—On the elasticity of gypsum and mica, by M. Coromilas.—On the influence of pressure on the temperature at which water shows a maximum density, by M. Van der Waals.—Apparatus for measurement of the expansion of rigid bodies by heat, by M. Reusch.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, November 1.—Dr. Gladstone in the chair.—The following papers were read:—On some hydrocarbons obtained from the homologues of cinnamic acid, by W. H. Perkin. These hydrocarbons were prepared either by heating the acids or by treating the hydrobromo acids with bases. The following acids were prepared and examined:—Hydrobromocinnenylacrylic, hydrobromocinnenylcrotonic, hydrobromocinnenylangelic. The following hydrocarbons were obtained:—Isopropylvinylbenzene, isopropylallylbenzene, isopropylbutenylbenzene, allylbenzene, and butenylbenzene; the dibromides of these bodies were also prepared and examined.—On anethol and its homologues, by W. H. Perkin. By heating methylparoxyphenylacrylic acid, vinylic anethol was obtained, similarly allylic or ordinary anethol and butenylic anethol were prepared. In conclusion the author discusses the formation of the hydrocarbons from the hydrobromo acids, and concludes that they are formed simply by the separation of hydrobromic acid and carbonic anhydride.—On two new methods for estimating bismuth volumetrically, by M. M. P. Muir. To a solution of bismuth in nitric acid an excess of sodium acetate is added, and then a measured volume of standard sodium phosphate solution also in excess; the bismuth is precipitated, the precipitate filtered off, and the excess of phosphoric acid determined in the filtrate by uranium acetate. The other method given does not yield such accurate results.—On the oxidation of ditolyl, by T. Carnelly. By the oxidation of solid ditolyl the author obtained diparatolylphenylcarbonic acid and diparadiphenyldicarbonic acid; liquid ditolyl yielded orthoparatolylphenylcarbonic acid, orthoparadiphenylcarbonic acid, and finally terephthallic acid.—On a new manganese reaction, by J. B. Hannay. If a solution of a manganous salt in strong nitric acid is warmed in the presence of an iron salt with some crystals of potassic chlorate, the iron and manganese are precipitated as a double manganate of iron and manganese. The author proposes this reaction for separating iron from aluminium, &c.

PARIS

Academy of Sciences, October 29.—M. Peligot in the chair.—The following papers were read:—*Résumé* of a history of matter (second article), by M. Chevreul.—On the solar photospheric system, by M. Janssen.—The telephone of Mr. Graham Bell, by M. Breguet.—On the determination of the quantity of mud contained in current water, by M. Bouquet de la Grye. He uses an instrument named a pelometer (πῆλος, mud), consisting of a V-shaped vessel whose rectangular faces, inclined one-tenth, are of thin glass, while its sides are of copper or white iron. One glass face has a centimetre scale commencing from the angle. The pelometer is filled and held vertical; it then presents a succession of vertical layers of increasing thickness upwards, and, by comparison with glass-ended tubes containing muddy water of various known densities, the proportion of mud may be ascertained. Other methods are given. Experiments made at Rochelle show that the quantity of mud per litre varies from one to ten times according to the depth. He considers regular measurements of the kind on watercourses desirable for agriculture, &c.—On an American vine-stock not attacked by phylloxera, by M. Fabre. This vine belongs to the species *Riparia*. Among other merits (besides its immunity) it gives cuttings readily, receives grafts from French species better than any other American variety, thrives in most arid soils, compact clays, and soils long exhausted by vine cultivation, and grows very rapidly.—On treatment of phylloxerised vines with sulphocarbonate of potassium applied with the distributing pail, in 1876-77, by M. Gueyraud.—Observations of the planet (175) Palisa, made at the Paris Observatory, with the west equatorial of the Garden, by MM. Paul and Prosper Henry.—Stellar systems of 36 Ophiuchus and 40 Eridan, by M. Flammarion.—General form of coefficients of certain developments, by M. André.—New mode of plane representation of classes of graduated surfaces, by M. Mannheim.—Experiments on the disruptive discharge made with the chloride of silver battery by MM. Warren de la Rue and H. W. Müller.—Rheostatic machine, by M. Planté. He combines a number of condensers (made of mica and tin), so as to be easily charged, from a secondary battery, in *quantity* and discharged in tension. The commutator is a long cylinder of hardened caoutchouc, having longitudinal metallic bands, and traversed by bent copper wire (for the two objects named). Metallic springs are connected with the two armatures of each condenser, and fixed on an ebonite plate on each side of the cylinder, which is rotated. A series of sparks can be got between the branches of the exciter in this arrangement, quite like those from electric machines with condensers. The discharges are always in the same direction, and the loss of force is less than in induction apparatus. A great many discharges can be had without the secondary battery being perceptibly weakened, as each discharge removes only a very small quantity of electricity.—On semi-diurnal barometric variations, by M. de Parville. The tropical hours may present, at a few days' interval, divergences amounting, during the great period, to forty-five minutes. The barometric variations in the tropical hours are not uniform; the maximum of descent of the mercury column occurs about three o'clock. Equality between the periods of day and night has never occurred. The amplitude of the variation is greater by day than by night, and during the dry season than during the wet.—On the action of anhydrous acids on anhydrous bases, by M. Bechamp. They are capable of uniting wholly.—On the determination of reducing sugar contained in commercial products, by M. Girard.—On the reducing sugar of commercial products in its relations to saccharimetry, by M. Morin. He shows the optical inactivity of this sugar.—On the production of racemic acid in the manufacture of tartaric acid, by M. Jungfleisch.—On some physical properties of *quercite*, by M. Prunier.—Action of solar light with variable degrees of intensity on the vine, by M. Macagno. Diminution of intensity hinders the production of glucose; the other elements (produced or assimilated) are in direct ratio of the luminous intensity. A portion only of potash is in inverse ratio of the luminous intensity: the contrary occurs in the case of potash combined with tartaric acid.—On the Orthonectida, a new class of animal parasites of Echinoderms and Turbellaria, by M. Giard.—On the calcareous algæ belonging to the group of verticillate Siphoneæ (Dasycladeæ, *Harv.*), and confounded with the Foraminiferæ, by M. Munier Chalmas.—Effects of faradisation in a case of hydrophobia in man, by M. Menesson. Considerable sedative effects were obtained; the patient, however, died after two days through a sudden stoppage of the heart's contractions.

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