THURSDAY, SEPTEMBER 26, 1895.

PERSONALITY.

The Diseases of Personality. By Th. Ribot. Authorised translation. Second revised edition. (Chicago: The Open Court Publishing Company, 1895.)

THE importance of a work bears little relation to its bulk, so no surprise need be felt at a masterly and very suggestive *résumé* of recent inquiries into a question of the highest interest being compressed into this thin volume of less than 160 pages of good readable type. The work itself is not new, though it is so in its present translated form. It is practically up to date, and affords an excellent study for those to whom what Tennyson calls "the abysmal deeps of Personality" are wholly mysterious, as well as to those others who have sounded them in part.

First as regards consciousness: there are two views, the old and the new. The old view regards it as the fundamental property of the soul or mind; the new view regards it as an event superadded to the more regular activity of the brain, depending on conditions as yet unknown, and appearing or disappearing according to their presence or absence. The old view fails to account for the vast substratum of unconscious mental activity whose existence is now beyond dispute, and it apparently fails to account for intermissions of consciousness, whose existence can hardly be denied even when the fullest allowance is made for the effects of forgetfulness. The new view is simpler than the old one, and much more consistent with observed facts, especially such as are obtained from the study of mental disease, which is a subtle analyser of mental functions. Many persons are loth to admit that the highest manifestations of the human mind are fugitive phenomena, subordinate to those of a lower grade; but whatever be the origin of consciousness, its value is none the less. From the point of view of the evolutionist, it is not the origin of a faculty that is of consequence, but the elevation to which that faculty attains. However consciousness may have come into existence, its first appearance on the earth must have been a fact of the first magnitude, for it is the basis of the recollections, which capitalise the past of each animal for the profit of its future, and give it new chances of survival. On the automaton view of life, consciousness changes the animal from a simple automaton into one of an incomparably higher order. The author quotes much from "Les colonies animales" of Perrier, to show the steps through which consciousness first became developed in the animal world, starting from associations of individuals that are almost independent of one another, but which, owing to their contiguity and mutual pressure, cannot be wholly unaffected by their neighbours. The next step is the appearance of a colonial consciousness, where a colony is formed of individuals in which some division of labour takes place, and the function of locomotion is centralised. But because a colony acquires colonial consciousness, it does not follow that each of the individuals that compose it loses its particular consciousness; thus the severed ray of a star-fish continues to creep, to follow, or, it may be, to deviate under conditions from a given route, and to quiver when excited, and thus to betray a consciousness of its own which, before it was severed, was subordinated to the consciousness of the whole star-fish. By degrees this colonial consciousness confiscates for its benefit all the particular ones.

The author maintains that consciousness is not like a central point from which alone feelings radiate and to which they all arrive, but that it is a complexus of separate phenomena, each of a particular class, bound up with certain unknown conditions of the brain, existing only when they exist, lacking when they disappear. Hence the sum of the states of consciousness in man is very inferior to the sum of all his nervous actions. Conscious personality is only an abstract of the vast amount of work that takes place in the nervous centres. Its basis is formed by the diffused bodily sensations which, being elementary causes, serve as a warp upon which is woven some gorgeous pattern of tapestry that corresponds to the higher feelings. The general consciousness of the organism serves as the support of all the rest, and forms, in the author's opinion, the real basis of conscious personality.

Personal identity is an unsatisfactory phrase. A man feels to be the same in his ego at different periods, because the great majority of his bodily feelings continue the same, owing to his structural sameness. The so-called identity is due to the large preponderance of unchanging elements, which characterise a healthy state; but in disease this habitual predominance may fail either wholly or temporarily, leading in the one case to a sense of a complete change of personality, in the other to that of multiple and alternating personalities. A few but adequate number of specimen cases are given. A somewhat comic instance is that by Hack Tuke, of a patient who had lost his ego (that is the one which was familiar to him), and was in the habit of searching for himself under his bed. (*Cf.* the speech of Saturn, "Search Thea, search . . ." in Keats' "Hyperion.")

The rather common cases in which a man believes himself to have become changed into a new person, are considered by the author to be mostly superficial; that is, to be due to local rather than to general disorder. I myself witnessed a case which showed that the imagined personality was not well sustained. It was at a lunatic asylum, where I went accompanied by a photographer to take specimens for composite photography. He mounted his camera in a ward, and a batch of patients were brought up. One of them was duly placed in front of the camera, the others were led to a bench behind the operator to wait their turn. It happened that one of these had the mania that he was a great commander, let us say, Alexander the Great, and he chafed internally at not having had precedence. When my photographer's head was under the dark cloth, and his body in the attitude appropriate to the occasion, Alexander the Great could restrain himself no longer, but nipped the projecting rotundity of the poor man's hinder end with his teeth. I abstain from dwelling on the tableau, or on the care with which the smarting photographer, in his further operations, squeezed himself into a corner that guarded his rear. The point is this, that a man who was thoroughly pervaded with the idea of being a mighty conqueror, would not have made that kind of attack.

Without attempting to condense further this already condensed and very readable little volume written by a distinguished inquirer, I will conclude by saying that it well deserves a place in any general library.

FRANCIS GALTON.

SATELLITE EVOLUTION.

Satellite Evolution. By James Nolan. Pp. 114. (Melbourne, &c.: George Robertson and Co., 1895.)

IN this book Mr. Nolan discusses the part played by tidal friction in the evolution of satellites. Although the subject is one of much scientific interest, his work is hardly likely to attract the attention it deserves, because the unmathematical reader will find the reasoning hard to follow, whilst the mathematician will be repelled by prolixity, due to the author's treatment of the problem by means of general reasoning.[1] The first fifty pages of the book appear to be virtually contained in the single equation which states the effect of tidal friction in increasing the mean distance of a satellite. It might perhaps be interesting to some to discuss the various elements of the problem in detail, but those who are able to comprehend an analytical formula are not very likely to have the patience to follow such a discussion.

I shall not accordingly follow Mr. Nolan in detail, but will pass at once to the conclusion to which he tends. On p. 9 he says:—

"Though Mr. Darwin made elaborate calculations to support his theory respecting the part played by tidal friction on the evolution of the earth and moon, he seems to have dismissed the Jovian and Saturnian systems with the conclusion that their satellites, unlike our moon, could not be traced much further in than the present distances of their respective planets; and that as the relation between the mass of the planet and satellite, or relation of rotational to orbital momentum is very different in the case of the earth and moon to that for other planets and satellites, their modes of evolution may have differed considerably. He seems to have gone something further into the possible effects of solar tidal friction on the planets revolving round the great central body, or at least has come to the correct conclusion that the efficiency of such tides would be too small to effect any appreciable change during the natural lifetime of a solar system."

He then proceeds to show that, if the earth and Jupiter rotate under the influence of tides subject to the same frictional resistance, the proportionate rate of increase of the moon's mean distance is much smaller than that of all of Jupiter's satellites, save one. In other words, four out of five of Jupiter's satellites would have their mean distances increased by, say, one per cent. in a much shorter time than would the moon. He then pursues the same train of reasoning with respect to Saturn and Mars.

It appears to me that Mr. Nolan is correct in these conclusions, and we are thus led to suppose that tidal friction may have played a much more important part in the evolution of satellites than I was disposed to allow it.[1] He points out (p. 70) that the satellites of Jupiter are probably much younger than the moon; "when the moon was younger, her relative rate of recession was faster, as now is the case for some satellites in other systems." He finally concludes (p. 78) that the majority of satellites in each system may be traced to a position corresponding with that of the rings of Saturn.

But before arriving at this result, the author has treated another problem, in which, in my opinion, his conclusion is incorrect. On p. 45, he considers the effects of tidal friction on such a ring as that of Saturn. He says:—

Tidal friction "could have no effect if the ring were perfectly even all round. When composed of individual bodies it could not be or remain so. Each individual would be unaffected by the tides of the others, and would recede at the same rate as if it were the only body in the ring. The moon recedes at exactly the same rate as she would were there no solar tides; and if there were a second moon there would be no interference with the recession of the first . . . Then if the bodies composing the rings are 'as the sand on the sea shore for multitude' tidal friction must still effect the usual progressive change, unless each individual body be small enough to be unaffected at the distance, whether composing a ring or not. This must have a dissolving effect on the ring, or tend to shape certain sections of it into so many bodies, which, having increased their mass at the expense of the ring, finally recede therefrom, either to circle round at a great distance or be precipitated into the planet increasing its rotation speed."

It would seem that the process here sketched is an essential part of Mr. Nolan's theory of the evolution of satellites, but I believe it to be founded on erroneous premises. He omits in fact to notice the necessary condition for neglecting the effects of the tides raised by one satellite on the mean distance of another; this is, that the periodic times of the two shall not be equal to one another. If the periodic times of two satellites are unequal, we need not invoke tidal friction to bring the two bodies near to one another. On the other hand, if four or eight satellites be equally spaced round a planet and revolve with the same periodic time, tidal friction would only influence their motions to a very small extent. I am therefore unable to follow Mr. Nolan in this part of his work.

Several other points in the early history of satellites are considered by Mr. Nolan, but I am unable to touch on them within the limits of a review.

Notwithstanding all that has been written by him and others, we are still far from a consistent theory of the formation of a satellite. In my own papers I have ventured to throw out suggestions (which have but too often been quoted as positive theories), and it still seems to me at least, that neither the present contribution of the author nor the theories of others are adequate.

This work touches on subjects of interest, and although it seems open to much criticism, I for my part welcome the extension given by Mr. Nolan to the part played by tidal friction in evolutionary astronomy.

G. H. DARWIN.

[1] The phraseology is somewhat lax, and it is not always easy to assure oneself of the correctness of the train of reasoning; but where the conclusion is correct, the reasoning probably is so also.

[1] The arguments by which I was led to an erroneous conclusion on this point, will be found in *Phil. Trans.*, part ii., 1881, p. 524.

OUR BOOK SHELF.

Die Lehre von der Elektrizität und deren Praktische Verwendung. By Th. Schwartze. (Leipzig: J. J. Weber, 1895.)

THE author in his preface says that his intention in writing this book was to give the bearing of the latest scientific results in electricity on electro-technology. He goes on to say that the contents will probably appear peculiar. The first of the above statements, taken in conjunction with the title of the book, will probably give as erroneous an idea of the contents as it is possible to obtain. For if there is one thing the author does not do, it is to give the bearing of the few modern discoveries, or lines of thought, which he mentions on the practical applications of electricity.

For all intents and purposes the book may be divided into two parts. The first of these deals with the question of the fundamental principles of general physics and with some mechanical problems, such as moment of inertia, oscillations of a pendulum, wave-motion, &c. The second part deals more particularly with electric and magnetic phenomena.

Throughout the greater part of the book, but particularly in the first part, the reader will probably heartily endorse the author's view, that the contents of the book are peculiar; for the subject of dimensions is treated at great length, so that, for at any rate the first three hundred pages, there is hardly a page without at least one dimensional equation. The appearance of some of these dimensional equations, however, are certainly peculiar, for the author attempts to introduce a set of dimensions in terms of what he calls "Linearkraft," "Flächenkraft," and "Volumenkraft." These quantities he indicates by the symbols L, L^2 and L^3, regardless of the fact that in those dimensional formulæ, in which length, mass, and time are taken as the fundamental units, the symbol L is used for a length. Even the author himself seems to have got muddled when such equations as $[M^2L^2]=[ML^2]$ are allowed to appear, and the state of mind of the student, whose command of dimensions is limited, after reading the book, is lamentable to think of. In the chapter dealing with the dimensions of the electrical and magnetic units, no mention is made of the effect of the properties of the medium, and although Rücker's name is mentioned in the preface in connection with the subject of dimensions, no mention is made of his proposal to consider the specific inductive capacity and the permeability of the medium as subsidiary fundamental units, and to indicate their presence in the dimensional formulæ. The more purely electrical portion of the book calls for little remark, and contains a somewhat elementary treatment of the subject of electrostatics, such as the calculation of the capacity of some simple forms of condensers, &c. There are also chapters dealing with uni-directed currents, thermo-electricity, electrolysis, electro-magnetic induction, and the dynamo. Finally, about seventy pages are devoted to what is called "electro-tectiniches," in which the commoner forms of electrical measuring instruments are shortly described.

While only a very short account is given of Hertz's work, contrary to what one would expect in a German work, considerable space is devoted to a description of Elihu Thomson's more showy experiments with rapidly alternating currents.

LETTERS TO THE EDITOR.

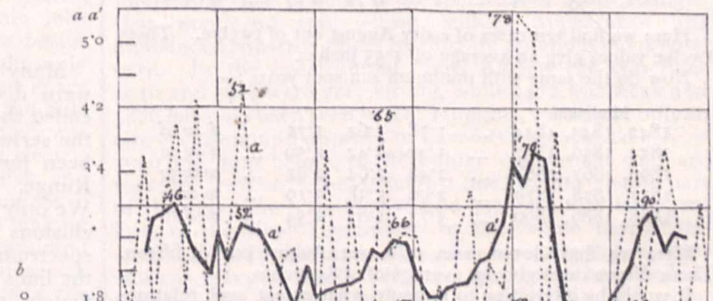
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Rain in August.

AUGUST being a harvest month, and the holiday month *par excellence* in this country, its weather is a matter of concern to multitudes. I propose to show how the rainfall of August at Greenwich has varied in the last half-century (1841–95).

This variation appears to me rather to suggest sun-spot influence; but whatever may be thought about this, it may be interesting to observe how far the kind of correspondence here pointed out is maintained in the future.

In the accompanying diagram we have (*a*) a dotted curve showing the variation of August rainfall, and the values have



a. Rainfall in August, Greenwich. *a'.* The same, smoothed (5-av.). *b.* Inverted sun-spot curve. *c, d, e.* Rainfall in August at Haverfordwest, Llandudno, and Boston (smoothed).

been smoothed with averages of 5, yielding the continuous curve *a'*. Underneath (*b*) is an inverted sun-spot curve.

A considerable correspondence may here be traced, especially in the last three waves; the crests or maxima of the smoothed rainfall curve coming near the sun-spot minima, and the hollows or minima of the former near the sun-spot maxima.

It seems specially noteworthy that in each year following a sun-spot maximum year we have had a very dry August. Thus (the August average being 2·38) we have:

Sun-spot max.	1848 ...	Rainfall of August	1849, 0·45 in.
,,	1860 ...	,, ,,	1861, 0·57 ,,
,,	1870 ...	,, ,,	1871, 0·86 ,,
,,	1883 ...	,, ,,	1884, 0·67 ,,

The data previous to 1841 are, I suppose, less reliable; but I may add these two cases of August rainfall under average:

Sun-spot max. 1830	...	Rainfall of August 1831,	2·14 in.
„ 1837	...	„ „ 1838,	0·93 „

By way of showing that in other parts of the country there has been, in recent years at least, a similar variation, I add three similarly smoothed curves of August rainfall for Haverfordwest, Llandudno, and Boston (Lincoln) respectively (*c*, *d*, *e*). The data, however, do not extend back further than 1866.

The case of Greenwich may be presented as follows:—Take each maximum sun-spot year, and a year on either side, and tabulate the August rainfall in each of these. Indicate by the letters *d* (for dry) and *w* (for wet) whether this rainfall has been below or above the average. Then we have:—

Maximum.					
1847, 1848, 1849	...	1·95	4·25	0·45	... *d w d*
1859, 1860, 1861	...	1·13	3·68	0·57	... *d w d*
1869, 1870, 1871	...	1·21	2·02	0·86	... *d d d*
1882, 1883, 1884	...	1·16	0·71	0·67	... *d d d*

Here we find ten cases of a dry August out of twelve. Those twelve values give an average of 1·55 inches.

Now do the same with minimum sun-spot years:—

Minimum.					
1842, 1843, 1844	...	1·78	3·62	1·71	... *d w d*
1855, 1856, 1857	...	1·40	2·42	2·50	... *d w w*
1866, 1867, 1868	...	2·42	2·64	2·61	... *w w w*
1877, 1878, 1879	...	2·90	5·38	5·19	... *w w w*
1888, 1889, 1890	...	3·73	1·81	2·54	... *w d w*

Here we find eleven cases of a wet August out of fifteen. Those fifteen cases give an average of 2·84 inches.

It would be interesting to know to what extent such relations subsist elsewhere, and perhaps some of your readers may be disposed to investigate the matter. A. B. M.

Alteration in the Colours of Flowers by Cyanide Fumes.

It is well known that the yellows of some insects are turned to red by the fumes from potassium cyanide; but I have not, after some inquiry, been able to obtain any literature describing the effects of such fumes upon the colours of flowers. The reactions I have observed are very curious, and while it seems improbable that they are hitherto wholly unknown, it may not be amiss to direct attention to them. A few lumps of the cyanide are placed in a corked tube, covered with a little cotton, and the flowers are placed on the cotton. It is probably necessary that the day should be hot, or the tube slightly warmed. The pink flowers of *Cleome integrifolia* and *Monarda fistulosa* turn to a brilliant green-blue, and finally become pale yellow. A purple-red *Verbena* becomes bright blue, then pale yellow. The purple flowers of *Solanum slœagnifolium* go green-blue and then yellow. The white petals of *Argemone platyceras* turn yellow—the natural colour of *A. mexicana.* The pale yellowish flowers of *Mentzelia nuda* turn a deeper yellow. Flowers of *Lupinus argenteus*, var., turn pale yellow. White elder (*Sambucus*) flowers turn yellow. The scarlet flowers of *Sphæralcea angustifolia* turn pale dull pink, resembling somewhat a natural variety of the same. Any of your readers will doubtless obtain similar results with the flowers growing in their vicinity. T. D. A. COCKERELL.

Las Cruces, New Mexico, U.S.A., September 3.

ON THE CONSTITUENTS OF THE GAS IN CLEVEITE.

WE have investigated the spectrum of the gas discovered in the mineral cleveite by Ramsay, and have found it to be most regular. It consists of six series of lines, the intensity of the lines in each series decreasing with decreasing wave-lengths. Similar series of lines have been observed in many spectra. The first series was discovered by Dr. Huggins in the ultra-violet spectra of a number of stars. It proved to belong to hydrogen, and to be the continuation of the four strong hydrogen lines in the visible part of the spectrum. Johnstone Stoney had already shown that three of the wave-lengths of the visible hydrogen lines were most accurately proportional to the values 9/5, 4/3, 9/8, when Balmer discovered that these values were given by the formula

$$\frac{m^2}{m^2 - 4}$$

for $m = 3, 4, 6$, and that the other wave-lengths of the series were proportional to the values obtained by substituting for m the other entire numbers greater than three. The series has now been followed from $m = 3$ to $m = 20$, the lines growing weaker and weaker to the more refrangible side, and approaching each other closer and closer. The formula shows that they approach a definite limit for large values of m. This is seen more clearly when we consider wave-numbers instead of wave-lengths, which according to the formula would be proportional to

$$1 - \frac{4}{m}2.$$

Many series of lines similar to the hydrogen series were discovered by Liveing and Dewar. They have called them harmonic series, and have compared them to the series of over-tones of a vibrating body. They have been further studied by Rydberg and by Kayser and Runge. We cannot here enter into any detailed account. We only want to explain so much as to make the conclusions understood which we have drawn from the spectrum of the gas in cleveite. The wave-lengths λ of the lines belonging to the same series are always approximately connected by a formula somewhat similar to Balmer's

$$1/\lambda = A - B/m^2 - C/m^4.$$

A determines the end of the series towards which the lines approach for high values of m, but does not influence the difference of wave-numbers of any two lines. B has nearly the same value for all the series observed, and C may be said to determine the spread of the series, corresponding intervals between the wave-numbers being larger for larger values of C. As B is approximately known, two wave-lengths of a series suffice to determine the constants A and C, and thus to calculate approximately the wave-lengths of the other lines. It was by this means that we succeeded in disentangling the spectrum of the gas in cleveite, and showing its regularity.

In the spectrum of many elements two series have been observed for which A has the same value, so that they both approach to the same limit. In all these cases the series for which C has the smaller value, that is to say which has the smaller spread, is the stronger of the two. In the spectrum of the gas in cleveite we have two instances of the same occurrence. One of the two pairs of series, the one to which the strong yellow double line belongs, consists throughout of double lines whose wave-numbers seem to have the same difference, while the lines of the other pair of series appear to be all single. Lithium is an instance of a pair of series of single lines approaching to the same limit. But there are also many instances of two series of double lines of equal difference of wave-numbers ending at the same place as sodium, potassium, aluminium, &c. There are also cases where the members of each series consist of triplets of the same difference of wave-numbers as in the spectrum of magnesium, calcium, strontium, zinc, cadmium, mercury. But there is no instance of an element whose spectrum contains two pairs of series ending at the same place. This suggested to us the idea that the two pairs of series belonged to different elements. One of the two pairs being by far the stronger, we assume that the stronger one of the two remaining series belongs to the same element as the stronger pair. We thus get two spectra consisting of three series each, two series ending at the same place, and the third leaping over the first two in large bounds and ending in the more refrangible part of the spectrum. This third series we suppose to be analogous to the so-called principal series in the spectra of the alkalis, which show the same features. It is not impossible, one may even say not unlikely, that there are principal series in the spectra of the other elements. But so far they have not been shown to exist.

Each of our two spectra now shows a close analogy to the spectra of the alkalis.

We therefore believe the gas in cleveite to consist of two, and not more than two, constituents. We propose to call only one of the constituents helium, the one to which the bright yellow double line belongs, whose spectrum altogether is the stronger one, while the other constituent ought to receive a new name.

We have confirmed this rather hypothetical conclusion by the following experiment. The connection leading from our supply of cleveite gas to the vacuum tube contained a side branch parting from it and joining it again. There were stopcocks on either side of the side branch, and a third one in the side branch. In the main tube between the ends of the side branch a plug of asbestos was tightly inserted. To prepare the vacuum tube only the tap leading to the supply was closed, the whole space up to this tap being carefully evacuated. Now the side branch was closed, and the tap leading to the supply was opened. Then we observed that the light of the electric discharge in the vacuum tube was at first greenish, and after a while grew yellow. By cutting off the current of gas after a sufficiently short time, we succeeded in making a vacuum tube which remained greenish. On examining it in a small spectroscope with which we could overlook the whole spectrum, we found that the intensities of the lines had changed. The yellow line was scarcely as bright as the green line 5016, and the red line 7065 had apparently decreased relatively to 7282 and 6678, although it was still stronger than 7282. The two lines that had decreased in intensity belong to the second set of series, while the others are members of the first set. The other visual lines of the second set could not very well be examined because they are more in the violet part.

This observation confirms our spectroscopic result. The gas in cleveite may be taken to be a mixture of two gases of different density, of which the lighter one is more rapidly transmitted through the plug of asbestos. There is, however, the objection to be raised, that in the green tube the pressure is less, and that the difference of intensities is due to the pressure being different. This must be further inquired into.

We were not satisfied with the visual observation of the change of intensities in our green tube, but thought it desirable to test the conclusion by the bolometric measurement of the two lines that we have discovered in the ultra-red part of the spectrum. If we were right, the ultra-red line of smaller wave-length, which belongs to the second set of series, ought to have decreased in intensity relatively to the other ultra-red line. This we found to be so indeed. In the yellow tubes the intensity of the smaller wave-length was to that of the other on an average as 3 to 1, while in the green tubes it was as 1·8 to 1. This confirmation we consider the more valuable as it does not depend on any estimation which may be biassed by the personal opinion of the observer, but is based on an objective numerical determination.

Another confirmation may be gathered from the spectrum of the sun's limb and that of several stars. Let us confine our attention to the six strongest lines in the visible part of the spectrum :

7066, 6678, 5876, 5016, 4922, 4472.

The first, third, and sixth belong to the second set of series ; the second, fourth and fifth to the first set. These six lines have all been observed in the spectrum of the sun's limb, as Norman Lockyer and Deslandres have pointed out. Now, according to their appearance in the spectrum of the sun's limb, they may be classed in two groups, one group being always present, the other group being sometimes present. C. A. Young long ago called attention to the difference in the frequency of appearance of the chromospheric lines. He has given them frequency numbers, roughly estimating the percentage of frequency with which the lines were seen during the six weeks of observation at Sherman in the summer of 1872. According to Young, 7066, 5876, 4472 have the frequency number 100, while 6678, 5016, 4922 have the numbers 25, 30, 30, showing that one of the two constituents was always present, while the other was only seen about once in every four cases.

The lines of both constituents have been observed in the spectra of a considerable number of stars β, δ, ϵ, ζ, γ Orionis, α Virginis, β Persei, β Tauri, η Ursæ majoris, β Lyræ. In the spectrum of β Lyræ, thirteen lines have been identified with certainty. But the most interesting case in point is the spectrum of Nova Aurigæ, that wonderful star whose sudden appearance was announced to astronomers in 1892 by an anonymous post-card. In the spectrum of Nova Aurigæ the two lines 5016 and 4922 were very strong, while 4472 was weak and 5876 has only been seen by Dr. Huggins, we believe only on one occasion, and appears to have been very weak. Now 5016 and 4922 belong to the lighter constituent, and are together with 6678 the strongest lines in the visible part of the spectrum ; while 5876 and 4472 are the strongest lines of the other constituent in the visible part of the spectrum. In Nova Aurigæ, therefore, the lighter constituent gave a much brighter spectrum than helium proper. But there may here be raised an objection, which indeed we do not know how to refute. Why has the line 6678 not been observed? It is a pity that the red part of the spectrum cannot be more easily photographed. Nova Aurigæ has now become very weak, and besides the spectrum is quite altered, so that we shall never know whether the red line 6678 was really absent or has only escaped notice.

From the fact that the second set of series is on the whole situated more to the refrangible part of the spectrum, one may, independently of the diffusion experiment, conclude that the element corresponding to the second set is the heavier of the two. In the spectra of chemically related elements like Li, Na, K, Rb, Cs, or Mg, Ca, Sr, or Zn, Cd, Hg, the series shift to the less refrangible side with increasing atomic weight. But it appears that in the spectra of elements following each other in the order of their atomic weights in a row of the periodic system like

Na, Mg, Al ;
K, Ca ;
Cu, Zn ;
Rb, Sr ;
Ag, Cd, In ;

the series shift the opposite way, so that the spectrum of the element of greater atomic weight is as a whole situated further to the more refrangible side. Now in our case the density of the gas has been determined by Langlet (published by Cleve) and by Ramsay to be about double the density of hydrogen. Assuming the atomic weights of the two constituents to be between that of lithium and that of hydrogen, they would both belong to the same row of the periodic system, and therefore the more refrangible set of series would correspond to the greater atomic weight.

For convenience of reference all the observed lines are given in the following table, the wave-lengths being abridged to tenth-metres.

Lighter Constituent.

Principal series.		First subordinate series. [1]		Second subordinate series.
20400	...	6678	...	7282
5016	...	4922	...	5048
3965	...	4388	...	4438
3614	...	4144	...	4169
3448	...	4009	...	4024
3355	...	3927	...	3936
3297	...	3872	...	3878
3258	...	3834	...	3838
3231	...	3806	...	3808
3213	...	3785		

Heavier Constituent (*Helium proper*).

		Double lines.		Double lines.
11220	...	5876	...	7066
3889	...	4472	...	4713
3188	...	4026	...	4121
2945	...	3820	...	3868
2829	...	3705	...	3733
2764	...	3634	...	3652
2723	...	3587	...	3599
2696	...	3555	...	3563
2677	...	3531	...	3537
		3513	...	3517
		3499	...	3503
		3488	...	3491
		3479	...	3482
		3472		
		3466		
		3461		

C. RUNGE AND F. PASCHEN.

NOTES.

THE third International Congress of Zoologists (an account of the proceedings at which will appear in a subsequent issue of NATURE) has just been held at Leyden, and appears to have been a great success. No fewer than twenty nationalities were represented, and the arrangements for the comfort of the members were all that could be wished. It was decided to hold the next meeting (in 1898) in England, and Sir William H. Flower was elected President. During the meeting it was announced that the Senate of the University of Utrecht had conferred degrees upon Sir William H. Flower, M. Milne-Edwards, of Paris, and Prof. Weismann, of Freiburg.

TELEGRAMS from St. John's, dated September 22, announced the return, in the steamer *Kite*, of the Peary Expedition. The result of the expedition was a most disappointing one, as Lieut. Peary and his companions were unable to extend their journeyings beyond Independence Bay, which point was the furthest north reached by Lieut. Peary in his expedition of 1892. The main cause of failure was the loss of all the stores of provisions, save one, which had been got together and deposited along the intended line of march last year, all having been buried by perhaps the heaviest snowfall known, which obliterated all traces of them. The sufferings endured by the explorers, on the verge of starvation as they were for the greater part of the time, can hardly be estimated, and when, on July 31, the *Kite* arrived, they were utterly broken down and ill, but they subsequently recovered under careful treatment. The expedition, according to a later telegram, will not be entirely barren of scientific results, as Lieut. Peary is reported to have mapped Whale Sound, and completed his studies of the Eskimo Highlanders. He has also brought back another year's meteorological record. The relief expedition, too, is credited with obtaining the largest collection of Arctic fauna and flora ever acquired, and Prof. Salisbury, of Chicago University, did good geological work.

A COMMUNICATION was made to the press on Friday last by Reuter's Agency with reference to the movements of the Jackson-Harmsworth Polar Expedition. It was admitted that the intelligence received had been made in a somewhat meagre and disjointed form; but from it could be gleaned that on September 7 of last year the expedition arrived safely on the coast of Franz Josef Land and in the locality of Cape Flora. On September 10 the ice closed round the *Windward*, and she was frozen in for the winter. On February 23 the sun returned, and on March 10 Mr. Jackson started on his northern journey, with a quantity of stores, and made his first depôt. Various journeys to and fro with provisions, &c., were made, and depôts formed, the most northern of which was about 100 miles from the camp. The *Windward* has, it is expected, now set sail for home, bearing letters and journals of the early part of the expedition.

THE expedition to Alaska of the United States Geological Survey, for the purpose of examining into the coal and gold mines of the territory, has returned safely to San Francisco after a successful and very interesting season, during which, incidentally, many of the glaciers and volcanos were studied. Messrs. Becker and Dall will return to Washington by October 1, to submit their report upon the mineral resources to the Director of the Survey, which will be printed as soon as the necessary analyses, &c., can be made.

WE have to record the death, at Berlin, at the age of seventy-six, of Prof. Bardeleben, the eminent surgeon and author of "Lehrbuch der Chirurgie und Operationslehre."

THE death is announced, from Bendigo, Victoria, of Dr. Paul Howard MacGillivray, well known as a medical man and for his researches on Polyzoa.

AT the meeting of the Entomological Society of London, to be held on Wednesday, October 2, the following papers will be read:—"Contributions towards the History of Maruina, a New Genus of Diptera" (*Psychodidæ*), by Dr. Fritz Müller; "Remarks on the Homologies and Differences between the First Stages of Pericoma and those of Maruina," by Baron Osten Sacken.

THE annual meeting of the Federated Institution of Mining Engineers has just taken place at Hanley, and papers were read on "The Use of Steel Girders in Mines," "Economic Minerals of the Province of Ontario," and "Gold Mining in Nova Scotia." The Institution seems to be in a flourishing condition, the membership having risen from 1189 in 1889–90, to 2199 at the present time. The prizes for papers on "The Prevention of Accidents in Mines" have been awarded as follows: (1) Mr. A. Kirkup (2) Mr. W. N. Drew; Messrs. E. A. Allport and A. Noble were bracketed for the third place.

THE Royal Society of New South Wales offers its medal and the sum of £25 for the best communications (provided such be deemed of sufficient merit) on original research in the following subjects:—"The Origin of Multiple Hydatids in Man"; "The Occurrence of Precious Stones in New South Wales, with a description of the Deposits in which they are found"; "The Effect of the Australian Climate on the Physical Development of the Australian-born Population"; "The Physiological Action of the Poison of any Australian Snake, Spider, or Tick"; "The Chemistry of the Australian Gums and Resins"; "The Embryology and Development of the Echidna or Platypus"; "The Chemical Composition of the Products from the so-called Kerosene Shale of New South Wales"; "The Mode of Occurrence, Chemical Composition and Origin of Artesian Water in New South Wales." The competition is open to all, and is not subject to any restriction, save that the communication to be successful must be either wholly or in part the result of the competitor's own original observation or research. The successful essays will be published in the Society's annual volume, and fifty copies of the paper will be supplied to their writers free of charge. Particulars as to the latest dates for sending papers, and all other necessary information, may be obtained from the Honorary Secretaries, at the house of the Royal Society of New South Wales, 5, Elizabeth-street, Sydney.

THE Manchester Trades Council has recently passed a resolution strongly in favour of the Report of the Select Committee of the House of Commons on Weights and Measures, in which the Council expresses the hope that no efforts will be spared to make the Committee's recommendations law. As can be readily understood, the New Decimal Association is much encouraged by the attitude taken in so important a commercial centre as Manchester, and it is to be hoped that at no distant date their efforts will be crowned with success, and that the present cumbrous system will be for ever abandoned.

THE metric system of weights and measures is to be obligatory in the United States of Mexico from September 16, 1896. This system has been in use in the Government departments of Mexico for some time past, but a decree recently passed makes it the sole legal system throughout the Republic, and will make an end of the various old Spanish measures hitherto in vogue in ordinary business transactions.

DR. VAN RIJCKEVORSEL and Herr van Bemmelen are engaged on a research which has for its object to determine the influence of elevation above sea-level on the magnetic elements. For this purpose an accurate magnetic survey must be made of some moderately high mountain, of non-magnetic material and sufficiently far removed from magnetic masses. The Righi seems to fulfil these conditions most satisfactorily; but in order to decide the matter, Herrn. van Rijckevorsel and van Bemmelen selected thirty stations, distributed on the low ground round the Righi in three concentric circles with the mountain as centre. The magnetic elements have been determined at these stations, but the calculations are not yet completed. If these indicate no traces of disturbance, due to the Righi or its surroundings, the survey of the mountain will be proceeded with.

THE latest number of the *Records* of the Geological Survey of India contains a translation of a paper by Dr. F. Kurtz, on the Lower Gondwana beds of Argentina (from *Revista del Mus. de la Plata*). In this is recorded an important discovery of plant-remains in shales at Bajo de Velis. These fossils are well-preserved, and while being quite different from the Argentine plant-remains already found, show a close affinity to the plants of the Kaharbari beds of the Lower Gondwanas of India, as well as to those of the Ekka-Kimberley beds of South Africa, the Newcastle and Baccus-Marsh beds of Australia, and the Mersey beds of Tasmania. The previously-known plant-bearing beds of Argentina consisted of two series—one containing a Rhætic flora, resembling that of the Stormberg (Upper Karoo) beds of South Africa, the Hawkesbury beds of Australia, and the Rajmahal (Upper Gondwana) series of India; the other containing a flora of Lower Carboniferous character. The newly-discovered flora must be intermediate in age between these two—that is to say, it cannot be older than Upper Carboniferous, nor younger than Triassic; and with it must go the flora of the important coal-bearing Upper Gondwana beds of India. These have already been assigned to the Upper Carboniferous (at lowest) by Messrs. Medlicott and Blanford, and the Indian Survey, and the new discoveries in Argentina give a satisfactory confirmation of their views.

WE note the publication of the first *Bulletin des Observations Météorologiques*, 1894, by the Observatory of St. Louis, St. Heliers, Jersey, containing monthly means from direct observations and from self-recording instruments. The Director of this new Observatory is the Rev. M. Dechevrens, who has already done good work at Zi-ka-wei, near Shanghai, and by the investigation of the typhoons of the China Seas, in connection with the Shanghai Meteorological Society. The St. Louis Observatory is provided with a tower about 150 feet high, for the special study of vertical wind currents and atmospheric electricity.

THE Acclimatisation Society of Moscow must be credited with more than ordinary originality and ingenuity in its efforts to improve the system of bee-keeping in vogue among the Russian peasants. Antiquated and unremunerative methods of hive management are still in general use in Russia; and, in order to diffuse a knowledge of the more rational methods of modern apiarists, the Society last year organised a travelling bee-keeping exhibition upon a novel and, as it proved, most successful plan. A barge, 70 metres long and 8 metres broad, was procured and fitted up with a museum, a garden with trees and flower-beds, hives of all kinds, old and new, and a number of hives with living bees; there were also dwelling-rooms for the travelling staff. The museum contained examples of bee-keeping appliances and products, together with a set of preparations illustrating the structure and life-history of bees and their natural enemies. The staff in charge of the exhibition consisted of a practical bee-keeper, two entomologists, and ten men-servants for the vessel. The floating exhibition was towed down the river out of Moscow by twenty horses, ten on each bank; and six towns and about twenty villages were visited between the old capital and the town Kaluga. The travelling was done during the night. During the day, from 8 a.m. to 9 p.m., a halt was made at some town or village; the objects in the museum were explained to visitors by the staff, and the methods of working the model hives were demonstrated to the bee-keeping country folk. The exhibition has worked with great success. The great expense which this interesting and instructive exhibition demanded was most willingly defrayed by Herr F. Motschalkin, who is himself an enthusiastic bee-keeper.

A NEW determination of the lowest temperature at which a hot body becomes visible is published by Sgr. P. Pettinelli, in the *Nuovo Cimento*. He heated a cast-iron cylinder 30 cm. long and 14 cm. broad in a wrought-iron jacket over a Bunsen burner to a temperature of 460° C., as indicated by an air thermometer, and then observed its flat end in a dark room from a point 60 cm. above it. When it had cooled to about 415°, the red heat vanished and gave way to an indefinite hazy glow. This glow completely disappeared at 404°, and repeated observations gave an error of only 3°. Highly emissive substances, such as the "mantles" made by Auer and others for incandescent gas lighting, became visible at the same temperature; but reflecting surfaces had to be heated 20° higher before they appeared to the eye, and glass still more. These low temperature rays were found to traverse glass and water like ordinary light rays, but they suffer a comparatively greater absorption. Different eyes differ slightly in their capacity of seeing them, the maximum divergence being about 6°. But then the extent of surface must be the same. Sgr. Pettinelli found that if he screened off all but 1/40th of the surface, the body had to be heated 6° higher than before to become visible; if 1/200th, 20° higher; and if 1/800th only was exposed to view, the minimum temperature of visibility was 460°. Hence he rightly concludes that the contradictory results obtained by previous experimenters are due to differences in the areas of the hot bodies investigated.

THE Irish elk (*Megaceros hibernicus*) has hitherto had a somewhat isolated position as the only species of its genus known to naturalists up to the present. A new claimant to the same generic title has, however, been recently unearthed in Germany, and has been described by Prof. Nehring, of Berlin, under the name *M. Ruffii*. The new species is intermediate in many of its characters between the Irish elk and the fallow deer (*Dama vulgaris*). It appears to have lived during the first interglacial epoch, while the Irish elk flourished at a somewhat later geological period. It may possibly, therefore, be regarded as the ancestor of the latter type. The antlers of *M. Ruffii* have fewer "points" or processes than those of *M. hibernicus*; and, although the skull of the animal was as large as or even larger than that of *M. hibernicus*, the antlers were markedly smaller and diverged from one another much less widely than in the case of the latter species. A restoration of the animal accompanies Prof. Nehring's description in *Wild und Hund* for July 19, 1895. From this picture the differences between this new species and *M. hibernicus* may be at once detected.

SOME important experiments of great practical interest have just been published by Dr. Breslauer on the antiseptic properties possessed by disinfectants mixed with different fats in the shape of ointments. As long as fourteen years ago Koch pointed out that carbolic acid combined with olive oil or "carbolised oil," contrary to the prevailing impression, possessed no antiseptic properties. Dr. Breslauer has extended these experiments to an exhaustive examination of various disinfectants, such as carbolic acid, corrosive sublimate, boric acid, nitrate of silver, &c., in combination with oil, vaseline, fat, lanolin anhydricum, lanolin, and unguentum leniens. It was found that the degree of antiseptic power possessed by the disinfectant depended, in a very remarkable manner, upon the particular diluent employed, and that in all cases the best antiseptic results were obtained with disinfectants in combination with lanolin or unguentum leniens. Thus in a series of experiments on the antiseptic effect produced by adding five per cent. of carbolic acid to various substances, it was ascertained that the *Staphylococcus pyogenes aureus* was still living after being immersed in carbolised oil for three days, in carbolised vaselin it survived one day, in fat four hours, in lanolin anhydricum two hours, in lanolin thirty minutes, and in unguentum leniens twenty minutes. Similar results were obtained not only with other bacteria, but also with different disinfectants. Dr. Breslauer has also examined the bactericidal properties of other ointments in frequent use, such as unguentum zinci, unguentum cinereum (benzoatum), and unguentum precipitatum album, and whilst the two latter were found to be possessed of highly antiseptic properties, the former exercised no perceptible effect whatever. In employing ointments it would appear, therefore, advisable to use the disinfectant selected in combination with lanolin or unguentum leniens instead of supplying vaseline, oil, or other fats, the addition of the latter, according to Dr. Breslauer, serving only to reduce the antiseptic action of the disinfectant. This subject is curiously one which has had, so far, hardly any attention bestowed upon it, and with the exception of some experiments by Gottstein, published in 1889, and, still more recently, an inquiry by Ludwig Bach into the antiseptic effect of various eye ointments, Dr. Breslauer's communication seems to be the only one which has appeared.

WE have recently received two new parts of the *Indian Museum Notes*, from the Trustees of the Museum, being vol. iii. parts 4 and 5. Part 4 is devoted to an account of the insects and mites which attack the tea-plant in India, and includes full descriptions and, in most cases, good figures of the principal insects, &c., discussed; and occasionally of their parasites also. The insects belong to all the more important plant-feeding orders; but what appears to us remarkable is the very large number of *Lepidoptera* which are injurious to the tea-plant, as compared with other insects. Thus, only three beetles are mentioned, belonging to the *Melolonthidæ*, *Chrysomelidæ*, and *Curculionidæ* respectively; as against nineteen *Lepidoptera Heterocera* of various families. The pamphlet concludes with a practical appendix on insecticides. It must not, however, be supposed that a treatise of seventy pages can possibly exhaust the subject of the enemies of any particular plant, especially when they are discussed in detail. A glance at the most important European book on entmological botany (Kaltenbach's "Pflanzenfeinde") is sufficient to show us that many plants are attacked by hundreds of different species of insects; and if this is the case in Europe, it cannot but be true to a still greater extent in tropical countries. But fortunately insects are not always uniformly abundant. They are affected by variations of the season: parasites, and many other influences which are more or less obvious to us; and it is only occasionally that one or other of the numerous species which feed upon any given plant becomes sufficiently abundant to cause any serious injury to it. The other number of the *Indian Museum Notes* before us (part 5) is more varied in its contents. It contains an account of the progress of entomology in the Indian Museum, from 1884–1894, by Mr. E. C. Cotes; some short papers by different entomologists on Indian *Diptera* and *Rhynchota*, and a series of miscellaneous notes on insects of all orders, by Mr. Cotes. This part is not only illustrated, like the other, by numerous woodcuts, but also contains three well-executed plain plates.

THREE important papers by Prof. E. D. Cope, and two by Prof. W. B. Scott, make up, with seven plates, the part recently distributed (vol. ix. part 4) of the *Journal* of the Academy of Natural Sciences of Philadelphia. Prof. Cope treats of new and little known Palæozoic and Mesozoic fishes, and describes Cyphornis—an extinct genus of birds. The genus is established on a species of bird represented by the superior part of a tarsometatarse, obtained by Dr. G. M. Dawson from a bed of indurated greenish clay of unknown age from Vancouver Island. The bird appears to possess real affinities with the Steganopodes, combined with affinities to more primitive birds with a simple hypotarsal structure. "The presumed affinity with the Steganopodes," remarks Prof. Cope, "indicates natatory habits, and probable capacity for flight. Should this power have been developed in *Cyphornis magnus*, it wlll have been much the largest bird of flight thus far known." Another paper by Prof. Cope is on extinct *Bovidæ*, *Canidæ* and *Felidæ*, from the Pleistocene of Southern Kansas and Western Central Oklahoma. Prof. W. B. Scott's memoir on the structure and relationships of *Ancodus* supplements the extensive investigations of Kowalevsky and Filhol by giving an account of the American species of that genus, and by showing the points of resemblance and differences between the approximately contemporaneous species of *Ancodus* in America and Europe. Prof. Scott concludes his valuable paper as follows:—"With the facts at present known, all seem to point to the origin of *Ancodus* in the Old World and its migration to America, in the interval between the Eocene and the Oligocene (Uinta and White River), yet until the American artiodactyls from the middle and upper Eocene are far better known than at present, such a conclusion cannot be regarded as final." The second paper by Prof. Scott deals with the osteology of *Hyænodon*—a genus described by him in 1877, so far as the materials then available would permit. The Princetown expedition of last year resulted in the collection by Mr. Hatchet of several more or less complete skeletons representing a number of species. These specimens of *Hyænodon* enabled Prof. Scott to supplement the earlier account with the present paper, in which is given a restoration of the skeleton of the very curious and remarkable animal with which it deals.

MESSRS. ROWLAND WARD AND CO., of Piccadilly, are sending out invitations to naturalists to inspect a mounted example of the White Rhinoceros (*Rhinoceros simus*) from Zululand. The two specimens brought home about two years ago were from Northern Mashonaland. Thus this animal, until lately supposed to be quite extinct, has now been found in a second locality. But these are now the only two spots on the face of the earth where this huge creature, formerly abundant in the Cape Colony, still exists, in very dwindling numbers, which will, no doubt, be now rapidly diminished.

A COMMITTEE of six gentlemen has been appointed by the Governor-General of Goa, India, to carry on excavations in the ancient city of Goa, in rearch of relics of the traditional grandeur of the past, and to take the necessary steps for the preservation of the monuments of Portuguese rule in India in the earlier time.

AN electrical forge, where the whole of the heating required is done by electricity, is in operation at Niagara Falls, the power being supplied by the great cataract. The cost of making a horse-shoe at the electric forge is, it is stated, much less than at an ordinary coal forge. We hear, too, that corn is being threshed by electricity, with very satisfactory results, at Mjölby in Sweden.

WE have received from Mr. W. Radcliffe, of Andreas School, Isle of Man, the inventor of the "Gonagraph," an instrument for drawing perfectly accurate equilateral triangles, squares, pentagons, hexagons, heptagons and octagons, an arithmetical puzzle. The puzzle consists of nineteen small cubes, having a face on each numbered with one of the first nineteen numbers, which are to be placed upon squares, symmetrically arranged on a board, five on the middle row, and two rows of four and three squares to right and left of this. The numbers are to be so arranged that their sum along each of twelve straight lines shall make up thirty-eight. This sum is also obtainable from other symmetrical arangements. It will thus be seen that the puzzle is of the nature of a magic square, and is a very ingenious one. The author has favoured us with his solution, which naturally is at present kept back. He has not furnished us with a clue to his arrangement, and we have in vain searched for it; nor does he say whether he has attempted any extension of the puzzle to thirty-seven or a higher number of cubes. The "thirty-eight" puzzle can be obtained direct from the inventor in a small box for sixpence.

A DESCRIPTION has been sent to us of a new arc lamp for projection purposes, which has been devised by Mr. Cecil M. Hepworth. The instrument has three regulating discs or milled heads of vulcanite, which project at the back, so as to be under the control of the lanternist. The top and bottom discs are for the purpose of regulating the positions of the carbons, and the middle disc has three duties to perform, viz. to bring the carbons slowly together as their points waste in consumption, by a push action to cause the carbons instantaneously to touch, and by a spring to as quickly separate, while by an upward movement the worm-wheel is thrown completely out of gear, and the carbons can be rapidly separated or brought together by hand, a provision necessary for the saving of time when inserting new carbons.

THE September part of the *Proceedings* of the Physical Society of London has reached us, and contains, in addition to the usual valuable supplement of "Abstracts of Physical Papers from Foreign Sources," the following papers:—"A Theory of the Synchronous Motor," by W. G. Rhodes (continuation); "On the Use of an Iodine Voltameter for the Measurement of Small Currents," by Prof. E. F. Herroun, "On the Condensation and the Critical Phenomena of Mixtures of Ethane and Nitrous Oxide," by Dr. Kuenen; "An Electro-Magnetic Effect," by F. W. Bowden; and "The Electrical Properties of Selenium," by Shelford Bidwell, F.R.S.

THE September–October part of the *Physical Review* (Macmillan) contains the following articles: "A Study of the Polarisation of the Light emitted by Incandescent Solid and Liquid Surfaces," by R. A. Millikan, "Alternating Currents when the Electromotive Force is of a Zigzag Wave Type," by E. C. Rimmington, "On Ternary Mixtures," by W. D. Bancroft, part 2; and minor contributions.

BOURNE'S Handy Assurance Manual for 1895, by William Schooling, has been published. It contains in a small compass a whole host of information likely to be of use to those who are interested in insurance matters, and appears to have been compiled with great care.

WE have received from Messrs. G. W. Wilson and Co., Limited, 2 St. Swithin Street, Aberdeen, copies of their catalogues of lantern slides. The list of subjects illustrated is a very full one, and the catalogues may be had upon application.

ON the completion of the fiftieth year of its existence, the editor of the *Botanische Zeitung* publishes a very useful index of the papers contained in the first fifty volumes.

THE September number of the *Irish Naturalist* has just appeared, and is entirely devoted to reports of the Galway conference and excursion of the Irish Field Club Union, held in July.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*, ♀) from India, presented by Miss Larkin; a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. W. Aldridge; a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by Mrs. Griffith; a —— Monkey (*Cercopithecus*, sp. inc.) from Africa, presented by Miss Pigott; two Vulpine Phalangers (*Phalangista vulpina*, ♂ ♀) from Australia, presented by Mr. F. J. Horniman; a Magpie (*Pica caudata*), British, presented by Mr. H. E. Blandford; an Orange-cheeked Amazon (*Chrysotis autumnalis*) from Central America, presented by the Rev. W. J. Loftie; a Martinique (*Ionornis martinicus*), captured off the Island of Ascension, presented by Mr. H. W. Power; a Smooth Snake (*Coronella lævis*), a Common Viper (*Vipera berus*), British, presented by Mr. G. J. S. Warner; a Brown Capuchin (*Cebus fatuellus*) from Guiana, three Grant's Francolines (*Francolinus granti*) from East Africa, two Egyptian Trionyx (*Trionyx niloticus*) from the Congo, deposited; a Two-toed Sloth (*Cholopus didactylus*) from Brazil, a Yellow-naped Amazon (*Chrysotis auripalliata*) from Central America, purchased.

OUR ASTRONOMICAL COLUMN.

THE ORBIT OF μ^2 BOOTIS (Σ 1938).—Dr. T. J. J. See gives in the *Astr. Nach.*, No. 3309, Bd. 138, the results of his researches on this star. This double was discovered by Sir William Herschel in 1781, and since the time of Struve it has been very abundantly observed. In all parts of the orbit the pair is sufficiently wide to be seen with a 6-inch telescope. The investigation gives the following elements of μ^2 Bootis; other elements are given for comparison.

	P	T	*e*	*a*	Ω	*i*	λ	Authority
				″	°	°	°	
1	146·649	1851·57	0·8529	1·320	94·7	49·4	87·1	Mädler 1847
2	182·6	66·0	0·491	1·165	166·1	47·5	23·0	Winagradsky 1872
3	314·34	60·88	0·5641	1·761	163·2	41·9	54·4	Hind 1872
4	200·4	65·2	0·51	—	172·0	45·0	20·1	Wilson 1872
5	198·93	65·5	0·4957	—	169·0	46·4	23·6	Klinkerfues
6	290·07	63·51	0·6174	1·500	183·0	44·4	17·7	Doberck 1875
7	280·29	60·51	0·5974	1·47	173·7	39·9	20·0	Doberck 1878
8	266·0	62·55	0·5668	1·057	166·7	35·2	40·9	Pritchard 1878
9	219·42	65·30	0·537	1·268	163·8	43·9	329·75	See 1895

The apparent orbit is:

Major axis = 2″·656
Minor axis = 1″·480
Angle of major axis = 173°·5
,, ,, periastron = 186°·7
Distance of star from centre = 0″·638

The computed and observed places seem to justify the new elements given above. The period thus will hardly be varied by as much as ten years, while the resulting alteration will be small in proportion.

THE BRITISH ASSOCIATION.

SECTION K.

BOTANY.

OPENING ADDRESS BY W. T. THISELTON-DYER, M.A., F.R.S., C.M.G., C.I.E., DIRECTOR OF THE ROYAL GARDENS.

THE establishment of a new Section of the British Association, devoted to Botany, cannot but be regarded by the botanists of this country as an event of the greatest importance. For it is practically the first time that they have possessed an independent organisation of their own. It is true that for some years past we have generally been strong enough to form a separate department of the old Biological Section D, on the platform of which so many of us in the past have acted in some capacity or other, and on which indeed many of us may be said to have made our first appearance. We shall not start then on our new career without the remembrance of filial affection for our parent, and the earnest hope that our work may be worthy of its great traditions.

The first meeting of the Section, or, as it was then called, Committee, at Oxford was held in 1832. And though there has been from time to time some difference in the grouping of the several biological sciences, the two great branches of biology have only now for the first time formally severed the partnership into which they entered on that occasion. That this severance, if inevitable from force of circumstances, is in some respects a matter of regret, I do not deny. Specialisation is inseparable from scientific progress; but it will defeat its own end in biology if the specialist does not constantly keep in touch with those fundamental principles which are common to all organic nature. We shall have to take care that we do not drift into a position of isolation. Section D undoubtedly afforded a convenient opportunity for discussing many questions on which it was of great advantage that workers in the two different fields should compare their results and views. But I hope that by means of occasional conferences we shall still, in some measure, be able to preserve this advantage.

RETROSPECT.

I confess I found it a great temptation to review, however imperfectly, the history and fortunes of our subject while it belonged to Section D. But to have done so would have been practically to have written the history of botany in this country since the first third of the century. Yet I cannot pass over some few striking events.

I think that the earliest of these must undoubtedly be regarded as the most epoch-making. I mean the formal publication by the Linnean Society, in 1833, of the first description of "the nucleus of the cell," by Robert Brown ("Misc. Bot. Works," i. 512). It seems difficult to realise that this may be within the recollection of some who are now living amongst us. It is, however, of peculiar interest to me that the first person who actually distinguished this all-important body, and indicated it in a figure, was Francis Bauer, thirty years earlier, in 1802. This remarkable man, whose skill in applying the resources of art to the illustration of plant anatomy has never, I suppose been surpassed, was "resident draughtsman for fifty years to the Royal Botanic Garden at Kew." And it was at Kew, and in a tropical orchid, *Phaius grandifolius*, no doubt grown there, that the discovery was made.

It was, I confess, with no little admiration that, on refreshing my memory by a reference to Robert Brown's paper, I read again the vivid account which he gives in a footnote of the phenomena, so painfully familiar to many of us who have been teachers, exhibited in the staminal hair of *Tradescantia*. Sir Joseph Hooker (*Proc. Linn. Soc.*, 1887-88, 65) has well remarked that "the supreme importance of this observation, . . . leading to undreamt-of conceptions of the fundamental phenomena of organic life, is acknowledged by all investigators." It is singular that so profound an observer as Robert Brown should have himself missed the significance of what he saw. The world had to wait for the discovery of protoplasm by Von Mohl till 1846, and till 1850 for its identification with the sarcode of zoologists by Cohn, who is still, I am happy to say, living and at work, and to whom last year the Linnean Society did itself the honour of presenting its medal.

The Edinburgh meeting of the Association, in 1834, was the occasion of the announcement of another memorable discovery of Robert Brown's. I will content myself with quoting Hofmeister's ("Higher Cryptogamia," 432) account of it. "Robert Brown was the discoverer of the polyembryony of the *Coniferæ*. In a later treatise he pointed out the origin of the pro-embryo in large cells of the endosperm, to which he gave the name of corpuscula." The period of the forties, just half a century ago, looks in the retrospect as one of almost dazzling discovery. To say nothing of the formal appearance of protoplasm on the scene, the foundations were being laid in all directions of our modern botanical morphology. Yet its contemporaries viewed it with a very philosophical calm. Thwaites, who regarded Carpenter as his master, described at the Oxford meeting in 1847 the conjugation of the *Diatomaceæ*, and "distinctly indicated," as Carpenter ("Memorial Sketch," 140) says, "that conjugation is the primitive phase of sexual reproduction." Berkeley informed me that the announcement fell perfectly flat. A year or two later Suminski came to London with his splendid discovery (1848) of the archegonia of the fern, the antheridia having been first seen by Nägeli in 1844. Carpenter (*loc. cit.*, 141) gave me, many years after, a curious account of its reception. "At the Council of the Ray Society, at which," he said, "I advocated the reproduction of Suminski's book on the 'Ferns,' I was assured that the close resemblance of the antherozoids to spermatozoa was quite sufficient proof that they could have nothing to do with vegetable reproduction. I do not think," he added—and the complaint is pathetic—"that the men of the present generation, who have been brought up in the *light*, quite apprehend (in this as in other matters) the utter darkness in which we were then groping, or fully recognise the deserts of those who helped them to what they now enjoy." This was in 1875, and I suppose is not likely to be less true now.

The Oxford meeting in 1860 was the scene of the memorable debate on the origin of species, at which it is interesting to remember that Henslow presided. On that occasion Section D reached its meridian. The battle was Homeric. However little to the taste of its author, the launching of his great theory was, at any rate, dignified with a not inconsiderable explosion. It may be that it is not given to the men of our day to ruffle the dull level of public placidity with disturbing and far-reaching ideas. But if it were, I doubt whether we have, or need now, the fierce energy which inspired then either the attack or the defence. When we met again in Oxford last year the champion of the old conflict stood in the place of honour, acclaimed of all men, a beautiful and venerable figure. We did not know then that that was to be his farewell.

The battle was not in vain. Six years afterwards, at Nottingham, Sir Joseph Hooker delivered his classical lecture on Insular Floras. It implicitly accepted the new doctrine, and applied it with admirable effect to a field which had long waited for an illuminating principle. The lecture itself has since remained one of the corner-stones of that rational theory of the geographical distribution of plants which may, I think, be claimed fairly as of purely English origin.

HENSLOW.

Addressing you as I do at Ipswich, there is one name written in the annals of our old Section which I cannot pass over—that of Henslow. He was the Secretary of the Biological Section at its first meeting in 1832, and its President at Bristol in 1836. I suppose there are few men of this century who have indirectly more influenced the current of human thought. For in great measure I think it will not be contested that we owe Darwin to him. As Romanes has told us ("Memorial Notices," 13): "His letters written to Prof. Henslow during his voyage round the world overflow with feelings of affection, veneration, and obligation to his accomplished master and dearest friend—feelings which throughout his life he retained with no diminished intensity. As he used himself to say, before he knew Prof.

Henslow the only objects he cared for were foxes and partridges." I do not wish to overstate the facts. The possession of "the collector's instinct, strong in Darwin from his childhood, as is usually the case in great naturalists," to use Huxley's (*Proc. R.S.*, xliv. vi.) words, would have borne its usual fruit in after life, in some shape or other, even if Darwin had not fallen into Henslow's hands. But then the particular train of events which culminated in the great work of his life would never have been started. It appeared to me, then, that it would not be an altogether uninteresting investigation to ascertain something about Henslow himself. The result has been to provide me with several texts, which I think it may be not unprofitable to dwell upon on the present occasion.

In the first place, what was the secret of his influence over Darwin? "My dear old master in Natural History" ("Life," ii. 317) he calls him; and to have stood in this relation to Darwin[1] is no small matter. Again, he speaks of his friendship with him as "a circumstance which influenced my whole career more than any other" (i. 52). The singular beauty of Henslow's character, to which Darwin himself bore noble testimony, would count for something, but it would not in itself be a sufficient explanation. Nor was it that intellectual fascination which often binds pupils to the master's feet; for, as Darwin tells us, "I do not suppose that any one would say that he possessed much original genius" (i. 52). The real attraction seems to me to be found in Henslow's possession, in an extraordinary degree, of what may be called the Natural History spirit. This resolves itself into keen observation and a lively interest in the facts observed. "His strongest taste was to draw conclusions from long-continued minute observations" (i. 52). The old Natural History method, of which it seems to me that Henslow was so striking an embodiment, is now, and I think unhappily, almost a thing of the past. The modern university student of botany puts his elders to blush by his minute knowledge of some small point in vegetable histology. But he can tell you little of the contents of a country hedgerow; and if you put an unfamiliar plant in his hands he is pretty much at a loss how to set about recognising its affinities. Disdaining the field of nature spread at his feet in his own country, he either seeks salvation in a German laboratory or hurries off to the Tropics, convinced that he will at once immortalise himself. But *cœlum non animum mutat;* he puts into "pickle" the same objects as his predecessors, never to be looked at again; or perhaps writes a paper on some obvious phenomena which he could have studied with less fatigue in the Palm House at Kew.

The secret of the right use of travel is the possession of the Natural History instinct, and to those who contemplate it I can only recommend a careful study of Darwin's "Naturalist's Voyage." Nothing that came in his way seems to have evaded him or to have seemed too inconsiderable for attention. No doubt some respectable travellers have lost themselves in a maze of observations that have led to nothing. But the example of Darwin, and I might add of Wallace, of Huxley, and of Moseley, show that that result is the fault of the man and not of the method. The right moment comes when the fruitful opportunity arrives to him who can seize it. The first strain of the prelude with which the "Origin" commences are these words: "When on board H.M.S. *Beagle* as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America." But this sort of vein is not struck at hazard or by him who has not served a tolerably long apprenticeship to the work.

When one reads and re-reads the "Voyage," it is simply amazing to see how much could be achieved with a previous training which we now should think ludicrously inadequate. Before Henslow's time the state of the natural sciences at Cambridge was incredible. In fact, Leonard Jenyns ("Memoir," 175), his biographer, speaks of the "utter disregard paid to Natural History in the University previous to his taking up his residence there." The Professor of Botany had delivered no lectures for thirty years, and though Sir James Smith, the founder of the Linnean Society, had offered his services, they were declined on the ground of his being a Nonconformist (*ibid.*, 37).

As to Henslow's own scientific work, I can but rely on the judgment of whose who could appreciate it in relation to its time. According to Berkeley (*ibid.*, 56), "he was certainly one of the first, if not the very first, to see that two forms of fruit might exist in the same fungus." And this, as we now know, was a fundamental advance in this branch of morphology. Sir Joseph Hooker tells me that his papers were all distinctly in advance of his day. Before occupying the chair of botany, he held for some years that of mineralogy. Probably he owed this to his paper on the Isle of Anglesey, published when he was only twenty-six. I learn from the same authority, that this to some extent anticipated, but at any rate strongly influenced, Sedgwick's subsequent work in the same region.

BOTANICAL TEACHING.

Henslow's method of teaching deserves study. Darwin says of his lectures "that he liked them much for their extreme clearness." "But," he adds, "I did not study botany" (i. 48). Yet we must not take this too seriously. Darwin ("Voyage," 421), when at the Galapagos, "indiscriminately collected every thing in flower on the different islands, and fortunately kept my collections separate." Fortunately indeed; for it was the results extracted from these collections, when worked up subsequently by Sir Joseph Hooker, which determined the main work of his life. "It was such cases as that of the Galapagos Archipelago which chiefly led me to study the origin of species" (iii. 159).

Henslow's actual method of teaching went some way to anticipate the practical methods of which we are all so proud. "He was the first to introduce into the botanical examination for degrees in London the system of practical examination" ("Memoir," 161). But there was a direct simplicity about his class arrangements characteristic of the man. "A large number of specimens . . . were placed in baskets on a side-table in the lecture-room, with a number of wooden plates and other requisites for dissecting them after a rough fashion, each student providing himself with what he wanted before taking his seat" (*ibid.*, 39). I do not doubt that the results were, in their way, as efficient as we obtain now in more stately laboratories.

The most interesting feature about his teaching was not, however, its academic aspect, but the use he made of botany as a general educational instrument. "He always held that a man of *no* powers of observation was quite an exception" (*ibid.*, 163). He thought (and I think he proved) that botany might be used "for strengthening the observant faculties and expanding the reasoning powers of children in all classes of society" (*ibid.*, 99). The difficulty with which those who undertake now to teach our subject have to deal is that most people ask the question, What is the use of learning botany unless one means to be a botanist? It might indeed be replied that as the vast majority of people never learn anything effectively, they might as well try botany as anything else. But Henslow looked only to the mental discipline; and it was characteristic of the man and of his belief in his methods that when he was summoned to Court to lecture to the Royal family, his lectures "were, in all respects, identical with those he was in the habit of giving to his little Hitcham scholars" ("Memoir," 149); and it must be added that they were not less successful.

This success naturally attracted attention. Botanical teaching in schools was taken up by the Government, and continues to receive support to the present day. But the primitive spirit has, I am afraid, evaporated. The measurement of results by means of examination has been fatal to its survival. The teacher has to keep steadily before his eyes the necessity of earning his grant. The educational problem retires into the background. "The strengthening of the observant faculties," and the rest of the Henslowian programme must give way to the imperious necessity of presenting to the examiner candidates equipped with at least the minimum of text-book formulas reproducible on paper. I do not speak in this matter without painful experience. The most astute examiner is defeated by the still more astute crammer. The objective basis of the study on which its whole usefulness is built up is promptly thrown aside. If you supply the apple blossom for actual description, you are as likely as not to be furnished with a detailed account of a buttercup. The training of observation has gone by the board, and the exercise of mere memory has taken its place. But a table of logarithms or a Hebrew grammar would serve this purpose equally well. Yet I do not despair of Henslow's work still bearing fruit. The examination system will collapse from the sheer impossibility of carrying it on beyond a certain point. Freed from its trammels, the teacher will have greater scope for individuality, and the result of his labours will be rewarded after some intelligent system of inspection. And here I may claim support from an unexpected quarter. Mr. Gladstone has recently written to a

[1] As I shall have frequent occasion to quote the "Life and Letters" I shall insert the references in the text.

correspondent:—"I think that the neglect of natural history, in all its multitude of branches, was the grossest defect of our old system of training for the young; and, further, that little or nothing has been done by way of remedy for that defect in the attempts made to alter or reform that system." I am sure that the importance and weight of this testimony, coming as it does from one whose training and sympathies have always been literary, cannot be denied. That there is already some revival of Henslow's methods, I judge from the fact that I have received applications from Board Schools, amounting to some hundreds, for surplus specimens from the Kew Museums. Without a special machinery for the purpose I cannot do much, and perhaps it is well. But my staff have willingly done what was possible, and from the letters I have received I gather that the labour has not been wholly misspent.

MUSEUM ARRANGEMENT.

This leads me to the last branch of Henslow's scientific work on which I am able to touch, that of the arrangement of museums, especially those which being local have little meaning unless their purpose is strictly educational. I think it is now generally admitted that, both in the larger and narrower aspects of the question, his ideas, which were shared in some measure by Edward Forbes, were not merely far in advance of his time, but were essentially sound. And here I cannot help remarking that the zoologists have perhaps profited more by his teaching than the botanists. I do not know how far Sir William Flower and Prof. Lankester would admit the influence of Henslow's ideas. But, so far as my knowledge goes, I am not aware that, at any rate in Europe, there is anything to be seen in public museums comparable to the educational work accomplished by the one at the College of Surgeons and the Natural History Museum, and by the other at Oxford.

I have often thought it singular that in botany we have not kept pace in this matter with our brother naturalists. I do not doubt that vegetable morphology and a vast number of important facts in evolution, as illustrated from the vegetable kingdom, might be presented to the eye in a fascinating way in a carefully arranged museum. The most successful and, indeed, almost the only attempt which has been made in this direction is that at Cambridge, which, I believe, is due to Mr. Gardiner. But our technical methods for preserving specimens still leave much to desire. Something more satisfactory will, it may be hoped, some day be devised, and the whole subject is one which is well worth the careful consideration of our Section. Henslow at least effected a vast improvement in the mode of displaying botanical objects; and a collection prepared by his own hands, which was exhibited at one of the Paris exhibitions, excited the warm admiration of the French botanists, who always appreciate the clear illustration of morphological facts.

OLD SCHOOL OF NATURAL HISTORY.

If the old school of natural history of which Henslow in his day was a living spirit is at present, as seems to be the case, continually losing its hold upon us, this has certainly not been due to its want of value as an educational discipline, or to its sterility in contributing new ideas to human knowledge. Darwin's "Origin of Species" may certainly be regarded as its offspring, and of this Huxley (*Proc. R.S.*, xliv. xvii.) says with justice: "It is doubtful if any single book except the 'Principia,' ever worked so great and rapid a revolution in science, or made so deep an impression on the general mind." Yet Darwin's biographer, in that admirable "Life" which ranks with the few really great biographies in our language, remarks (i. 155): "In reading his books one is reminded of the older naturalists rather than of the modern school of writers. He was a naturalist in the old sense of the word, that is, a man who works at many branches of science, not merely a specialist in one." This is no doubt true, but does not exactly hit off the distinction between the kind of study which has gone out of fashion and that which has come in. The older workers in biology were occupied mainly with the external or, at any rate, grosser features of organisms and their relation to surrounding conditions; the modern, on the other hand, are engaged on the study of internal and intimate structure. Work in the laboratory, with its necessary limitations, takes the place of research in the field. One may almost, in fact, say that the use of the compound microscope divides the two classes. Asa Gray has compared Robert Brown with Darwin as the "two British naturalists" who have "more than any others, impressed their influence upon science in the nineteenth century" (NATURE, x. 80). Now it is noteworthy that Robert Brown did all his work with a simple microscope. And Francis Darwin writes of his father: "It strikes us nowadays as extraordinary that he should have had no compound microscope when he went his *Beagle* voyage; but in this he followed the advice of Robert Brown, who was an authority on such matters" (i. 145). One often meets with persons, and sometimes of no small eminence, who speak as if there were some necessary antagonism between the old and the new studies. Thus I have heard a distinguished systematist describe the microscope as a curse, and a no less distinguished morphologist speak of a herbarium having its proper place on a bonfire. To me I confess this anathematisation of the instruments of research proper to any branch of our subject is not easily intelligible. Yet in the case of Darwin himself it is certain that if his earlier work may be said to rest solely on the older methods, his later researches take their place with the work of the new school. At our last meeting Pfeffer vindicated one of his latest and most important observations.

The case of Robert Brown is even more striking. He is equally great whether we class him with the older or the modern school. In fact, so far as botany in this country is concerned, he may be regarded as the founder of the latter. It is to him that we owe the establishment of the structure of the ovule and its development into the seed. Even more important were the discoveries to which I have already referred, which ultimately led to the establishment of the group of Gymnosperms. "No more important discovery," says Sachs ("History," 142), "was ever made in the domain of comparative morphology and systematic botany. The first steps towards this result, which was clearly brought out by Hofmeister twenty-five years later, were secured by Robert Brown's researches, and he was incidentally led to these researches by some difficulties in the construction of the seed of an Australian genus." Yet it may be remembered that he began his career as naturalist to Flinders's expedition for the exploration of Australia. He returned to England with 4000 "for the most part new species of plants." And these have formed the foundation of our knowledge of the flora of that continent. Brown's chief work was done between 1820 and 1840, and, as Sachs (*loc. cit.*, 139, 140) tells us, "was better appreciated during that time in Germany than in any other country."

MODERN SCHOOL.

The real founder of the modern teaching in this country in both branches of biology I cannot doubt was Carpenter. The first edition of his admirable "Principles of Comparative Physiology" was published in 1838, the last in 1854. All who owe, as I do, a deep debt of gratitude to that book will agree with Huxley ("Memorial Sketch," 67) in regarding it as "by far the best general survey of the whole field of life and of the broad principles of biology which had been produced up to the time of its publication. Indeed," he adds, "although the fourth edition is now in many respects out of date, I do not know its equal for breadth of view, sobriety of speculation, and accuracy of detail."

The charm of a wide and philosophic survey of the different forms under which life presents itself could not but attract the attention of teachers. Rolleston elaborated a course of instruction in zoology at Oxford in which the structures described in the lecture-room were subsequently worked out in the laboratory. In 1872 Huxley organised the memorable course in elementary biology at South Kensington which has since, in its essential features, been adopted throughout the country. In the following year, during Huxley's absence abroad through ill-health, I arranged, at his request, a course of instruction on the same lines for the Vegetable Kingdom.

That the development of the new teaching was inevitable can hardly be doubted, and I for my part am not disposed to regret the share I took in it. But it was not obvious, and certainly it was not expected, that it would to so large an extent cut the ground from under the feet of the old Natural History studies. The consequences are rather serious, and I think it is worth while pointing them out.

In a vast empire like our own there is a good deal of work to be done and a good many posts to be filled, for which the old Natural History training was not merely a useful but even a necessary preparation. But at the present time the universities almost entirely fail to supply men suited to the work. They neither care to collect, nor have they the skilled aptitude for observation. Then, though this country is possessed at home of incomparable stores of accumulated material, the class of competent amateurs who were mostly trained at our universities, and who did such good service in working that material out, is fast disappearing. It may not be easy indeed in the future to fill important posts even in this country with men possessing the necessary qualifications. But there was still another source of naturalists, even more useful, which has practically dried up. It is an interesting fact that the large majority of men of the last generation who have won distinction in this field have begun their career with the study of medicine. That the kind of training that Natural History studies give is of advantage to students of medicine which, rightly regarded, is itself a Natural History study, can hardly be denied. But the exigencies of the medical curriculum have crowded them out; and this, I am afraid, must be accepted as irremediable. I cannot refrain from reading you, on this point, an extract from a letter which I have received from a distinguished official lately entrusted with an important foreign mission. I should add that he had himself been trained in the old way.

"I have had my time, and must leave to younger men the delight of working these interesting fields. Such chances never will occur again, for roads are now being made and ways cut in the jungle and forest, and you have at hand all sorts of trees level on the ground ready for study. These bring down with them orchids, ferns, and climbers of many kinds, including rattan palms, &c. But, excellent as are the officers who devote their energy to thus opening up this country, there is not one man who knows a palm from a dragon-tree, so the chance is lost. Strange to say, the medical men of the Government service know less and care less for Natural History than the military men, who at least regret they have no training or study to enable them to take an intelligent interest in what they see around them. A doctor nowadays cares for no living thing larger or more complicated than a *bacterium* or a *bacillus*."

But there are other and even more serious grounds why the present dominance of one aspect of our subject is a matter for regret. In the concluding chapter of the "Origin," Darwin wrote: "I look with confidence to the future—to young and rising naturalists." But I observe that most of the new writers on the Darwinian theory, and, oddly enough, especially when they have been trained at Cambridge, generally begin by more or less rejecting it as a theory of the origin of species, and then proceed unhesitatingly to reconstruct it. The attempt rarely seems to me successful, perhaps because the limits of the laboratory are unfavourable to the accumulation of the class of observations which are suitable for the purpose. The laboratory, in fact, has not contributed much to the Darwinian theory, except the "Law of Recapitulation," and that, I am told, is going out of fashion.

The Darwinian theory, being, as I have attempted to show, the outcome of the Natural History method, rested at every point on a copious basis of fact and observation. This more modern speculation lacks. The result is a revival of transcendentalism. Of this we have had a copious crop in this country, but it is quite put in the shade by that with which we have been supplied from America. Perhaps the most remarkable feature is the persistent vitality of Lamarckism. As Darwin remarks: "Lamarck's one suggestion as to the cause of the gradual modification of species—effort excited by change of conditions—was, on the face of it, inapplicable to the whole vegetable world" (ii. 189). And if we fall back on the inherited direct effect of change of conditions, though Darwin admits that "physical conditions have a more direct effect on plants than on animals" (ii. 319), I have never been able to convince myself that that effect is inherited. I will give one illustration. The difference in habit of even the same species of plant when grown under mountain and lowland conditions is a matter of general observation. It would be difficult to imagine a case of "acquired characters" more likely to be inherited. But this does not seem to be the case. The recent careful research of Gaston Bonnier only confirms the experience of cultivators. The modifications acquired by the plant when transported for a definite time from the plains to the Alps, or *vice versâ*, disappear at the end of the same period when the plant is restored to its original conditions (*Ann. d. Sc. nat.*, 7e sér. xx. 355).

Darwin, in an eloquent passage, which is too long for me to quote ("Origin," 426), has shown how enormously the interest of Natural History is enhanced "when we regard every production of nature as one which has had a long history," and "when we contemplate every complex structure . . . as the summing up of many contrivances." But this can only be done, or at any rate begun, in the field, and not in the laboratory.

A more serious peril is the dying out amongst us of two branches of botanical study in which we have hitherto occupied a position of no small distinction. Apart from the staffs of our official institutions, there seems to be no one who either takes any interest in, or appreciates in the smallest degree, the importance of systematic and descriptive botany. And geographical distribution is almost in a worse plight, yet Darwin calls it, "that grand subject, that almost keystone of the laws of creation" (i. 356).

I am aware that it is far easier to point out an evil than to remedy it. The teaching of botany at the present day has reached a pitch of excellence and earnestness which it has never reached before. That it is somewhat one-sided cannot probably be remedied without a subdivision of the subject and an increase in the number of teachers. If it has a positive fault, it is that it is sometimes inclined to be too dogmatic and deductive. Like Darwin, at any rate in a biological matter, "I never feel convinced by deduction, even in the case of H. Spencer's writings" (iii. 168). The intellectual indolence of the student inclines him only too gladly to explain phenomena by referring them to "isms," instead of making them tell their own story.

ORGANISATION OF SECTION.

I am afraid I have detained you too long over these matters, on which I must admit I have spoken with some frankness. But I take it that one of the objects of our Section is to deliver our minds of any perilous stuff that is fermenting in it. But now, having taken leave of the past, let us turn to the future.

We start at least with a clean slate. We cannot bind our successors, it is true, at other meetings. But I cannot doubt that it will be in our power to materially shape our future, notwithstanding. When we were only a department I think we all felt the advantage of these annual meetings, of the profitable discussion, formal and informal, and of the privilege of meeting so many of our foreign brethren who have so generously supported us by their presence and sympathy.

I am anxious, then, to suggest that we should conduct our proceedings on as broad lines as possible. I do not think we should be too ready to encourage papers which may well be communicated to societies, either local or central.

The field is large; the labourers as they advance in life can hardly expect to keep pace with all that is going on in it. We must look to individual members of our number to help us by informing and stimulating addresses on subjects they have made peculiarly their own, or on important researches on which they have been specially engaged.

NOMENCLATURE.

There is one subject upon which, from my official position elsewhere, I desire to take the opportunity of saying a few words. It is that of Nomenclature. It is not on its technical side, I am afraid, of sufficient general interest to justify my devoting to it the space which its importance would otherwise deserve. But I hope to be able to enlist your support for the broad common-sense principles on which our practice should rest.

As I suppose, every one knows we owe our present method of nomenclature in natural history to Linnæus. He devised the binominal, or, as it is often absurdly called, the binomial system. That we must have a technical system of nomenclature I suppose no one here will dispute. It is not, however, always admitted by popular writers who have not appreciated the difficulty of the matter, and who think all names should be in the vernacular. There is the obvious difficulty that the vast majority of plants do not possess any names at all, and the attempts to manufacture them in a popular shape have met with but little success. Then, from lack of discriminating power on the part of those who use them, vernacular names are often ambiguous; thus Bullrush is applied equally to *Typha* and to *Scirpus*, plants extremely different. Vernacular names, again, are only of local utility, while the Linnean system is intelligible throughout the world.

A technical name, then, for a plant or animal is a necessity, as without it we cannot fix the object of our investigations into its affinity, structure, or properties ("Linn. Phil.," 210). "Nomina si nescis perit et cognito rerum."

In order to get clear ideas on the matter let us look at the logical principles on which such names are based. It is fortunate for us that these are stated by Mill, who, besides being an authority on logic, was also an accomplished botanist. He tells us ("System of Logic," i. 132): "A naturalist, for purposes connected with his particular science, sees reason to distribute the animal or vegetable creation into certain groups rather than into any others, and he requires a name to bind, as it were, each of his groups together." He further explains that such names, whether of species, genera, or orders, are what logicians call connotative; they *denote* the members of each group, and *connote* the distinctive characters by which it is defined. A species, then, connotes the common characters of the individuals belonging to it; a genus, those of the species; an order, those of the genera.

But these are the logical principles, which are applicable to names generally. A name such as *Ranunculus repens* does not differ in any particular from a name such as John Smith, except that one denotes a species, the other an individual.

This being the case, and technical names being a necessity, they continually pass into general use in connection with horticulture, commerce, medicine, and the arts. It seems obvious that, if science is to keep in touch with human affairs, stability in nomenclature is a thing not merely to aim at, but to respect. Changes become necessary, but should never be insisted upon without grave and solid reason. In some cases they are inevitable unless the taxonomic side of botany is to remain at a standstill. From time to time the revision of a large group has to be undertaken from a uniform and comparative point of view. It then often occurs that new genera are seen to have been too hastily founded on insufficient grounds, and must therefore be merged in others. This may involve the creation of a large number of new names, the old ones becoming henceforth a burden to literature as synonyms. It is usual in such cases to retain the specific portion of the original name, if possible. If it is, however, already preoccupied in the genus to which the transference is made, a new one must be devised. Many modern systematists have, however, set up the doctrine that a specific epithet once given is indelible, and whatever the taxonomic wanderings of the organism to which it was once assigned, it must always accompany it. This, however, would not have met with much sympathy from Linnæus, who attached no importance to the specific epithet at all: "Nomen specificum sine generico est quasi pistillum sine campana" ("Phil.," 219). Linnæus always had a solid reason for everything he did or said, and it is worth while considering in this case what it was.

Before his time the practice of associating plants in genera had made some progress in the hands of Tourneford and others, but specific names were still cumbrous and practically unusable. Genera were often distinguished by a single word; and it was the great reform accomplished by Linnæus to adopt the binominal principle for species. But there is this difference. Generic names are unique, and must not be applied to more than one distinct group. Specific names might have been constituted on the same basis; the specific name in that case would then have never been used to designate more than one plant, and would have been sufficient to indicate it. We should have lost, it is true, the useful information which we get from our present practice in learning the genus to which the species belongs; but theoretically a nomenclature could have been established on the one-name principle. The thing, however, is impossible now even if it were desirable. A specific epithet like *vulgaris* may belong to hundreds of different species belonging to as many different genera, and taken alone is meaningless. A Linnean name, then, though it consists of two parts, must be treated as a whole. "Nomen omne plantarum constabit nomine generico et especifico" ("Phil," 212). A fragment can have no vitality of its own. Consequently, if superseded, it may be replaced by another which may be perfectly independent.[1]

It constantly happens that the same species is named and described by more than one writer, or different views are taken of specific differences by various writers; the species of one are therefore "lumped" by another. In such cases, where there is a choice of names, it is customary to select the earliest published. I agree, however, with the late Sereno Watson (NATURE, xlvii. 54) that "there is nothing whatever of an ethical character inherent in a name, through any priority of publication or position, which should render it morally obligatory upon any one to accept one name rather than another." And in point of fact Linnæus and the early systematists attached little importance to priority. The rigid application of the principle involves the assumption that all persons who describe or attempt to describe plants are equally competent to the task. But this is far from being the case that it is sometimes all but impossible even to guess what could possibly have been meant.[1]

In 1872 Sir Joseph Hooker ("Flora of British India," i. vii.) wrote: "The number of species described by authors who cannot determine their affinities increases annually, and I regard the naturalist who puts a described plant into its proper position in regard to its allies as rendering a greater service to science than its describer when he either puts it into a wrong place or throws it into any of those chaotic heaps, miscalled genera, with which systematic works still abound." This has always seemed to me not merely sound sense, but a scientific way of treating the matter. What we want in nomenclature is the maximum amount of stability and the minimum amount of change compatible with progress in perfecting our taxonomic system. Nomenclature is a means, not an end. There are perhaps 150,000 species of flowering plants in existence. What we want to do is to push on the task of getting them named and described in an intelligible manner, and their affinities determined as correctly as possible. We shall then have material for dealing with the larger problems which the vegetation of our globe will present when treated as a whole. To me the botanists who waste their time over priority are like boys who, when sent on an errand, spend their time in playing by the roadside. By such men even Linnæus is not to be allowed to decide his own names. To one of the most splendid ornaments of our gardens he gave the name of *Magnolia grandiflora*: this is now to be known as *Magnolia fœtida*. The reformer himself is constrained to admit, "The change is a most unfortunate one in every way" ("Garden and Forest," ii. 615). It is difficult to see what is gained by making it, except to render systematic botany ridiculous. The genus *Aspidium*, known to every fern cultivator, was founded by Swartz. It now contains some 400 species, of which the vast majority were, of course, unknown to him at the time; yet the names of all these are to be changed because Adamson founded a genus, *Dryopteris*, which seems to be the same thing as *Aspidium*. What, it may be asked, is gained by the change? To science it is certainly nothing. On the other hand, we lumber our books with a mass of synonyms, and perplex every one who takes an interest in ferns. It appears that the name of the well-known Australian genus *Banksia* really belongs to *Pimelea*; the species are therefore to be renamed, and *Banksia* is to be rechristened *Sirmuellera*, after Sir Ferdinand von Mueller; a proposal which, I need hardly say, did not emanate from an Englishman.

I will not multiply instances. But the worst of it is that those who have carefully studied the subject know that, from various causes which I cannot afford the time to discuss, when once it is attempted to disturb accepted nomenclature it is almost impossible to reach finality. Many genera only exist by virtue of their redefinition in modern times; in the form in which they were originally promulgated they have hardly any intelligible meaning at all.

It can hardly be doubted that one cause of the want of attention which systematic botany now receives is the repulsive labour of the bibliographical work with which it has been overlaid. What an enormous bulk nomenclature has already attained may be judged from the "Index Kewensis," which was prepared at Kew, and which we owe to the munificence of Mr. Darwin. In his own studies he constantly came on the track of names which he was unable to run down to their source. This the "Index" enables to be done. It is based, in fact, on a manuscript index which we compiled for our own use at Kew. But it is a mistake to suppose that it is anything more than the name signifies, or that it expresses any opinion as to the validity of the names themselves. That those who use the book must judge of for themselves. We have indexed existing names, but we have not added to the burden by making any new ones for species already described.

What synonymy has now come to may be judged by an example supplied me by my friend Mr. C. B. Clarke. For a single species of *Fimbristylis* he finds 135 published names under six

[1] As Alphonse de Candolle points out in a letter published in the *Bull. de la Soc. bot. de France* (xxxix.), "the real merit of Linnæus has been to combine, for all plants, the generic name with the specific epithet." It is important to remember that in a logical sense the "name" of a species consists, as Linnæus himself insisted, in the combination, not in the specific epithet, which is a mere fragment of the name, and meaningless when taken by itself.

[1] Darwin, who always seems to me, almost instinctively, to take the right view in matters relating to natural history, is ("Life," vol. i. p. 364) dead against the new "practice of naturalists appending for perpetuity the name of the *first* describer to species." He is equally against the priority craze:—"I cannot yet bring myself to reject very *well-known* names" (*ibid.*, p. 369).

genera. If we go on in this way we shall have to invent a new Linnæus, wipe out the past, and begin all over again.

Although I have brought the matter before the Section it is not one in which this, or indeed any collective assembly of botanists, can do very much. While I hope I shall carry your assent with the general principles I have laid down, it must be admitted that the technical details can only be appreciated by experienced specialists. All that can be hoped is a general agreement amongst the staffs of the principal institutions in different countries where systematic botany is worked at; the free-lances must be left to do as they like.

PUBLICATIONS.

I have dwelt at such length on certain aspects of my subject that perhaps, without great injustice, you may retort on me the complaint of one-sidedness But when I survey the larger field of botany in this country, the prospect seems to me so vast that I should despair even if I had my whole address at my disposal of doing it justice. I think that its extent is measured by the way in which the publications belonging to our subject are maintained. First of all we have access to the Royal Society, a privilege of which I hope we shall always continue to take advantage for communications which either treat of fundamental subjects, or at least are of general interest to biologists. Next to this we have our ancient Linnean Society, with a branch of its publications handsomely and efficiently devoted to systematic work. Then we have the *Annals of Botany*, which has now, I think, established its position, and which brings together the chief morphological and physiological work accomplished in the country. Lastly, we have the *Journal of Botany*, a less ambitious but useful periodical, which is mainly devoted to the labours of English botanists. I remember there was a time when I thought that this, at any rate, was an exhausted field. But it is not so; knowledge in its most limited aspects is inexhaustible if the labourer have the necessary insight. The discoveries of Mr. Arthur Bennett amongst the potamogetons of the Eastern Counties is a striking and brilliant instance.

Besides the publication of the *Annals* we owe to the Oxford Press a splendid series of the best foreign text-books issued in our own language. If the thought has sometimes occurred to one's mind that we were borrowers too freely from our indefatigable neighbours, I, at least, remember that the late Prof. Eichler paid us the compliment of saying that he preferred to read one of these monumental books in the English translation rather than in the original. I believe it is no secret that botany owes the aid that Oxford has rendered it in these and other matters in great measure to my old friend the Master of Pembroke College, than whom I believe science has no more devoted supporter.

PALÆOBOTANY.

I have said much of recent botany; I must not pass over that of past ages. Two notable workers in this field have passed away since our last meeting. Saporta was with us at Manchester, and we shall not readily forget his personal charm. If some of his work has about it a too imaginative character, the patience and entire sincerity with which he traced the origin of the existing forms of vegetation in Southern Europe to their ancestors in the not distant geological past will always deserve attentive study. But in the venerable, yet always useful, Williamson we lose a figure whose memory we shall long preserve. With rare instinct he accumulated a wealth of material illustrative of the vegetation of the Carboniferous epoch, which, I suppose, is unique in the world. And this was prepared for examination with incomparable patience either by his own hands or under his own eyes. He illustrated it with absolute fidelity. And if he did not in describing it always use language with which we could agree, nothing could ruffle either his imperturbable good nature or the noble simplicity of his character. Truth to tell, we were often in friendly warfare with him. But I rejoice to think that before his peaceful end came he had patiently reconsidered and abandoned all that we regarded as his heresies, but which were, in truth, only the old manner of looking at things. And I think that if anything could have contributed to make his departure happy, it was the conviction that the completion of his work and his scientific reputation would remain perfectly secure in the hands of Dr. Scott.

VEGETABLE PHYSIOLOGY.

Turning again to the present, the difficulty is to limit the choice of topics on which I would willingly dwell. In an address which I delivered at the Bath meeting in 1888, I ventured to point out the important part which the action of enzymes would be found to play in plant metabolism. My expectations have been more than realised by the admirable work of Prof. Green on the one hand, and of Mr. Horace Brown on the other. The wildest imagination could not have foreseen the developments which in the hands of animal physiologists would spring from the study of the fermentative changes produced by yeast and bacteria. These, it seems to me, bid fair to revolutionise our whole conceptions of disease. The reciprocal action of ferments, developed in so admirable a manner by Marshall Ward in the case of the ginger-beer plant, is destined, I am convinced, to an expansion scarcely less important.

But, perhaps, the most noteworthy feature in recent work is the disposition to reopen in every direction fundamental questions. And here, I think, we may take a useful lesson from the practice of the older Sections, and adopt the plan of entrusting the investigation of special problems to small committees, or to individuals who are willing to undertake the labour of reporting upon special questions which they have made peculiarly their own. These reports would be printed *in extenso*, and are capable of rendering invaluable service by making accessible acquired knowledge which could not be got at in any other way.

We owe to Mr. Blackman a masterly demonstration of the fact, long believed, but never, perhaps, properly proved, that the surface of plants is ordinarily impermeable to gases. Mr. Dixon has brought forward some new views about water-movement in plants, which I confess I found less instructive than many of my brother botanists. They are expressed in language of extreme technicality; but, as far as I understand them, they amount to this. The water moving in the plant is contained in capillary channels; as it evaporates at the surface of the leaves a tensile strain is set up, as long as the columns are not broken, to restore the original level. I can understand that in this way the "transpiration current" may be maintained. But what I want to know is how this explains the phenomena in the sugar maple, a single tree of which will yield, I believe, 20–30 gallons of fluid before a single leaf is expanded.

We owe to Messrs. Darwin and Acton the supply of a "Manual of Practical Vegetable Physiology," the want of which has long been keenly felt. Like the father of one of the authors, "I love to exalt plants" (i. 98). I have long been satisfied that the facts of vegetable physiology are capable of being widely taught, and are not less significant and infinitely more convenient than most of those which can be easily demonstrated on the animal side. How little any accurate knowledge of the subject has extended was conspicuously demonstrated in a recent discussion at the Royal Society, when two of our foremost chemists roundly denied the existence of a function of respiration in plants, because it was unknown to Liebig!

ASSIMILATION.

The greatest and most fundamental problem of all is that of assimilation. The very existence of life upon the earth ultimately depends upon it. The veil is slowly, but I think surely, being lifted from its secrets. We now know that starch, if its first visible product, is not its first result. We are pretty well agreed that this is what I have called a "proto-carbohydrate." How is the synthesis of this effected? Mr. Acton, whose untimely end we cannot but deeply deplore, made some remarkable researches, which were communicated to the Royal Society in 1889, on the extent to which plants could take advantage of organic compounds made, so to speak, ready to their hand. Loew, in a remarkable paper, which will perhaps attract less attention than it deserves from being published in Japan (*Bull. College of Agric. Imp. Univ. Tokio*, vol. i.), has from the study of the nutrition of bacteria, arrived at some general conclusions in the same direction. Bokorny appears recently to have similarly experimented on algæ. Neither writer, however, seems to have been acquainted with Acton's work. The general conclusion which I draw from Loew is to strengthen the belief that form-aldehyde is actually one of the first steps of organic synthesis, as long ago suggested by Adolph Baeyer. Plants, then, will avail themselves of ready-made organic compounds which will yield them this body. That a sugar can be constructed from it has long been known, and Bokorny has shown that this can be utilised by plants in the production of starch.

The precise mode of the formation of form-aldehyde in the process of assimilation is a matter of dispute. But it is quite clear that either the carbon dioxide or the water, which are the materials from which it is formed, must suffer dissociation. And this requires a supply of energy to accomplish it. Warington has drawn attention to the striking fact that in the case of the nitrifying bacterium, assimilation may go on without the intervention of chlorophyll, the energy being supplied by the oxidation of ammonia. This brings us down to the fact, which has long been suspected, that protoplasm is at the bottom of the whole business, and that chlorophyll only plays some subsidiary and indirect part, perhaps, as Adolph Baeyer long ago suggested, of temporarily fixing carbon oxide like hæmoglobin, and so facilitating the dissociation.

Chlorophyll itself is still the subject of the careful study by Dr. Schunck, originally commenced by him some years ago at Kew. This will, I hope, give us eventually an accurate insight into the chemical constitution of this important substance.

The steps in plant metabolism which follow the synthesis of the proto-carbohydrate are still obscure. Brown and Morris have arrived at the unexpected conclusion that "cane-sugar is the first sugar to be synthesised by the assimilatory processes." I made some remarks upon this at the time (*Journ. Chem. Soc.*, 1893, 673), which I may be permitted to reproduce here.

"The point of view arrived at by botanists was briefly stated by Sachs in the case of the sugar-beet, starch in the leaf, glucose in the petiole, cane-sugar in the root. The facts in the sugar-cane seem to be strictly comparable (*Kew Bulletin*, 1891, 35-41). Cane-sugar the botanist looks on, therefore, as a 'reserve material.' We may call 'glucose' the sugar 'currency' of the plant, cane-sugar its 'banking reserve.'

"The immediate result of the diastatic transformation of starch is not glucose, but maltose. But Mr. Horace Brown has shown in his remarkable experiments on feeding barley embryos that, while they can readily convert maltose into cane-sugar, they altogether fail to do this with glucose. We may conclude, therefore, that glucose is, from the point of view of vegetable nutrition, a somewhat inert body. On the other hand, evidence is apparently wanting that maltose plays the part in vegetable metabolism that might be expected of it. Its conversion into glucose may be perhaps accounted for by the constant presence in plant tissues of vegetable acids. But, so far, the change would seem to be positively disadvantageous. Perhaps glucose, in the botanical sense, will prove to have a not very exact chemical connotation.

"That the connection between cane-sugar and starch is intimate is a conclusion to which both the chemical and the botanical evidence seems to point. And on botanical grounds this would seem to be equally true of its connection with cellulose.

"It must be confessed that the conclusion that 'cane-sugar' is the first sugar to be synthesised by the assimilatory processes seems hard to reconcile with its probable high chemical complexity, and with the fact that, botanically, it seems to stand at the end and not at the beginning of the series of metabolic change."

PROTOPLASMIC CHEMISTRY.

The synthesis of proteids is the problem which is second only in importance to that of carbohydrates. Loew's views of this deserve attentive study. Asparagin, as has long been suspected, plays an important part. It has, he says, two sources in the plant. "It may either be formed directly from glucose, ammonia (or nitrates) and sulphates, or it may be a transitory product between protein-decomposition and reconstruction from the fragments" (*loc. cit.*, 64).

In the remarks I made to the Chemical Society I ventured to express my conviction that the chemical processes which took place under the influence of protoplasm were probably of a different kind from those with which the chemist is ordinarily occupied. The plant produces a profusion of substances, apparently with great facility, which the chemist can only build up in the most circuitous way. As Victor Meyer (*Pharm. Journ.*, 1890, 773) has remarked: "In order to isolate an organic substance we are generally confined to the purely accidental properties of crystallisation and volatilisation." In other words, the chemist only deals with bodies of great molecular stability; while it cannot be doubted that those which play a part in the processes of life are the very opposite in every respect. I am convinced that if the chemist is to help in the field of protoplasmic activity he will have to transcend his present limitations, and be prepared to admit that as there may be more than one algebra, there may be more than one chemistry. I am glad to see that a somewhat similar idea has been suggested by other fields of inquiry. Prof. Meldola (NATURE, xlii. 250) thinks that the investigation of photochemical processes "may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree." I am delighted to see that the ideas which were floating, I confess, in a very nebulous form in my brain are being clothed with greater precision by Loew.

In the paper which I have already quoted, he says of proteids (*loc. cit.*, 13): "They are *exceedingly labile compounds* that can be easily converted into relatively stable ones. A great lability is the indispensable and necessary foundation for the production of the various actions of the living protoplasm, for the mode of motions that move the life-machinery. There is a *source of motion* in the labile position of atoms in molecules, a source that has hitherto not been taken into consideration either by chemists or by physicists."

But I must say no more. The problems to which I might invite attention on an occasion like this are endless. I have not even attempted to do justice to the work that has been accomplished amongst ourselves, full of interest and novelty as it is. But I will venture to say this, that if capacity and earnestness afford an augury of success, the prospects of the future of our Section possess every element of promise.

PHYSICS AT THE BRITISH ASSOCIATION.

THE proceedings of this Section were commenced by the delivery of the presidential address by Prof. W. M. Hicks. In seconding the vote of thanks to the President, Prof. Fitzgerald referred to the possible change of mass with temperature, suggested in the address, and pointed out that such a phenomenon would show itself by a deviation of planetary motions from strict conformity to Kepler's laws, owing to their change of mass on cooling.

Sir Douglas Galton exhibited plans of the German Reichsanstalt, and of the new buildings in course of construction, and gave a more detailed account of the management of this institution than is contained in his presidential address to the Association. His object in reading the paper was to revive a movement set on foot at a previous meeting by Prof. Oliver Lodge. The Committee appointed at that time to consider the question of a National Physical Laboratory for the United Kingdom made but little progress, possibly because they did not propose to develop any existing institution. He suggested that the scope of the Kew Observatory should be extended so as to include research, and that it be made the starting-point for the national laboratory.

A discussion followed, in which several members took part. Prof. Rücker lamented the want of concentration and organisation in research work, and thought a national laboratory might remedy this. He regretted that the day was passing away when a man could undertake both teaching and research, because, in his opinion, teachers should not give up research. Prof. Oliver Lodge drew attention to the enormous advantages possessed by a national institution, for carrying on researches extending over a long period. In a university laboratory such research would possibly be discontinued with a change of professor. The universities would still do pioneer work, discovering new fields of research and obtaining preliminary results. Prof. Fitzgerald, on the other hand, did not think it advisable to hand over research to a national laboratory, whereas he strongly advocated an extension of the standardising work performed at Kew. He believed that the highest kind of instruction was training in research work, and it was the function of the universities to give this instruction. Instead of that, the professors are called upon to cram old knowledge into immature and stupid students. The Section has appointed a Committee to reconsider the question of a national laboratory.

Prof. Henrici read a paper on the teaching of geometrical drawing in schools, which was, he said, as a rule very bad. He pointed out that Euclid's constructions are generally followed, the use of the set-square being discarded and only straight-edges and compasses used. He urged the desirability of discarding Euclid in the teaching of geometrical drawing, advocating the use of the set-square from the very commencement. The examples ought to be so arranged that a student can verify his constructions for himself; he therefore suggested the appointment of a Committee to report on the whole question and issue a syllabus of examples. This suggestion was adopted by the Section.

The range of subjects included in the work of the Section was perhaps nowhere better exemplified than in the passage to the next paper, a report on cosmic dust, by Dr. J. Murray. An examination of the red clay from the bottom of the Pacific Ocean, in places 1000 miles from any coast, enables three classes of magnetic particles to be distinguished; these are—crystalline fragments of magnetic or titanic iron, dark shiny spherules containing metallic iron, and the brownish spherules known as chondres. The various layers of manganese nodules found surrounding nuclei of tertiary teeth or bones contain these black and brown spherules, and there is every indication that the brown ones are of extra-terrestrial origin. In this case they ought to occur at all, or at any rate many, points on the earth's surface; Dr. Murray has, however, looked for them in vain both in the dust of Greenland glaciers and on the summit of Ben Nevis. He is of opinion that the accumulation of meteoric dust takes place with exceeding slowness, say about 20 lbs. of dust per square mile per century, and that the bed of the Pacific Ocean has not received one foot of deposit since the tertiary period. Consequently any attempt to gather these particles will probably be fruitless, unless continued over a long period. He wished for suggestions as to the best method of procedure in the future. It was pointed out that a good opportunity for the collection of meteorites will be afforded by the meteor shower of November 1899.

The Committee on underground temperature have been fortunate this year in obtaining records from a bore-hole in New South Wales, the first observations made in the southern hemisphere. The bore-hole is situated near Port Jackson, close to Sydney Harbour; it is 2929 feet deep, and contains water. The gradient observed was a small one, being a rise of 1° F. in descending 80 feet vertically. The observers suspected that the temperature of the rock was influenced by the proximity of the water in the harbour, but an examination of the temperature distribution in the harbour did not confirm this. Lord Kelvin suggested the African mines as a new field for observations.

Prof. S. P. Thompson reported the recommendations of the Committee on the size of pages of scientific periodicals. It is considered advisable to retain quarto and octavo sizes, and certain limits for text and margin are given for each of these sizes. There appeared to be a strong feeling against any change in the sizes of the Royal Society's publications. During the year the Committee will endeavour to induce other scientific societies to adopt the standard sizes recommended.

Prof. Rücker communicated the results of a comparison of magnetic standard instruments, made by himself and Mr. W. Watson. In his presidential address to the Section last year he showed that it was useless to proceed further with a magnetic survey until a direct comparison of standards used in the various observatories had been made, because it was well known that instruments differed greatly. During the year he has visited the various magnetic observatories, carrying a portable declinometer of the Kew pattern, and with Mr. Watson's assistance has directly compared the simultaneous readings of his declinometer and that of the observatory. Errors are found in the latter, which are in every case traceable to magnetic material in or on the wooden box containing the suspended magnet. If this box be replaced by an ebonite one, the error disappears. It is, however, easier to allow for the error than to get rid of it; its amount is perfectly definite.

On Friday the Section sat jointly with Section B. Lord Rayleigh read a paper on the refractivity and viscosity of these gases. He described how, by means of an electric arc, kept up for several weeks in a mixture of oxygen and atmospheric nitrogen, he finally obtained more than a litre of argon at atmospheric pressure. This proved to have the same density as the specimen obtained by the magnesium method. The refractive index was measured by the interference method of Fizeau, the two beams being separated by slits in front of the lens nearest the eyepiece. The latter was constructed of cylindrical lenses. To avoid the use of cross-wires, the tubes containing the gases under comparison were arranged so as not to occupy the whole field of view, some light passing parallel to, and outside them; two sets of fringes were thus obtained, which could be brought to coincidence by varying the pressure of either gas. Adjustments were made for several pressures, one of the tubes always containing air. The values of the refractivity ($\mu - 1$) were, for argon 0·961, and for helium 0·146, that of air being taken as unity. The viscosity of each gas was measured by its rate of flow through a capillary tube, the results being (air = 1) argon 1·21, helium 0·96. Lord Rayleigh mentioned that a sample of nitrogen collected from a Bath spring, where it bubbles out along with the water, gave the D_3 line of helium. Dr. Gladstone showed that the results of these experiments assign to argon the atomic weight 20, its specific refractive energy being intermediate between those of fluorine and sodium, but not between those of potassium and calcium.

Prof. Schuster then opened a discussion on the evidence to be gathered as to the simple or compound character of a gas from the constitution of its spectrum. Recent spectroscopic work in connection with argon and cleveite gas has directed attention to the double spectra exhibited by these substances, and conjectures have been made that the two spectra indicate the gases to be mixtures. Prof. Schuster expressed strongly the view that gases with double spectra are not necessarily mixtures or compounds. He quoted in support of this the cases of sodium and mercury vapours, and oxygen, in all of which the absorption spectrum differs from that of the luminous vapour. The difficulty is not explained by assuming dissociation to occur, because some substances have three or more spectra. He thought mere examination of spectra would not suffice to determine whether an unknown substance is an element, mixture of elements, or compound.

The despondent view of Prof. Schuster was not shared by Prof. Runge, of Hanover, who at this point contributed an account of the researches of himself and Prof. Paschen on the spectrum of cleveite gas, showing that it is a mixture. (An account of this work by the authors themselves will be found on p. 520.)

Dr. G. J. Stoney contributed to the discussion by a paper on the interpretation of spectra.

On Saturday the Section was subdivided into two departments, mathematics and meteorology.

In the department of mathematics, Lord Kelvin read a paper on the translational and vibrational energies of vibrators after impacts on fixed walls, in which he sought to find an exception to the Maxwell-Boltzmann theorem relating to the average translational energy of the molecules of a gas. He calculated the time-average of the translational energy of a free particle after coming into contact with a vibrating particle, and found it always in excess of that which would be given by the Maxwell-Boltzmann law, though approximating more nearly to that average when the number of encounters was considerable; and that it seemed ultimately to give a total average out of accordance with the law. In the discussion which followed, Mr. G. H. Bryan pointed out that the Maxwell-Boltzmann law referred to the statistical average energy of a great number of particles, not to the time-average energy of a single particle.

Prof. Hicks, in his paper on a spherical vortex, stated that he had proved the possibility of building up a compound spherical vortex consisting of successive shells in which the rotation is oppositely directed, the vorticity and size of each shell satisfying a definite relation. In a paper on bicyclic vortex aggregates, he stated that it was possible, with given current and vortex-sheets, to have a steady *spiral* motion round an axis, compounded of motion in planes through the axis and motion in circles round the axis, the cyclic constants of the two component motions being independent of each other.

Mr. G. T. Walker showed an ingenious top in the shape of a flattened ellipsoid in which rotation could become converted into oscillations, and *vice versâ*, by means of an adjustable piece which could be arranged unsymmetrically.

Dr. Burton made some suggestions as to matter and gravitation in the cellular vortex ether described in Prof. Hicks's presidential address.

Mr. P. H. Cowell read an important paper on recent developments of the lunar theory, chiefly by Dr. G. W. Hill, extended in the current number of the *American Journal* by an admirable paper by Prof. E. W. Brown. The order of work in attacking problems in the lunar theory is quite altered and much simplified in the new method. In a short discussion which followed, Mr. Cowell stated that Prof. Brown was engaged in bringing out a treatise on the lunar theory.

Prof. J. D. Everett read a paper on absolute and relative motion; and Mr. W. H. Everett made a communication on the calculation of the magnetic field due to a current in a solenoid.

In pure mathematics, Major MacMahon gave an interesting method of graphically representing partitions of numbers.

Colonel Cunningham read a paper on Mersenne's numbers, which are numbers of the form $2^q - 1$, where q is a prime, and which were first discussed by Mersenne about the year 1664. Colonel Cunningham also described a book of tables which he proposed to calculate, giving the solution of the congruence $2^x \equiv R$ (mod. p) for all moduli (p) which are primes, or powers of primes, up to 1000. There are to be two tables for each modulus, one giving the values of R for a series of values of x; and the other giving the smallest values of x for a series of values of R. He described some of the uses of such a table, and stated that the plan on which it would be drawn up would be precisely like a somewhat similar table by Jacobi, described in Prof. Cayley's report on mathematical tables in the British Association Report of 1876.

Prof. Alfred Lodge drew the attention of the Section to a multiplication table up to 1000 × 1000, drawn up by Mr. M. B. Cotsworth, of Holdgate, York, which was exhibited; it is similar to Crelle's table of the same extent, though in some respects more convenient.

Prof. M. J. M. Hill described two species of tetrahedron, the volume of any member of which can be determined without using the proposition that tetrahedrons on equal bases, and having equal altitudes, are equal.

In the department of meteorology, Mr. Eric S. Bruce put forward a new theory of lightning flashes, based on the principle of the pin-hole camera. The light from a concealed flash might, he supposed, pass through a small aperture in the concealing cloud and fall on another cloud, forming an inverted image of the flash. If there were several apertures we should have as many images. They would be faint, possibly too faint to affect a photographic plate. Moreover, if the receiving cloud were of irregular shape, an originally straight flash would appear distorted into a zig-zag line on the cloud. Mr. Symons thought a brighter patch of light ought to occur at the angles of the image thus distorted, and he scarcely thought the conditions imagined by Mr. Bruce corresponded with those of nature.

The report of the Committee on earth tremors was presented by Mr. Symons, who, in referring to the delicacy of the instruments used in their observations, said that an angle equal to that subtended by a chord 1 inch long at the centre of a circle 1000 miles in radius could be detected. Since last report two bifilar pendulums have been purchased, of the kind described in NATURE, vol. l. pp. 246–249 (1894); each possesses its own photographic recording apparatus. One of these has been recently erected in the cellar of Mr. Davison's house in Birmingham; the other should have been placed in a house three-quarters of a mile to the east, but this was found impracticable. It will be placed somewhere in the neighbourhood, and comparisons of the records of the instruments will be made during the year, after which the second one will be available for another station. An appendix to the report by Mr. Davison gives the bibliography and classification of horizontal pendulums.

Prof. John Milne gave an account of the long report of the Committee on seismological phenomena in Japan. This commences by a reference to the great loss caused by the recent fire at Prof. Milne's house and observatory, after which follows a description of the records of the Gray-Milne seismograph. Attached to the report is a catalogue of 8331 earthquake shocks recorded in Japan between 1885 and 1892, giving full particulars of the centre and area of disturbance. It enables the approximate *weight* of each to be found, and permits the division of Japan into fifteen distinct seismic districts. The next section of the report deals with the rate of propagation of earthquake disturbances from Japan to Europe. The small tremors which occur in the ten seconds or so before an earthquake shock are transmitted to Europe, but they are spread over half an hour; it appears, therefore, that the preliminary tremors either travel more quickly, or reach Europe by a shorter route than the main shock. The latter is known to travel along the surface at about 3000 metres per second. Do the tremors travel at 8000 to 11,000 metres per second, or do they pass through the earth, not round it? If the latter, we may hope for some further knowledge concerning the interior of the globe. Prof. Milne has set up horizontal pendulums in nearly a score of places, and finds great differences in their behaviour. They all exhibit a general displacement, *i.e.* tilt, in the same direction, and similar long-period movements. Examined from hour to hour, however, some of them show the existence of a diurnal wave. After a long and very laborious search, graphically described to the Section by Prof. Milne, he succeeded in tracing this diurnal effect to the local removal of load from the alluvium by greater evaporation from exposed areas. At night the movement is slight, and is probably accounted for by the condensation, at the cold surface, of aqueous vapour after rising through the warm earth. Some observations have been made on the disturbance of the pendulums by earth tremors. Their cause has not been ascertained, but they always occur with greatest intensity between 5 and 9 a.m. They are most marked with a steep barometer gradient and consequent wind, local or distant.

As Prof. Milne has now returned from Japan, and the earthquake catalogue is completed, the Committees on earth tremors and seismological phenomena have united under the latter name. The new Committee is a large one, and with Prof. Milne and Mr. Davison as joint secretaries, it ought to do good work.

A new theory of thunderstorms was advanced by Prof. Michie Smith in his paper on Indian thunderstorms. His observations, made at Madras, showed that sheet-lightning occurs there every evening during several months of the year, always in the south-west and near the horizon. Lightning phenomena in the morning occur, on the other hand, in the north-east. The phenomena consist of actual discharges between two clouds, or two portions of the same cloud, and are not reflections of distant lightning; they take place in the upper portions of low-lying cumulus clouds. Prof. Smith attributes them to the clouds formed in the regions of still air at the meeting of the land and sea breezes, and has observed in these regions the simultaneous rise of two close parallel clouds from the edge of the cumulus; such clouds are scarcely distinguishable except with oblique illumination, and it is within, or between, them that the discharges occur. The time of their formation depends on the hour at which the sea breeze sets in, being roughly three hours later. The land breeze being dry and dusty is negatively charged, while the sea breeze is known to carry a strong positive charge; equalisation of the electrical states of the clouds formed out of these will, therefore, give rise to lightning. Prof. Smith referred to the iridescence or nacreous appearance of the edges of the clouds when rapidly sinking, and considered this effect to be due to the dust left behind by them.

This paper gave rise to an interesting discussion, chiefly with reference to the origin of dust in clouds, and the source of their electricity. Mr. John Aitken pointed out that thunderstorms are most probably the effect, not the cause, of purifying the air. He gave instances of thunderstorms on several successive days, all of which left the air dusty and impure; eventually the air cleared, and no more thunder occurred. Prof. Schuster alluded to the fact that twenty-five theories of thunderstorms had been put forward in a dozen years, and in a single year five appeared. He attributed the positive charge of the sea breeze to the electrification of the air by the spray from the breaking waves; Lenard has shown that the spray of pure water gives a negative charge to the air, while that of salt water communicates a positive charge. He believed the dust of clouds to be acquired locally, except that at high altitudes, which we know to be carried long distances. A proof of this is to be found in the Himalayas where certain valleys are dusty and others fairly free from dust, although all receive the wind from the Indian plains. His observations of nacreous clouds in England had led him to connect them rather with the ice particles of cirrus clouds than with dust. To this latter point Prof. Michie Smith replied that the nacreous appearance fits the edge of the cumulus so closely that he believes the two to be connected.

The Committee on the application of photography to meteorology are proceeding with the photography of clouds near the sun by means of two cameras at a fixed distance apart, and exposed simultaneously by an electrical arrangement. In this way they hope to obtain absolute measurements of cloud altitudes. For purposes of measurement the sun's image appears in all the photographs. A photograph of the rainbow, by Mr. Andrews of Coventry, is the first of its kind received by the Committee. It shows the secondary bow, and the greater brightness of the region within the bow.

During a recent visit to the Engadine, Prof. Schuster has made observations on the atmospheric electricity near the ground at different heights above sea-level. The readings were taken with Lord Kelvin's portable electrometer, which worked very satisfactorily and seems well adapted for such purposes. In all cases positive charges were found, increasing with height but in an apparently erratic fashion. The normal positive charge at the foot of a glacier was found to be strengthened by a wind blowing down it, and Lenard's observations on the negative electricity of waterfalls were all confirmed. The daily curve of atmospheric potential in the valley of Pontresina shows a maximum at 11 a.m., dipping a little and rising again to an afternoon maximum at 5 p.m., then rapidly descending as the evening breeze sets in. Discussion on the paper related chiefly to the behaviour and temperature errors of portable electrometers, the latter being somewhat large and quite unexplained. Prof. Ayrton suggested a crucial experiment to determine whether atmospheric electricity is due to an actual distribution in the air, or to induction from the earth's surface.

The report of the Ben Nevis Observatory for 1894 was presented. The mean hourly velocity of the wind at the top of the mountain, and the mean rainband, are included in the report for the first time. Dr. Buchan and Mr. Omond have made progress in collating the simultaneous records made at Fort William and the summit; the differences between them are to be examined especially with respect to their bearing on coming storms. Even at this stage the results indicate that the present theory of cyclones requires great modification.

The first part of Monday's sitting was devoted to a discussion on the nature of combination tones. Prof. Rücker gave an admirable account of the history of the subject, pointing out that Helmholtz originated both the theory that they are objective, and that which supposes them subjective. He reviewed the theories of Prior and others, according to which summation and difference tones are explained as beat tones of various kinds; and he called attention to Helmholtz's proof that an asymmetrical elastic body, such as the disc of a microphone or the drumskin of the ear, would resound to the difference tone between two notes. Prof. S. P. Thompson regretted that in his historical survey Prof. Rücker did not refer to his own work. He read communications from König and Hermann, defining their views. König distinguishes between beat tones, which can be resonated, and difference tones, to which the resonator does not respond; the latter are subjective. Hermann objects to Helmholtz's theory that it is inadequate to account for the loudness of the combinational tones. Prof. Thompson mentioned experiments to show that difference tones may be obtained by sending one sound to each ear, and in other cases where the drumskin does not receive the sounds. He described also the effect of periodically intermitting a single tone, or of suddenly and periodically changing its phase, in both of which cases a tone is heard the pitch of which is the frequency of phase-change or intermittence.

Prof. Everett sought for the cause of combination tones in the air itself, which would be disturbed unsymmetrically by two sounds of finite amplitude. He thought, however, that in the combined effect of two tones, the vibration corresponding to the fundamental Fourier term common to each would be louder than the difference tone, a view in which Lord Kelvin concurred. Dr. Burton pointed out that Prof. Everett's explanation of combination tones would apply also to phase tones and intermittence tones. Dr. G. J. Stoney thought resonance by the mouth-cavity was an important factor in hearing, and in the selection of separate sounds from among a number. There was a general agreement that summation tones have never been heard, and probably do not exist.

Mr. E. H. Griffiths opened a discussion on the desirability of a new Practical Heat Standard. He showed that the use of water as the standard substance in heat measurements had led to great confusion, on account of the various assumptions as to its variation of heat-capacity with temperature. The curves of heat-capacity of water and temperature, used by different experimenters, were exhibited; according to which the author's results furnished a value about the mean of those of recent observers. Mr. Griffiths suggests as a heat unit, absolute, independent of any one person's results, and convenient in magnitude, the heat energy of 42 million ergs. To interpret it as a water standard he proposes to take it as the thermal capacity of a gramme of water at 10° C., as measured by the hydrogen thermometer; and he gives a formula to find the heat-capacity at other temperatures than 10° C. Lord Kelvin said that Prof. Rankine had previously suggested the dynamical specific heat of water as a standard. Mr. W. N. Shaw thought it advisable to make a distinction between the numbers for the absolute thermal capacity and the specific heat of a substance. He believed this would be done most simply by taking the thermal unit as the heat energy of a million ergs; the specific heat of water at 10° C. would then be unity, and its thermal capacity 42 units. The choice of a thermal unit has been referred to the Electrical Standards Committee.

Dr. C. H. Lees gave an account of the method and results of experiments on the thermal conductivity of mixtures of liquids. The method used was that of Christiansen, in which the heat is conducted through the liquid enclosed between two copper discs, and confined by an ebonite ring if necessary. The results show that the conductivity of mixtures of two liquids is less than the value calculated by the ordinary law of mixture, at any rate for water, ethyl alcohol, methyl alcohol and glycerine. Dr. Lees undertook the experiments to verify certain relations suggested by Prof. H. F. Weber between molecular weight, density, specific heat and thermal conductivity.

A paper by Prof. Ramsay and Miss Dorothy Marshall was read by the latter, the subject being a method of comparing heats of evaporation of liquids at their boiling-points. After remarking that the data of heats of evaporation are very scanty and discrepant, Miss Marshall described a method by which two liquids, kept at their boiling points by jackets of their vapour surrounding them, are boiled by means of equal bare platinum wires heated by an electric current. A comparison of the amounts of the liquids evaporated in a given time gives the ratio of heats of evaporation. For absolute values a special determination was made on benzene by Mr. Griffiths and Miss Marshall. Alcohol was carefully compared with benzene, and all other liquids were then compared with alcohol. Water was very erratic in its behaviour, probably because of its greater electric conductivity.

Mr. G. U. Yule exhibited a harmonic analyser.

At the meeting on Tuesday, Lord Kelvin described the results of experiments for the electrification and diselectrification of air and other gases, made by Messrs. Maclean and Galt, and himself. In the earlier experiments the air inside a metal can was electrified by points, the can being put to earth; on insulating the can and blowing out the air, the charge acquired by the can was equal and opposite to that of the air. Electrification of air and other gases in gas-holders over water, by points and flames, was also tried, greater electric densities being thus obtained than by the previous method. The maximum effects were $1 \cdot 5 \times 10^{-4}$ electrostatic units per c.c. for air, and $2 \cdot 2 \times 10^{-4}$ for CO_2. The gases were diselectrified by "filtering" them through metal tubes containing conducting wire gauze and cotton wool. Very little electrifying effect was found when uncharged air passed through a platinum tube 100 cm. long and 1 mm. diameter, until the tube was made red-hot, in which case the air acquired a strong positive charge. Prof. Oliver Lodge suggested the use of a filter consisting of a metal tube, highly polished inside and illuminated by an electric beam shining into its interior. Lord Kelvin said that in all Hertz's or Elster and Geitel's experiments on diselectrification by light, the charge of the air round the illuminated body should be examined.

Prof. Rücker made a communication on vertical (earth-air) electric currents. At the meeting of the Association last year, Dr. Adolph Schmidt accounted for a portion of the earth's magnetism by assuming electric currrents to pass vertically between earth and air. Such currents would be shown by the non-vanishing of the line-integral of magnetic force when taken round a closed circuit on the earth's surface. The matter was tested in this way by Messrs. Kay and Whalley, using four independent circuits, three in Great Britain and one in Ireland, and obtaining the data of magnetic force from the surveys of 1886 and 1891. The results do not decide the general question, but they show that in the United Kingdom the upward current has certainly not more than one-tenth of the value required in Dr. Schmidt's theory. Lord Kelvin calculated that the current assumed by Dr. Schmidt (0·1 ampere per square kilometre of surface) amounts to a removal of the fine-weather charge of the air near the earth 36 times per second. Dr. Rijchevorsel said he understood that magnetic observations were about to be made in Switzerland, which would furnish data for similar calculations there.

Mrs. Ayrton made a communication on the connection between potential difference, current and length of arc, in the electric arc. The results of carefully-performed experiments, verified also by recalculation from the data of other observers, show that the following relations hold:—(1) For constant length of arc the power (number of watts used in the arc) is a linear function of the current; (2) for constant currents the power is a linear function of the length of arc; (3) for constant length of arc the curve of potential difference and current is a rectangular hyperbola. All these laws are included in the President's statement that the surface with potential difference, current and arc length as coordinates, is a hyperbolic paraboloid.

Prof. Ayrton read a paper by Mr. Mather and himself, in which arguments were advanced against the existence of a back electromotive force in the electric arc. The authors describe a method of measuring the true resistance of the arc, namely the ratio of a small increase of potential difference to the corresponding increase in the current; this, of course, is a negative quantity. The same authors described a magnetic field-tester, an application of the ordinary exploring coil and ballistic galvanometer method, with a spiral spring to effect rapid rotation of the exploring coil, and a modified D'Arsonval galvanometer with shuttle-wound coil capable of rotating through several turns without losing the proportionality of angular displacement and restoring force.

The velocity of light in vacuum tubes conveying an electric discharge formed the subject of a paper by Messrs. Edser and Starling. Vacuum tubes were placed in the path of the two beams of a Fizeau interference apparatus, and the position of the bands observed. No appreciable shift of the bands was obtained either by setting up an induction-coil discharge, or by the discharge of ten gallon jars through the tubes when placed in series with a piece of wet string. The discharge in the latter case lasted one-thirtieth of a second, and the authors show that a disturbance of the bands of so long duration would have been observed.

Mr. F. G. Baily read a paper on hysteresis of iron in an alternating magnetic field, in which he showed that the hysteresis of iron increases with the field up to a maximum value, in accordance with Ewing's theory. The experiments were made by the isthmus method, using a small laminated armature consisting of thin discs of charcoal iron; the most intense magnetic field used was 22,000 C.G.S. units, and the hysteresis was measured by the rise of temperature of the armature.

On Wednesday, Dr. Gladstone and Mr. W. Hibbert made a communication on the change of molecular refraction in salts or acids dissolved in water. The molecular refraction of a substance is altered when the substance changes its state, and a further slight alteration takes place on diluting its solution; the authors have obtained some evidence of a close connection between these changes and the variations of electric conductivity of the substance and its solutions. Such a connection would have an important bearing on the theory of solution.

The report of the Electrical Standards Committee was read. The Committee hope during the year to institute a comparison between the British and German standards of resistance, and have procured coils for this purpose, which have already been tested at the Reichsanstalt. The Committee, recognising the need for practical units of magnetic field and magnetic potential, recommend for tentative adoption (1) a unit equal to 10^8 C.G.S. lines, to be called a *weber*, (2) the C.G.S. unit of magnetic potential, to be called a *gauss*. They also recommend that the termination *ance* be used in describing the properties of a piece of matter, *e.g.* the resist*ance* of a copper wire, and the termination *ivity* or *ility* for the specific properties of the material, *e.g.* the resist*ivity* of copper would mean the resistance of a centimetre cube of it. Prof. Oliver Lodge explained, and advocated the use of, the proposed units. Prof. S. P. Thompson, while agreeing with the Committee as to the desirability of having units of magnetic field and magnetic potential, thought the choice of their names should be left with the practical men who use them. He believed the proposed *weber* was too large, and advocated the retention of the C.G.S. "line," using the kilo- and mega- for its multiples; further, he did not see any necessity for abandoning the ampere-turn in order to replace it by the *gauss*. Prof. Thompson pointed out a more formidable objection, namely, that the American Institute of Electrical Engineers have attached the name *weber* to a different unit, and have suggested the name *gilbert* for the gauss. Several members continued the discussion, and Prof. Perry expressed his opinion that the question of names ought to be settled by a general congress.

Two pieces of apparatus for tracing the form of the wave of potential in an alternate current circuit were exhibited and described, the one by Messrs. Barr, Burnie and Rodgers, the other by Prof. Ayrton and Mr. Mather.

Mr. E. H. Griffiths exhibited the apparatus designed for the calibration of high-temperature thermometers at Kew Observatory, and described it. A Callendar and Griffiths platinum thermometer is enclosed in a glass or porcelain tube, and can be immersed, along with the thermometer to be calibrated, in a bath of molten metal or sulphur vapour, according to the temperatures required. Its resistance is measured by a Wheatstone bridge, the coils of which are enclosed in a copper box, five sides of which are immersed in a water-bath of constant temperature, while the top is surmounted by a case similar to that of a chemical balance. The coils of the bridge are of platinum-silver, wound double, and are not embedded in paraffin, the object being to allow them to assume the temperature of the box and surrounding water as quickly as possible.

A vote of thanks to the Chairman and Secretaries terminated the proceedings.

CHEMISTRY AT THE BRITISH ASSOCIATION.

WITH the exception of Prof. Runge's announcement of the undoubtedly compound nature of helium, few of the communications laid before Section B at Ipswich are likely to awaken great interest outside chemical circles. The discussions, however, which are now a recognised feature of these meetings, were especially successful, and it is not too much to hope that the joint meeting with the newly-formed Botanical Section may be the means, if only indirectly, of bringing about results of great importance to the agricultural community.

Following the President's valuable address, Sir Henry Roscoe and Dr. A. Harden communicated to the Section an interesting discovery in historical chemistry. It has been generally assumed that Dalton arrived at the idea of atoms with definite weights from a consideration of the proportions in which certain elements combined. From the examination of a number of manuscript volumes of Dalton's own laboratory notes, which they have recently discovered in the library of the Manchester Literary and Philosophical Society, Sir Henry Roscoe and Dr. Harden conclude that Dalton worked out his theory solely from physical considerations as to the constitution of gases. His mind being saturated with Newton's ideas concerning atoms, it was from these that his own atomic theory was developed.

Later on, quoting not only his own results but those of other chemists, he seems to have been led to the law of multiple proportions as the only conceivable mode of combination between atoms. Extracts were given from his notes showing that certain numbers, usually quoted as having led him to his atomic theory, *e.g.* the analyses of marsh gas and olefiant gas, were only inserted in his tables some time after the publication of his ideas.

Prof. Armstrong said it was satisfactory to learn that Dalton had really arrived at his conclusions from truly philosophical considerations, without reference to the very crude numbers, usually quoted as sufficient basis for the laws that he worked out.

The report of the Committee on the teaching of science in elementary schools was read by Dr. J. H. Gladstone. During past years there has been an increase in the number of subjects taught, and in the number of pupils receiving instruction. The alteration in the system of inspection will have an especially useful effect in the teaching of science. The question of the training of teachers is discussed in the report. A course for mistresses on domestic science, dealing as far as possible with the nature of the processes and materials employed in the household, has been found successful. The great obstacles to good science teaching at the present time in elementary schools are: (1) Large classes; (2) multitude of subjects; (3) insufficiency of the training course for teachers in science subjects; (4) effects of the old science and art system, which is clearly far too formal, and pays far too little attention to ordinary requirements.

The courses on elementary physics and chemistry, and the science of common things are found to be more attractive than pure chemistry.

Other subjects dealt with in the report are school visits to museums; the right method of giving object lessons; and the teaching of the metric system. Finally it is suggested as a question worth consideration, whether the recognised school age should not be raised from thirteen to fourteen.

In the discussion which followed the reading of the report, the relation of County Councils to elementary schools was debated, and it was contended that these are helped indirectly by the Councils providing facilities for the training of teachers.

Mr. G. J. Fowler read a paper on the action of nitric oxide on certain salts, by H. A. Auden and G. J. Fowler, in which the action of nitric oxide on different salts at various temperatures is described. Oxy-salts have been chiefly examined, the most interesting results being obtained with the chlorates and iodates of potassium and silver. With potassium chlorate action takes place at the ordinary temperature, chlorine being evolved, but no potassium chlorate being formed. With silver chlorate, chlorine is also evolved, but some chloride is obtained. Potassium iodate yields iodine but no potassium iodide at a low temperature, while silver iodate is completely converted into iodide, no iodine being liberated, or silver nitrate formed. It is suggested that these results tend to show a difference in constitution between the silver and potassium salts.

Prof. Clowes gave an account of further experiments on the respirability of air, in which a candle flame has burnt till it is extinguished. He finds that an atmosphere, which contains oxygen 16·4 per cent., nitrogen 80·5 per cent., carbon dioxide 3·1 per cent., will extinguish a candle flame, but is still, according to the experiments of Haldane, not only respirable, but would be breathed by a healthy person for some time without injury. An atmosphere which extinguishes a coal-gas flame, however, appears to approach closely to the limits of respirability, as far as the proportion of oxygen which it contains is concerned. The candle and lamp flames should be discarded as tests of the respirability of air in favour of the coal-gas flame.

A paper was read by Mr. D. J. P. Berridge, on the action of light upon the soluble metallic iodides in presence of cellulose, in which it was shown that the amount of iodine liberated from potassium iodide by the combined action of light, air and moisture, is greatly increased by the presence of cellulose, this substance probably combining with the potassium hydrate liberated in the reaction. By investigating the conditions of formation of the chocolate stain obtained when note-paper containing starch, and soaked in potassium iodide solution, is exposed to light, evidence is obtained of the formation of a tri-iodide of potassium. The iodides of sodium, calcium, strontium, barium, iron, and zinc, all behave like the potassium salt; cadmium seems alone unable to form a higher iodide.

Dr. C. A. Kohn read the second report of the Committee on quantitative analysis by means of electrolysis. The bibliography of the subject has been completed. The experimental work has been carefully organised, and the results on the determination of bismuth and of tin are nearly complete.

Sir H. E. Roscoe presented the report of the Committee appointed to prepare a new series of wave-length tables of the spectra of the elements.

Some interesting communications were made to a joint sitting of Sections A and B; and the account of these, which we give in our report of the work of the former Section, is supplemented by the following notes on Dr. Gladstone's and Prof. Schuster's communications.

Dr. Gladstone's paper was on specific refraction and the periodic law, with special reference to argon and other elements. In former years he had shown that the specific refractive energies of the elements in general were, to a certain extent, a periodic function of their atomic weights. With regard to argon, the specific refractive energy of argon gas as reckoned by Lord Rayleigh's data is 0·159. At the suggestion of Deeley, the bearing of this result on the atomic weight of argon was considered. If the atomic weight be 19·94, the molecular refraction will be 3·15. This figure is almost identical with that belonging to oxygen and nitrogen gas, and differs considerably from that of calcium, which has a molecular refraction of 10·0 and a specific refractive energy of 0·248. These facts tend to suggest an atomic weight of 20 for argon, and to place it in the vicinity of the alkali metals.

The discussion, which was opened by Prof. Schuster, on the evidence to be gathered as to the simple or compound nature of a gas from the constitution of its spectrum, dealt with matters of rather more physical than chemical bearing. Of special interest to chemists, however, was the evidence cited by Prof. Schuster for considering that the variations noticed in the spectra of sodium, nitrogen, and mercury under different conditions were due to differences in atomic aggregation.

Monday's sitting was devoted to a discussion, held in conjunction with Section K (Botany), on the relation of agriculture to science. It was introduced by Prof. R. Warington in a paper entitled, "How shall agriculture best obtain the help of science?" This was devoted to a consideration of the best means for diffusing a knowledge of the scientific principles of agriculture. Certain things could be usefully done by a Board of Agriculture, and others by County Councils. The formation of a really complete agricultural and horticultural library, freely open to the public, and the maintenance of an English agricultural journal, are matters which might fall to the Board of Agriculture. The advantages to be derived from a Government laboratory and experimental station were dwelt upon. Local stations and secondary agricultural schools should be maintained by the County Councils, who also should inspect the technical instruction in their locality. The foundation of habits of observation and logical reasoning must be laid in the elementary school if higher instruction is afterwards to be given. Higher qualifications should be required for agricultural lecturers than is at present the case.

Mr. T. Hendrick contributed a second paper. He spoke of the apathy and even hostility to science shown by the practical agriculturist, and considered the reasons for this attitude.

In other countries national systems of agricultural education and research have been founded by the State. It is hopeless to look to local effort and support, because the practical man expects immediate results, and results out of all proportion to the time and money expended in obtaining them. The time has come when the State must take part in the work and devote to it much larger sums than at present.

Mr. Thiselton-Dyer said that the matter had been carefully considered by the last Government. It was difficult, however, to persuade the Treasury that agriculture was entitled to receive special aid of a kind not given to any of our other great industries, such as iron and textiles. Personally he looked to individual effort and munificence to supply what was needed.

Prof. Marshall Ward pointed out that it was of extreme importance that the results of any investigations should be made known at once and accurately to the practical man, and this was work which might very well be undertaken by Government, but he deprecated any direction or control from a Government department in any matters of original research.

Prof. J. R. Green pointed out the necessity for investigations on vegetable physiology, as bearing on the growth of crops.

Sir Douglas Galton agreed with Mr. Dyer that agriculturists must look to themselves for help, rather than to the Government. The obtaining of really good teachers was the great difficulty.

Lord Walsingham spoke of the difficulty in producing crops which would realise a profit. Wheat-growing was unprofitable in England, and his own attempts to grow tobacco were frustrated by the heavy duty.

Sir J. Evans and Sir H. Roscoe spoke of the work of the County Councils, and Prof. Perceval gave an account of the courses at Wye College.

Mr. J. Long considered that schools and colleges for boys and youths and demonstration plots for adult farmers were the best means of bringing home the benefits arising from the application of science to agriculture.

Mr. J. R. Dunstan, in a paper on the subject under discussion, contended that courses of lectures were necessary as pioneer work. Unless farmers have a general knowledge of the principles of science, they cannot really understand the results of experiments.

Prof. Liveing advised the co-operation of County Councils in maintaining a central experimental station. He described the system of agricultural teaching adopted at Cambridge.

Mr. Avery gave some account of the agricultural side attached to the Ashburton School in Devon, and spoke of the difficulty of obtaining pupils.

Mr. T. S. Dymond emphasised the necessity of a knowledge of scientific principles, if farmers were to properly understand experimental results.

Mr. C. H. Bothamley considered agricultural sides to secondary schools much better than schools restricted to farmers' sons. The value of demonstration plots, as distinguished from experimental plots, was very great.

Prof. Warington, in reply, remarked that the whole agricultural position was such that if anything was to be done, it must be done at once, they could not afford to wait.

Mr. T. B. Wood gave an account of work at the experimental plots in Suffolk and Norfolk. The experiments in Suffolk are conducted at two stations with soils typical of large areas in the neighbourhood, viz. at Higham, where the soil is thin and light with a chalk sub-soil, and at Lavenham, where it is a much deeper loam. The experiments at both stations consist in the growth of various crops in rotation with various manures. Each year a report of these experiments is printed and circulated, and during the summer, lectures and demonstrations are given on the plots. In Norfolk there are no definite fixed stations, but the use of land has been granted by farmers for experiments on the effect of manures on crops grown in the ordinary course of farming. Feeding experiments have also been conducted.

A paper from Prof. H. W. Vogel was read, in his absence, by the Secretary, dealing with the history of the development of orthochromatic photography. Photographs were shown illustrating the advantages of the use of eosin-silver as a sensitiser, the plates being more sensitive to the yellow rays than plates prepared with ordinary eosin.

Mr. C. H. Bothamley read a paper, illustrated by lantern slides and specimens, on the sensitising action of dyes on gelatino-bromide plates. The manner in which the dye acts was discussed, experimental evidence being given against Abney's view that an oxidation product, formed by the action of light on the dye, is the active agent in assisting the reduction of the silver bromide by the developer. The probabilities appear more in favour of Eder's view that the dye or sensitiser absorbs the energy of the light waves, and passes that energy on to the silver bromide with which it is associated, the silver bromide being thereby decomposed, and the so-called latent image being formed.

In reply to questions by Lord Rayleigh, Dr. Kohn, and Dr. Harden, Mr. Bothamley said that, so far as he was aware, photo-chemical action is always preceded by the absorption of light-waves, and in the case of colourless substances it is the ultra-violet rays that are absorbed and do the chemical work. Although the quantitative composition of the latent image is not known, we have, as a matter of fact, considerable knowledge as to its properties. There is no difficulty in determining the absorbing action and the sensitising effect on two contiguous strips of the same plate, and therefore under strictly comparable conditions. No relation can be traced between the fluorescence of a dye and its sensitising action.

The report of the Committee for investigating the action of light upon dyed colours was read by the President. With some few exceptions, all the available red, orange, and yellow colours, as applied to wool and silk, have now been exposed. (Tables are appended giving the general result of the exposure.) As before, it is found that many natural dye-stuffs are by no means so fast as is generally supposed, and are exceeded in this respect by artificial colouring matters.

Two papers on organic chemistry were contributed by Dr. J. J. Sudborough. In the first paper, the author describes the preparation of a monochloro-stilbene from deoxy-benzoin, differing from that described by Linin, as it is a solid, crystallising from alcohol in large colourless plates. An oily compound, corresponding to that of Linin, has been prepared, and is being further investigated. Other stilbene derivatives are described.

In a note on the constitution of camphoric acid, the author draws attention to the fact that, as regards its etherification, camphoric acid shows a marked resemblance to some of the poly-carboxylic acids investigated by Victor Meyer and Sudborough, and to hemi-mellitic acid. The formulæ of Armstrong and of Bredt are regarded as best agreeing with the behaviour of camphoric acid in this respect.

Mr. H. J. H. Fenton gave an account of the preparation and properties of a new organic acid obtained by oxidising tartaric acid under certain conditions in presence of a ferrous salt. It can be obtained by the oxidation of moist ferrous tartrate in the air, and it is found that this reaction is much accelerated by light. The acid has been isolated, and proves to be a dibasic acid having the formula $C_4H_4O_6 + 2H_2O$. It gives a beautiful violet colour with ferric salts in presence of alkali. The constitution of the acid is under investigation. Heated with water it is resolved into carbon dioxide and glycollic aldehyde, the latter substance polymerising to form a sweet-tasting solid gum having the formula $C_6H_{12}O_6$.

The Committee for investigating isomeric naphthalene derivatives report that the fourteen isomeric tri-chlor derivatives have been obtained.

Dr. M. Wildermann read two papers on physical chemistry. In the first, experimental evidence was quoted, showing the validity of Van 't Hoff's constant, Dalton's law, &c., for very dilute solutions. In the second paper, on the velocity of reaction before perfect equilibrium takes place, an attempt was made to develop equations of equilibrium from experiments made by others on the rate of solidification of phosphorus and other substances.

Messrs. C. F. Cross and C. Smith contributed a paper on the chemical history of the barley plant. The work had been carried out during the two years 1894 and 1895 on the experimental plots at Woburn, and the general conclusions drawn were that the conditions of soil nutrition had very little influence upon the composition of the plant; that the straw grown in wet seasons had a high feeding value and conversely a low paper-making value; and that the compounds known as furfuroids were continuously assimilated to permanent tissue in a normal season, but in a very dry season the permanent tissue is drawn upon by the growing plant for nutrient material which is ordinarily drawn from the cell contents.

THE RETIREMENT OF PROFESSORS.

THE report of the Committee appointed by the Treasury to consider the question of the desirability of a fixed age for the compulsory retirement of professors serving under the Crown has been recently published as a Parliamentary paper. The Committee consisted of Lord Playfair, Lord Welby, and Sir M. W. Ridley, M.P. Mr. C. L. Davies was secretary. The report, which is addressed to the Lords Commissioners of her Majesty's Treasury, is in the following terms:—

We have taken the evidence of presidents and professors of the Queen's Colleges in relation to their retirement upon superannuation at fixed ages, as determined by the Order in Council of August 15, 1890. We are of opinion that the Commission of 1888, upon the report of which, to some extent, that Order in Council was based, did not intend that the limitations of age applied to Civil servants generally should be deemed applicable to presidents and professors of colleges, who are appointed and serve under different conditions from those which prevail in the Civil Service.

These presidents and professors are appointed at a maturer age, and have, by the nature of their employment at seats of learning, less tendency than Civil servants to become inefficient at the age of sixty-five. Indeed, up to that age it is often found that their efficiency increases, by experience in teaching, as their age progresses, though undoubtedly a time does arrive when advancing age weakens the receptivity of the professor to new discoveries in science, and diminishes the inclination to alter his instruction in order to adapt it to these changes. When this occurs the students are the sufferers. In the German Universities this well-known degeneration of intellectual activity among the aged is partly compensated by the appointment of active young "extraordinary professors," who, though not on the ordinary staff of the colleges, are allowed to give competing lectures within their walls. In Edinburgh an extra-mural competition is encouraged, and in each Scotch University, when professors show diminished efficiency through age, it is the duty of the University court to superannuate the professor under a pension scheme, which is charged upon a fixed Parliamentary vote for all the Scotch Universities. The Queen's Colleges in Ireland are in a different position, for they are only to a small extent dependent upon votes in Parliament, being mainly supported out of the Consolidated Fund. They are, in consequence of this peculiarity, in more intimate connection with the executive Government, with which the presidents are in frequent communication as to the working of the college and the efficiency of the professors, who are appointed by the Crown and can be dismissed by the Crown. The statutes which govern the Colleges also emanate from the Crown, and are not, like those of other colleges, the product of academic autonomy.

Under these circumstances, we are of opinion that there should be fixed rules as to superannuation of presidents and professors, and that they should be made by college statutes and not by an Order in Council.

We are of opinion that when a professor reaches sixty-five years of age the president of the college should be bound to report to the Government the condition and efficiency of the teaching. If these are and continue to be satisfactory, the professor need not be superannuated till seventy, but at this age his retirement should be absolute.

In regard to presidents, we are of opinion that the age of seventy should be the period of retirement, but, should the visitors of the college formally report that the college would suffer by the loss of the experience which the president has acquired, we think that the Treasury, and not the Irish Office, should have power to continue him as president for a certain number of years not exceeding five, so that at the age of seventy-five the retirement of a professor should be absolute.

We are quite aware that there are cases where professors at seventy and presidents at seventy-five are fully competent to discharge their duties, but the advantages derived from superannuation would be seriously diminished if, to meet these rare cases, there were uncertainty in regard to the application of a general rule. We have observed with regret that the *alumni* of the Queen's Colleges do not seek to go back to them as professors, and it was explained to us that one reason for this is that it is useless for them to prepare for a professorial career in these colleges while so much uncertainty prevails as to when the chairs will become vacant.

We also took the evidence of Profs. Lockyer and Rücker as to the conditions which prevail in the Government School of Science at South Kensington, and we found that the age of seventy for professors was considered a proper age for retirement under ordinary circumstances.

In our opinion, as the professors are not appointed till middle life, the addition of seven years to their period of service in calculating the amount of their superannuation obviously tends to secure eminent specialists as candidates for office. The power of voluntary retirement at the age of sixty has also much to commend it in this sense.

We have the honour to be
Your Lordships' obedient servants,
PLAYFAIR.
WELBY.
August 5, 1895. M. W. RIDLEY.

The report is followed by the minutes of evidence taken on June 17, 18, and 19, during which nine witnesses were examined.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

WE learn from *Science*, that Prof. Strahl, of Marburg, has been called to the chair of Anatomy in the University of Giessen, Prof. Hans Lenk, of Leipzig, to that of Geology in the University of Erlangen, and that Dr. Haecker, of Freiburg i.B., and Dr. v. Dalla-Torre, of the University of Innsbruck, have been made assistant Professors of Zoology.

PROF. CHAPMAN having resigned the professorship of Geology and Mineralogy in the University of Toronto, that chair is now vacant.

ACCORDING to *Science*, the conditions attached to the bequest made by the late Sir William Macleay to the Sydney University, to found a chair of Bacteriology, are such that the University has decided to decline the bequest. The money will therefore revert to the Linnean Society of New South Wales, to maintain a bacteriologist, who will carry on bacteriological investigations and also take pupils.

THE Examinations for the Royal Agricultural Society's Junior Scholarships have been fixed to take place on November 12 and 13 next, at the schools of candidates and at the Society's house, 13 Hanover Square. Ten scholarships of £20 each are thrown open for competition by candidates between the ages of fourteen and eighteen, and the subjects of examination comprise: (1) The Principles of Agriculture, especially with reference to the rotation of crops, the nutrition of plants and animals, and the mechanical cultivation of the soil; (2) Chemistry as applied to Agriculture; (3) Elementary Mechanics as applied to Agriculture; (4) Land Surveying. The latest date for receiving entries is October 15.

THE following courses of Gresham Science lectures are announced:—"Physic," by Dr. Symes Thompson, on October 8 to 11; "Astronomy," by Rev. E. Ledger, on October 22 to 25; "Geometry," by Mr. W. H. Wagstaff, on November 19 to 22. The lectures will all be delivered at six o'clock in the theatre of Gresham College, E.C.

THE London Society for the Extension of University Teaching announces that, in co-operation with the Royal Geographical Society, arrangements have been made for the delivery at Gresham College of a course of twenty-five lectures by Mr. H. J. Mackinder, on "The Principles of Geography." The course is specially arranged for pupil teachers, and the Sessional Certificate, granted in connection therewith, will carry marks at the Queen's Scholarship Examination. The lectures will be given on Monday evenings at six o'clock, beginning October 7.

AT the City of London College, Moorfields, a course of twenty-five lectures on "The History of Chemical Discovery" will be delivered, under the auspices of the London Society for the Extension of University Teaching, by Prof. W. Ramsay, F.R.S. The course will be begun on Tuesday evening, October 8, at eight o'clock, and be continued weekly.

ON Tuesday evening, October 1, Sir Henry E. Roscoe will preside at a meeting at the Royal Victoria Hall, Waterloo Bridge Road, when the presentation of certificates to students of the Morley Memorial College will take place. The lecture arrangements at the Royal Victoria Hall for the month of October are as follow:—On the 8th, Mr. W. P. Bloxam will lecture on "Combustion"; on the 15th, Dr. W. D. Halliburton will lecture on the "Human Brain"; and on the 22nd, Mr. P. J. Hartog will lecture on "Lavoisier."

SCIENTIFIC SERIALS.

American Journal of Science, September.—Distribution and secular variation of terrestrial magnetism, by L. A. Bauer. Starting from the supposition that the earth is magnetised symmetrically to its axis of rotation, the author shows that the chief cause of distortion of this primary field can be represented as due to a secondary polarisation approximately equatorial in direction. Of these two systems, the polar systems would have to be five or six times stronger than the equatorial. Since, in going round the earth along a geographical parallel of latitude, the deflections due to the secondary system almost balance each other, the inference might be drawn that the secondary field is in some way connected with the earth's rotation.—Relations of the diurnal rise and fall of the wind in the United States, by Frank Waldo. For January the rise of wind towards the mid-day maximum is followed by a more rapid fall over nearly the whole of the United States. For July the same law holds, except in the Western States, where the morning rise is more rapid. As regards the time during which the wind rises, this is about seven hours in the Mississippi valley. On the Atlantic coast there is a decrease from ten hours in the north to five hours on the coast of Florida.—The rate of increase varies from 0·4 to 0·6 miles per hour. Native sulphur in Michigan, by W. H. Sherzer. During the past year interesting deposits of sulphur have been discovered in the Upper Helderberg limestone, of Monroe County, Michigan. The sulphur bed lies from sixteen to eighteen feet below the surface between a compact, dolomitic limestone and a calcareous sand rock. The sulphur generally occurs in bright lustrous masses towards the centre of the cavity, intermatted frequently with the above minerals. Fragments as large as a fist are readily removed. Some of the smaller cavities contain nothing but sulphur, and one was found filled with selenite crystals. About an acre of this bed had been removed when the locality was visited, and from this the superintendent estimated that one hundred barrels of pure sulphur had been obtained.

Wiedemann's Annalen der Physik und Chemie, No. 8.—Simple objective presentation of the Hertzian reflection experiments, by Victor Biernacki. The author places one of Lodge's "coherers" in the focal line of the secondary mirror. Under these conditions, mirrors with a length as small as 45 cm. and an aperture of 30 cm., with a focal length of 3 cm., exhibit the reflection phenomena well. The coherer employed is a horizontal glass tube filled with copper filings, whose resistance is reduced as soon as electric oscillations impinge upon it. The polarisation experiment is easily performed with a tiled wall, which behaves as a transparent solid to the electric rays. A striking experiment analogous to the introduction of a doubly-refracting crystal between two crossed nicolls is the introduction of a thick slab of good ice between the two crossed mirrors, with its axis of 45° to both the focal lines. The galvanometer connected with the coherer, which before was motionless, now gives a distinct reflection, thus showing the doubly-refracting nature of ice.—A convenient method for showing the electric refractive powers of liquids, by P. Drude. For this purpose, strong oscillations are necessary. These may be produced by a modification of Blondlot's arrangement, using an exciter without a condenser, whose total length is slightly smaller than half the wave-length required. The wave-lengths in water and other liquids are obtained by conducting the parallel wires through a long trough filled with the liquid. A bridge is put across them where they enter the water. Another bridge is placed on the wires in air on the other side of a Zehnder tube connected with a gold-leaf electroscope. This is shifted until the gold-leaves collapse. The distance between the two bridges is then, say, 36 cm. The bridge on the water's edge is then gradually shifted along the immersed wires, and the points at which the gold-leaves diverge and collapse are noted. The distance between two successive nodes is 4 cm., so that the refractive index of water for electric waves is 9, and the specific inductive capacity 81. Alcohol, glycerine, and other not very highly conducting liquids may be similarly investigated.—Inconstancy of spark potential, by G. Jaumann. The author shows that the potential which leads to a spark discharge depends upon several elements besides the thickness and nature of the dielectric, the chief one being the presence of variations of electric force, which hasten the discharge and lower the necessary difference of potential. When these variations are avoided, differences of potential amounting to several times the ordinary ones may become necessary for discharge. The spark gap is also affected by previous sparks and by a delay in discharging.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 16.—M. A. Cornu in the chair.—A memoir by M. F. V. Maquaire, on protection against naval collisions, was referred to a Committee.—The perpetual Secretary, in presenting vol. vi. of "Œuvres de Christiaan Huygens," reminded the Academy of the loss of M. Bierens de Hahn. The Haarlem Society will continue the publication of this work, so ably edited by M. Hahn.—On the "équilatères" included in the equations $O = \Sigma_1^{2n} - {}^2l_1 T_1^n \equiv H_n$, $O = \Sigma_1^{2n} - {}^1l_1 T_1^n \equiv H_n + \lambda H'_1$, by M. Paul Serret.—Researches on Algerian phosphates. The case of a phosphatic rock from Bougie, having the composition of a superphosphate, by MM. H. and A. Malbot. The results of a number of analyses of rocks from various sources are tabulated. The Bougie rock is described in detail, as it presents several peculiarities. With regard to the method of analysis, the conclusions are drawn: (1) The presence of organic matter may produce a loss when the phosphoric acid is estimated by direct precipitation as magnesium ammonium phosphate in citric liquor, and this error is not always diminished by a preliminary evaporation with nitric acid on the sand bath. (2) The same error does not occur if the phosphoric acid be first precipitated as ammonium phosphomolybdate. (3) The agreement between the two methods is exact when the organic matter is first destroyed by calcination at a red heat.—The neoformation of nerve cells in the brain of the monkey, following the complete ablation of the occipital lobes, by M. Alex. N. Vitzou, of Bucharest. A detailed account is given of the gradual recovery of the power of perceiving external objects by a monkey after complete ablation of the occipital lobes. An examination revealed the fact that the space formerly filled by the occipital lobes had been filled up by new tissue which was found to consist throughout of pyramidal nerve cells and nerve tissues, the cells being less numerous than in the ordinary occipital lobes of the adult. The new tissue was not due to hypertrophy of the anterior lobes. On repeating the ablation the monkey again lost the power of perceiving external objects, and is still under observation.—M. Ch. V. Zenger records in a note the occurrence of atmospheric disturbances at certain points in Central Europe on September 10 and 11, as predicted by him.

NEW SOUTH WALES.

Linnean Society, July 31.—Mr. Henry Deane, President, in the chair.—Catalogue of the described Coleoptera of Australia. Supplement part i.—*Cicindelidæ* and *Carabidæ*, by George Masters. It is proposed to give as far as possible a complete list of all the Australian Coleoptera described since the year 1886, also to fill in the omissions previous to that date. The present part contains references to 429 species, besides many corrections, and additional localities.—Australian *Termitidæ*, part i., by W. W. Froggatt. The author gives an account of the distribution of *Termites* in general and of the damage done by them, and passes on to a consideration of the habits and range of Australian forms, concluding with a general account of the structure of the termitaria of both the common mound-building species, and of those of *Eutermes* which form arboreal nests as well as on the ground. (*a*) Report on a fungus (*Meliola amphitricha*, Fries.) on *Dysoxylon*. The fungus is found on the leaves of *Dysoxylon rufum*, Benth., on the Richmond River, N.S.W., and has not previously been recorded for this colony. (*b*) Notes on *Uromyces amygdali*, Cooke—a synonym of *Puccinia pruni*, Pers.—Prune rust. This leaf rust is of great economic importance, since it attacks such valuable fruit trees as peach and nectarine, plum and apricot, cherry and almond, causing them prematurely to shed their leaves, and as a consequence, either to bear no fruit, or only small quantities of an inferior kind. Though sometimes called "Peach Yellows," it must not be confounded with the dreaded disease, due to bacteria, known by that name in the United States. Specimens of affected peach leaves, forwarded by Mr. Tryon from Queensland, yielded both uredospores and two-celled teleutospores. In Victoria in the summer season, even as late as July, only the uredospores are at all common. (*c*) Groundsel rust, *Puccinia erechtitis*, McAlp., with trimorphic teleutospores. The æcidial stage is common on groundsel; but this is the first record for teleutospores in Australia. The rust is identical with that on *Erechtites*, described last year. The specimens were procured at Hobart, Tasmania.—By D. McAlpine, Government Pathologist, Melbourne. (Communicated by J. H. Maiden). On a new species of *Elæocarpus* from Northern New South Wales, by J. H. Maiden and R. T. Baker. A large tree (height 80–100 feet, and a trunk diameter of 2–3 feet as seen), found on the Brunswick River. The affinities of this species lie between *E. sericopetalus*, F.v.M., and *E. ruminatus*, F.v.M.; it differs from these two species in the number of stamens, lobed petals, bracts, and fruits. It is named in honour of Mr. William Baeuerlen, Botanical Collector to the Technological Museum, Sydney.—On a new cone from the Solomon Islands, by John Brazier.

BRISBANE.

Royal Geographical Society of Australasia.—Annual meeting, July 22.—Mr. J. P. Thomson, President, in the chair.—The Secretary, Mr. J. Fenwick, read the yearly report of the Council, which stated that during the year sixteen ordinary members had been added to the roll of the Society. The library had received some valuable donations and exchanges, and the finances of the Society were in a satisfactory condition. The President read an address on the subject of the physical geography of Australia, after which the election of officers took place.

CONTENTS.