THURSDAY MARCH 11, 1875

AGRICULTURE IN VICTORIA

Department of Lands and Agriculture, Victoria. Second Annual Report of the Secretary for Agriculture. (Melbourne: Published by authority. John Ferres, Government Printer, 1874.)

THE Government of the flourishing colony of Victoria has had in active operation, for two years, a Department of Agriculture, the history, constitution, and working of which may be discussed with advantage at a time when it is proposed, in many quarters, to establish a somewhat similar department in the mother country.

The work done by the Victorian Department for 1873 is detailed in the Report sent to us by the Secretary. The origin of the Department is given in the Introduction to the First Report, which we happen to possess, and from which we learn that the Port Phillip Agricultural Society was instrumental in inducing the Legislature of the day to pass the Act, 22nd Victoria, No. 83, which established and endowed a Board of Agriculture for the colony. It would appear that the Board spent all its funds in making grants to local Agricultural Societies; and thus failed, as might have been expected, to produce results commensurate with the grant. This failure induced a number of thoughtful men to urge on the Government the propriety of establishing an independent department for promoting the agricultural interests. The executive received the matter favourably; and appointed, on the 19th of June, 1872, the Hon. J. J. Casey first Minister of Agriculture. It became necessary to appoint a permanent executive officer as head of the Department, and the course adopted for securing the services of such an officer was novel. It was called a competitive examination, but the competition was confined to an essay on the means of promoting the object. The examiners unanimously selected as the best the essay written by Mr. A. R. Wallis, who was at once appointed. Mr. Wallis holds the diploma of the Royal Agricultural College, Cirencester; and, fortunately for the colony, possesses essential qualities, such, *e.g.*, as energy, which could not be tested by the writing of an essay. The paper which secured the appointment for Mr. Wallis is published in the First Annual Report of the Department, and is the production of a thoughtful mind.

The first "Report" was made up chiefly of papers supplied by the Secretary himself. He had to discuss vine-culture, vine-disease, and other subjects which were new to him. On the whole, however, the volume was a respectable production.

In the Report for 1874 he was able to obtain papers on various agricultural subjects from the most competent men in the colony. The volume begins with the general report by the Secretary himself, which is followed by a report from the pen of the recently appointed chemist, Mr. W. E. Ivey, and a report on the state forests apparently written by the same Mr. Ivey. In addition to these reports the volume contains a great many original papers on important subjects. On the whole, the volume is creditable to the Secretary, on whom the direction of the Department devolves. He is a young man. Everything was new to him in his adopted country. He had to deal with subjects which he could not by any possibility have mastered at the time he entered on his duties. Viewing his labours in the light of this fact, they give promise of a useful career. The recent scientific training which Mr. Wallis received at Cirencester must have aided him in overcoming many difficulties. He would do well to exercise great caution. We would advise him, and all those who break new ground, to avoid disquisitions or discussions on subjects with which they are not thoroughly conversant. We find an instance in the Report for 1874. In suggesting the propriety of instituting an agricultural survey of Victoria, a thing in itself most useful, the Secretary writes a rank heresy in political economy. "It seems to me," he says, "a monstrous thing that a man who, by the combined application of industry, capital, and intelligence, has converted a barren schistose hill into a well-managed and productive vineyard, should be subject to a higher assessment than the person who owns or occupies the adjacent lands of equal natural fertility, or than one who owns a vast extent of the most naturally productive lands of the colony, because such lands are devoted to none other than pastoral purposes."

In writing this passage Mr. Wallis overlooked an elementary principle of taxation, namely, that as one of the objects of taxation is to create a fund for the protection of property, men should pay this tax in proportion to their property or ability to pay. A well-managed and productive vineyard would be a source of loss to its owner if every dishonest man living in the colony of Victoria were allowed to seize the crop. It is unnecessary to waste time in elucidating so simple a matter. The wonder is, how a man of Mr. Wallis's intelligence and position could have entertained and expressed a view which is at variance alike with the elements of economic science and common sense. We fully believe the passage was written hurriedly and without thought. The subject was of the most incidental character; and there is a very general tendency to deal in an "offhand" manner with topics which arise in this way. The Secretary passes in review the leading crops and interests with which his department is concerned. We are sorry to learn that the experiments made with flax in various parts of the colony have not been satisfactory. The vine crop of 1874 was good, and it was comparatively free from disease. Fruit culture, entomology and meteorology, and a great many other subjects, are briefly noticed. The topic which appears to interest the Secretary most is agricultural education, which is treated at considerable length in a paper distinct from the Report. "It is high time," he says, "now that the Church, the Law, and the Sword have their Colleges supported by the State, that the Plough should have hers." And he urges that "it is as much a matter of national policy to teach the people how to feed men scientifically as to kill them." His paper on agricultural education is most interesting. Of his own Alma Mater, Cirencester, he speaks more reservedly than we could expect. His success, which we sincerely and ardently wish, will do more for Cirencester than mere words of praise. He describes its arrangements briefly and correctly. Of the Irish national system of agricultural education he speaks in the warmest terms. Through its instrumentality, we are informed, the knowledge of the rotation of crops was introduced into districts where rotation cropping had been previously unknown, and where the potato and oats were the only crops formerly cultivated. Before embarking in any scheme of agricultural education, the people of Victoria would do well to study the "ups" and "downs" of this Irish system, which has been in operation for upwards of thirty years, and which, if report be true, is about being freely pruned by the Treasury. This Irish system of agricultural education is directed by a body of twenty Commissioners, of whom one is a paid administrator, nineteen being unpaid. We take it for granted that they and the Government of the day concur in the action of the Treasury. There is a widespread feeling that there are, or have been, men at the Treasury who are opposed to public grants for agricultural education, and who say there is no reason why farmers should be taught their business any more than shoemakers or carpenters.

But all that the best friends of agricultural education claim is, that the fundamental truths of agricultural science should be taught in our rural schools, and that there should be a few normal schools or colleges in which the best minds of the country could be thoroughly educated in the science of agriculture, so as to qualify them for making investigations, and for taking a leading part in agricultural progress. This is, according to our interpretation, all that the Secretary of the Agricultural Department of Victoria asks; and we trust the Government of Victoria will carry out his views. If they carefully study the several sides of the Irish system, they cannot fail to devise a system of agricultural education which would confer lasting benefits on the colony.

It has been already stated that Mr. Ivey contributes two papers, one on Chemistry and the other on the State Forests. It is not often that a man professes chemistry and forestry. Many a chemist is also a naturalist, and why should not a man study the habits of forest trees as well as those branches of knowledge included in natural history? Mr. Ivey's report on the forests is interesting, but his chemical report concerns us more. He gives us several chemical analyses of virgin soils, and endeavours to show that such analyses are of direct use to the farmer. We agree with Mr. Ivey when he says that the chemist, by discovering some compound in the soil unfavourable to crops, can afford the settler information which will save him from the loss of pitching his tent on a barren location. We must, however, assure Mr. Ivey that he pushes a little too far his argument in favour of the value of chemical analyses of soil. We have now before us a most remarkable sheet, drawn up by an advanced agriculturist, in which appear thirteen chemical analyses of soils and subsoils, and the rents of these soils, and we must say that we have never seen any return showing a great discordance between the indications of analyses and the judgment of men who know to a shade the actual value of land. If Mr. Ivey is ambitious to make his investigations in this department of chemistry of real use and benefit to the farmer, he must strike out a new line of thought. Until he does this he should, if he would retain the good opinion of men who are competent to form a correct estimate of his work, confine himself to those fields of labour in which there is ample room for the application of the established principles of chemistry.

Mr. R. L. J. Ellery, F.R.S., Government Astronomer, contributes to the Report now under review an able and interesting report on the meteorology of Victoria. Many of the rising generation cast their thoughts on the colonies with a view to emigration; and to these Mr. Ellery's report must be instructive. In the following passage we get a general notion of the physical features of the country:—

"By an examination of a contoured plan of the colony, we find that the most prominent feature is an extensive mountain range running approximately east and west, rising somewhat abruptly about lat. 37° 30′, and long. 141° 40′, varying in altitude from 1,000 to 5,000 feet, and culminating in the N.E. in lat. 36° 30′, long. 148° 20, at Mount Kosciusko, the highest part of Australian Alps, where it attains an altitude of over 7,000 feet. The higher parts of this range are covered with snow for several months in the year. The mountain country is for the most part densely wooded with fine timber, even to the very summits; at some of the higher elevations, however, especially in the N.E., many of the peaks are quite bare, or only partially covered with dwarfed trees or shrubs. The country north and south of this great dividing range is moderately undulating or flat, consisting often of large plains, in some parts quite destitute of trees, but closely wooded in others. Along some parts of the coast-line, however, especially in the Cape Otway, Western Port, and Wilson's Promontory districts, the land rises to considerable altitude (from 2,000 to 3,000 feet) by ranges generally well covered by timber to their summits. On the whole, the country is not well watered; the rivers are few and insignificant and are often nearly dry in summer; there are several lakes, both salt and fresh, in different parts, but not of sufficient extent to have any marked influence on the climate. The coast-line itself is for the most part flat, with a moderate elevation; although, as just stated, at some places lofty ranges abut on the sea, and the coast becomes precipitous and rugged. An extensive sea-board, open to polar winds and oceanic currents, modified, no doubt, by the presence of the island of Tasmania, an extensive and wooded mountain range running across the whole breadth of the colony, the higher portions of which are often clothed in snow, and the generally arid sub-tropical Australian interior, dominating on its northern and western boundary, must each necessarily exercise considerable influence in producing conditions of climate varying with the locality."

The notion is generally entertained in these countries that the climate of Victoria is extremely dry. Mr. Ellery shows that the rainfall attains to the average of similar latitudes in other parts of the globe. He puts the average at 25·66 inches per annum. Spontaneous evaporation is, however, very great; and a large quantity of the rainfall is also lost in consequence of the vast area of the country which has been unbroken.

The mean temperature of the year is given as follows:—

Melbourne	57°·5	Bush Waste	57°·2
Portland	60°·9	Stawell	57°·7
Cape Otway	55°·1	Berwick	57°·1
Port Albert	56°·4	Daylesford	53°·1
Saba Island	58°·6	Heathcote	57°·4
Ararat	58°·0	Castlemain	56°·2
Ballarat	53°·6	Camperdown	54°·6
Sandhurst	58°·7		

The minimum of heat occurs in June, July, and August. The lowest known at Melbourne is 27°, or 5° below the freezing-point; at Portland, 27°; at Sandhurst, 27°·5, and at Ballarat, 22°.

The highest recorded temperature in the shade occurs at Sandhurst in January, and was 117°; at Melbourne 111°. "There are other localities in which higher temperatures prevail in the same month, especially in the plains north of the dividing range, and along the banks of the Murray, in which the temperature has been as high as 123° to 125° for several days together. It is during the hot winds to which the climate is subject in summer that our highest temperatures occur, but they seldom last many hours, and are usually followed by a change in the direction of the wind, and by a comparatively low thermometer, when a fall of 20° to 25° often occurs in as many minutes."

We intended to make some remarks on the general advantages of a Department of Agriculture, but shall reserve them for a review of a similar volume which has come to us from the United States of America.

OUR BOOK SHELF

The Pathological Significance of Nematode Hæmatozoa. By T. R. Lewis, M.B., Staff-Surgeon H.M.B.F., on Special Duty. (Calcutta: 1874).

THIS little work may be regarded as a companion volume to Dr. Lewis's essay "On a Hæmatozoon in Human Blood." Both are reprints from the Annual Reports of the Sanitary Commissioner with the Government of India, for the years 1871 and 1873 respectively, and as such testify to the high class of scientific labour performed by the staff officers on special duty.

The main points brought out by Dr. Lewis are such as afford proof that chyluria (or a milky-looking condition of the urine) and the elephantoid state of the tissues are associated with the presence of a microscopic nematode entozoon in the human blood. Having fairly established that conclusion, he next proceeds to show that the disorders in question are immediately "due to the mechanical interruption to the flow of the nutritive fluid in the capillaries and lymphatics." No one who takes the trouble to look into the evidence so carefully collected by the author can fail to see that he has thrown a great deal of light upon the pathology of chyluria, elephantiasis, and other more or less closely allied morbid conditions; but Dr. Lewis has done more than this, for he has extended our knowledge of the habits and genetic relations of the microscopic hæmatozoa of the dog (so long a puzzle to helminthologists), and has shown that the so-called *Filariæ sanguinis hominis* are perfectly distinct from the canine *filariæ*, which latter, moreover, he proves to be the progeny of the *Filaria sanguinolenta*. Further than this, the author has detected numerous specimens of an aberrant type of nematode worm in the walls of the stomach of pariah dogs. These parasites occupy small tumours, two or more being usually coiled together in the centre of each swelling. He speaks of them as *Echinorhynchi*, which, indeed, they somewhat resemble; but it is quite clear from the very admirable figures accompanying the description, that the worms are not members of the order *Acanthocephala*. They are, in fact, examples of the *Cheiracanthus robustus* hitherto found only in various species of *Felis*. The illustrations, throughout, are remarkably clear, and show the internal structure of the parasites to perfection.

T. S. COBBOLD

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 Origin of the Jewish Week

MR. R. A. PROCTOR's paper on "Saturn and the Sabbath of the Jews," in the *Contemporary Review* of this month, reopens one of the oldest and most interesting questions in the history of astronomy. Unfortunately, the writer is very imperfectly acquainted with the literature of his subject, and in consequence has, I think, imported not a little confusion into the discussion. That the week of seven days is directly connected with the worship of the seven planets known to the ancients, is a theory which has always had many supporters. It is at once suggested by the familiar names of the seven days, and would be absolutely proved if we could show that these names are as old as the division of the lunar month into four weeks. Again, it is also a well-known, though less wide-spread doctrine, that the Jewish Sabbath passed into Mosaism from an earlier planetary religion. Of course, if it can be shown that the Sabbath was originally sacred to Saturn, we have a strong proof of the antiquity of the names of the week-days, and a probability that these names are as old as the seven day week itself. In this way a question in the history of Semitic religions comes to have an important bearing on a question in the history of astronomy. Mr. Proctor reverses the argument. He assumes that we have the clearest possible evidence that all nations that adopted the seven-day week named the days after the planets, and did so in that peculiar order which is generally explained by assuming that a new planet presides over every successive hour of the week, and that each day takes the name of the planet of its first hour. It is then argued that Saturn, as the highest planet, was the supreme god of Assyria, and so also of the Egyptians who received their astrological lore from Chaldea. The Egyptians, we are told, certainly consecrated the seventh day of the week to Saturn, and since the Israelites left Egypt observing the Sabbath, while there is no evidence of a Sabbath in patriarchal times, "it is presumable that this day was a day of rest in Egypt." Now, whatever may be the ultimate solution of the problem of the origin and diffusion of the seven-day week, this theory rests partly on uncertain assumptions, partly on undoubted blunders. It is notorious that several Semitic nations, not to speak of the Peruvians, had a seven-day week without planetary names; so that Mr. Proctor's fundamental assumption begs the whole question. Then, again, it is the opinion of so great an authority as Lepsius that the Egyptians had no seven-day week, but divided the month into three decades. The passage of Dion Cassius from which the contrary opinion is drawn is certainly not decisive for ancient Egyptian usage, and Mr. Proctor seems to quote his author at second hand; for he asserts, in flat contradiction to Dion, that when the latter wrote, neither Greeks nor Romans used the week. For the supposition that Saturn was the supreme god of the Egyptians, not a shadow of proof is offered, while what is said of the Assyrian Saturn is directly in the teeth of the most recent researches. If Mr. Proctor had read Schrader's essay on the Babylonian origin of the week, he would have known that Adar or Saturn is quite distinct from the supreme god Asur. Thus, apart from the late and doubtful testimony of Dion, Mr. Proctor has no other evidence for his Egyptian theory of the week than that which he derives from the presumed non-existence of the Sabbath among the Hebrews before they entered Egypt. But the seven-day week appears in the narrative of the flood, which is certainly not an Egyptian legend. I say nothing of numerous minor inaccuracies in Mr. Proctor's paper, but repeat that the point on which new light requires to be thrown is whether it can be made out that the names of the seven days are as old as the week itself. This again seems to depend partly on the question whether the division of the day into twenty-four hours is older than the week, and partly on what can be determined as to early Egyptian and Chaldean subdivisions of the month. The Egyptians had a day of twenty-four hours, but had they a week? The Chaldeans may have had the week, but they seem to have divided the day (including the night) into twelve hours. Perhaps, however, it ought to be borne in mind that Dion gives another way of accounting for the names of the day, depending not on the division of the day into hours, but on the analogy of musical harmony (ἡ ἁρμονία ἡ διὰ τεσσάρων). The Jewish Sabbath can contribute little to the argument unless one is prepared with Lagarde to maintain that Shabbat is a name of Saturn.

W. R. SMITH

Kirkes' Physiology

I HAVE observed in your issue of Jan. 28 (vol. xi. p. 248) a letter in answer to some previous remarks of mine concerning the true function of the sinuses of Valsalva. Your correspondent, Mr. Prideaux, does not, it seems, quarrel with the actual method of my reasoning, but urges that the conditions necessary for the existence of the premisses do not practically obtain. I may remark, however, that Mr. Prideaux does not show how or in what manner my arguments are inapplicable, but contents himself with pointing out what he imagines to be an error in my conception of the mechanism of the part in question. Now, I candidly confess that my knowledge of the state of things at the base of the aorta was not based upon practical observation, but at the same time I must, in justice to myself, say that in the mental review which I took of the possibilities of construction of the valves, I recognised the probable existence of the case which forms the subject of Mr. Prideaux's demonstration. But as he seems to think that if this error be granted the whole reasoning which follows is consequently invalid, I assert that it is by no means obviously certain, *à priori*, that an alteration in the conditions of its application must necessarily modify the conclusion. On the contrary, this very point which he deems it needless to prove because he has no doubt that it will be allowed, is the very point on which the whole question turns. I think also that in the further illustration of this I shall be able to show that Mr. Prideaux has missed the sole idea for which I was anxious to contend, viz., "that no mechanical advantage is gained by the expansion of the aorta towards its termination." Moreover, if I can point out the occasion of his difference from myself, I shall at the same time be rendering my own assurance the more complete.

In the first place, then, I think the difference is more verbal than real, and depends upon a certain ambiguity in the term "force of reflux." This I have interpreted to mean the pressure which would be represented by an area equal to the normal calibre of the vessel, being of opinion that it cannot naturally be applied to the multiplied pressure which would be given by taking the total area of expansion as its equivalent. The former pressure is transmitted without diminution to the unsupported area of the valves.

Again, the statement that "owing to the expansion of the aorta towards its termination, the force of reflux is most efficiently sustained by the muscular substance of the ventricle," is undoubtedly true in one sense; but in this case it is reduced to a mere truism, and amounts simply to this, that "the muscular substance of the ventricle being partially exposed to the contact of the column of blood, the latter rests upon it," and this, indeed, holds good whether the valves be mediate between the blood and the structure of the ventricle or not. However, I cannot help crediting the enunciation of Mr. Savory's theory with more than this, and maintain that it naturally induces the idea that the arrangement is in some way advantageous to the valves, *i.e.*, that the pressure is lessened on the unsupported portion.

That this conclusion was contrary to mechanical laws was what I endeavoured to show in my first letter, and that my arguments are equally applicable in the present instance is evident from the fact that the existence of that portion of the valves which rests upon the ventricle is mechanically unimportant and need not be considered, since the remainder of their surface bears just the same pressure as if they were attached directly to the margin of the ventricular ring.

It is possible, however, to make one other supposition on behalf of Mr. Savory's theory, that the error lies in its statement, and not in the theory itself. If this be the case it would at any rate be much better expressed thus: "That though the aorta expands towards its termination, the increase of pressure which the valves would thus have to bear is compensated by the support which they receive from the muscular substance of the ventricle."

With regard to the last paragraph of your correspondent's letter, in which he denies the possibility of contraction of the aortic orifice during the diastole, I can only say that instead of imagining this to be the case, I expressed a strong doubt as to its occurrence. For the original statement the text-book and not myself is responsible, as may be seen from the following quotation: "The reflux of blood is most efficiently sustained by the ventricular wall, which at the moment of its occurrence is probably in a state of contraction." That this, however, should take place is, as Mr. Prideaux justly observes, an impossibility, and only proves the existence of another error either of theory or enunciation. W. PERCY ASHE

51, Palace Gardens Terrace

Flight of Birds

THE Duke of Argyll appears to maintain that a bird can remain at rest in a uniform horizontal current by simply placing and maintaining itself in a certain *fixed* attitude.* He seems likewise to think that the muscular effort required to maintain this attitude is somehow an explanation of the phenomenon.

But would a dead bird, of precisely the same weight, size, shape, &c., rigidly fixed in the same attitude, also remain poised under like conditions? Of course I do not refer to the practical difficulty of maintaining an exact balance in the case of a dead bird, but in order to test the theory I suppose a mathematically uniform current and a mathematically perfect poise.

The live bird being perfectly motionless, the two would be precisely in the same mechanical condition, although the rigid attitude of the live bird would be maintained by dint of muscular exertion, and that of the dead bird by *rigor mortis*. Under these circumstances, would the dead bird fall to the ground or remain poised? If the former, what mechanical forces would apply to it which do not apply to the live bird? If the latter, then it would clearly follow that both birds could without change of attitude move with a uniform velocity, in a horizontal line, through still air; for it is clear that the mechanical problem is precisely the same, whether the air be in motion and the bird at rest, or the bird in motion and the air at rest. In each case the *relative* motion is the same.

Suppose, for example, a bird were poised at rest in a westerly breeze, moving over the earth's surface at the rate of twelve miles an hour, and suppose also the surface of the earth on account of latitude to be moving at an equal rate in the opposite direction. To anyone stationed on the surface of the earth this would be a case of the bird remaining still in a moving current. Yet, in fact, the bird would really be moving through still air at the same rate as the surface of the earth. This, I think, will be sufficient to illustrate the fact that the motionless poising of a bird in a uniform current is identical with its uniform motion through still air without change of attitude.

I need hardly point out that the muscular effort necessary to maintain the required attitude, producing no actual motion, can do no mechanical work. It cannot overcome atmospheric friction, nor the effect of the attraction of the earth.

Perhaps, indeed, the following simple way of viewing the subject may render it still more obvious:—

1. If the bird were deprived of its motor weight, *i.e.* if it were exactly of the weight of the atmosphere, then, whatever might be its motionless attitude, it would clearly float away like a balloon with the atmospheric current in which it was immersed.

2. If the air were at rest, then also under the same circumstances it must necessarily fall towards the ground, either vertically or obliquely, owing to its weight.

3. Therefore, by the most elementary law of the composition of motions, it follows that, taking into account the weight of the bird and the motion of the atmosphere, the actual resultant motion will be a motion combined of a motion vertically downwards and one or more horizontal motions.

4. The resistance of the air on the relatively still wings of the bird enables it to convert its downward motion partially into a forward motion also; but it is quite obvious that a motion combined of horizontal motions and a downward motion must result in a downward motion, and cannot produce equilibrium.

The Duke of Argyll's testimony to the fact that birds hover *apparently* without motion in horizontal air currents is valuable, and no doubt increases the difficulty of accounting for the phenomenon on the hypothesis of upward currents.

Graaf Reinet College F. GUTHRIE

To Microscopists and Entomologists

CAN any of your readers who are microscopists and entomologists help me to a successful method of killing and mounting *Hoplophora decumana*—belonging to the order Acarina?

The difficulties it presents are, that on being touched it contracts its head and legs and withdraws them into the horny envelope which surrounds its body, and that portion of the envelope extending over the head then closes tightly upon the aperture, completely shutting in the head and legs, so that in this condition the creature appears like a very minute seed covered with a few spinous projections. I can find no certain method of causing it to die unclosed, or so to mount it as to exhibit its form; and as the creature is not easily met with, I shall feel much indebted by any suggestions. I may add that I have consulted experienced mounters without success.

Hill Top, Midhurst, Feb. 22 R. C. FISHER

* See NATURE, vol. x. p. 262.

"Chameleon Barometer"

In my first communication (vol. xi. p. 307) upon this subject, I stated that the actual temperature had apparently no effect upon the colour of the paper. Since then I have had reason to change my opinion. During the late severe weather I have had better opportunities of studying the behaviour during frost, and I have observed that though in summer the paper will remain red for a difference of 3° between the thermometers, in very cold weather it is only red when that difference falls to 0°, or perhaps ·5°. This seems to agree with the fact that cold air cannot dissolve so much aqueous vapour as warm air. A. PERCY SMITH

Rugby, March 6

OUR ASTRONOMICAL COLUMN

TOTAL SOLAR ECLIPSE OF 878, OCTOBER 29.—In a communication to the *Times* in August 1872, this eclipse, in the days of King Alfred, was pointed out by the Rev. S. J. Johnson, of Upton Helions, Devon, as having been probably total in London. In the Saxon Chronicle it is merely stated that "the sun was eclipsed one hour of the day," without reference to any phenomena of totality; the *Chronicon Scotorum* records "a dark noon;" in the *Annales Fuldenses* we read: "Sol quoque in 4 Kal. Novembris post horam nonam ita obscuratus est per dimidiam horam, ut stellæ in cœlo apparent et omnenoctem sibi imminere putarent." This night-like appearsance of nature clearly indicates that the eclipse was total at Fulda (Hesse-Cassel), and if our calculations assign elements for the eclipse, which show totality at this spot, it may fairly be assumed that they will give very nearly the true phase for London. Correcting the arguments of Damoiseau's Lunar Tables of 1824, so as to bring them into agreement with Hansen for moon and Le Verrier for sun, and taking the minor equations from the Tables, we find the following elements for 878, Oct. 29:—

Conjunction in R.A., 0h. 51m. 24s. M. T. at Greenwich.

	°	′	″	
R.A.	218	6	10	
Moon's hourly motion in R.A.		37	25	
Sun's ,, ,, ...		2	29	
Moon's Declination	14	6	44	S.
Sun's ,, ...	15	4	40	S.
Moon's hourly motion in Decl.		8	25	S.
Sun's ,, ,, ...		0	48	S.
Moon's horizontal parallax		60	35	
Sun's ,, ,, ...		0	9	
Moon's true semi-diameter		16	31	
Sun's ,, ,, ...		16	12	

Assuming the position of Fulda to be in longitude 0h. 38m. 41s. E., and latitude 50° 33′·7, we find by direct calculation from the above elements a total eclipse, totality commencing at 2h. 9m. 32s. local mean time, and continuing 1m. 41s. with the sun at an altitude of 19°. The partial phase began at 0h. 56m. and ended at 3h. 24m. The Fulda annalist has "post horam nonam" for the time of the eclipse, but the times we have found cannot be very much in error. The sun rose at Fulda on this day at 7h. 12m. apparent time, or at 6h. 57m. mean time, so that the ninth hour from sunrise would be 4 P.M. To reconcile this difference, Dr. Hartwig, of Leipsic (who calculated the eclipse in 1853 from the best data then available, without finding it quite total at Fulda), conjectured that the author of the Chronicle might have reckoned his time from the commencement of twilight at the beginning of the month. However this may be, our elements, which may be expected to be pretty near the truth, have indicated a very measurable duration of totality at Fulda. Calculating now for London (St. Paul's), we again find a total eclipse commencing at 1h. 16m. 20s. mean time, and ending at 1h. 18m. 10s., or with a duration of 1m. 50s. If any reader should have the curiosity to examine the track of totality further, the following formulæ will assist him. Putting l for the geocentric latitude of place, and L for its longitude from Greenwich, reckoned positive eastward, t for Greenwich mean time—

$$\text{Cos. } w = 136{\cdot}55c0 - [2{\cdot}13760] \sin. l + [1{\cdot}70924] \cos. l, \cos. (L + 155^\circ\ 31'{\cdot}7)$$
$$t = 1\text{h. } 17\text{m. } 15\text{s.} \mp [1{\cdot}76081] \sin. w - [3{\cdot}32433] \sin. l$$
$$- [3\ 91281] \cos. l, \cos. (L + 109^\circ\ 10'{\cdot}4)$$

Upper sign for beginning of totality, lower one for ending; the quantities within the brackets are logarithms.

The Rev. S. J. Johnson found no other total eclipse in London during the long interval from 878 to 1715, and we are able to confirm his inference that there is not likely to be another one visible in the metropolis for five hundred years from the present time. Less than seven years after the eclipse of 878, or on June 16, 885, a very great eclipse passed over Scotland and Ireland. By a similar accurate computation to that detailed above, it is found to have been total not far from Nairn, and the duration of totality was little less than five minutes, a most unusual length for so high a latitude. In *Chronicon Scotorum* we read, "The stars were seen in heavens."

ENCKE'S COMET.—The ephemeris of this comet for the present appearance, communicated by Dr. von Asten, of Pulkova, to the St. Petersburg Academy, not having been yet transferred to the *Astronomische Nachrichten*, where such matters are commonly looked for, we continue our reduction of the places to 8 P.M. Greenwich time for the period when the comet is likely to be most easily found in these latitudes:—

	R.A. h. m. s.	N.P.D. ° ′	DISTANCE from Earth.
March 20	1 19 27	75 0·0	1·433
,, 22	1 25 58	74 32·8	
,, 24	1 32 43	74 6·7	1·350
,, 26	1 39 41	73 42·3	
,, 28	1 46 50	73 20·4	1·258
,, 30	1 54 8	73 2·1	
April 1	2 1 28	72 48·8	1·156
,, 3	2 8 42	72 42·4	
,, 5	2 15 37	72 45·3	1·042
,, 7	2 21 53	73 0·5	
,, 9	2 27 1	73 31·9	0·918

The distance from the earth is expressed, as usual, in parts of the earth's mean distance from the sun.

VARIABLE STARS.—Next week we shall give the times of maxima and minima of the better known variable stars for two or three months in advance, calculated from the elements in Prof. Schönfeld's last catalogue. It does not appear that an ephemeris for 1875 has been circulated as in several previous years.

THE FRENCH TRANSIT EXPEDITION TO NEW CALEDONIA

WE have received the following interesting communication from a correspondent:—

The French Transit of Venus Expedition to New Caledonia was the result of an after-thought on the part of the French Academy, which only took a definite form in the shape of active preparations for the great event in May last, months, if not years, after the other stations had been fixed on and the construction of the necessary instruments commenced. The New Caledonian observers were consequently at a great disadvantage, being obliged to complete all their arrangements within the short space of ten weeks, and to start for this *Ultima Thule* of civilisation in the middle of July. Everything, however, was got in readiness at home with so much care and despatch that nothing of the slightest importance, either in the astronomical or photographic department of the expedition, has been found wanting. The observatory has been fitted up and the observations made with as much completeness as if the centre of France, and not a convict settlement at the very opposite extremity of the world, had been the scene of operations, and the results, though not all that could be desired, are nevertheless well worthy of the time and money expended in obtaining them.

M. André, of the Paris Observatory, a well-known French astronomer, was appointed director of the expedition, whilst to M. Angot, Professor of Physics in the Normal School, Paris, the photographic portion of the work was entrusted. The instruments to be used consisted of five telescopes of various powers; a very complete photographic apparatus which will be described hereafter; a meridian instrument; an apparatus for producing an artificial transit, with electric chronograph carrying four pens attached; and lastly, two instruments for accurately determining the magnetic inclination and declination of Nouméa, which up to the present time have never been exactly known. The largest of the telescopes (7·5 in.), as well as three others (5 in.), was provided with an objective silvered by M. Foucault's process, the fifth having an unsilvered lens of 3¾ in. diameter, and of extremely good definition. All the instruments were equatorially mounted, three of them being connected with the chronograph, whilst the other two obtained their time by means of clock and chronometer. The telescope used for the photographic part of the work had an objective of 5 in. diameter and 13 ft. focal length, and was firmly fixed in a horizontal position on stone pillars, the image of the sun being directed along the axis by a large silvered mirror placed outside and moved at will from the interior by means of long wooden rods on either side of and parallel to the telescope. During the transit an assistant stood near this mirror, and at every command "*Découvrez*," removed the cover (placed on the mirror to prevent it becoming heated, and thereby causing distortion of the sun's image), and replaced it immediately after the plate had been exposed. With this apparatus, the daguerreotype process of sensitising a silvered plate of copper by means of iodine and bromine, developing in a mercury bath and fixing with hyposulphite of soda, was alone employed, and with the greatest success.

Though the day was somewhat cloudy, considerably over 100 very well-defined pictures of Venus during the Transit were obtained, together with 130 others, rendered less distinct by the intervention of clouds. When it is known that for several days previous to the 9th, the weather had been so bad that all hopes even of a glimpse of the transit of the planet were abandoned, and that dense clouds hung over the whole sky, and heavy showers of rain fell up to within four hours of the first contact, M. Angot may well be congratulated on the success of his labours. These daguerreotype pictures are not quite $1\frac{1}{2}$ in. in diameter, and were obtained by exposures of the plates varying from $\frac{1}{100}$ to $\frac{1}{80}$ of a second in duration. M. Janssen's method was not employed, but a very simple plan was adopted of placing the sensitised plate in a frame fixed at the focus of the chemical rays, and causing the exposure by sliding in front of it a metallic screen with a slit in it, whose width of course varied with the time necessary for exposure. A clock connected electrically with the sidereal one in the main observatory was placed in a convenient position above the telescope, and the instant of each exposure accurately noted. The assistants in this work, four in number, were all convicts, who performed their share with the neatness and readiness for which Frenchmen, whatever their position in life may be, are so remarkable; and, indeed, nothing has struck me more during the progress of the work here than the aptitude which seems innate in the French race for work of this kind; and it is no disparagement to English soldiers to say that it would have taken them days to learn to read chronometers with the accuracy which their French brethren-in-arms acquired in a few hours and apparently without the slightest difficulty. The main features in all the telescopic observations are the $3\frac{1}{2}$ minutes' difference between the estimated and observed times of first contact, the absence of the drop, and, in the case of the instruments furnished with silvered objectives, the clear tangential contact of the planet and the sun's limb, which enabled four out of the five observers to obtain the instant of second contact with very great accuracy. With these objectives, which appear to be especially well adapted for observations of this nature, the planet was seen to pass clear and distinct on to the sun's disc, without any appearance of distortion or cloudiness whatever; but with the unsilvered objective an appearance was observed as if a drop, such as those described by English astronomers, was about to form. Without forming, however, it changed almost imperceptibly into a tremulous haziness, which rendered it impossible to say when the actual contact took place, and compelled the observer to note two instants, one when this haziness first appeared, and the other when it had so far disappeared in the increasing brightness in the rear of the planet that he was confident that Venus was fairly on the solar disc. These two instants are separated by an interval of thirty-four seconds, and their mean corresponds within two or three seconds with the instant of tangential contact observed with the other instruments. Whether the slight cloudiness of the sky, or a constant error peculiar to all unsilvered objectives, or the fact that the latter telescope was focussed on a spot much nearer to the sun's limb than the other instruments, is to be put down as the cause of this difference or not, seems at present a matter of doubt only to be cleared up when other observations with unsilvered lenses are recorded.

The third and most important contact in New Caledonia was not observed, owing to a cloud which, much to our chagrin, strayed over the sun's face some 6′ before the estimated time of egress, and completely shut out our view for about 20′, after which the fourth contact was observed, but with a considerable degree of uncertainty, on account of the undulatory appearance of the sun's limb.

I may mention, in conclusion, that the times of duration of the whole transit, *i.e.* the interval between the first and fourth contacts, obtained by three of the observers, differed by only 8″, but these were considerably at variance with the estimated duration of the transit as given in the *Nautical Almanac*. Besides MM. André and Angot, three French officers, Capts. Derbés, Bertin, Ribout, and Mr. Abbay, took part in the observations. A.

On board the *Rangatira*,
Jan. 5, 1875

SCIENTIFIC REPORT OF THE AUSTRO-HUNGARIAN NORTH POLAR EXPEDITION OF 1872–74 *

THE real object of the expedition was not particularly that of reaching high latitudes, but rather the investigation of the large unknown sea north of Siberia; the explorers thought they might eventually reach Behring's Straits, without cherishing very sanguine hopes on this point. When during 1871 Lieut. Weyprecht made a preliminary expedition into those regions, he found the whole large sea between East Spitzbergen and Nowaja Semlja so completely unknown, that in spite of his stopping six weeks at Tromsö, and making inquiries of all Finnmark skippers and whalers, he could not learn anything definite as to the conditions of climate and ice in those parts; few vessels had succeeded in reaching the 76th degree of north latitude. During the two Austrian expeditions this unknown sea has been investigated from 40° to 70° East long. (from Greenwich), and beyond the 79th degree of latitude on the west side and the 80th on the east side; an extensive, hitherto unknown tract of land has been discovered, and Lieut. Julius Payer has made sledge journeys into this land, reaching very nearly 83° N. lat.

In 1871 the explorers had found the sea completely free from

* Die 2. Oesterr.-Ungarische Nord Polar Expedition unter Weyprecht und Payer, 1872–74. (Petermann's Geogr. Mittheilungen, 1875; heft ii.)

ice as far as 78° N. lat., north of Nowaja Semlja, and their intention at the second expedition was to investigate this sea in an easterly direction, taking the Siberian coast as basis, and depending on the influence of the great Siberian rivers, whose great quantities of comparatively warm water probably free the coast from ice almost every summer.

Unfortunately the year 1872 was one of the most unfavourable ever seen. Already in 74°·5 N. lat. the explorers found ice; they could only reach Cape Nassau with great difficulty, and were finally blocked up by packed ice in a locality where, in the previous and following years, there was no ice for one hundred German miles round. They never got within the reach of the Siberian rivers, and the uncertainty with regard to their influence upon the ice along the Siberian coast is still the same as ever. But one point is clearly proved, namely, that the conditions of ice between Spitzbergen and Nowaja Semlja are highly variable from year to year; this circumstance, more than any other, speaks against the advisability of future expeditions to be made on the basis of Franz-Joseph's Land. In 1874 the explorers found the ice again in the same position as in 1871; there is perhaps a certain periodicity in this.

Lieut. Weyprecht formerly thought that marine currents were the principal cause of the general motion of the ice in Arctic regions; he is now of a different opinion, as he maintains that during the drift of their vessel, which was frozen in, in packed ice, and drifted in this state for over fourteen months, the influence of currents was imperceptible compared to that of winds upon the drifting ice. The existence of Gulf-stream water in the great area between Norway, Spitzbergen, and Nowaja Semlja is undeniable; the current cannot, however, be traced directly by its course, but rather by the unproportionally high sea-temperatures in those high latitudes. As a natural consequence of this, the Gulf-stream does not regulate the limits of ice, but the ice, set in motion by winds, regulates the limits of the warmer Gulf-stream water, depriving the same of the last degrees of heat which it contains. A comparison of the *Hansa* drift with the winds would show whether on the east coast of Greenland the drift of ice is only produced by the latter; Sir L. M'Clintock proves with figures that this decidedly is the case in Baffin's Bay. The speed of the drift of course depends upon the force of the winds, local conditions, vicinity of coasts, and the more or less open water. The great influence of the wind upon the ice-fields is explained by their ruggedness; each projecting block represents a sail.

In the vicinity of coasts it is somewhat different; immense currents are often perceived there, originating through the tides, or perhaps through the motion of the ice itself and the winds.

There is a decided general tendency in the ice to move southward during the summer; the reason of this may be the flowing off of melted water in all directions, which causes a breaking-up of the whole Arctic mass of ice. But all other influences upon the motion of the ice are nearly imperceptible when compared to that of winds, and can only be traced in their most general effects. It is quite certain, however, that in the south of Franz-Joseph's Land there is a constant flow of ice from east to west, *i.e.* from the Siberian sea. If the field of ice which held Lieut. Weyprecht's ship a prisoner had not attached itself to Wilczek Island, it would have drifted towards the northern end of Spitzbergen; he arrives at this conclusion from observing the winds of last winter.

To the influence of winds Lieut. Weyprecht also ascribes the existence of open water near all west coasts in those regions: he found the main direction of winter-storms in Franz-Joseph's Land to be E.N.E.; the ice under west coasts is therefore constantly broken up. Lieut. Payer, on the northernmost point he reached, was stopped from extending his sledge journeys further by open water near a west coast, upon which he was travelling.

Also, with regard to quality, the ice in those seas is very variable. While in the summer of 1873 the explorers could not see the end of the field in which their ship was frozen in, they never met fields of such an extent during their retreat; also, with regard to thickness, there was great variety. In 1873 their field formed an irregular frozen mass, with high ice walls in all directions and immense protuberances; in 1874 they found much greater evenness, and although thawing had begun so late that they almost perished with thirst during a month and a half, the ice was so thin in some places at the end of July that they often broke through while drawing their sledges. During the drift the whole mass was doubtless packed very closely; the field, in spite of the constant drifting motion, did not turn round, the bow of the ship pointing always in the same direction; only in September, when the field was greatly reduced, it began to turn; in October and November large holes were seen in it in the vicinity of the coast, towards the south.

Whether Franz-Joseph's Land can again be reached by ship, Lieut. Weyprecht thinks mainly dependent on favourable conditions of weather and ice; in any case a very warm summer will be necessary, and then it could be done only late in the year. As the most favourable point to start from in such an expedition, he indicates 45° East long., as here he found the barrier of ice in 1871 to be fifty German miles more to the north than in 60° E. long.

In the preliminary expedition of 1871, Lieut. Weyprecht found sure signs of the vicinity of land in 43° E. long. and 78°.75 N. lat., and accordingly he proposed this unknown land as the basis for future expeditions sent to reach the pole. The mysterious Gillis-Land is situated upon 30° E. long. The south coast of Franz-Joseph's Land was seen by Payer at least as far as 50° E. long. Lieut. Weyprecht now thinks he may be permitted to conclude that these three points are connected. Thus Franz-Joseph's Land would become greatly extended in a western direction. Numerous icebergs floating along the coast seem to confirm this idea, and it is hardly necessary to point out how much the interest in Arctic investigation would be increased by this idea proving a correct one.

During a year and a half the explorers had constant opportunities closely to observe the behaviour and formation of packed ice. The phenomenon is instructive, as it is the same in the whole of the Arctic regions. With the exception of land-ice, which clings to the coasts and never reaches far out into the sea, all ice—icebergs as well as fields—is in constant motion, winter and summer; and this, as has been shown, is through the influence of winds. The motion, however, is a different one almost with every field, and thus a certain pressure results wherever two fields touch; this naturally leads to the breaking up of the fields, and the contraction of the ice during sudden low temperatures plays its part in a similar way. If one considers the great extent of the fields, sometimes of many miles, and their enormous masses, one can easily imagine the colossal forces which are active in these phenomena, and the greatness of their effects. When two fields meet, a combat body to body ensues, often lasting only a few minutes, but sometimes even for days and weeks. The edges are then turned up on both sides, upwards and downwards, an irregular wall of ice consisting of wildly-mixed blocks begins to build itself, the pressure increases more and more, masses of ice eight feet long and broad are lifted 30 to 40 feet high, and then fall to make room for others. At last one of the fields begins to shift itself for some distance underneath the other one; often they separate for a while, only to renew the struggle. But the end of it always is that the intense cold unites all into one solid mass; a single field results from the two, and the next storm or quick change of temperature cracks the new field in some other direction, the pieces renewing the old struggle. This is the origin of the ice-fields, which are quite irregular above and below, sometimes only consisting of blocks that have frozen together, and filling up the whole Arctic region as so-called pack-ice.

During winter, snow-storms fill up all smaller irregularities completely. As soon as the sun begins its action, the crushing of the ice decreases, the winterly ice walls diminish considerably, immense masses of ice and snow are melted, and the resulting sweet water forms large lakes on all the lower even parts of the fields. During the summer, about four feet of ice are thus melted down from above; of course the whole field and everything upon it—the explorer's ship, for instance—is raised so much higher. In the following winter it grows below in the same ratio, and thus the whole of the ice is in an uninterrupted process of renovation, from below upwards; we may conclude that all the old pack-ice is replaced by new in the course of two years.

The spaces of open water which naturally occur during the great crushes are soon again covered by fresh ice in winter; the intense cold keeps repairing the broken field of ice. Lieut. Weyprecht observed that within twenty-four hours, and with a temperature of $-30°$ to $40°$ R. ($37·5 - 50°$ C.), the new crust becomes about a foot thick. The salt of the sea-water has not time to be displaced entirely, the formation of ice going on too quickly, and a considerable quantity freezes into the upper strata of the ice; this quantity decreases downwards as the ice takes more time to form. Beginning at a certain thickness, the ice is almost free from salt. The upper strata, however, on account of the salt they contain, attract moisture in a great degree, and form a tough, leathery mass which bends under foot without breaking. This, however, is only the case with new ice, as after a short time the salt crystallises out of the ice, and the surface covers itself with a snowy layer of salt, sometimes reaching two inches of thickness. Even in the most intense cold this layer retains so much moisture that it makes the impression of a thaw; only little by little, evaporation and drizzling snow do their work, and the ice itself becomes brittle.

In this way almost all the salt, which was frozen in, crystallises out, and is washed off and back into the sea by the melted water in the next summer. The melted water at the end of the summer is therefore almost free from salt, and has a specific gravity of 1·005. It is evident that a smooth plane of ice, as is found on sweet water, is a very rare occurrence in Arctic regions.

The finest and most interesting phenomenon, the only change in the long night of winter, is the Aurora Borealis; no pen can describe the magnificence of this phenomenon in its greatest intensity. In February 1874 Lieut. Weyprecht saw an aurora, which ran beyond the zenith from east to west like an immense stream of fire, and constantly showed intense prismatic colours running like flames, and as quick as lightning, from one side of the horizon to the other. At the same time flashes of fire came from the southern horizon and reached to the magnetic pole; it was the most stupendous natural display of fireworks he had ever been able to imagine. With regard to the intensity of the aurora, Lieut. Weyprecht says he can prove by data that it differs, independently of the geographical latitude, in the different parts of the Arctic zone, and that the district he visited was a maximal district; when the sky was clear, traces of auroræ could be uninterruptedly observed; in the second winter he even kept an "aurora journal," which, however, gave only few positive results, and was left behind in the ship. The phenomenon is past all description and classification, changing constantly and showing new forms at every moment. Lieut. Weyprecht was never able to describe the origin of an aurora; the phenomenon is there, and it is impossible to say whence it came.

Only in a very general way three forms of auroræ can be distinguished: first, quiet, regular arcs, slowly passing from the southern horizon and disappearing in the northern one; then, bands of light of great variety of forms, ever changing place and intensity; and lastly, the so-called corona, *i.e.* radiations from or towards the magnetic pole. Generally the colour is an intense white with a greenish hue; with greater motions and stronger radiations the prismatic colours are often seen in great intensity.

Lieut. Weyprecht spent much time and trouble on spectral observations of the auroræ, but unfortunately his spectroscope was too small and imperfect. He could never see more than the well-known green line; compared with the spectral observations of the Swedish Expedition, which were made with much more perfect instruments, his observations are of no value. One interesting fact with regard to the auroræ was, however, ascertained. It was found that upon very intense auroræ storms followed almost every time; this is proved by meteorological data, and Lieut. Weyprecht thinks he is justified in the conclusion that the Aurora Borealis is an atmospheric phenomenon and closely connected with meteorological conditions; he arrived at this conviction through observing hundreds of auroræ, but says he cannot give any positive or important reason for his conclusion.

(To be continued.)

JOHN EDWARD GRAY, F.R.S.

WE have to record the death, on Sunday morning last, at his residence in the British Museum, of Dr. J. E. Gray, late Keeper of the Zoological portion of the National Collection.

Dr. Gray was born in 1800 at Walsall, in Staffordshire, being the eldest of the three sons of Mr. S. F. Gray, a chemist of that town. He was educated for the medical profession, and very shortly exhibited his biological taste, by writing a work on the then new "natural" arrangement of plants. In 1824 Dr. Gray was appointed an assistant in the Natural History department of the British Museum, where, with the assistance of Dr. Leach, he commenced the study of zoology to such good purpose that in 1840 he succeeded Mr. Children as Keeper of the Zoological Collection of the Museum. At that time biology held but a small place in popular favour, especially in the eyes of those most active in the superintendenc of the extension of the British Museum. Against the opposing influences thus affecting his department, not the least of which was the antagonism of Mr. Panizzi, Dr. Gray, by his indefatigable zeal and courage to face obstacles, nevertheless succeeded in bringing the national collection of osteological and skin specimens, during the thirty-five years of his keepership, to so high a standard of excellence, that no other museum, not even Leyden itself, is equal to it.

Most of the biological societies which now exist include Dr. Gray amongst their founders or earliest members. The Zoological Society owes much to him, the number of papers communicated to it by him being very great. He was the leading spirit of the *Annals and Magazine of Natural History*, and was the author of the *Zoological Miscellany*, *Knowsley Menagerie*, and other works. In his Catalogue of the Mammals in the British Museum, which is far advanced towards completion, is incorporated much of the author's work in that direction, published originally in separate short papers.

The qualities which most distinguished Dr. Gray as a naturalist were his great industry in combination with an acute perception of minute distinctions. His imperfect acquaintance with anatomy in many of its branches much limited his generalising powers, and in some cases distorted his view of the relative importance of character based only on osteological features. To all students of the groups of animals which were touched upon by Dr. Gray—and there are but few that were not—that author's work will be found invaluable, both from the independent light which it throws on the subject, and from the careful review which it gives of the previous investigations of other naturalists.

Dr. Gray was elected a Fellow of the Royal Society in 1832; he resigned the Keepership of the British Museum at Christmas last. He leaves a widow, but no children.

NEW ORDER OF EOCENE MAMMALS

AT the last meeting of the Connecticut Assembly, February 17, Prof. O. C. Marsh made a communication on a new order of Eocene Mammals, for which he proposed the name *Tillodontia*. These animals are among the most remarkable yet discovered in American strata, and seem to combine characters of several distinct groups, viz., Carnivores, Ungulates, and Rodents. In *Tillotherium*, Marsh, the type of the order, the skull has the same general form as in the bears, but in its structure resembles that of Ungulates. The molar teeth are of the ungulate type, the canines are small, and in each jaw there is a pair of large scalpriform incisors faced with enamel, and growing from persistent pulps, as in Rodents. The adult dentition is as follows:—Incisors $\frac{2}{2}$; canines $\frac{1}{1}$; premolars $\frac{3}{2}$; molars $\frac{3}{3}$. The articulation of the lower jaw with the skull corresponds to that in Ungulates. The posterior nares open behind the last upper molars. The brain was small, and somewhat convoluted. The skeleton most resembles that of Carnivores, especially the *Ursidæ*, but the scaphoid and lunar bones are not united, and there is a third trochanter on the femur. The radius and ulna, and the tibia and fibula are distinct. The feet are plantigrade, and each had five digits, all terminated with long, compressed and pointed, ungual phalanges, somewhat similar to those in the bears. The other genera of this order are less known, but all apparently had the same general characters. There are two distinct families, *Tillotheridæ*, in which the large incisors grew from persistent pulps, while the molars have roots; and the *Stylinodontidæ*, in which all the teeth are rootless. Some of the animals of this group were as large as a Tapir. With *Hyrax* or the *Toxodontia* the present order appears to have no near affinities.

METEOROLOGICAL OBSERVATIONS IN THE PYRENEES

M. DURUOF, the French aëronaut, has just completed a series of three ascents executed from Pau, for the purpose of studying the state of the atmosphere during the recent cold season. Thrice M. Duruof started with a north wind at the surface of the earth, and thrice he was able to find an upper current blowing from the south. The last time he started at 1.30 P.M., travelled upward until 2.30 P.M., moving southwards, when having reached a higher level he was carried northwards. He landed safely at 4 P.M. in the department of Gers.

He found in his last trip that the wind was veering regularly with increasing altitude, and was steady at certain levels, so that it was possible to go in any direction by keeping the proper altitude for a sufficient length of time. All his changes of direction were traced on an Ordnance Survey map. His readings and observations will be sent to the Academy of Sciences for further discussion.

It was observed during the recent cold period that the barometer was low with a northern wind, which is unusual. The three ascents of Duruof may be regarded as affording an explanation of the fact, if we suppose the southern current to have been general at an altitude of 4,000 to 9,000 feet above the earth.

The superior current on the 4th of March was carrying immense quantities of snow at a temperature of 0° C. The snow rapidly melted in its descent, as the air was mild below. It is probable that this snow was caused by the influence of the Pyrenean range, which is very cold. I observed at Paris an effect which can be ascribed to similar causes, from hilly parts of our geological basin situated in the south. On that very day the sky was covered in the south and blue in the north, where immense plains extend to any distance.

At all events the southern aërial stream which carried the balloon northwards was very thick. M. Duruof was unable to find its upper surface, although he reached the level of 11,000 feet.

Other ascents will be made by the same enterprising aëronaut, whose special attention has been so long devoted to the utilisation of various currents according to altitude.

W. DE FONVIELLE

SCIENCE AT THE NEW PARIS OPERA*

II.

ALL branches of Physics are represented in the New Opera; Heat, Light, Optics, Electricity, Acoustics play their different parts. So far as acoustic instruments are concerned, we may refer to an organ constructed by M. Cavaillé-Coll, and formed of eighteen registers, distributed over two key-boards, and a complete foot-board. This organ is worked by four pedals, vibrating the air contained in 1,032 pipes, of which some are more than five metres in height, and above ·30 metre in diameter, But it is the electric light which has most interest for us.

After giving a brief account of the invention and history of the voltaic pile, M. Tissandier proceeds to describe the battery connected with the New Opera, which has been organised by M. Duboscq.

The electric light may be thrown upon the magnificent stage by means of a Bunsen battery of 360 elements, which is established in a room on the ground-floor, the length of which is not less than seven metres. M. Duboscq has here arranged six tables of 2·75 metres long by ·75 metre broad, which each support a Bunsen battery of sixty elements (Fig. 5). This battery is placed upon the table which is made of very thick unpolished glass that cannot be injured by the acids. The elements are arranged in four rows of fifteen each. The table is provided underneath with a board which supports a large rectangular basin, in which the plates are placed after they have been used. The jars of the battery, filled with nitric acid, are, after being used, placed in a tub containing the acid and closed with a wooden lid.

In order to work a battery of such power under favourable conditions, M. Duboscq has had to make special arrangements for the preparation of the sulphuric acid liquid as well as for the zinc amalgams necessary to put the system of batteries in action.

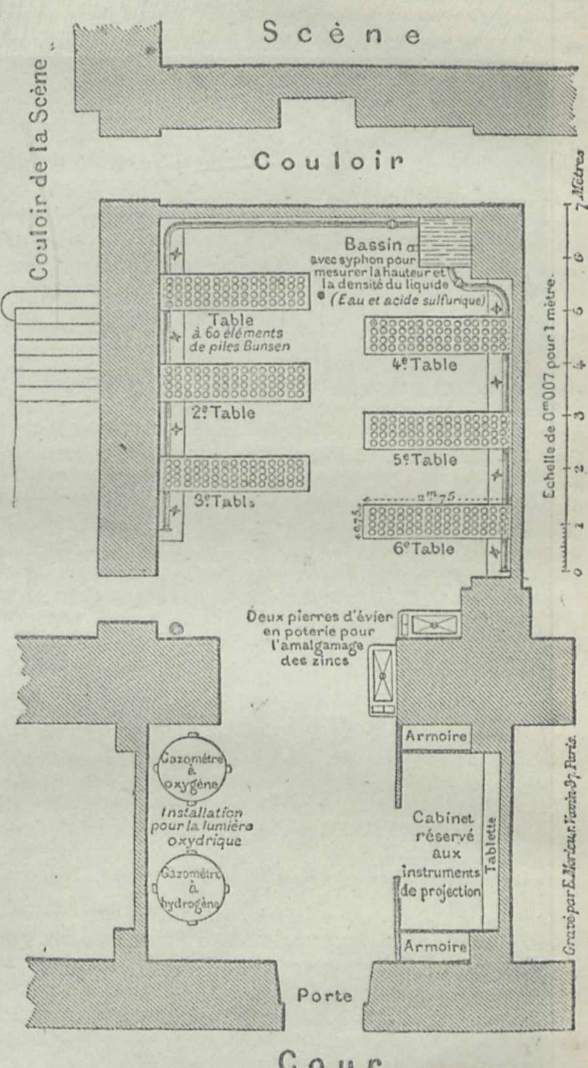

FIG. 5.—Plan of the Electric Room at the New Opera.

At the right corner of the electric room is a large reservoir, of the capacity of about one cubic metre, where water mixed with one-tenth of sulphuric acid can be stored. A spigot permits this liquid to run into a vertical siphon formed of a large tube, into which an areometer is plunged to ascertain its quality, and make sure that the preparation has been made in the proper proportions. The reservoir is furnished at its lower part with an earthenware pipe which is conducted along the walls of the room, opposite the six battery tables. Beside each table an earthenware spigot enables the operators to run the liquid into earthenware jugs, from which they fill the battery jars with the liquid.

By an excellent precaution M. Duboscq has obviated

* Continued from p. 351.

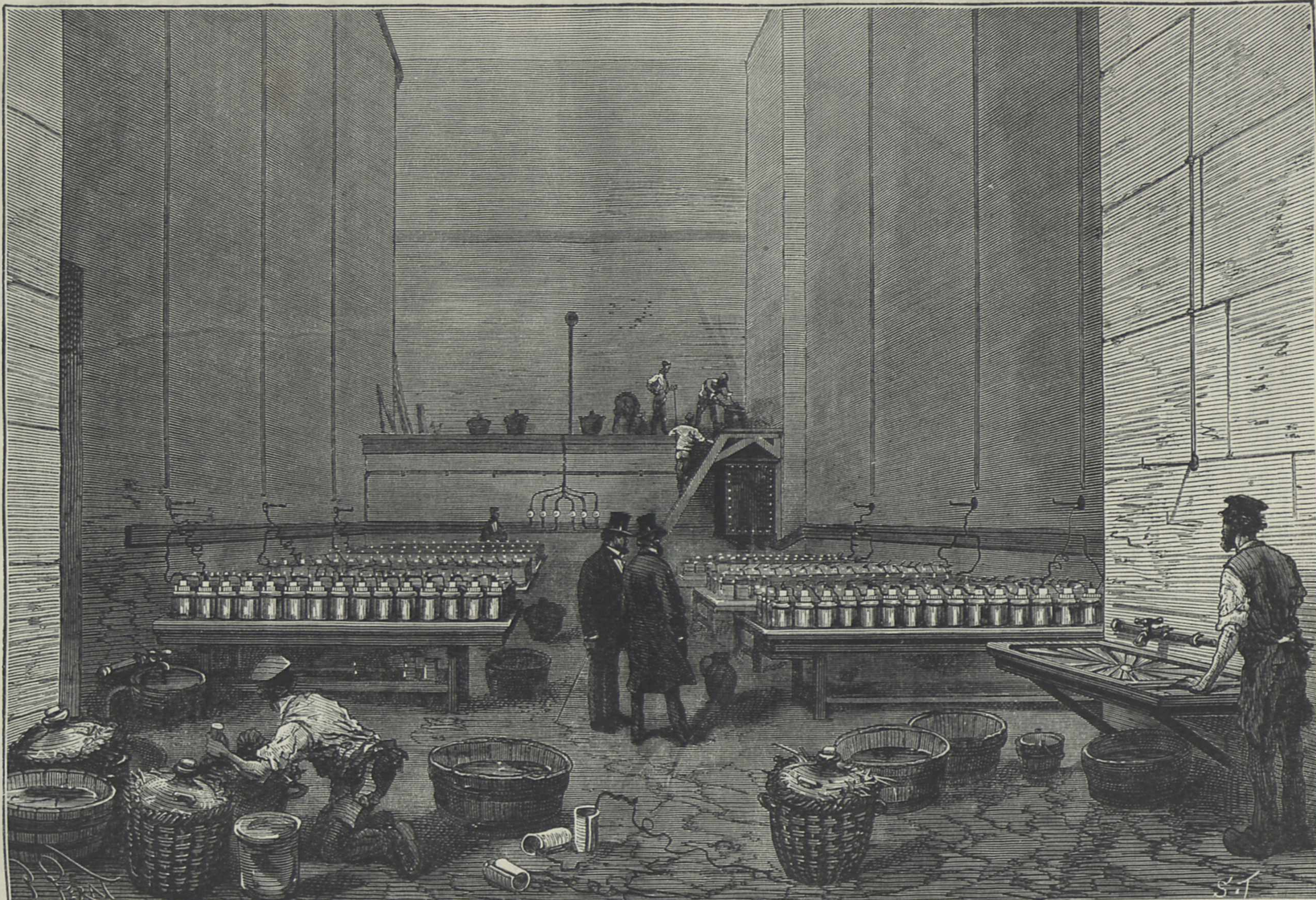

FIG. 6.—View of the Electric Room at the New Opera.

the dangerous action of the nitrous vapours, by placing here and there upon the piles saucers containing ammonia, which condenses them.

Each table, as we have said, forms a battery of sixty elements. The electric wires are conducted along the wall at the bottom of the room, where they traverse six galvanometers (Fig. 6). Each of these galvanometers indicates, by means of the needle with which it is provided, the condition of the battery to which it corresponds. The six isolating wires, after leaving the six galvanometers, pass along the walls to the stage, where the currents which they carry may be utilised either singly or by twos or threes, according to the degree of intensity which it is wished to give to the light. The distance which the current runs from the electric room to the most distant point of the stage is about 122 metres; the total length of all the wires is about 1,200 metres.

M. Duboscq, imitating the systems of telegraphic wires, makes use of the earth as a return current; one of the poles of each battery is in communication with the iron of the building. Without this arrangement it would have been necessary to double the length of the wires.

In most instances M. Duboscq places his electric lamp on one of the wooden galleries which run along the higher regions of the scenery above the stage. It is from this artificial sky that he, a new Phœbus, darts upon the nymphs of the ballet the rays of his electric sun. It is from here, decomposing the light by means of the vapour of water, he throws upon the stage a veritable rainbow, as in *Moses*; again, it is thus that he causes the light from the painted windows to fall upon the flags of the church where Margaret is in the clutches of remorse. Sometimes the electric apparatus is placed on a level with the stage, when it is sought to produce certain special effects, such as that of the fountain of wine in Gounod's opera. The lime-light is also used to produce certain brilliant effects in the New Opera.

It will thus be seen that the electrical arrangements in the New Opera leave little to be desired. There is an electric battery of extraordinary power, which might be profitably used for certain experiments of high interest, requiring an electric power of great intensity. M. Tissandier makes the very happy suggestion that this powerful battery might be utilised for the purpose of scientific research, and we hope that those who have the management of the Opera will take his hint; they ought to remember how much their art owes to the researches of science. He also very appropriately suggests that the Government which has made such a lavish expenditure, forty million francs, on a place of amusement, might also benefit the country even more by doing something to restore to efficiency the buildings in which the work of science is carried on. At all events it will be seen that in this magnificent building Science occupies a place of no mean importance.

NOTES

LETTERS have been received from the Eclipse Expedition from Suez. They had heard from the Viceroy that arrangements had been made to have a vessel awaiting them at Galle.

THE following telegram has been received by the *Times* from its St. Petersburg correspondent, with regard to the Transit of Venus:—"Herr Struve reports that at Hakodaki both interior contacts were observed. At Wakhodka, on the coast of the Pacific east of Vladivostock, only the first interior contact was observed. At Kamen Riboloff, on Lake Hanka, all four contacts were satisfactorily observed, but no heliometric measurements. At Ashooradeh, on the Caspian Sea, some diameters and chords were measured; but the sun was covered by clouds at the moment of contact. No report yet from Pekin." We would also call attention to the account of the French observations in New Caledonia, which we publish this week, and to the interesting letter in yesterday's papers from Capt. Fairfax, of the *Volage*, to the Admiralty, giving some details of the Kerguelen Island parties. The astronomers, he says, are pleased with their success. News has now been received more or less from all the Kerguelen parties; we hope to be able to summarise them next week.

PROF. C. S. LYMAN writes to the *New York Tribune* to say that he observed the planet Venus on the 8th of December, a few hours before its transit began, and found that from the time when it was 1° 50′ distant from the sun's centre, up to the time of its passage across its disc, it was apparently surrounded by a ring of light, which appearance was due to the refraction of the sun's light passing through the planet's atmosphere on its way to the earth. This phenomenon was first observed by Prof. Lyman in 1866, and will again occur in 1882, being repeated, in fact, as often as the planet approaches within the limiting distance above mentioned. When further from the sun than this limit, the circle of light becomes a segment only, whose size diminishes as the planet recedes from the sun.

MR. SLATER, one of the naturalists sent out by the Royal Society with the Transit of Venus Expedition to Rodriguez, is now on his way home. Dr. Balfour, who, after his special work, has devoted a month to the Island of Bourbon, is expected to arrive in England at the end of the present month. The collections made have been embarked, and there is reason to hope that in the course of a few weeks we shall be in possession of a complete report of all that has been accomplished by the three young men appointed to explore the singular island Rodriguez. An instalment of their results has already appeared in the Proceedings of the Royal Society. In like manner, Mr. Gulliver is devoting a month to marine zoology at Zanzibar.

THE list of candidates for the Fellowship of the Royal Society is closed for the present session. The number up is fifty-four.

WE hope that advantage will be taken of the *conversazione* of the Royal Society which is to be held on the 7th April, to exhibit the improvements effected in philosophical apparatus during the past year. It has happened more than once that an important improvement has been shown for the first time at the Royal Society, and we shall be glad if the practice can be continued. The rapidity with which instruments become obsolete in these days is perhaps the most remarkable evidence of the advance of science.

THE large and influential deputation from University College which waited upon the Duke of Richmond and Viscount Sandon on Tuesday received what we think may be regarded as on the whole a satisfactory reply. The deputation showed that the means and buildings and apparatus at the command of the College are totally inadequate to the present advanced position of science and to the efficient discharge of the work which the much underpaid professors have to perform. The Duke of Richmond's reply shows, we think, that the Government are really anxious to help the cause of science and of education as far as the means at their command will enable them. He rightly said that the movement which caused the deputation to wait upon him and his colleague is a legitimate one. "I think," he said, "it would be advantageous to us in considering this question if the Council of the College could see their way to lay before us some estimate of the sum of money that they would seek from the Government, and the mode in which they would propose to spend the money if a sum were granted." This seems to us quite reasonable, and augurs well for the cause of those institutions which can really prove that they deserve to be helped.

As was to be expected, the estimates for the Arctic Expedition were passed by the House of Commons last Friday with complete unanimity. The sum asked for was 98,620*l*. There was appended to the estimate a further sum of 16,000*l*. for the next financial year; and for future years, while the expedition is out, there will be an additional sum of 13,000*l*. In addition to all this, there is a contingent possibility of about 50,000*l*. being required in case of its being thought necessary or desirable to send out a relief ship in consequence of the expedition not having returned as soon as was expected. We do not think it likely that this last item will ever be required, though it is creditable to the House that not a voice was raised against any of the items in the estimate. It has been decided that a man-of-war will accompany the expedition as far as Upernivik, where she will fill the ships up with coals and provisions. It is stated that the *Pandora*, which was one of the vessels named for the expedition, but was condemned on survey, has been purchased from the Admiralty by Mr. Allen Young, a lieutenant in the Royal Naval Reserve, and it is rumoured that he will assume command of her, and accompany the *Alert* and *Discovery* during the summer. Mr. Young served with Admiral Sir Leopold M'Clintock on board the *Fox* in the Franklin Search Expedition.

Some official papers concerning the Arctic Expedition have just been published by the Admiralty; these contain the arguments which have been urged on behalf of the Smith Sound route, as well as details concerning the fitting of the ships, appointment of officers and men, &c., with which our readers are already familiar. The chosen route offers the only promise of a continuous coast-line stretching far northwards, and upon this fact the prospect of reaching the Pole by travelling parties mainly depends. It is, moreover, the only route, so far as our knowledge extends, where the operations of an expedition can be confined within such limits that succour would be reasonably certain of reaching it. Along with the papers an Admiralty Chart of the Polar Sea is published. Rear-Admiral Sir F. Leopold M'Clintock will supply each of the two ships with a copy of his own manuscript notes on the fitting of sledges and tents, the scale of clothing and provisions, and all the results of his own experience in sledge travelling. The article on the work of the Arctic Expedition, in the last number of the *Geographical Magazine*, is mainly taken from these notes.

We regret very much that it has been finally decided that no professional geologist shall accompany the Arctic Expedition, the main reason, we believe, being the want of accommodation. The fact is that a botanist is to be sent out who is not wanted, as one of the surgeons is a good botanist; while the place required for a geologist is thus uselessly occupied. The expedition is nothing if not scientific, and surely geology is one of the sciences in which some of the most valuable results would be obtained by an expedition to high polar lands. In this connection we would draw our readers' attention to the first instalment of a paper in this week's Nature, giving some valuable details of the scientific results of the Austro-Hungarian Expedition. If the results of our expedition be as valuable in proportion to its size and equipment, we may expect science to reap a large harvest indeed.

A letter from Captain David Gray appears in Heft iii. of Petermann's *Mittheilungen*, giving reasons for his preference of the East Greenland-Spitzbergen route for Polar exploration over the Smith Sound route. It is accompanied by an illustrative map.

To note the appearance of a new scientific society is one of the chief pleasures in recording the progress of science; and when the incident occurs in the midst of a community given up to commerce, the pleasurable feeling is enhanced. A Society has been started in Trieste, that busy port at the head of the Adriatic, under the title "Società Adriatica di Scienze naturali," or, as the German-speaking portion of the inhabitants call it, "Naturwissenschaftliche Adriatische Verein." We have received a list of the members, a copy of the statutes, and the first number of the *Bollettino*. This, an octavo of about sixty pages, published in December last, contains an address by Dr. Syrski on the objects of the Society, and on the advantages generally of the study of natural history; a paper, with illustrations, on the "Organi della riproduzione e della fecondazione dei pesci ed in ispecialità delle Anguille;" and one of much interest, "Sulle attuali cognizioni chimiche del mare Adriatico." These papers exemplify the scheme which the Society has formed—investigation of the Adriatic and its coasts, and the promotion of a knowledge of natural history. In carrying out this scheme there are many important questions which may be elucidated, especially in a southern latitude, and we offer to the new Society our best wishes for its success. We hope it will find many correspondents in this country.

The Ateneo Propagador de las Ciencias Naturales offers a prize of 500 pesetas (about 20 guineas) for the best original memoir on the mineralogy, botany, or zoology of Spain. Any person, whether a member of the society or not, can compete for this prize. Memoirs must be sent in to the secretary of the society before the 30th September, 1875. A printed paper with further particulars may be procured from the secretary, whose address is Calle Ancha de San Bernardo, 15, Madrid.

The new part of the official Topographical Atlas of Switzerland contains the first part of a new hydrographic map, in four sheets, of the Lake of Geneva, the result of a recent minute examination of the lake by the Government engineer, M. Ph. Gosset. From these sheets a clear and precise idea of the configuration of the lake may be obtained, and M. Gosset's examination confirms generally that of De la Beche made about fifty years ago, the former, however, being infinitely more precise and detailed. The bottom of the lake forms a large valley bordered by two slopes (*talus*). The length of this plain is about six kilometres; its bottom is very flat, and the inequalities never exceed ten metres in a transverse section of the lake. Profiles taken perpendicularly to the axis of the lake are nearly all contained between two curves of ten metres in height. There is nothing in the axis of the lake like a longitudinal valley; on the contrary, there is rather a slight median elevation, and two lateral valleys, not strongly marked, along the foot of the slope. One interesting result of M. Gosset's examination is to confirm the absence, in the depths of the lake, of accidents, inequalities, rocks, glacial moraines, and erratic blocks. Further details of this valuable map may be obtained in an article by Dr. Forel in the January number of the *Archives des Sciences* of the *Bibliothèque Universelle*. The article has also been separately reprinted.

We regret very much the news that the expedition which started from Burmah into China some time ago (see Nature, vol. xi. pp. 175 and 209), has met with a disaster. On February 22, at a place called Mauwine, it was attacked by several hundred Chinese, together with a large number of the hill tribes. The main body of the expedition escaped with three wounded, but losing, it is feared, either the greater part or the whole of its baggage. Moreover, a distinguished Engineer officer, Mr. Margary, who had made his way overland from Burmah to form the expedition, was separated from it, and with five Chinese servants surrounded and killed.

The recent polar weather has told heavily upon French men of science. Every week a fresh death is reported, and this week we are apprised of the death of M. Louis Mathieu, at the age of ninety years. M. Mathieu was elected fifty years ago to fill the place vacated by the death of Mestier. That celebrated comet-seeker of the eighteenth century had been himself a member of the Academy for fifty years. Two persons occupying the same seat for a period of more than a century is an example of academical hereditary longevity which is likely very seldom to occur. M. Mathieu was the brother-in-law of Arago, a circumstance which had added much to his personal credit and influence. He was a member of the Bureau des Longitudes, and editor of the *Annuaire* for more than sixty years. He had been employed in the first part of the century in connecting French and English triangulations.

THE supplementary part No. 42 of Petermann's *Mittheilungen*, advance sheets of which have been forwarded us, contains the first half of a translation from the Russian of the celebrated traveller Sewerzow's exploration of the Thian Shan Mountain System in 1867-68. A translation of the same traveller's exploration of the Tschu and Syr Darya region in 1864-65 appeared in the *Journal* of the Royal Geographical Society for 1870, by Mr. R. Mitchell. The present translation is accompanied with a magnificent chromolithographic map of the mountainous region around Lake Issyk-Kul, from Russian official surveys. Sewerzow made a careful study not only of the geography, but of all departments of the natural history, of the meteorology, and general physical characteristics of the region which he explored.

THE Council of the Senate of Cambridge University have had under their consideration the duties and stipend of the Jacksonian Professor. The Council are of opinion that it will be advantageous to the University, as well as in direct conformity with the design of the professorship, that the lectures of the professor should be directed hereafter, at least in part, to the illustration and advancement of the knowledge of some branch or branches of applied physics. They further recommend that the next Jacksonian Professor receive from the University chest such a sum as will with his endowment stipend raise the income of the professorship to 500*l*. per annum; that he shall be required to reside within the precincts of the University for eighteen weeks during term time in every academical year, to give one course of lectures in each of two terms at least, and to give not fewer than forty lectures in every academical year.

THE same body have recommended that a managing council, consisting of the Vice-Chancellor and twelve other members of the Senate, be appointed in connection with lectures and classes at populous centres; and that the Syndics be required to make an annual report to the Senate.

THE Council of the Pathological Society, we learn from the *British Medical Journal*, have arranged that a discussion shall be opened, by Dr. Charlton Bastian, F.R.S., at the meeting of April 6th, on the Germ-theory of Disease, being a discussion of the relation of Bacteria and allied organisms to virulent inflammations and specific contagious fevers. It is expected that Dr. Burdon-Sanderson will take part in the discussion; and it is hoped that, besides the members of the Society interested in this important subject, Prof. Lister of Edinburgh, and it may be Prof. Billroth of Vienna, will find opportunity of being present and taking part in the debate.

AT the last *soirée* of the Paris Observatory, M. Cornu made some exceedingly interesting experiments with his apparatus for measuring the velocity of light. The mirror for reflecting the ray had been placed on the top of a barrack at only 1,280 yards from the Observatory. The wonderful effect of the extinction of the ray by a certain speed of rotation of the wheel was easily observed, as also its reappearance with an increased velocity. The cloudy state of the atmosphere did not prevent the experiment from being a success. It is expected that the apparatus will be sent to the next meeting of the British Association.

AT a recent meeting of the Senate of the University of London, it was resolved that there is no sufficient reason for perpetuating the slight differences which at present exist between the curricula of the Women's General Examination and the Matriculation Examination; and that in and after the year 1876 the curriculum of the Women's General Examination be the same as the curriculum for the time being of the Matriculation Examination, except that, in the year 1876, women shall have the option of being examined according to the present instead of the altered curriculum.

THE meeting of delegates of the French Sociétés Savantes will take place at the Sorbonne after Easter, as usual, and will have a special interest for meteorologists. M. Leverrier, who will be appointed the President of the Commission of Sciences, has sent a circular to the several presidents of the Meteorological Commissions, asking them to send as many meteorologists as they can to Paris on that occasion; the intention of the Ministry being to call a special Congress for Meteorology in order to group together the various Departments into natural meteorological districts.

THE destruction of seals in the Arctic seas has been carried on to such an extent that fears are entertained of the annihilation of these animals. The Peterhead sealers and whalers have therefore determined to agree to a "close time," during which it shall be unlawful for any sealing-ship to kill seals, or even to leave port for the fishing-grounds; thus giving the newly-born seals time to develop into a useful size, and enabling even the parent-seals to escape. It is hoped to extend this regulation to other countries engaged in the industry; and the Board of Trade has been in correspondence with various authorities on the subject. The papers in connection with the case have been presented to Parliament, and will shortly be printed, when the decision of the Government will probably be made known.

THOUGH Indian tobacco is not much esteemed in this country, owing to its being badly prepared, some 796,000 acres of land are under tobacco cultivation, distributed as follows:—In the Bombay Presidency over 40,000 acres; in the Punjâb, over 90,000; in Oude, 69,574; in the Central Provinces, 55,000; in Behar, 18,500; in Mysore, 20,000; in Burmah, 13,000; while in Bengal there are some 500,000 acres.

WE learn that the export of cinchona bark from the Nilgiri hills, on the part of the Government, during 1872–73, the first regular year of export, amounted to over 20,000 lbs., which realised 4,000*l*. in the London market. It is anticipated that the returns of the exports for the past year, 1873–74, would show a similar quantity, and that the trade in future years will rapidly increase. Bark from private cinchona plantations in the East Indies and Ceylon appears regularly in the London market, fetching from 10*d*. to 4*s*. per lb. "Very good average prices," it is said, "as compared with those obtained by the South American barks."

THE additions to the Zoological Society's Gardens during the past week include a Hog Deer (*Cervus porcinus*) from Kurrachee, presented by Mr. H. Hughes; a White-crowned Mangabey (*Cercocebes æthiops*) from West Africa, presented by Mr. W. Gordon Patchett; an Egyptian Jerboa (*Dipus ægyptius*) from Egypt, presented by Mr. A. Carey, R.N.; an Anubis Baboon (*Cynocephalus anubis*) from W. Africa, presented by Mr. R. B. N. Walker; an Indian Wild Dog (*Canis primævus*) from India, presented by H.E. the Governor-General of India; three Crested Falcons (*Baza lophotes*), two Indian Cobras (*Naia tripudians*), two Indian Eryx (*Eryx johnii*) from India, purchased.

ON THE DYNAMICAL EVIDENCE OF THE MOLECULAR CONSTITUTION OF BODIES *

II.

LET us now return to the case of a highly rarefied gas in which the pressure is due entirely to the motion of its particles. It is easy to calculate the mean square of the velocity of the particles from the equation of Clausius, since the volume, the pressure, and the mass are all measurable quantities. Supposing the velocity of every particle the same, the velocity of a molecule of oxygen would be 461 metres per second, of nitrogen 492, and of hydrogen 1844, at the temperature 0° C.

The explanation of the pressure of a gas on the vessel which contains it by the impact of its particles on the surface of the vessel has been suggested at various times by various writers. The fact, however, that gases are not observed to disseminate themselves through the atmosphere with velocities at all approaching those just mentioned, remained unexplained, till Clausius, by a thorough study of the motions of an immense number of particles, developed the methods and ideas of modern molecular science.

To him we are indebted for the conception of the mean length of the path of a molecule of a gas between its successive encounters with other molecules. As soon as it was seen how each molecule, after describing an exceedingly short path, encounters another, and then describes a new path in a quite different direction, it became evident that the rate of diffusion of gases depends not merely on the velocity of the molecules, but on the distance they travel between each encounter.

I shall have more to say about the special contributions of Clausius to molecular science. The main fact, however, is, that he opened up a new field of mathematical physics by showing how to deal mathematically with moving systems of innumerable molecules.

Clausius, in his earlier investigations at least, did not attempt to determine whether the velocities of all the molecules of the same gas are equal, or whether, if unequal, there is any law according to which they are distributed. He therefore, as a first hypothesis, seems to have assumed that the velocities are equal. But it is easy to see that if encounters take place among a great number of molecules, their velocities, even if originally equal, will become unequal, for, except under conditions which can be only rarely satisfied, two molecules having equal velocities before their encounter will acquire unequal velocities after the encounter. By distributing the molecules into groups according to their velocities, we may substitute for the impossible task of following every individual molecule through all its encounters, that of registering the increase or decrease of the number of molecules in the different groups.

By following this method, which is the only one available either experimentally or mathematically, we pass from the methods of strict dynamics to those of statistics and probability.

When an encounter takes place between two molecules, they are transferred from one pair of groups to another, but by the time that a great many encounters have taken place, the number which enter each group is, on an average, neither more nor less than the number which leave it during the same time. When the system has reached this state, the numbers in each group must be distributed according to some definite law.

As soon as I became acquainted with the investigations of Clausius, I endeavoured to ascertain this law.

The result which I published in 1860 has since been subjected to a more strict investigation by Dr. Ludwig Boltzmann, who has also applied his method to the study of the motion of compound molecules. The mathematical investigation, though, like all parts of the science of probabilities and statistics, it is somewhat difficult, does not appear faulty. On the physical side, however, it leads to consequences, some of which, being manifestly true, seem to indicate that the hypotheses are well chosen, while others seem to be so irreconcilable with known experimental results, that we are compelled to admit that something essential to the complete statement of the physical theory of molecular encounters must have hitherto escaped us.

I must now attempt to give you some account of the present state of these investigations, without, however, entering into their mathematical demonstration.

I must begin by stating the general law of the distribution of velocity among molecules of the same kind.

If we take a fixed point in this diagram and draw from this point a line representing in direction and magnitude the velocity of a molecule, and make a dot at the end of the line, the position of the dot will indicate the state of motion of the molecule.

If we do the same for all the other molecules, the diagram will be dotted all over, the dots being more numerous in certain places than in others.

The law of distribution of the dots may be shown to be the same as that which prevails among errors of observation or of adjustment.

The dots in the diagram before you may be taken to represent the velocities of molecules, the different observations of the position of the same star, or the bullet-holes round the bull's-eye of a target, all of which are distributed in the same manner.

The velocities of the molecules have values ranging from zero to infinity, so that in speaking of the average velocity of the molecules we must define what we mean.

The most useful quantity for purposes of comparison and calculation is called the "velocity of mean square." It is that

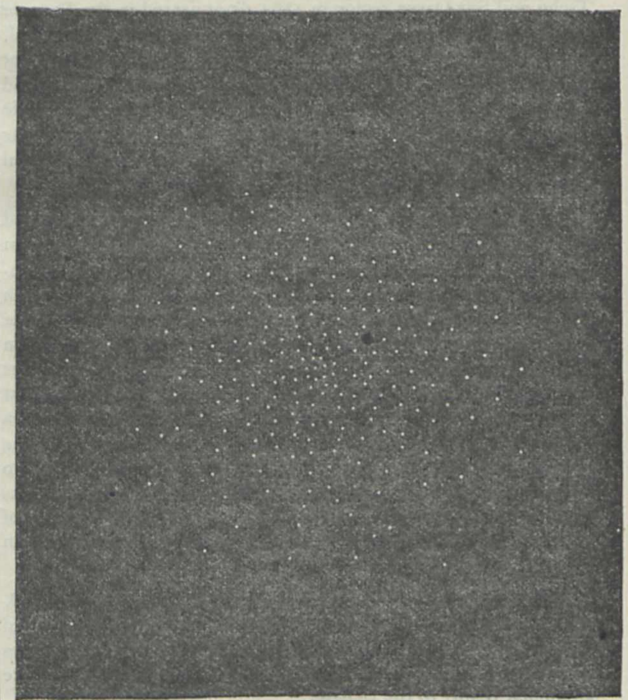

Diagram of Velocities.

velocity whose square is the average of the squares of the velocities of all the molecules.

This is the velocity given above as calculated from the properties of different gases. A molecule moving with the velocity of mean square has a kinetic energy equal to the average kinetic energy of all the molecules in the medium, and if a single mass equal to that of the whole quantity of gas were moving with this velocity, it would have the same kinetic energy as the gas actually has, only it would be in a visible form and directly available for doing work.

If in the same vessel there are different kinds of molecules, some of greater mass than others, it appears from this investigation that their velocities will be so distributed that the average kinetic energy of a molecule will be the same, whether its mass be great or small.

Here we have perhaps the most important application which has yet been made of dynamical methods to chemical science. For, suppose that we have two gases in the same vessel. The ultimate distribution of agitation among the molecules is such that the average kinetic energy of an individual molecule is the same in either gas. This ultimate state is also, as we know, a state of equal temperature. Hence the condition that two gases

* A lecture delivered at the Chemical Society, Feb. 18, by Prof. Clerk-Maxwell, F.R.S. (Continued from p. 359.)

shall have the same temperature is that the average kinetic energy of a single molecule shall be the same in the two gases.

Now, we have already shown that the pressure of a gas is two-thirds of the kinetic energy in unit of volume. Hence, if the pressure as well as the temperature be the same in the two gases, the kinetic energy per unit of volume is the same, as well as the kinetic energy per molecule. There must, therefore, be the same number of molecules in unit of volume in the two gases.

This result coincides with the law of equivalent volumes established by Gay Lussac. This law, however, has hitherto rested on purely chemical evidence, the relative masses of the molecules of different substances having been deduced from the proportions in which the substances enter into chemical combination. It is now demonstrated on dynamical principles. The molecule is defined as that small portion of the substance which moves as one lump during the motion of agitation. This is a purely dynamical definition, independent of any experiments on combination.

The density of a gaseous medium, at standard temperature and pressure, is proportional to the mass of one of its molecules as thus defined.

We have thus a safe method of estimating the relative masses of molecules of different substances when in the gaseous state. This method is more to be depended on than those founded on electrolysis or on specific heat, because our knowledge of the conditions of the motion of agitation is more complete than our knowledge of electrolysis, or of the internal motions of the constituents of a molecule.

I must now say something about these internal motions, because the greatest difficulty which the kinetic theory of gases has yet encountered belongs to this part of the subject.

We have hitherto considered only the motion of the centre of mass of the molecule. We have now to consider the motion of the constituents of the molecule relative to the centre of mass.

If we suppose that the constituents of a molecule are atoms, and that each atom is what is called a material point, then each atom may move in three different and independent ways, corresponding to the three dimensions of space, so that the number of variables required to determine the position and configuration of all the atoms of the molecule is three times the number of atoms.

It is not essential, however, to the mathematical investigation to assume that the molecule is made up of atoms. All that is assumed is that the position and configuration of the molecule can be completely expressed by a certain number of variables.

Let us call this number n.

Of these variables, three are required to determine the position of the centre of mass of the molecule, and the remaining $n - 3$ to determine its configuration relative to its centre of mass.

To each of the n variables corresponds a different kind of motion.

The motion of translation of the centre of mass has three components.

The motions of the parts relative to the centre of mass have $n - 3$ components.

The kinetic energy of the molecule may be regarded as made up of two parts—that of the mass of the molecule supposed to be concentrated at its centre of mass, and that of the motions of the parts relative to the centre of mass. The first part is called the energy of translation, the second that of rotation and vibration. The sum of these is the whole energy of motion of the molecule.

The pressure of the gas depends, as we have seen, on the energy of translation alone. The specific heat depends on the rate at which the whole energy, kinetic and potential, increases as the temperature rises.

Clausius had long ago pointed out that the ratio of the increment of the whole energy to that of the energy of translation may be determined if we know by experiment the ratio of the specific heat at constant pressure to that at constant volume.

He did not, however, attempt to determine *à priori* the ratio of the two parts of the energy, though he suggested, as an extremely probable hypothesis, that the average values of the two parts of the energy in a given substance always adjust themselves to the same ratio. He left the numerical value of this ratio to be determined by experiment.

In 1860 I investigated the ratio of the two parts of the energy on the hypothesis that the molecules are elastic bodies of invariable form. I found, to my great surprise, that whatever be the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of the two parts of the energy must be always the same, the two parts being in fact equal.

This result is confirmed by the researches of Boltzmann, who has worked out the general case of a molecule having n variables.

He finds that while the average energy of translation is the same for molecules of all kinds at the same temperature, the whole energy of motion is to the energy of translation as n to 3.

For a rigid body $n = 6$, which makes the whole energy of motion twice the energy of translation.

But if the molecule is capable of changing its form under the action of impressed forces, it must be capable of storing up potential energy, and if the forces are such as to ensure the stability of the molecule, the average potential energy will increase when the average energy of internal motion increases.

Hence, as the temperature rises, the increments of the energy of translation, the energy of internal motion, and the potential energy are as 3, $(n - 3)$, and e respectively, where e is a positive quantity of unknown value depending on the law of the force which binds together the constituents of the molecule.

When the volume of the substance is maintained constant, the effect of the application of heat is to increase the whole energy. We thus find for the specific heat of a gas at constant volume—

$$\frac{1}{2J}\,\frac{p_0 V_0}{273^\circ}(n + e)$$

where p_0 and V_0 are the pressure and volume of unit of mass at zero centigrade, or 273° absolute temperature, and J is the dynamical equivalent of heat. The specific heat at constant pressure is

$$\frac{1}{2J}\,\frac{p_0 V_0}{273^\circ}(n + 2 + e)$$

In gases whose molecules have the same degree of complexity the value of n is the same, and that of e *may* be the same.

If this is the case, the specific heat is inversely as the specific gravity, according to the law of Dulong and Petit, which is, to a certain degree of approximation, verified by experiment.

But if we take the actual values of the specific heat as found by Regnault and compare them with this formula, we find that $n + e$ for air and several other gases cannot be more than 4·9. For carbonic acid and steam it is greater. We obtain the same result if we compare the ratio of the calculated specific heats

$$\frac{2 + n + e}{n + e}$$

with the ratio as determined by experiment for various gases, namely, 1·408.

And here we are brought face to face with the greatest difficulty which the molecular theory has yet encountered, namely, the interpretation of the equation $n + e = 4{\cdot}9$.

If we suppose that the molecules are atoms—mere material points, incapable of rotatory energy or internal motion—then n is 3 and e is zero, and the ratio of the specific heats is 1·66, which is too great for any real gas.

But we learn from the spectroscope that a molecule can execute vibrations of constant period. It cannot therefore be a mere material point, but a system capable of changing its form. Such a system cannot have less than six variables. This would make the greatest value of the ratio of the specific heats 1·33, which is too small for hydrogen, oxygen, nitrogen, carbonic oxide, nitrous oxide, and hydrochloric acid.

But the spectroscope tells us that some molecules can execute a great many different kinds of vibrations. They must therefore be systems of a very considerable degree of complexity, having far more than six variables. Now, every additional variable introduces an additional amount of capacity for internal motion without affecting the external pressure. Every additional variable, therefore, increases the specific heat, whether reckoned at constant pressure or at constant volume.

So does any capacity which the molecule may have for storing up energy in the potential form. But the calculated specific heat is already too great when we suppose the molecule to consist of two atoms only. Hence every additional degree of complexity which we attribute to the molecule can only increase the difficulty of reconciling the observed with the calculated value of the specific heat.

I have now put before you what I consider to be the greatest difficulty yet encountered by the molecular theory. Boltzmann has suggested that we are to look for the explanation in the mutual action between the molecules and the ætherial medium which surrounds them. I am afraid, however, that if we call in the help of this medium, we shall only increase the calculated specific heat, which is already too great.

The theorem of Boltzmann may be applied not only to determine the distribution of velocity among the molecules, but to determine the distribution of the molecules themselves in a region in which they are acted on by external forces. It tells us that the density of distribution of the molecules at a point where the potential energy of a molecule is ψ, is proportional to $e^{-\frac{\psi}{\kappa\theta}}$ where θ is the absolute temperature, and κ is a constant for all gases. It follows from this, that if several gases in the same vessel are subject to an external force like that of gravity, the distribution of each gas is the same as if no other gas were present. This result agrees with the law assumed by Dalton, according to which the atmosphere may be regarded as consisting of two independent atmospheres, one of oxygen, and the other of nitrogen; the density of the oxygen diminishing faster than that of the nitrogen, as we ascend.

This would be the case if the atmosphere were never disturbed, but the effect of winds is to mix up the atmosphere and to render its composition more uniform than it would be if left at rest.

Another consequence of Boltzmann's theorem is, that the temperature tends to become equal throughout a vertical column of gas at rest.

In the case of the atmosphere, the effect of wind is to cause the temperature to vary as that of a mass of air would do if it were carried vertically upwards, expanding and cooling as it ascends.

But besides these results, which I had already obtained by a less elegant method and published in 1866, Boltzmann's theorem seems to open up a path into a region more purely chemical. For if the gas consists of a number of similar systems, each of which may assume different states having different amounts of energy, the theorem tells us that the number in each state is proportional to $e^{-\frac{\psi}{\kappa\theta}}$ where ψ is the energy, θ the absolute temperature, and κ a constant.

It is easy to see that this result ought to be applied to the theory of the states of combination which occur in a mixture of different substances. But as it is only during the present week that I have made any attempt to do so, I shall not trouble you with my crude calculations.

I have confined my remarks to a very small part of the field of molecular investigation. I have said nothing about the molecular theory of the diffusion of matter, motion, and energy, for though the results, especially in the diffusion of matter and the transpiration of fluids are of great interest to many chemists, and though from them we deduce important molecular data, they belong to a part of our study the data of which, depending on the conditions of the encounter of two molecules, are necessarily very hypothetical. I have thought it better to exhibit the evidence that the parts of fluids are in motion, and to describe the manner in which that motion is distributed among molecules of different masses.

To show that all the molecules of the same substance are equal in mass, we may refer to the methods of dialysis introduced by Graham, by which two gases of different densities may be separated by percolation through a porous plug.

If in a single gas there were molecules of different masses, the same process of dialysis, repeated a sufficient number of times, would furnish us with two portions of the gas, in one of which the average mass of the molecules would be greater than in the other. The density and the combining weight of these two portions would be different. Now, it may be said that no one has carried out this experiment in a sufficiently elaborate manner for every chemical substance. But the processes of nature are continually carrying out experiments of the same kind; and if there were molecules of the same substance nearly alike, but differing slightly in mass, the greater molecules would be selected in preference to form one compound, and the smaller to form another. But hydrogen is of the same density, whether we obtain it from water or from a hydrocarbon, so that neither oxygen nor carbon can find in hydrogen molecules greater or smaller than the average.

The estimates which have been made of the actual size of molecules are founded on a comparison of the volumes of bodies in the liquid or solid state, with their volumes in the gaseous state. In the study of molecular volumes we meet with many difficulties, but at the same time there are a sufficient number of consistent results to make the study a hopeful one.

The theory of the possible vibrations of a molecule has not yet been studied as it ought, with the help of a continual comparison between the dynamical theory and the evidence of the spectroscope. An intelligent student, armed with the calculus and the spectroscope, can hardly fail to discover some important fact about the internal constitution of a molecule.

The observed transparency of gases may seem hardly consistent with the results of molecular investigations.

A model of the molecules of a gas consisting of marbles scattered at distances bearing the proper proportion to their diameters, would allow very little light to penetrate through a hundred feet.

But if we remember the small size of the molecules compared with the length of a wave of light, we may apply certain theoretical investigations of Lord Rayleigh's about the mutual action between waves and small spheres, which show that the transparency of the atmosphere, if affected only by the presence of molecules, would be far greater than we have any reason to believe it to be.

A much more difficult investigation, which has hardly yet been attempted, relates to the electric properties of gases No one has yet explained why dense gases are such good insulators, and why, when rarefied or heated, they permit the discharge of electricity, whereas a perfect vacuum is the best of all insulators.

It is true that the diffusion of molecules goes on faster in a rarefied gas, because the mean path of a molecule is inversely as the density. But the electrical difference between dense and rare gas appears to be too great to be accounted for in this way.

But while I think it right to point out the hitherto unconquered difficulties of this molecular theory, I must not forget to remind you of the numerous facts which it satisfactorily explains. We have already mentioned the gaseous laws, as they are called, which express the relations between volume, pressure, and temperature, and Gay Lussac's very important law of equivalent volumes. The explanation of these may be regarded as complete. The law of molecular specific heats is less accurately verified by experiment, and its full explanation depends on a more perfect knowledge of the internal structure of a molecule than we as yet possess.

But the most important result of these inquiries is a more distinct conception of thermal phenomena. In the first place, the temperature of the medium is measured by the average kinetic energy of translation of a single molecule of the medium. In two media placed in thermal communication, the temperature as thus measured tends to become equal.

In the next place, we learn how to distinguish that kind of motion which we call heat from other kinds of motion. The peculiarity of the motion called heat is that it is perfectly irregular; that is to say, that the direction and magnitude of the velocity of a molecule at a given time cannot be expressed as depending on the present position of the molecule and the time.

In the visible motion of a body, on the other hand, the velocity of the centre of mass of all the molecules in any visible portion of the body is the observed velocity of that portion, though the molecules may have also an irregular agitation on account of the body being hot.

In the transmission of sound, too, the different portions of the body have a motion which is generally too minute and too rapidly alternating to be directly observed. But in the motion which constitutes the physical phenomenon of sound, the velocity of each portion of the medium at any time can be expressed as depending on the position and the time elapsed; so that the motion of a medium during the passage of a sound-wave is regular, and must be distinguished from that which we call heat.

If, however, the sound-wave, instead of travelling onwards in an orderly manner and leaving the medium behind it at rest, meets with resistances which fritter away its motion into irregular agitations, this irregular molecular motion becomes no longer capable of being propagated swiftly in one direction as sound, but lingers in the medium in the form of heat till it is communicated to colder parts of the medium by the slow process of conduction.

The motion which we call light, though still more minute and rapidly alternating than that of sound, is, like that of sound, perfectly regular, and therefore is not heat. What was formerly called Radiant Heat is a phenomenon physically identical with light.

When the radiation arrives at a certain portion of the medium, it enters it and passes through it, emerging at the other side. As long as the medium is engaged in transmitting the radiation it is in a certain state of motion, but as soon as the radiation has passed through it, the medium returns to its former state, the motion being entirely transferred to a new portion of the medium.

Now, the motion which we call heat can never of itself pass from one body to another unless the first body is, during the whole process, hotter than the second. The motion of radiation, therefore, which passes entirely out of one portion of the medium and enters another, cannot be properly called heat.

We may apply the molecular theory of gases to test those hypotheses about the luminiferous æther which assume it to consist of atoms or molecules.

Those who have ventured to describe the constitution of the luminiferous æther have sometimes assumed it to consist of atoms or molecules.

The application of the molecular theory to such hypotheses leads to rather startling results.

In the first place, a molecular æther would be neither more nor less than a gas. We may, if we please, assume that its molecules are each of them equal to the thousandth or the millionth part of a molecule of hydrogen, and that they can traverse freely the interspaces of all ordinary molecules. But, as we have seen, an equilibrium will establish itself between the agitation of the ordinary molecules and those of the æther. In other words, the æther and the bodies in it will tend to equality of temperature, and the æther will be subject to the ordinary gaseous laws as to pressure and temperature.

Among other properties of a gas, it will have that established by Dulong and Petit, so that the capacity for heat of unit of volume of the æther must be equal to that of unit of volume of any ordinary gas at the same pressure. Its presence, therefore, could not fail to be detected in our experiments on specific heat, and we may therefore assert that the constitution of the æther is not molecular.

J. CLERK-MAXWELL

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Feb. 18.—"On the number of Figures in the Reciprocal of each Prime Number between 30,000 and 40,000," by William Shanks. Communicated by the Rev. Dr. Salmon, F.R.S.

"On the Nature and Physiological Action of the *Crotalus*-poison as compared with that of *Naja tripudians* and other Indian Venomous Snakes," by T. Lauder Brunton, F.R.S., and J. Fayrer, M.D.

It appears that there is little difference between the physiological effects of the crotaline or viperine, and the colubrine virus. The mode in which death is brought about is essentially the same in all; though there are evidences, even when allowing for individual peculiarities, that the action is marked by some points of difference sufficiently characteristic to require notice in detail.

We have already expressed our belief that death is caused by the cobra-, *Daboia*-, and *Hydrophis*-poison, 1st, through its action on the cerebro-spinal nerve-centres, especially on the medulla, inducing paralysis of respiration; or 2nd, in some cases where the poison has entered the circulation in large quantities and has been conveyed more directly to the heart, by arrest, tetanically in systole, of cardiac action, probably owing to some action on the cardiac ganglia; 3rd, by a combination of the two previous causes; 4th, by a septic condition of a secondary nature, and which, being more essentially pathological in its bearings, the details were not considered suitable for discussion here.

There is reason to believe that death is caused in the same way by the *Crotalus*-poison also; and it appears, from the experiments recently performed in Calcutta by Dr. Ewart and the members of the Committee appointed by Government upon *Pseudechis porphyriacus*, or the black snake, and *Hoplocephalus curtus*, or the tiger-snake of Australia, that their virus causes death in the same manner. These reptiles had been sent from Melbourne to Calcutta for the purpose of investigation and comparison. (*Vide* Committee's Report, p. 58 *et seq.*, Appendix.)

But though the actual cause of death is essentially the same, the phenomena which precede and accompany it differ in some degree according to the nature of the poison, the quantity and site of the inoculations, and the individual peculiarities of the creature inoculated, as may be seen in the experiments herewith recorded.

The condition of an animal poisoned by the rattlesnake-venom, then, essentially resembles that of one subjected to the influence of the colubrine or viperine poison of Indian snakes:—

Depression, hurried respiration, exhaustion, lethargy, unconsciousness, nausea, retching, and vomiting.

Muscular twitchings, ataxy, paralysis, and convulsions, the latter probably chiefly, though not entirely, due to circulation of imperfectly oxygenated blood, the result of impeded respiration, and, finally, death.

Hæmorrhages or hæmorrhagic extravasations and effusions, both local and general, occur in all varieties of snake-poisoning.

But we observe (and in this our observations are in accord with those of Weir Mitchell) that there is a greater tendency to both local and general hæmorrhage and extravasation of blood and of the colouring matter of the blood, especially as observed in the peritoneum, intestines, and mesentery, and also probably to a more direct action on the cord than in poisoning by either cobra or viper.

The viscera and other tissues after death are found congested and ecchymosed, and in some cases to a great extent, seeming to show that either a preternatural fluidity of blood or some important change in the vessels, favouring its exudation, has occurred.

Several experiments were made on the physiological action of the virus of the rattle-snake, with the view of comparison with that of the cobra and *Daboia*.

We are indebted to Dr. Weir Mitchell, of Philadelphia, for a supply of the virus. He was good enough to send about six grains of the dried poison of *Crotalus*—the species not named, but it is believed to be of *Crotalus durissus*.

It has the appearance of fractured fragments of dried gum-arabic and of rather a darker yellow colour, but otherwise resembling the dried cobra-virus sent from Bengal.

There were no very marked differences to be observed in the action of the poison except in the energy with which the cobra exceeded the *Crotalus*.

It appears that the direct inoculation of large doses of the virus, whether viperine or colubrine, into the circulation have the power in some cases of annihilating almost instantaneously the irritability of the cord and medulla, as in others they have of arresting the heart's action.

The local as well as the general effect of the cobra- and *Crotalus*-poisons, *i.e.* colubrine and viperine, is to cause hæmorrhage, ecchymosis, and sanguinolent effusions into the areolar tissue, not only at the seat of inoculation and its neighbourhood, but also in the mucous membranes and other vascular parts. It is obvious also that the *Crotalus*-poison acts more energetically in this respect than the cobra-poison, and that this is perhaps one of the most marked distinctions between them.

Cobra venom is a muscular poison, and the gastrocnemius of a frog immersed in a watery solution of it contracts immediately upon immersion, and loses its irritability very much sooner than one placed in pure water.

In our experiments cobra-poison appeared first to stimulate and then to paralyse the motions of cilia from the mouth of a frog.

It arrests very rapidly the movements of infusoria and of the cilia upon them, but the cilia upon the mantle of a fresh-water muscle continued to move for many hours in an extremely strong solution of dried cobra-venom. In the case of white blood-corpuscles no very distinct action was observed. When applied to a piece of *Vallisneria spiralis* it appeared to have almost no effect, for the motion of the granules within the cells continued with undiminished rigour for two hours afterwards.

Feb. 25.—"On the Forms of Equipotential Curves and Surfaces and Lines of Electric Force," by W. Grylls Adams, M.A., Professor of Natural Philosophy and Astronomy in King's College, London.

The paper contains an account of certain experimental verifications of the laws of electrical distribution in space and in a conducting sheet, such as a sheet of tinfoil. When two battery poles are attached to any two points of an unlimited plane sheet, or to two points on the edge of a circular disc, or if the disc be bounded by arcs of circles passing through the two battery poles, the lines of force and also the equipotential curves are circles. The equipotential circles have their centres on the straight line joining the battery poles, and the lines of force pass through these poles. In any limited space, whether in the plane or in the solid, which is bounded entirely by lines of force, no alteration is made in the distribution of the current when that limited space is entirely removed from the conducting space around it.

Several cases are taken in a sheet of tinfoil 18 inches square, with several battery poles about 3 inches apart near the centre of the sheet, and the equipotential curves traced out by means of two poles attached to a delicate galvanometer, these poles being at points of the same potential when the galvanometer needle is at zero; a sheet limited in size by cutting along lines of force is then taken, and in each case it is shown that there is no alteration of the equipotential curves. The forms of these curves are traced out for one positive, and four negative poles at equal distances from it at the corners of a square in the centre of a large sheet of tinfoil; also the curves for one positive and two negative poles at equal distances on either side of it on the same straight line.

When there are four electrodes, two of each kind on an unlimited sheet, an equipotential curve is given by the equation,

$$rr' = cr_1r_1'.$$

If the four points lie on a circle, and the complete quadrilateral be drawn through them, the circles which have their centres at the intersections of opposite sides of the quadrilateral, and which cut the first circle at right angles, will also cut one another at right angles. One of these circles is shown to be an equipotential curve for the four electrodes, and the other is a line of force.

Hence, if we cut the unlimited sheet along the edge of this latter circle, we shall not alter the forms of the equipotential curves; and within it we shall have one electrode of each kind, the others being their electric images, the product of the distances of an electrode and its image from the centre being equal to the square of the radius of the disc. If an electrode is at the edge of the disc, then the electrode and its image coincides, and the equation to the equipotential curve is

$$r^2 = cr_1r_2.$$

When one pole is at the edge and the other is at the centre of a circular disc, since the electric image of the centre is at an infinite distance, the equation to the equipotential curve is

$$r^2 = cr_1.$$

This is an interesting case, as showing that the equipotential curves do not always cut the edge of the disc at right angles.

On placing one of the galvanometer electrodes at the extremity of the diameter through the battery electrodes, and tracing with the other, it is found that the equipotential curve through that point cuts the edge of the disc at an angle of 45°, and that there are two branches cutting one another at right angles.

These peculiarities are explained on tracing the curve

$$r^2 = 4ar_1$$

corresponding to this case. The extremity of the diameter is a point through which two branches of the curve pass at right angles to one another.

The forms of the equipotential surfaces and lines of force in space may be determined experimentally by taking a large vessel containing a conducting liquid and placing two points, the ends of two covered wires, for battery electrodes at a given depth in the liquid and away from the sides and ends of the vessel, taking similar covered wires immersed to the same depth for galvanometer electrodes.

For two electrodes the equipotential surfaces will be surfaces of revolution around the straight line joining them, and so will cut any plane drawn through this straight line or axis everywhere at right angles.

Hence we may suppose sections of the liquid made along such planes without altering the forms of the equipotential surfaces. This shows that we may place our battery electrodes at the side of a rectangular box containing the liquid, and with the points only just immersed below the surface of the liquid, and the equipotential surfaces will be the same as if the liquid were of unlimited extent in every direction about the electrodes.

We shall obtain the section of the equipotential surface by taking for galvanometer electrodes two points in the surface of the liquid, keeping one fixed and tracing out points of equal potential with the other.

The potential at any point in space, due to two equal and opposite electrodes, is

$$A\left(\frac{1}{r} - \frac{1}{r_1}\right)$$

where r and r_1 are the distances of the point from the electrodes, so that for an equipotential surface

$$\frac{1}{r} - \frac{1}{r_1} = \text{constant.}$$

These surfaces are cut at right angles by the curves

$$\cos\theta - \cos\phi = c,$$

which are also the magnetic lines of force, θ and ϕ being the angles which the distances from the electrodes make with the axis. That the lines of force in a vessel of finite size should agree with the lines of force in space, the form of the boundary of the vessel in a plane through the axis should everywhere be a line of force; but the ends of a rectangular vessel coincide very closely with certain lines of force, either when the electrodes are at the ends, or when there are two electrodes within the vessel, and two supposed electrodes at their electrical images at an equal distance outside the ends of the vessel.

The equipotential surfaces are given in this case by the equation,

$$\frac{1}{r} + \frac{1}{r'} - \frac{1}{r_1} - \frac{1}{r_1'} = \text{constant},$$

and the lines of force by the equation,

$$\cos\theta + \cos\theta_1 - \cos\phi - \cos\phi_1 = c.$$

The curve, for which $c = 2$ coincides very closely with the ends of the box.

The equipotential surfaces were traced out in sulphate of copper and in sulphate of zinc by the following method:—

A rectangular box was taken, and the battery electrodes attached to pieces of wood which could be clamped at the centre of the end of the box, and could be brought to any required point in the line joining the middle points of the end of the box. The galvanometer-electrodes were attached to T pieces which rest on the ends and side of the box, and the position of the electrodes read off by millimetre-scale placed on the ends and sides of the box.

When the electrodes are parallel lines extending throughout the depth of the liquid the equipotential surfaces are cylindrical, and their sections are given by the equation,

$$\log(rr'\ldots) - (\log r_1r_1'\ldots) = \log c,$$

where there are several positive and several negative electrodes, $r \cdot r' \ldots$ &c. being measured from the points where the electrodes cut the plane of the section.

Hence the forms of these equipotential curves are the same as in a plane sheet, so that the forms traced out in tinfoil will be the same as the corresponding forms in space for line electrodes. These forms may be traced out in sulphate of copper with copper electrodes, or in sulphate of zinc, with amalgamated zinc electrodes.

The results of these investigations show how closely the experimental determination of equipotential surfaces and lines of force agrees with the theory of electrical distribution in space.

Linnean Society, March 4.—Dr. G. J. Allman, president, in the chair.—Messrs. W. W. Scofield and T. Atthey were elected fellows.—Mr. Hanbury exhibited a fungus from South America, a species of *Phallus* allied to *P. impudicus*.—Mr. J. G. Baker exhibited specimens of the two species of plane-tree, *Platanus occidentalis* and *orientalis*, and of the variety of the latter known as *acerifolia*, and pointed out the distinctions between them; also a curious modification of bulb-form in a species of *Drimia*.—Mr. J. R. Jackson read a paper on plants in which ants make their homes; exhibiting specimens of two of the most remarkable of these, *Myrmecodia* and *Hydnophyllum*.—Prof. Thiselton Dyer read a brief note on the structure of the so-called *membrana nuclei* in the seeds of Cycads. Heinzel had described this as a cellular structure, the cells of which had thick walls penetrated by ramifying tubes. There was reason, however, for believing that the membrane only represented the wall of a single cell, and was in fact probably the greatly enlarged primary embryo-sac. What Heinzel had taken for tubes seemed really to be solid. They were arranged all over the membrane after the fashion of what carpet-manufacturers call a "moss-pattern." They were possibly the débris of the thickened walls of the cells of the nucleus which had been destroyed by the enlargement of the primary embryo-sac.—Prof. Dickson exhibited and described a series of microscopic slides illustrating the mode of growth of *Tropæolum speciosum*.—A paper was taken as read by Mr. Bentham, on the classification of the natural orders Campanulaceæ and Oleaceæ.

Geological Society, Feb. 24.—Mr. John Evans, V.P.R.S., president, in the chair.—Before proceeding to the business of he meeting the President spoke of the death of Sir C. Lyell.

"By every one of us," he said, "he was regarded as the leader of our science, by most of us as our trusted master, and by many of us as our faithful friend. He has lived to see the truth of those principles for which he so long and earnestly contended accepted by nearly all whose opinions he valued; and in future times, wherever the name of Lyell is known, it will be as that of the greatest, most philosophical, and most enlightened of British, if not indeed of European geologists."—The following communications were read:—On the Murchisonite beds of the estuary of the Ex, and an attempt to classify the beds of the Trias thereby, by Mr. G. Wareing Ormerod. This paper may be regarded as a continuation of one read by Mr. Ormerod before this Society in 1868. After noticing the mineralogical character of the Murchisonite, Mr. Ormerod described, first, the Red Sandstone beds by the sea-shore. To the east of Exmouth he considered that they were "Keuper," which extended inland to a fault running to the south of Lympstone. A conglomerate rock at the Beacon at Exmouth was probably the upper bed of the "Bunter," and this he considered to be the same rock that occurred at Cockwood on the right bank of the Ex. This overlay soft red rock, containing occasionally fragments of various rocks, and in the upper part a slight trace of Murchisonite. At Dawlish a soft conglomerate containing Murchisonite in great abundance occurred; this extended inland about two miles. On the westerly side of Dawlish conglomerate beds cropped out, containing fragments of granitic and porphyritic rocks, quartz, Lydian-stone; and here the limestone fragments containing animal remains first occurred. After passing the Parson-and-Clerk Tunnel, these conglomerate beds ceased until reaching Teignmouth, and the cliffs consist of soft beds. At Teignmouth the conglomerates, with limestone, again commenced, and continued to near St. Mary Church, in this part alternating with soft sandy or clayey beds. To the north of the fault at Lympstone the Keuper did not appear by the Ex, and the conglomerate with limestone had not been noticed, being possibly buried under the Greensand of Haldon. The beds north of this point on both sides of the Ex were the soft Red Sandstone, with a trace of Murchisonite, and the underlying Murchisonite conglomerates, and near Haldon House beds that it was considered were possibly those to the west of Dawlish occurred. These beds were broken up by various faults running in both north and south and east and west directions. In the district under consideration it was shown that the soft sandy beds, with a trace of Murchisonite, and the underlying bed of Murchisonite conglomerate, occurred in various places, and in such a manner that there could not be any doubt of their identity; these the author considered as marking a clear division in the Red Sandstone. The paper was illustrated by a map and three sections, and photographs of the cliffs, and by numerous specimens.—On some newly exposed sections of the "Woolwich and Reading beds" near Reading, Berks, by Prof. T. Rupert Jones, F.R.S., and Mr. C. Cooper King. The authors described the section of the lowest Tertiary beds lately exposed at Coley Hill, Reading, Berks, comparing it with other sections in the neighbourhood described by Buckland, Rolfe, Prestwich, and Whitaker. At one point in the section oyster-shells are wanting in the bottom bed, as observed also by Whitaker at Castle Kiln. At the same part of the section the leaf-bearing blue clays are also absent, but are continued by irregular thin seams of derived clay and clay-galls, with broken lignite, occasional grey flints, and by at least one green-coated flint and pebble of lydite. At another point, where the blue clay still exists, very numerous and large lumps of clays, rolled and often enclosing sub-angular flints, lie in the sand over the leaf-bed. Some of these clay-galls have passed into concentric nodules of ochre and limonite. The probable derivation of the two sets of clay-galls is from pre-existing clay beds—probably the blue shale, one from its worn end, and the other (upper one) from a terrace or ledge in its thickness—by the action of varying currents in an estuary at different levels. The clay-galls of the upper series vary much in character; some are of dense dark brown and light coloured clays, others of sandy blue and grey clays, many have involved sand and flints from an old shoal or beach. A probably analogous band of flints has been noticed at Red Hill, Berks, by Prestwich. The direction of the currents wearing away the clay bands and depositing the galls and sands was suggested; and these observations were offered as further materials in working out the hydrography and history of the Lower Tertiaries.—On the origin of Slickensides, with remarks on specimens from the Cambrian, Silurian, Carboniferous, and Triassic formations, by Mr. D. Mackintosh. This paper was founded on specimens a selection of which was exhibited. The author stated that his observations led him to believe that true slickensides are produced by the movement of one face of rock against another, accompanied by partial fusion. He indicated that in many cases the slickensided surfaces are not only polished and striated, but also hardened, and that there is an imperceptible gradation from this hardened film to the ordinary structure of the rock.

Chemical Society, March 4.—Prof. Odling, F.R.S., in the chair.—A paper on the dissociation of nitric acid, by Messrs. P. Braham and J. W. Gatehouse, was read by the former, and an experiment performed showing the action which takes place.—Dr. Thudichum then addressed the meeting on the chemical constitution of the brain, exhibiting a large number of the products obtained from that organ.—There were also papers on calcic hypochlorite from bleaching powder, by Mr. C. T. Kingzett; and on a simple method of determining iron, by Mr. W. Noel Hartley.

Zoological Society, March 2.—Mr. Osbert Salvin, F.R.S., in the chair.—An extract was read from a letter addressed to the Secretary by Dr. W. Peters, pointing out that the *Sternothœus* figured by Dr. Gray in the Society's "Proceedings" for 1873, to which neither specific name nor locality had been assigned, was *S. niger*, and that its habitat was the Cameroons, from which place Dr. Peters had received specimens.—Mr. H. E. Dresser read some notes on the *Falco labradorus* of Audubon, *Falco sacer* of Forster, and *Falco spadiceus* of the same author.—Mr. A. Boucard communicated a monographic list of the Coleoptera of the genus *Plusiotis* of North America, and gave the description of several new species.—A communication was read from Mr. E. P. Ramsay, giving the descriptions of some rare eggs of Australian birds.—Mr. G. B. Sowerby, jun., communicated the descriptions of ten new species of shells from various localities.—Dr. A. Günther, F.R.S., communicated on behalf of Dr. T. Thorell, of Upsala, the description of a collection of spiders made by Dr. Vinson in New Caledonia, Madagascar, and Reunion, amongst which were a few new species.—A communication was read from Mr. E. L. Layard, H.B.C., administering the government at Fiji, giving the description of some supposed new species of birds from the Fiji Islands.—Mr. A. H. Garrod read a paper containing the description of the lower larynx in some of the rarer species of Anatidæ. To this was added an account of the tracheal arrangement in *Platalea ajaja*, which differs much from that of the common Spoonbill. Reference was also made to the manner of development of the tracheal loop in those of the Cracidæ which have recently died in the Society's Gardens.

Royal Microscopical Society, March 3.—Mr. H. C. Sorby, F.R.S., the new president, having been formally introduced by Mr. Chas. Brooke, expressed his sense of the honour conferred upon him, regarding it as a mark of approval of new methods and kinds of investigation, to which, rather than to the more ordinary and general subjects of microscopical inquiry, he had for many years devoted his attention.—Mr. H. J. Slack read some notes translated from Von Baer, &c., which described an organism closely allied to that recently exhibited by Mr. Badcock and assumed to be a species of *Bucephalus*.—A paper by Dr. Royston Pigott, on the principle of testing object glasses by means of images produced by reflection from globules of mercury, &c., was read by the Secretary.—Mr. H. J. Wenham described, by means of black board illustrations, a new method of viewing objects at extreme angles, and the value of this new mode of examination was explained.—Mr. C. Stewart called attention to some new and beautiful specimens of Polycistinæ exhibited in the room by Mr. John Stephenson.

Anthropological Institute, Feb. 23.—Col. A. Lane Fox, president, in the chair.—Mr. R. B. Holt exhibited a collection of models of Esquimaux: Caïques, baidars, winter huts, summer huts, sleighs, and other objects of native manufacture.—Capt. Harold Dillon exhibited and described a series of flint arrow- and spear-heads found by him near Ditchley, Oxon. The following papers were read:—On the Milanows of Borneo, by Lieut. C. C. de Crespigny, R.N..; Further notes on the rude stone monuments at the Khasi hills, by Major Godwin-Austen; Report on the Congress of Anthropology and Prehistoric Archæology held at Stockholm in 1874, by H. H. Howorth; History of the Heung-Noo in their relations with China, translated by A. Wylie, of Shanghae, with notes by H. H. Howorth.

Physical Society, Feb. 13.—The report of the President (Prof. Gladstone, F.R.S.) and Council shows that a gratifying number of physicists responded to the circular issued by Dr.

Guthrie in the autumn of 1873, and that the formation of the Society has been attended with much success in every way. The meetings were commenced under singularly favourable circumstances, as the Lords of the Committee of Council on Education generously placed the physical laboratories and lecture-rooms at the disposal of the Society, which was thus afforded unusual facilities for experimental illustrations. The first paper was on the new contact theory of the galvanic battery, by J. A. Fleming, B.Sc., and it was followed by many valuable communications. Two papers may be mentioned as being of special interest, these are: "On the combination of colours by polarised light," by Mr. W. Spottiswoode, F.R.S., and "On the application of wind to stringed instruments," by Mr. J. Baillie Hamilton.—The Society has already lost a very able member by the death of Dr. W. S. Davis, of Derby, at the early age of thirty-two.—The following is the list of officers and Council for the present year:—President, Prof. J. H. Gladstone, F.R.S. Vice-presidents: Prof. W. G. Adams, F.R.S.; Prof. G. C. Foster, F.R.S. Secretaries: Prof. A. W. Reinold, M.A.; W. Chandler Roberts. Treasurer, Dr. E. Atkinson. Demonstrator, Dr. F. Guthrie, F.R.S. The other members of the Council are:—Latimer Clark, C.E., W. Crookes, F.R.S., Prof. A. Dupré, Prof. O. Henrici, F.R.S., W. Huggins, F.R.S., Prof. M'Leod, W. Spottiswoode, F.R.S., Dr. H. Sprengel, D. W. Stone, E. O. W. Whitehouse.

Royal Horticultural Society, Feb. 9.—Annual Meeting. Viscount Bury, K.C.M.G., president, in the chair.—The Report of the Council, which dealt principally with the financial position of the Society, was taken as read. The statement of accounts for 1874 showed an expenditure of 11,673*l*. 3*s*. 2*d*., against an income of 10,877*l*. 9*s*. 11*d*., leaving a deficit of 795*l*. 13*s*. 3*d*. This did not include rent, on account of which 2,400*l*. must be paid to H.M. Commissioners for the Exhibition of 1851 during 1876, otherwise the lease of the gardens at South Kensington will be avoided. On the other hand, the income was increased by a sum of 768*l*. 17*s*. 6*d*. paid to the society for the use of the arcades during the International Exhibition of last year. The result of the ballot for officers and council for the ensuing year was as follows:—President, Viscount Bury, K.C.M.G.; Treasurer, Bonamy Dobree; Secretary, W. A. Lindsay. New members of Council, Hon. and Rev. J. T. Boscawen, W. Longman, J. D. Chambers, F. Campion. On the motion of Mr. Godson the discussion of the report was adjourned to March 9.

EDINBURGH

Royal Society, March 1.—Sir William Thomson, president, in the chair.—The Chairman announced that the Council had awarded the Makdougall Brisbane Prize for the Biennial Period, 1872–74, to Prof. Lister for his paper on the germ theory of putrefaction and other fermentative changes, communicated to the Society, 7th April 1873. The following communications read:—Obituary notice of Mr. William Euing, by Prof. W. P. Dickson, Glasgow, communicated by the President; on a faulty construction common in skewed arches, by Mr. Edward Sang; on the mode of growth and increase amongst the corals of the Palæozoic Period, by Prof. H. Alleyne Nicholson, M.D.

PARIS

Academy of Sciences, March 1.—M. Frémy in the chair.—The following papers were read:—On the generalisation of the theory of the normals of geometrical curves, where for every normal a number of straight lines is substituted, by M. Chasles.—On some problems of molecular mechanics, by M. Berthelot. This paper was based principally on the experiments of MM. H. Sainte-Claire Deville and Debray, with perruthenic acid and oxide of silver, of which an account was read at the last meeting; it treats of certain facts, newly discovered and relating to the direct formation of compounds, which upon decomposition evolve a considerable degree of heat; these facts are quoted with special reference to butyrate of soda, and from them some general deductions are made with regard to molecular mechanics.—On the capillary theory applied to Tiliaceæ, by M. A. Trécul.—Experiments on the artificial imitation of native magneto-polar platinum, by M. Daubrée.—A note on magnetism, by M. Th. du Moncel.—M. Leverrier then explained to the Academy the new organisation of the meteorological service of ports, which came in force on March 1. Reports are made twice daily, morning and evening, and it is expected that the evening reports will be specially beneficial to fishing vessels.—Don Pedro II., Emperor of Brazil, is nominated correspondent to the section for Geography and Navigation, in lieu of the late Admiral de Wrangel. A telegram was read from his Majesty expressing thanks for the distinction.—A memoir by M. Cabieu, on a new manure, consisting of the ashes of Medusæ, picked up on the coasts, and fæcal matter.—A note, by M. Chapelas, in defence of the phenomenon observed by him on Feb. 10, at Paris, which was supposed by others to be a large bolide.—On the geometrical solution of some problems relating to the theory of surfaces, and depending from infinitesimals of the third order, by M. A. Mannheim.—On the simplest modes of limit equilibrium, which can be present in a body without cohesion and strongly compressed, by M. J. Boussinesq.—A note by M. G. Fouret, on the geometrical construction of the moments of bending power acting upon the supports of a beam with several joists.—A note by M. V. Feltz, on experimental researches on the toxical principle in putrefied blood; account of experiments made upon dogs into whose veins putrefied blood was injected.—A memoir by M. Macario, on the employment of electricity in hydrocele, iliac passion (*ileus*), and paralysis of the bladder; accounts of cases that were successfully cured by electricity.—A memoir on the chemical manure for beet, by MM. H. Woussen and B. Corenwinder.—M. de Maximowitch then presented a note on a theory of integration of equations with partial derivatives of the second order.—M. P. P. Mestre made a communcation respecting Phylloxera.—A note by M. H. Renan, elements and ephemerides of planet (141).—On a purple colouring matter derived from cyanogen, by M. G. Bong.—On the separation of boracic acid from silica and fluorine, by M. A. Ditte.—On the reciprocal substitution of the volatile fatty acids, by M. H. Lescœur. The author maintains that he has permanently established the following facts, namely, that acetic acid can displace formic acid from its compounds in considerable quantity, that this displacement can take place in the cold, that the presence of water does not notably affect the phenomenon, and the quantity of formic acid displaced varies according to the excess of acetic acid added.—A note by M. G. Hinrichs on the calculation of the moments of maximum inertia in the molecules of the chloro-derivatives of toluene.—Note by M. W. Louguinine on the quantities of heat evolved in the formation of the potash salts of some acids of the fatty series.—On a new psychrometer which avoids all calculation, called *hygrodeik*, by M. Lowe.—On a new pourer for volumetric analysis, by M. A. Pinchon.—Finally, five letters from different correspondents were read, all with regard to the bolide of Feb. 10, first mentioned by M. Chapelas, who afterwards thought it was only the strongly illuminated edge of a cloud.

BOOKS AND PAMPHLETS RECEIVED

COLONIAL.—The Pathological Significance of Nematode Hæmatozoa: T. R. Lewis, M.B. (Calcutta).—Report of Microscopical and Physiological Researches into the Nature of the Agent or the Agents producing Cholera. Second Series: T. R. Lewis and D. D. Cunningham (Calcutta).

FOREIGN.—Principes des Sciences Absolues: James Thomson (J. Rothschild Paris).—La Terre Végétale, Géologie Agricole: Stanislas Meunier (J. Rothschild, Paris).—Sulle Variazioni periodiche e non periodiche della temperatura nel Clima di Milano: Giovanni Celoria (Milan, Ulrico Hoepli).

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