THURSDAY, SEPTEMBER 27, 1877

URBAN J. J. LEVERRIER

OUR readers will not have been altogether surprised at the announcement of the death of the Director of the Paris Observatory. We have had frequent occasion recently to refer to the unsatisfactory state of M. Leverrier's health, and only a fortnight since we announced his return to his post. He died, however, on Sunday last at the age of sixty-six years, singularly enough on the thirty-first anniversary of the discovery of Neptune.

The gigantic extent and utility of Leverrier's work has been such that it is not possible at such short notice to present anything like a worthy estimate of it. We shall therefore in the present notice content ourselves with referring to a few of the chief events in his career.

Urban Jean Joseph Leverrier was born at St. Lô, March 11, 1811. He was educated at the École Polytechnique, where he so distinguished himself as to be allowed to choose what branch of the public service he wished to enter. He selected a post under the "Administration des Tabacs," and while in that position published his first paper—a chemical one. But he soon gave up chemistry to devote himself almost entirely to mathematical astronomy. In 1839 he contributed two papers to the Paris Academy on the secular variations of the orbits of the planets. These papers attracted the attention of Arago, who requested Leverrier to calculate afresh the perturbations of Mercury with reference to the attraction of other bodies. This may be said to have been the beginning of the great work which he carried on till his death, and with which his name will be for ever associated.

The nature and the stupendous extent of this work were most admirably stated by Prof. Adams, in presenting last year to Leverrier the gold medal of the Astronomical Society. This address is referred to elsewhere. Though drawn aside for a little into the political arena by the events of 1848, he never discontinued his work in connection with the planetary orbits. While in the Legislative Assembly, as member for his native department, La Manche, he gave his attention mainly to subjects connected with public education and scientific discoveries. He has had much influence in these directions, and it is largely owing to him that the École Polytechnique has attained its present high organisation. In 1852 Leverrier was a Senator and Inspector-General of Superior Education. On the death of Arago, Leverrier, as might have been expected, was appointed to succeed him as head of the Paris Observatory. That institution he found in a most disorganised and altogether unsatisfactory condition, and Leverrier set himself earnestly to raise it to the level which it ought to occupy. A work of this kind he could not accomplish without giving offence to many interested in the continuation of the old ways. Indeed, Leverrier's rigid rule caused such discontent among his staff, that the Government were actually compelled to dismiss the great astronomer from his post in 1870; he was, however, restored again in 1873.

While Leverrier's great work was in the sphere of mathematical astronomy, he by no means neglected other departments of science connected with the work of an observatory. To him is mainly due the organisation of the existing meteorological service in France, which depends largely upon local effort. He was ever ready to afford facilities to others to carry on their own researches within the Observatory precincts, and he saw with pleasure the erection of new Observatories both in Paris itself and in the provinces.

"If on no other than selfish grounds," to quote the *Times* notice, "England, as a maritime country, cannot fail to pay a tribute of respect to a man whose work has been of the utmost practical importance in the construction of tables used in guiding ships across the seas. Nor has England been, in fact, niggardly in rendering him honour. On four occasions living words of respect and friendship from England have been addressed to M. Leverrier by presidents of the Royal Society and the Royal Astronomical Society when presenting medals, which are by tradition regarded as the highest tribute the societies can offer of their appreciation of the value of work done. In 1846 the Royal Society, under the presidency of Lord Northampton, presented to him the Copley medal. In 1848 the Royal Astronomical Society, under the presidency of Sir John Herschel, awarded a testimonial; in 1868, under the presidency of the Savilian Professor, the gold medal; and again in 1876, under the presidency of Prof. Adams, M. Leverrier's 'rival' in the discovery of Neptune, a second gold medal. Two years ago the University of Cambridge, at the suggestion of Prof. Adams, conferred on him the honorary degree of LL.D. Perhaps the most valued, because most practical, recognition that could be offered was the fact that for years past his tables have been employed in our 'Nautical Almanac,' superseding all others for the computation of the places of the planets."

Leverrier's work has been of such value to a maritime nation that a mark of appreciation on the part of our Government, as well as on the part of our societies, would not have been out of place. At the funeral on Tuesday, English science was represented by Mr. Hind, the distinguished superintendent of the *Nautical Almanac*—we hope in an official as well as in his private capacity.

M. Leverrier was Inspector-General of Universities, one of the highest dignitaries in the Legion of Honour, and a member of almost every Academy and order of merit in the world. Of his two sons one died two years ago, and the other is an engineer in the Ponts et Chaussées. Madame Leverrier has not been well for a long period, and no doubt the shock of her husband's death will tell upon her constitution.

Owing to the great loss sustained by science in the death of Leverrier, the Paris Academy of Sciences closed its session on Monday immediately after the letter from M. Tresca announcing the sad event had been read. M. Tresca was able to state that the great life-work of Leverrier had just been completed.

RECENT BOTANICAL BOOKS

Text-book of Structural and Physiological Botany. By Otto W. Thomé. Translated and Edited by Alfred W. Bennett, M.A., B.Sc. (London: Longmans, Green, and Co., 1877.)

THERE is a manifest want at the present time of a text-book of moderate size which would supply English students with a general view, not running too far into details, of the distinct types of organisation existing in the vegetable kingdom. Such a book should, as far as possible, draw its illustrations from plants which the teacher could without any very great difficulty place before his pupils. After intelligently comparing the descriptions with as many of the structures described as opportunity afforded, a student should have a tolerably clear idea of the leading facts in the comparative vegetable physiology and morphology of plants. Sachs's "Text-book" is a perfect mine of information of the most accurate and recent kind on all manner of special points, but it is rather a book for the advanced student to consult than for the beginner to study. It is often difficult in it "to see the wood for the trees," to keep well in view the firm grasp of fundamental principles of organisation which the distinguished author undoubtedly possesses, but which the richness of the materials that he has on the whole so ably marshalled cannot, especially on a first reading, but very considerably obscure. Huxley and Martin's "Elementary Biology" is undoubtedly very useful for first breaking ground, but the vegetable types are of course treated in relation to the animal and with the view of bringing out certain general laws applicable to the whole of organised nature. The commencing botanist of course wants something more specialised than this, and for a long time past there has been perhaps little better in English than the able and philosophical sketch of the vegetable kingdom which is contained in the opening chapter of Dr. Carpenter's "Comparative Physiology."

At first sight it seemed as if the desideratum had been supplied by Thomé's "Lehrbuch der Botanik," of which an English translation has recently been published by Mr. A. W. Bennett. An examination of its pages is, however, disappointing. There is an entire want on the part of the author of any definite grasp of his subject, and this, combined with a good deal of vagueness and inaccuracy in the facts, preclude the book being regarded as possessing any higher value than a mere compilation which is only not very bad because recourse has been had to fairly good sources of information. All through there are evidences that the author has not a practical familiarity with the subject on which he is writing. A few instances will suffice to illustrate this defect. On p. 10 we are told "Protoplasm . . . which is inclosed in a cell-wall has . . . no power of escaping from its envelope;" but the author confutes himself on p. 284 by stating *à propos* of Myxomycetes: "The germinating spore, now provided with a cell wall, allows the whole of its protoplasmic contents to escape." On p. 24 there is the extraordinary statement that in some *Muscineæ* starch-grains "are points of crystallisation around which the mass of chlorophyll has been deposited." And a little further on, speaking of the chlorophyll corpuscles of *Metzgeria*, we are told "They multiply also by division, splitting up into two new bodies, each capable of independent life;" yet chlorophyll-corpuscles are incapable of life independent of the cell of which they are specialised constituents. On p. 29 it is stated that the "purpose of the formation of starch is that it may be stored up in the cells as a reserve food material," which is about as just a view of this important process of nutrition as if we were to say of wheat that the purpose of its cultivation was to fill granaries. After this we may pass over as comparatively unimportant, the queer statement that starch is "deposited in especially large quantities in . . pollen-grains" of all things in the world.

On p. 32 we are told that cell-division commences by "the protoplasm . . . contracting into a spherical form." The figure on the opposite page tacitly corrects this error.

On p. 44 we learn that "*Periderm* consists of tabular cells with thicker walls, which, when looked at vertically, have a regular polygonal or stellate appearance." This is far from clear, and certainly not universally true. On p. 52 we are informed:—

"The laticiferous vessels and the true vessels together have been compared to the venous and arterial blood-vessels of animals; but since a direct connection between them has not been proved, and the mature vessels are normally filled with air, this comparison cannot be maintained."

In an educational book surely we may ask that the *débris* of effete hypothesis should be left to slumber in its appropriate oblivion. Any one who has ever examined students will shudder to think how often this quaint relic of the phytotomy of two centuries ago will be trotted out triumphantly when far more important things are altogether forgotten.

Passing over the histology—the treatment of which cannot be regarded as satisfactory—we find a section on "The external form of plants" which, under this apparently philosophical heading, simply conceals the old dreary sterility of the descriptive terminology of flowering plants. Even this is wanting in accuracy. Thus, to take a single page (79): "scandent" is given as a synonym of twining, the fact being that it includes every form of climbing *except* twining; again, Solomon's seal is said to develop its flowering stems from terminal buds, while it is obvious that they are axillary, and therefore lateral.

The "Special Morphology" is far better planned, and if thoroughly revised and published separately, might make—as it is well-illustrated, though chiefly with borrowed woodcuts—a handy little text-book. There is the same want of severe accuracy, however, to the confusion o students. Thus (p. 192), the suspensor in phanerogams is termed the pro-embryo; p. 315, the same term is applied to the prothallus of ferns; the structures, of course, are morphologically in no way homologous. Merely to mention defects as they catch the eye in turning over the pages, on p. 274, ascospores are said to be contained in perithecia, oblivious of the asci; p. 275, saprophytic fungi are mentioned when saprogenous are intended; p. 296, *Hepaticæ* are said to form "a beautiful transition from the Thallogens to the Acrogens," which only shows how easily persons may be deluded by mere "adaptive" characters; on p. 309 the author confuses—which is almost incredible—the vascular bundle-sheath with the sclerenchyma in the fern-stem; p. 337, the North American *Callitris* is mentioned, North African being intended, and the two-lobed anthers of *Taxineæ* are mentioned, when *Taxus*, for example, has usually a six-lobed anther, as he tells us on p. 336.

We have not examined very critically the concluding chapters on Fossil and Geographical Botany, but the latter, at any rate, seems too vague to be very useful.

Altogether, we cannot but feel sorry that Mr. Bennett has spent his labour on a work of such little real value. He seems, indeed, to have had some misgivings as to its shortcomings, and has largely borrowed from Huxley and Martin's "Elementary Biology." Thus the student is left as best he may to harmonize Thomé's account of the antheridium of Characeæ, on p. 293, with Huxley's independent description on p. 294, which is transferred almost bodily.

It can hardly be doubted that Mr. Bennett, with his experience as a teacher, could have supplied us from his own pen with a text-book which would have been much more useful.

OUR BOOK SHELF

Natural Geometry; an Introduction to the Logical Study of Mathematics, for the Use of Schools and Technical Classes, with Explanatory Models. By A. Mault. (London: Macmillan, 1877.)

THIS is a good elementary text-book, founded on the work by M. E. Lagout ("Takimetry"), which we have already noticed (NATURE, vol. xvi. p. 226). The ground covered by the work before us is not quite so extensive in one direction as that covered by Dr. Gwynne's translation; but it has an introduction to pure geometry which is likely to be of service to junior pupils. We are disposed to think that some such practical training as that indicated here, with the aid of the accompanying models, and a short course of "practical" geometry would be a capital thing for our junior pupils. Boys who are exceedingly dull and stupid over their "Euclid" often, as we have repeatedly seen, take much interest in these concrete exhibitions of geometrical truths. The book has been very carefully got out; there are a few loose expressions which might be improved. On p. 32 is the statement, "in equal circles equal arcs are those which have equal chords," a distinction should be made between major and minor arcs. Another trifling matter (but some boys would at once notice it) is that some equilateral figures are drawn on p. 33, which are not equilateral by scale. There are two parts—geometry by sight, which treats of the measurement of flat surfaces and of solids, and scientific geometry, or reasoning helped by sight. The latter is concerned with the measurement of accessible and inaccessible things and with the incommensurable (as the circle, sphere, cylinder, and cone). We can recommend the book for school use.

LETTERS TO THE EDITOR

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

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

The Cycle of Sun-spots and Rainfall

MAY I venture to ask insertion for the following remarks on the cycle of sun-spots and rainfall? I frequently receive inquiries regarding the meteorological aspects of the Indian famine and the prospects of Madras during the coming monsoon. But I beg to state that the object of my investigations is not at present to predict the future, but simply to ascertain the past facts. When we are quite sure of the data it will be time enough to apply them. In order to secure a stable basis it has been necessary to work up a vast accumulation of meteorological returns which have never been previously collated, and to make further references to India, Germany, and America. Some time must still elapse before the results can be presented as a whole. Meanwhile I should be obliged if you would give insertion to the facts with regard to two points which seem at present to have a special interest to the public.

In the first place, I think I may now safely say that the coincidence of the cycle of rainfall with the sun-spot period is not confined to Madras, but is common to various points around the great ocean tract on which Madras lies. The three such points of observation for which materials exist during the most considerable period of years are Madras, Bombay, and the Cape of Good Hope. I have made up the following table from the monthly, and, when available, from the hourly, returns of these stations; and as there are a few errors (probably clerical) in a return recently given for Bombay by the meteorological reporter at Calcutta, I ought perhaps to add that these returns have been tested by a trained computator in the Scottish Meteorological office. The sun-spot column was worked out from an old list, the only one available to me in India, and will hereafter be revised from the more complete returns issued this year by Dr. Rudolph Wölf. The differences do not, however, affect the general results. My cycle of eleven years starts back from 1876; and the *minimum* group of my cycle, namely, the eleventh, first, and second years, include all the years of *minimum* sun-spots from 1877 to 1810. It will be seen that the coincidence in the cycle at Madras and the Cape of Good Hope is very strongly marked, while that at Bombay is less so, and somewhat lags behind the other two. An explanation exists for this, but it would trespass too much on your space to enter into that side of the question.

TABLE I.—*Eleven Years' Cycle of Rainfall and Sun-spots shown in Periods of Two Years.*

	Average of rainfall in inches, registered at Madras. (1813-76.)	Average of rainfall registered at Cape of Good Hope. (1842 70.)	Average of rainfall registered at Bombay. (1817-76)	Average relative number of sun-spots (Wölf's old list). (1810-60.)
Min. group. Eleventh series of years in the cycle of eleven years ...	37·03	21·19	70·32	10·92
	Av. 40·39	Av. 21·05	Av. 68·78	Av. 10·32
Min. group. First and second series of years in the cycle of eleven years	42·07	20·98	63·c2	10·02
Third and fourth series of years in the cycle of eleven years ...	49·12	23·92	67·36	39·89
Fifth and sixth series of years in the cycle of eleven years ...	54·64	28·11	71·22	73·44
Seventh and eighth series of years in the cycle of eleven years	52·36	27·80	79·34	53·78
Ninth and tenth series of years in the cycle of eleven years ...	49·02	23·26	76·42	33·54
Eleventh series of years in the cycle of eleven years	37·03	21·19	70·32	10·92

The cyclic coincidence may be tested in another way. If it really exists there should be a well-marked minimum group at the extremities of the cycle (in the eleventh, first, and second years), and a well-marked maximum group in the middle of the cycle (the fifth and following years). The years on both sides of the central maximum group, *i.e.*, between it and the minimum group at the two extremities, should yield intermediate results, and, when taken together, should form an equally well-marked intermediate group. I therefore divided the cycle (so far as the number 11 permitted) into three equal groups. The "minimum group" is formed by the three series of years at the extremities of the cycle, which include *all* the years of minimum sun-spots in this century from 1810 to 1877. The "maximum group" embraces the four central years from the true maximum year of the rainfall and sun-spot cycle (the fifth) to the second maximum in the sub-cycle of sun-spots in the eighth year. The "intermediate group" consists of the two series of years on both sides of the central maximum group, namely, the third and fourth years on the one side, and the ninth and tenth years on the other side.

TABLE II.—*Cycle of Rainfall and Sun-spots shown in the Minimum, Intermediate, and Maximum Groups.*

	Average of rainfall in inches, registered at Madras.	Average of rainfall in inches, registered at Bombay.	Average of rainfall in inches, registered at Cape of Good Hope.	Average relative number of sun-spots (old list).
Minimum group: eleventh, first, and second years... ...	40·39	68·78	21·05	10·32
Intermediate group: third and fourth, with tenth and ninth years	49·07	71·89	23·59	36·71
Maximum group: fifth, sixth, seventh, and eighth years ...	53·50	75·23	27·95	63·61

I regret that I have not the materials for showing the rainfall for Bombay and the Cape during the whole sixty-four years for which the returns exist for Madras.

The other point on which I venture to trouble you at present has reference to a different class of atmospheric phenomena. I think that it may now be affirmed that a cycle of wind-disturbances exists and is coincident with, although slightly lagging behind, the cycle of sun-spots. M. Poëy called the attention of the French Académie des Sciences to this subject five years ago, and published, as far back as 1873, a list of hurricanes in the West Indies from 1750 to 1873 in support of his views. Dr. Meldrum has worked out the same question, as regards the [East] Indian Ocean, with admirable patience and success. On the publication of my cycle of Madras rainfall it struck Mr. Henry Jeula, Honorary Secretary to the late Statistical Committee at Lloyd's, that the subject might have a practical bearing upon under-writing and marine risks. He collected from Lloyd's Loss Book the statistics for the two last eleven-year cycles (1876-66, and 1865-55), the only ones for which materials were available; and conjointly, we have tabulated the results. We found that the percentage of losses on registered vessels of the United Kingdom was 17½ per cent. greater during the two years of maximum sun-spots (the eleventh and first of the cycles) than during the two years of minimum sun-spots (the fifth and sixth of the cycles). In the same way we found that the percentage of the total losses (calculated on the eleven years) posted on Lloyd's Loss Book was 15 per cent. greater during the two years of maximum than during the two years of minimum sun-spots. We further found that the increase and decrease of losses follows a cycle, closely following (although for sufficient reasons somewhat lagging behind) the cycle of sun-spots. These results can be tested from the succeeding tables.

TABLE III.—*Percentage of Losses posted on Lloyd's Loss-Book, compared with the Eleven-Year Cycles of Sun-spots and of the Rainfall at Madras.*

		On registered vessels of the United Kingdom.	On total losses posted in eleven years.	Average of rainfall at Madras. Inches.	Average relative number of sun-spots (Wolf's lists).
Minimum group.	Eleventh series of years of the cycles	9·93	7·64	37·03	10·9
	Average of first and second years of the cycles	11·91	9·33	42·07	10·0
Average of third and fourth years of the cycles		11·05	8·64	49·12	39·8
Average of fifth and sixth years of the cycles		12·21	9·31	54·64	73·4
Average of seventh and eighth years of the cycles		12·82	9·81	52·36	53·7
Average of ninth and tenth years of the cycles		11·84	9·09	49·02	33·5
Average of eleventh years of the cycles		9·93	7·64	37·03	10·9

Dividing the eleven years as nearly as the number admits by three, into a minimum, an intermediate, and a maximum group, we get the following results:—

TABLE IV.—*Percentage of Losses posted on Lloyd's Loss-Book, compared with the Eleven-Year Cycles of Sun-spots and of the Rainfall at Madras.*

	On registered vessels of the United Kingdom (1855-1876.)	On total losses posted in each eleven years. (1855-1876)	Average of rainfall at Madras. Inches (1813-1876.)	Average of relative number of sun-spots (Wolf's lists). (1810-1860.)
Minimum Group.				
Average of first, second, eleventh, and tenth years of the cycles	11·13	8·64	41·58	14·26
Intermediate Group.				
Average of third, fourth, ninth, and eighth years of the cycles	11·90	9·21	51·37	42·46
Maximum Group.				
Average of fifth, sixth, and seventh years of the cycles...	12·49	9·53	53·22	64·10

Again, testing the cyclic coincidence, by taking the first four years, the middle or maximum four years, and the remaining three years at the end, a similar result is obtained.

TABLE V.—*Percentage of Losses posted on Lloyd's Loss Book, compared with the Eleven-Year Cycles of Sun-spots and of the Rainfall at Madras.*

	On registered vessels of the United Kingdom.	On total losses posted in eleven years.	Average of rainfall at Madras. Inches.	Average relative number of sun-spots (Wolf's lists).
Average of first, second, third, and fourth years of the cycles	11·49	8·98	45·00	24·90
Maximum Group.				
Average of fifth, sixth, seventh, and eighth years of the cycles	12·52	9·56	53·50	63·55
Average of the ninth, tenth, and eleventh years of the cycles	11·20	8·61	45·02	25·99

I think, therefore, that we are justified in concluding that the periodicity observed by M. Poëy in the hurricanes of the Antilles, and by Dr. Meldrum in the cyclones of the Bay of Bengal is of such a character as to exercise a widespread effect upon the commerce of the world. How far these wind-disturbances may be eventually proved to be general throughout the earth's atmosphere, or throughout any given belt of it, I am not yet prepared to offer an opinion. But that the practical results of such wind-disturbances on maritime commerce are of a general character the foregoing tables now place beyond dispute.

In conclusion, I beg to caution fellow-workers that no really trustworthy results are to be obtained from the old plan of jumbling together a number of unhomogeneous stations in a bag and shaking out averages. The true method is to take certain recognised factors in the rainfall, such as the monsoons, and to examine whether any common periodicity exists between the operation of these factors and the sunspots. This is what I have attempted to do for various points around the great basin which stretches southwards from the Bay of Bengal, and what Mr. Archibald has so carefully done in NATURE for Northern India. I am now conducting a similar inquiry into the American and Australian rainfall, but, as already stated, some time must elapse before the results can be presented. W. W. HUNTER

Allanton House, Lanarkshire, September 20

The Discovery of the Satellites of Mars

As some of the earlier newspaper accounts of Prof. Hall's discovery of satellites of Mars are said to have produced, in some quarters, an impression not fair to him, and as the same accounts may produce the same impression abroad, it seems proper to make the following statement :—

When on the morning of August 17 Prof. Hall showed me his observations, the communication was purely confidential and friendly, and was not made either in the line of duty or because he failed to recognise the significance of his observations, or because any special skill he did not possess would aid in interpreting them. I suggested that, from the few measures he had made, it was possible to estimate the time of revolution of the satellite, if the object really were one ; and thus ventured the prediction that it would be hidden during most of the following night, but would reappear toward morning near the position in which it was seen the night before. The fulfilment of this prediction facilitated the establishment of the true character of the object, but, without it, an equally certain hold on the satellite would very soon have been obtained by Prof. Hall alone. The credit of sole discoverer is therefore due to him. SIMON NEWCOMB

The Satellites of Mars

IT may interest some of the readers of NATURE to know that one of the recently-discovered satellites of Mars appears to have been certainly seen with the six-foot reflector at Parsonstown. My assistant writes me on the 17th instant :—

"On the 8th instant (before receiving the Washington circular) I suspected very strongly at 11·45 P.M., while using the six-foot, that a satellite was visible, following the disc, about 1½ diameters. It appears now from the elements that it must have been the outer one. On the 15th instant, at 11·30 P.M., I saw it *quite distinctly* preceding the planet, however not well enough to measure it, as I lost it again after a couple of minutes, owing to the strong glare of Mars. Last night I saw it again, but only by glimpses, twice or three times."

The unfavourable weather prevented the satellites from being looked for between the 8th and 15th instant.

I may add that it seems probable that the satellites might have been measurable on the 15th instant with the bright line micrometer had it not been in the maker's hands. The low meridian altitude of the planet (25° at Parsonstown as compared with 40° at Washington) is of course a serious drawback to observations at the former place. ROSSE

Yorkshire, September 20

THE weather in this neighbourhood has been very unfavourable for observation ever since the announcement of the discovery of two satellites of Mars. Last night, however (about 9.30), during little more than half-an-hour's interval of clear sky, the air being extremely steady, and the planet beautifully defined, I succeeded in seeing the outer satellite of the two. With the full aperture of my 18-inch silvered-glass equatorial reflector, and an ordinary achromatic eye-piece with a bar across the field hiding the planet, the satellite was but glimpsed occasionally ; with a single double-concave lens (power about 180) it was visible, in spite of the brilliant light of the planet. Had I not known its exact position, however, I question whether I should have seen it at all. It is a most difficult object.

HENRY COOPER KEY

Stretton Rectory, Hereford, September 19

A Good Suggestion

THE approaching meeting in London of librarians representing the most important English, and, I believe, foreign, collections of books, makes the present a suitable time to offer suggestions as to the management of such collections.

It has long seemed to me that an improvement might be made of a very simple nature, but capable of greatly increasing the working value of reference libraries, especially those of the first rank ; namely, to provide, somewhat as follows, for their being consulted by those who cannot personally visit them.

Suppose that the authorities of such an institution as the British Museum or the Bodleian designate certain persons, not paid officers of the library, but known to its directors as well-educated, trustworthy, and acquainted with the resources of the particular library ; publishing the names and addresses of these gentlemen as willing, and believed to be competent, to undertake researches amongst the books for those who may write to them from a distance ; the official authorities assuming no actual responsibility for the work so done, but merely recommending the persons to do it ; publishing at the same time a definite statement of the payment expected per day or hour by these persons.

Often when one would desire to consult a great public library in a foreign land, or in a distant part of one's own country, nothing short of a personal visit would be of use, but in very many cases it would be quite possible to obtain all that one desired by a simple business-like correspondence with a proper agent. Sometimes the question is merely whether such or such a book exists in the library, with perhaps an accurate copy of its title ; sometimes a special reference to a single page in an old and scarce scientific journal or set of transactions is to be verified ; sometimes a few paragraphs are to be copied in the exact words of the author ; sometimes a name, date, or number is to be sought out ; sometimes a larger amount of work would be needed, but so definitely shaped out that instructions in writing could easily be given for it to an intelligent person on the spot. As it is, the consulting of such a distant library in person is often simply impossible, and even when possible, often involves such expense and delay as to make themselves seriously felt ; whereas by the plan proposed, the object in view might often be attained at a cost of time and money altogether trifling. In my own very small experience I once found it necessary to travel some 700 miles, losing three days, and spending about 7*l*., in order to refer to a book for about ten minutes, while directions for making the same search could have easily been put upon half a sheet of note-paper, and carrying them out would have occupied a person living in the city in which the library was situated altogether not more than an hour or an hour and-a-half.

In the neighbourhood of almost every large library competent men might readily be found to undertake such work as is suggested, and to whom the opportunity of increasing their income, or probably in time earning from this source alone a satisfactory income, would be welcome. The plan would admit of being carried out upon a small or an extended scale ; a library of the third or fourth class might afford a field for a single man only, while one of the first class would be likely gradually to enlist the services of a number ; if this were so sub-division of labour would be desirable, one person undertaking researches in natural history, another in mathematics, physics, or chemistry, another in classical learning, &c.

While such work could not properly be done by the regular officers of a public library, it would be important that the private individuals who were to enter upon it should have the approval of, and should be recommended by, the library authorities, who might also very properly fix the rate of payment, recommending only those who were willing to accept the rules laid down.

This plan has at least the merit that it might be tested with very little trouble, risk, or disturbance of existing arrangements. I believe that even in England with great libraries situated at comparatively moderate distances from almost every one in the kingdom, it would prove a great convenience ; to persons placed as are those who live here in America, with no library of the first rank on this side of the ocean, and with hundreds of miles often separating one from the larger of even those libraries which do here exist, the boon of access by letter to the greatest collections of the world would be inestimable. It would be in a new direction, and a noble one, carrying out the tendency of the most modern civilisation which looks to placing, as far as possible, the resources of the whole earth within the reach of him who lives at any one spot upon its surface J. W. MALLET

University of Virginia, Setember 5

Some of the Troubles of John O'Toole respecting Potential Energy[1]

II.

B.—*Potential E., as meaning "energy related to Potential Functions."*

WE now pass to the second meaning of "potential E." It happens, by a most singular and unfortunate coincidence, that this class of E. can very well be called by that title for a reason quite distinct from that which we have been deprecating. The idea of the potential function, or briefly, potential, was first formed and thus named by Green. It has no reference whatever to existing in possibility ; it is concerned with present potency or power ; and it happens that potential E. of unit of mass may

[1] Continued from p. 441.

be very appropriately so termed by some physicists as being E. of potential, or rather E. of difference of potential, or E. which is the complement of potential. This is evidently sometimes in the minds of our teachers: indeed, Clerk Maxwell directly tells us[1] that this is one sense in which the title is suitable; he calls it a "very felicitous expression," because it has the two differently applicable meanings we have mentioned.

7. Here, then, is our next gravamen. These two characters of this type of E. are quite heterogeneous and unconnected. Now a *simple name* can only refer to one character of a person or thing; and if it happens, by accident, that the *word* that constitutes the name has two quite different meanings or references, and that both are applicable to the person or thing, if we mentally apply them both to that individual, we are guilty of a sort of punning or verbal skylarking; as if for instance we should call Mr. Smith an upright man on account both of his erect carriage and of his moral probity. This is bad enough; but further, the two characters which might be implied in the name "potential E." are not merely heterogeneous; they are mutually incompatible; they cannot be put together into the same complex idea, at least by ordinary mortals. Surely there is no occasion to stop to prove this.

It is evident that the majority of our teachers feel this and the preceding inconveniences themselves. And I confess that I am now going to bring against them a more serious accusation than that of merely using unsuitable language. There is a most singular and apparently significant omission to be noticed in nearly all the manuals, referring to this subject, into which I have looked; we have already alluded to this.

8. But we must take this opportunity of numbering it as the eighth of our gravamina. It is this, while teaching us about the different classes of E. and telling us that one is called "potential," they abstain from telling us why it is so called! This omission is so remarkable, in itself, as occurring in books intended to impart instruction, and so unlike the ordinary behaviour of our doctors, that there must be some very particular reason for it. A single person might make this omission by pure accidental inadvertence; but when a number of persons do so it cannot be thus accounted for. There is no explanation but this—that they perceive the botherations connected with this confounded, I mean confounding, name, "potential E.," and rather than acknowledge how matters stand, and own themselves to blame, they try to slur the thing over by giving no meaning of the name at all. Rankine, indeed,[2] just alludes to the last meaning of "potential E.," which refers to its connection with potential (function); but that is all; not a word about the incongruity between it and his own original meaning, just as if none existed. Clerk Maxwell, however, as we have just seen, boldly takes the bull by the horns, and tries to make both himself and us believe that he is delighted with this Janus-like name and with the compounding of its two incompatible meanings. It so happens that the writings of that distinguished physicist contain as striking an illustration as could be conceived of the inconvenience of the ambiguity of this "very felicitous expression." We have already mentioned it, but with a different object in view. Having told us in one place that "potential E." is "the E. which the system has not in actual possession," he also tells us elsewhere[3] that "the leaden weight of a clock when it is wound up has potenial E., which it loses as it descends." The weight sets-to and works with E., which it has not in possession, but only has the power to acquire, and which it loses the power of acquiring!![4] In the first statement he was thinking of the first meaning of potential E. and in the second statement of the other meaning.

It might be said that if we discard the first meaning of "potential E." on account of its intrinsic wrongness, we shall, at the same time, abolish this last difficulty, which arises from its relation with the second meaning, and that this second meaning, which is admittedly good *in itself*, will then have nothing against it. But in the first place the associations of the name "potential E." with the first meaning are too strong to be easily got rid of. It would be all but impossible to retain the word and confine it strictly to the second meaning.

9. But besides, "potential E." in this second meaning, though good in itself, has inconveniences independent of this when applied as I believe it universally is, to E. conceived of as existing in *the body* moved; for potential (function) does not appertain to the *body* moved, but entirely to the *force* concerned.

C.—*Potential E., as meaning "Energy of Potency."*

As to the third meaning of "potential E.," it has been said (and indeed Rankine may have had this in view in one place[1]) that it need not be taken to imply anything more than E. of potency or power without reference to Potential (function)

10. But according to this, "potential E." would mean the power of doing work which consists in power; and it would be as great a tautology as "umbrageous shades."

11. And again, if it be the special *distinction* of one class of E. that it is E. of potency it necessarily follows that the other class, observe the so-called actual E., is *E. of impotency!*

12. And besides, there is the same incompatibility between this meaning, C, and meaning A, as there is between B and A.[2]

As to the whereabouts of Potential Energy.

We shall now pass from the perplexities connected with this unlucky name, "potential E." to consider the behaviour of our teachers towards the thing itself. It will conduce to clearness to drop this name now, since our objections are no longer directed against *it*, and adopt another very common one for the same thing, viz., "E. of position."

13. The E. of position is usually regarded and spoken of as belonging to, or being *in the body* in question which may be about to move and acquire E. of motion. This puzzles poor P. terribly; not only on account of the difficulty of grasping the thing mentally and of putting any clear meaning into it, but also because the doctors, both individually and collectively, often display such curious inconsistency respecting it.

But before proceeding to consider directly the undesirableness of this way of viewing E. of position, let us observe that it is the cause of all the above perplexities, which, indeed, seems to be sufficient objection to it; and let us endeavour to find out why the doctors should have had recourse thereto.

The physicists having determined, for the reasons below, to talk of this E. of position as being in the body, and that body being just the same (and, when regarded as attract*ed* or repell*ed*, as is usually done, equally inert) in whatever position it stands, it becomes necessary to provide for this by a little ingenious dodge; for such the phrase "potential E.," as now generally used, really is. "Potential E." plays the same part as a conjuror's empty case or shape, which is made to represent some solid object which is really lying elsewhere, or is perhaps actually doing duty there. Our physical prestidigitateurs tell poor P. that this E. is "in the body," that the body "*has* it," that the body "possesses it," with other similar expressions. But what is it that they are presenting to him all the time? "Potential E.," which sounds to him very fine, and which he thinks must be something very serviceable, but which is in reality only an empty shape, for it is "E. which the body *has not* in actual possession"!! They have adopted the precise inverse of the famous device employed by Ulysses when he told Polyphemus that his name was Οὖτις. We have mentioned and reckoned this grievance already, on its own account; we have returned to it now only to show how the present one necessitates the use of this delusive name "potential E."

Why then is it that our *teachers* (save the mark!) wish thus to make-believe that they have got their E. of position in the body? The principal reason is this—They have to keep straight with the metaphysicians. In these days it is generally perceived that we should, as much as possible, avoid treating force as an objective *something*. When energy does not come prominently forward into discussion they can use the same forms of expression about force as their grandfathers did, though intending them only as such. The term "force" is "very useful," in that "it enables us to abbreviate statements which would otherwise be long and tedious;" and no harm is done by using it when the necessary reservation as to its being only a convenient mode of speech is known to underlie all the statements and discussion. But when Energy, which must be taken as real and objective in some sense, is the subject of their talk, they become extra cautious, and, fearing to put this objective affair into non-objec-

[1] "Heat," p. 91; "Matter and Motion," p. 81:—"Potential E.—A very felicitous expression, since it not only signifies the E. which the system has not in actual possession, but only has the power to acquire, but it also indicates its connection with what has been called (on other grounds) the Potential Function."

[2] *Phil. Mag.*, February, 1867.

[3] "Theory of Heat," p. 281.

[4] We have already seen above, that the weight never acquires more than a quite insensible amount of "actual E.," so called.

[1] "Encyclop. Brit." (1857), vol. xiv., article "Mechanics."

[2] In Nicholl's "Cyclop. of Phys. Sciences" potential E. is said to mean E. of *a* power or force, but it is easy to see that this does not mend the matter at all. As a *distinguishing* title it implies that a moving body has no force nor power, no "power of performing work," that is to say no E.

tive force, or tension, or stress, they are driven to thrust it into the body, notwithstanding the perplexities and contradictions caused by so doing, and notwithstanding the painful necessity incurred thereby of hoodwinking poor P. in the above manner, and endeavouring to hoodwink the metaphysicians. But there is really no reason why force with the saving reservation should not be introduced as freely into the discussion of E. as into other questions of dynamics; and the physicists often do introduce it thereinto; but then, when frightened at what they have done, they will silently withdraw it again. All the inconsistencies of the doctors, and their capriciously varying moods of freedom and shyness respecting "force," and their stepping up and down from one platform of thought to another, perplex poor P. beyond measure. He knows nothing but what they tell him; and he dares not attribute his difficulties to anything but either the abstruseness of the subject or his own stupidity.

But probably there was another motive, also, for this melancholy idea of putting the E. of position into the body, viz., the desire for simplicity of arrangement. Since E. is E., and the kinetic E. is undeniably in the body, it would seem to be an orderly proceeding to put the other there too. But this would be as if a methodical housekeeper should keep her coals and her blankets in the same "hole" because they are both warming apparatus, though in very different ways. And besides, we shall find that whatever may be the gain in this respect in putting both the types of E. into the body, it is outweighed by a certain loss of true correspondence and clear analogy, which will be mentioned farther on.

We now come to the more direct consideration of the merits of this procedure of putting the E. of position into the body. Let us begin with an interesting little illustration of its character. It is the ordinary and legitimate mode of expression to say that when a stone is projected vertically upwards, the gravitational attraction between the earth and stone draws the stone down again and gives it the kinetic E. with which it strikes the earth. And the gravitation attraction is usually and conveniently conceived and spoken of as being all the earth's: and the stone is usually regarded as being simply attract*ed*. Every doctor will frequently speak thus; and nevertheless he will also, and sometimes in the same breath,[1] tell us that it is the *stone*, say at the highest point of its ascent, that has E. of position due to its height from the ground. So then the connecting attractive force, which is to do the work of drawing the stone down again, and which is therefore one factor of the E. present, is regarded as being in the earth, but the E. as being in the stone! This is *one* way no doubt of teaching poor P. the difference between force and E! Take another illustration. Some of our foremost doctors[2] tell us that when a bow is drawn and about to discharge the arrow or the bolt it is the arrow or the bolt that has E. of position; in this they have at least the merit of consistency. Poor P. generally feels that this conveys no distinct idea at all to his mind; of course he dares not thing it wrong. Then he finds other doctors[3] who tell him (though in so doing they are inconsistent[4] with themselves) that in this case the E. is in the bow. What is to be done now? Is this distracting E. of position "like a bird so that it can be both here and there at the same time"? Or are the doctors on one side—how shall we write it—wrong? At any rate, since the doctors differ, poor P. must needs choose for himself, and in order to escape the above perplexities and also for the following reasons, he elects to conceive of the E. of position as not in the body but in the force or forces concerned which are at least virtually there; it being an ulterior and quite another question, what is force?

The discussion is of course now, as it has been all along, only as to modes of conception or of expression, and not as to the science of our doctors. All agree that if you *spend* E. against the resistance of the inertia of a mass in giving it velocity or acceleration, you have bestowed your E. on the inertia of that body, you have transferred your E. to that inertia. So, in exact correspondence and analogy, if you spend E. against the resistance of the gravitation attraction, for instance, in raising a stone to a certain height you have bestowed your E. on that attraction, you have transferred your E. to gravity. That attraction was beforehand pulling at the stone as hard as it could; but it had no power of doing work, according to the definition of work, *i.e.*, it had no energy according to the definition of E. You have given it E., or the power of performing work by affording it the condition necessary for its doing work, viz., space to work through. Why will not the doctors say this in so many words, when they do say it virtually in various forms? From Newton down they tell us this, that the work done by a force is fs (s being the space through which the force f acts); but the work done is the measure of the preceding E. or power which of course the force had of doing that work; why then will they scarcely ever say that the E. of a force is fs (s being now the space through which he force will have opportunity of acting)? When they do say in substance what we want them to say, they avoid most carefully the direct clear statement of it in so many words.[1] "This kind of E. [potential] depends upon the work which the forces of the system would do if the parts of the system were to yield to the action of those forces." That, of course, means *precisely the same* as the following, which, however, expresses the thing more directly. This kind of E. (potential) is the E. which the forces of the system possess in consequence of the possible displacements of the parts of the system under the action of those forces. Tait himself, both in his Glasgow lecture and in his "Recent Advances," tells us that a wound up spring or bent bow has potential E. Clerk Maxwell tells us the same. If so we have a right to speak of the energy of the gravitation attraction. In a certain respect the cases are different, but not so as to affect the present point.

This, our putting of the E. of position into the forces, instead of into the body or bodies, does not, of course, *explain* the action any more than the other does, but it gives a conception (provisional, if you like) which is much clearer and *in better analogy*, and, as we have said, free from all the above-recounted confusions. Moreover, the expression "E. of a force" has the great advantage of keeping before the mind of poor P. the fact that force and energy are not the same, a distinction which he is slow to apprehend, and which it is of the utmost importance to him that he should get proper hold of.

And now that we have got our E. of position into its most convenient seat, what shall we call it, and how shall we speak of its action? We cannot be dreadfully wrong if we call it by a name suggested by an expression of Helmholtz; let it be "*Energy of Tension*." Does it not seem more logical to designate it by its essential characteristic than by what is only a condition though an indispensable one; for this latter we do when we call it E. of position or configuration. And as to its action let us say that when E. is being, as it is usually expressed, transformed from potential to actual E., or *vice versâ*, it is *transferred* from the forces to the bodies of the system, or *vice versâ*. If these expressions are unsuitable and erroneous, then let every one abstain from language which is precisely tantamount to them. But our doctors do not do this; and it fortifies us greatly in the belief that we are right to know that our doctors, when they are quite themselves, say the very same in substance, though not in so many words. On the other hand, if these expressions recommend themselves to us, let us use them boldly and consistently without mincing matters. Deschanel seems to have been on the point of using them in one place.[2] However, the fear of his *confrères* suddenly rose before his eyes, and having written (or his translator for him) the word "transferred," he stops short without telling us from what and to what the transference is made; he leaves us to complete for ourselves the sense of the passage, which clearly is that the transference is from the forces to the bodies, and *vice versâ*.

Poor Publius and myself have *several* other complaints to make; but probably we have said enough to excite the sympathy of all considerate persons.

Dublin X.

[1] Clerk Maxwell's "Heat," p. 281 bottom; see Willson's "Dynamics," p. 147.

[2] *E.g.* Balfour Stewart, "Cons. of E.," p. 25 (but see his "Elem. Phys.," p. 106).

[3] Tait, "Recent Advances," p. 18; Willson, "Dynamics," p. 278.

[4] The inconsistency is startlingly exhibited in a single sentence for which two doctors are responsible, "Uns. Univ.," p. 111, "the potential E. of a raised weight or bent spring." If the potential E. is in either one of these it cannot be in the other. We have the same in a single sentence in Thomson and Tait, p. 178 (two doctors, again, responsible); also in Tait's Glasgow lecture.

[1] The only exception that I remember to have seen is afforded, curiously enough, by Rankine himself, the inventor of E. *in posse*. In *Phil. Mag.*, February, 1853, he says, "E. of gravitation;" and in "Encycl. Brit.," vol. xiv., "Mechanics," he speaks of the E. of an effort.

[2] "Nat. Phil.," p. 79

New Electric Lights

UNDER the above title Mr. Munro describes, in NATURE, vol. xvi. p. 422, M. Lodighin's device for an electric light. This is no novelty but a simple repetition of an invention made by Mr. Starr, a young American, and patented in this country under the title of "King's Patent Electric Light," specification enrolled March 25, 1846. An account of it, with drawings, may be found in the *Mechanic's Magazine*, April 25, 1846, p. 312. To this are appended some editorial remarks in which the novelty of the invention was at that date disputed. Those who care to follow the subject further may find a letter of mine replying to this editorial criticism in the *Mechanic's Magazine* of May 9, 1846, p. 348.

I constructed a large battery and otherwise assisted Mr. Starr in his experiments on this light. The "wick," as Mr. Munro aptly calls it, was a stick of gas retort carbon, like that pictured (NATURE, p. 423), excepting that it was affixed to supports of porcelain in order to remedy the fracture which occurred to our first apparatus in which the carbon stick was rigidly held in metallic forceps. Thus the improvement of M. Kosloff was also anticipated.

The lamp-glass was a thick barometer tube about thirty-six inches long, with its upper end blown out to form a large bulb or expanded chamber. The carbon and its connections were mounted in this with a platinum wire passing through and sealed into the upper closed and expanded end of the tube.

The whole of the tube was then filled with mercury and inverted in a reservoir, and thus the carbon stick, &c., were left in a Torricellian vacuum. The current was passed by connecting the electrodes of the battery with the mercury (into which a wire from the lower end of the carbon dipped) and with the upper platinum wire respectively. A beautiful steady light was produced accompanied with a very curious result which at the time we could not explain, viz., a fall of the mercury to about half its barometrical height and the formation within the tube of an atmosphere containing carbonic acid.

I have now little doubt that this was due to the combustion of some of the carbon by means of the oxygen occluded within itself.

In pointing out this anticipation of M. Lodighin's invention I do not assume or suppose that any piracy has been perpetrated. It is one of those repetitions of the same idea which are of such common occurrence and which cost the re-inventor and his friends a vast amount of trouble and expense that might be saved if they knew what had been done before.

I may add that the result of our battery experiments was to convince Mr. Starr that a magneto-electric arrangement should be used as the source of power in electric illumination; and that he died suddenly in Birmingham in 1846, while constructing a magnetic battery with a new armature which, theoretically, appeared a great improvement on those used at that date. Of its practical merits I am unable to speak.

Twickenham, September 18 W. MATTIEU WILLIAMS

Serpula Parallela

Two or three years ago I read somewhere that *Serpula parallela* of M'Coy is probably a vitreous sponge. Can any of your readers give me a reference for this? I wish to give the authority for this happy suggestion to which Mr. Young and I referred last year. JOHN YOUNG

Glasgow University, September 19

HYDROGRAPHIC SURVEY OF THE BALTIC

WE learn from the Stockholm *Nya Dagligt Allehanda* that during the month of July last a hydrographical survey of the Baltic was carried out by two vessels belonging to the Swedish navy, which were placed for this purpose at the disposal of the Swedish Royal Academy of Sciences for a month. A grant of about 550*l.* is intended to cover the expenses of three such expeditions. The whole of the Baltic, from a line drawn from Arendal to Jutland to the head of the Gulf of Bothnia and from the Swedish coast on the one side to the Finnish, Russian, German, Danish, and Norwegian on the other, was examined for temperature and salinity along thirty-four lines, measuring together more than 23,000 English miles, and including 200 stations. At every such station the temperature and salinity of the sea water were ascertained at the surface and at several different depths down to the bottom, about 1,800 different determinations of temperature having been made and a corresponding number of samples of water obtained. The nature of the bottom has also been ascertained by instruments which brought up samples not only from the surface of the bottom, but also from a variable depth, occasionally several feet, under it. The plan of this survey, which is said to be the most complete that has yet been made for its special objects, the determination of the salinity and temperature, was drawn up and carried out by Prof. F. L. Ekman. New instruments for taking samples of sea-water at different depths were employed, and as the temperature of the water did not undergo any perceptible alteration during the time required for getting it to the surface, for every sample that was obtained, the temperature of the depth from which it was raised was ascertained simultaneously, without any great loss of time. The survey shows the Baltic and the Gulf of Bothnia to consist of three strata, differing greatly in temperature, and often very sharply defined, viz., an upper stratum, which is warmed during the summer by the heat of the sun to a pretty high temperature, a lower, in which the cold of winter still prevailed to a great extent, and under the latter still another of a somewhat higher temperature than the intermediate stratum, the third stratum being of great thickness where the depth was considerable. In the Gulf of Bothnia, as in Skagerack and Kattegat, on the other hand, the temperature diminished steadily in proportion to the depth, as is commonly the case in the ocean. The uppermost summer-warm stratum of water was found to be of variable thickness at different places in the Baltic; at some it was scarcely perceptible at the period of observation. This and other peculiarities will probably be explained in the course of the working out of the observations which is now proceeding.

OUR ASTRONOMICAL COLUMN

THE SATURNIAN SATELLITE HYPERION.—The following ephemeris of this satellite for the next period of absence of moonlight is founded upon the elements calculated by Prof. Asaph Hall, of Washington, from his measures in 1875. Though limited to dates when Saturn may be observed while the moon is absent, probably her presence, except when very near the planet, is less an impediment to viewing so faint an object than the unavoidable proximity of the planet itself.

At 10h. Greenwich M.T.

	Pos.	Dist.		Pos.	Dist.
Sept. 30	261·8	47·8	Oct. 6	277·4	219·5
Oct. 1	270·6	118·6	„ 7	278·8	177·4
„ 2	273·0	176·6	„ 8	281·2	130·8
„ 3	274·3	215·8	„ 9	286·7	76·5
„ 4	275·3	234·1	„ 10	321·0	22·1
„ 5	276·3	232·3			

An ephemeris of the five inner satellites of Saturn, by Mr. Marth, appears in No. 2,154 of the *Astronomische Nachrichten*. It is elaborately compiled, but this the first portion, extending to September 20, only reached this country on the date of its expiration. It is to be regretted that a work of this interest involving so much care and trouble in its preparation, should not have been in the hands of astronomers earlier; it is not the first instance of unfortunate delay in the publication of communications of immediate utility in this periodical of late.

THE NEW COMET (1877, IV.).—A first approximation to the orbit of the faint comet discovered at Marseilles on the 14th inst. calculated by Mr. Hind upon M. Coggia's observation on that date, and observations at Leipsic by

Prof. Bruhns, and Strasburg by Prof. Winnecke on the 17th and 18th respectively, gives the following elements:—

Perihelion Passage, September 6·4104 Greenwich M.T.

Longitude of the Perihelion ...	111 47 45	True equinox,
,, Ascending Node ...	251 45 50	Sept. 16.
Inclination to Ecliptic	78 35 58	
Log. Perihelion Distance	0·198282	

Motion—retrograde.

The middle observation is represented with errors of + 7″ in longitude and − 2″ in latitude.

A few computed positions are subjoined, but they are to be regarded only as rough ones:—

At 12h. G.M.T.	R.A. h. m. s.	N.P.D. ° ′	Distance from the earth.
Sept. 26	8 21 52	45 9·9	1·655
,, 28	8 19 34	45 46·6	1·622
,, 30	8 17 4	46 24·4	1·588
Oct. 2	8 14 21	47 3·2	1·554
,, 4	8 11 24	47 43·2	1·520
,, 6	8 8 13	48 24·8	1·486

According to the above orbit the comet will remain visible for many weeks, approaching the earth, though receding from the sun, as it descends to the node; the intensity of light, however, is not likely to much exceed twice its actual amount. The elements do not present a striking similarity to those of any comet previously computed.

It is the fourth comet newly discovered in the present year, the others having been found (1) by M. Borrelly, February 8; (2) by Prof. Winnecke, April 5; and (3) by Mr. Swift on April 11. D'Arrest's comet of short period, which has been observed on its fourth return, is to be added to these, and as this comet is still in a position to be observed with our larger instruments a few places are subjoined:—

At Paris Noon.

	R.A. h. m. s.	N.P.D. ° ′	Distance from the earth.
Sept. 29	4 51 2	89 56·4	1·425
Oct. 3	4 51 12	90 32·6	1·416
,, 7	4 50 44	91 8·5	1·409
,, 11	4 49 41	91 43·5	1·403
,, 15	4 48 2	92 17·3	1·398

FAMINES AND SHIPWRECKS

THE following letter from Dr. Balfour Stewart appeared in the *Times* of Saturday last:—

I have read with much interest your various articles on Dr. Hunter's researches into the Madras rainfall and the possible connection of famines with sun-spots, and I perceive from the letter in your columns of yesterday by Mr. Henry Jeula, of Lloyd's, that he has found most shipwrecks during periods of maximum solar activity.

I consider it a fortunate thing for science that the physics of the sun are now judged of sufficient importance to occupy the attention of the leading journal, inasmuch as the duty of the man of science is rather quietly to continue investigating than to endeavour to force prominently before the public the results of his work.

It has been recognised now for several years that in this particular case of shipwreck as in others the study of solar physics must ultimately lead to results of national importance. In illustration of this I may quote from a lecture delivered by Mr. Lockyer in October, 1872 ("Solar Physics," p. 423):—

"Mr. Meldrum, a distinguished meteorologist, who lives, not in the temperate zones of the earth, where the meteorological conditions are irregular, but in the torrid zone, where regular meteorological phenomena, and among them cyclones, abound, tells us that it is no longer correct to merely associate cyclones with the tropics. He tells us that the whole question of cyclones is a question of solar activity, and that if we write down in one column the number of cyclones in any given year and in another column the number of sunspots in any given year, there will be a strict relation between them—many sun-spots, many hurricanes; few sun-spots, few hurricanes. Only this morning I have received a letter from Dr. Stewart, who tells me that Mr. Meldrum has since found that what is true of the storms which devastate the Indian Ocean is true of the storms which devastate the West Indies; and, on referring to the storms of the Indian Ocean, Mr. Meldrum points out that at those years where we have been quietly mapping the sun-spot maxima the harbours were filled with wrecks, vessels coming in disabled from every part of the great Indian Ocean. Now that surely is something worth considering, because, if we can manage to get at these things, to associate them in some way with solar activity, so that there can be no mistake about it, the power of prediction—that power which would be the most useful one in meteorology, if we could only get at it—would be within our grasp."

I will, with your permission, make a few remarks on the present position of the problem and on the scientific policy for the future which this position naturally suggests. In the first place, what are the facts? Without the sun, the atmosphere of our earth would be as dead and inactive as a cotton mill without fire in its boiler. As in the case of such a mill, the work done will depend upon the strength of the furnace fire, so that if the fire be variable the work will vary with it. As regards our earth, we know, to begin with, of two distinct periods of meteorological variation. The one of these is due to the change of apparent position of the sun in the heavens caused by the earth's rotation, and its length is one day. The other is due to a similar change caused by the earth's revolution, and its length is one year. If the sun were intrinsically constant we should not be justified in looking for any other variation (unless we attribute some influence to the moon); but if the sun be variable in its power we are led to look for a corresponding variation in terrestrial activity. Now we know that the sun is a variable factor. At certain periods his disc is absolutely free from spots, while at others it is studded with these curious objects. On these last occasions we have other lines of evidence, which lead us to believe in the intense activity of the sun, or, in the forcible language once used by the late Sir J. Herschel to myself, the solar pot seems then to be boiling very rapidly. If we are to reason by analogy at all, the terrestrial pot must follow the solar one, and occurrences denoting a deficiency of energy, such as periodical famines, depending on local failures of vegetable food, will be grouped round years of deficient solar activity, while other occurrences, depending on great energy, such as cyclones and shipwrecks, will rather follow the periods of maximum solar activity. I have taken two instances (famines and cyclones), but I might have taken others of a more recondite nature, such as the phenomena of terrestrial magnetism, concerning which I believe I am justified in stating that those who work at this branch of science are satisfied that a decided effect has been traced to the variability of the sun as a cause.

It is quite right that men of science who are pursuing other branches of inquiry, that statesmen who have to utilise the teachings of science for the benefit of nations, and, permit me to add, editors of powerful journals like your own, should wish that the proof might be of a more absolutely convincing kind than that which has satisfied the workers that they are on the right way. At present the problem has not been pursued on a sufficiently large scale or in a sufficient number of places. In fine, if the attack is to be continued, the skirmishers should give way to heavy guns, and these should be brought to bear without delay now that the point of attack is known.

There are, of course, two ways of treating the problem. The evidence may be pronounced insufficient and the workers stopped, when the research will gradually die out from want of support. On the other hand, if, as you, Sir, admit, we are on a track of a discovery which will in time enable us to foretell the cycle of droughts, public opinion should demand that the investigation be prosecuted with redoubled vigour and under better conditions.

My object in writing this letter is to express a hope that your powerful influence may not be wanting in furtherance of a branch of inquiry from which I, as an individual worker in it, am undoubtedly of opinion that the greatest national benefit must in time arise. If forewarned be forearmed, then such a research will ultimately conduce to the saving of life both at times of maximum and minimum sun-spot frequency.

Owens College, Manchester, September 20

PROF. ADAMS ON LEVERRIER'S PLANETARY THEORIES

THE following admirable statement concerning Leverrier's more recent work was made on the occasion of the presentation of the gold medal of the Astronomical Society, in February, last year, to him by Prof. J. C. Adams, the president. It will be read with a mournful interest at the present time:—

It is not many years since our medal was awarded to M. Leverrier for his theories and tables of the four planets nearest the sun, viz., Mercury, Venus, the Earth, and Mars. Long before this he had been occupied with the larger planets, but before proceeding further with their theories he found it necessary to establish on solid foundations the theory of the motion of the earth, on which all the rest depend, and this again naturally led him to investigate the theories of the three nearer planets which, with the earth, constitute the inferior portion of the planetary system.

By the comparison of these theories with observations, M. Leverrier was led to two interesting results. He found that in order to bring the theories of Mercury and Mars into accordance with observation, it was necessary and sufficient to increase the secular motion of the perihelion of Mercury, and also the secular motion of the perihelion of Mars.

Hence M. Leverrier inferred that there existed on the one hand, in the neighbourhood of Mercury, and on the other, in the neighbourhood of Mars, sensible quantities of matter the action of which had not been taken into account.

This conclusion has been verified with respect to Mars. The matter which had not been considered, turns out to belong to the earth itself, the mass of which had been taken too small, having been derived from too small a value of the solar parallax. A similar increase of the mass of the earth is indicated by the theory of Venus, and a corresponding increase of the solar parallax is likewise derived from the lunar equation in the motion of the sun.

With respect to Mercury, a similar verification has not yet taken place, but the theory of the planet has been established with so much care, and the transits of the planet across the sun furnish such accurate observations, as to leave no doubt of the reality of the phenomenon in question; and the only way of accounting for it appears to be to suppose, with M. Leverrier, the existence of several minute planets, or of a certain quantity of diffused matter circulating about the sun within the orbit of Mercury.

The results which M. Leverrier had thus obtained from his researches on the motions of the inferior planets added to the interest with which he now entered upon similar researches on the system of the four great planets which are the most distant from the sun. Such researches might furnish information respecting matter hitherto unknown existing in the neighbourhood of these planets. Possibly they might afford indications of the existence of a planet beyond Neptune, and at any rate they would provide materials which would facilitate future discoveries.

As I shall have occasion to explain later on, the theories of the mutual disturbances of the larger planets are far longer and more complicated than those of the smaller, so that all that M. Leverrier had yet done might be almost regarded as merely a prelude to what still remained to be done. Increased difficulties, however, far from deterring, seemed rather to stimulate him to greater exertions.

On May 20, 1872, M. Leverrier presented to the Academy an elaborate memoir, containing the first part of his researches on the theories of the four superior planets, Jupiter, Saturn, Uranus, and Neptune. This memoir contains an investigation of the disturbances which each of these planets suffers from the action of the remaining three. Throughout this investigation the development of the disturbing function, as well as that of the inequalities of the elements is given in an algebraical form, in which everything which varies with the time is represented by a general symbol, so that the expressions obtained hold good for any time whatever. Thus the eccentricities and inclinations, the longitudes of the perihelion and of the nodes are all left in the condition of variables. The mean parts of the major axes, which suffer no secular variations, are alone treated as given numbers.

At the end of the *résumé* of the contents of this memoir, given in the *Comptes Rendus*, M. Leverrier lays down the following almost appalling programme of the work still remaining to be done.

It would be necessary, he says,—

1. To calculate the formulæ, and to reduce them into provisional tables.

2. To collect all the exact observations of the four planets, and to discuss them afresh, in order to refer their positions to one and the same system of co-ordinates.

3. By means of the provisional tables, to calculate the apparent positions of the planets for the epochs of the observations.

4. To compare the observed with the calculated positions, to deduce the corrections of the elliptic elements of the four planets, and to examine whether the agreement is then perfect.

5. In the contrary case, to find the causes of the discrepancy between theory and observation.

Extensive as is this programme, it has already been completely carried out as regards the planets Jupiter and Saturn, and partly so as regards Uranus and Neptune.

Having received from the Academy the most effectual encouragement to pursue his researches, M. Leverrier lost no time in bringing them gradually to completion, so that they might become available for practical use.

Accordingly, on August 26, 1872, he presented to the Academy a memoir containing a complete determination of the mutual disturbances of Jupiter and Saturn, and thus serving as a base for the theories of both these planets, which are closely connected with each other.

Again, on November 11, 1872, he presented his determination of the secular variations of the elements of the orbits of the four planets, Jupiter, Saturn, Uranus, and Neptune. These variations are mutually dependent on each other, and must be treated simultaneously. Their determination consequently involves the solution of sixteen differential equations, which are very complicated in form, and can only be integrated by repeated approximation.

This part of the work forms a necessary preliminary to the treatment of the theory of any one of these planets in particular.

On March 17, 1873, M. Leverrier presented to the Academy the complete theory of Jupiter; and on July 14 in the same year he followed it up by the complete theory of Saturn.

On January 12, 1874, he presented his tables of Jupiter, founded on the theory which has just been mentioned, as compared with observations made at Greenwich from 1750 to 1830, and from 1836 to 1869, and with observations made at Paris from 1837 to 1867.

Again, on November 9, 1874, he presented to the Academy a complete theory of Uranus. Already in 1846, in his researches which led to the discovery of Neptune, M. Leverrier had given a very full investigation of the perturbations of Uranus by the action of Jupiter and Saturn. In the memoir just mentioned he gives a fresh investigation, including a full treatment of the perturbations of Uranus by the action of Neptune.

On December 14, 1874, he presented a new theory of the planet Neptune, thus completing the theoretical part of the immense labours which he had undertaken with respect to the planetary system.

Finally, on August 23, 1875, he presented to the Academy the comparison of the theory of Saturn with observations.

Such is a bare enumeration of the various labours for which our science is already indebted to our illustrious Associate.

That any one man should have had the power and perseverance required thus to traverse the entire solar system with a firm step, and to determine with the utmost accuracy the mutual disturbances of all the primary planets which appear to have any sensible influence on each other's motions, might well have appeared incredible if we had not seen it actually accomplished.

I will now proceed to give a brief outline of the investigations relating to the motions of the four larger planets, with which we are now more particularly concerned. The most important parts of these investigations are printed in full detail in the volumes of *Memoirs* which form part of the *Annals of the Observatory of Paris*.

As in his former researches, M. Leverrier here also exclusively employs the method of variation of elements, and the investigations are based on the development of the disturbing function given by him, in the first volume of the *Annals of the Paris Observatory*, with greater accuracy and to a far greater extent than had ever been done before.

The eighteenth chapter of M. Leverrier's researches, which forms nearly the whole of the tenth volume of the *Memoirs*, is devoted to the determination of the mutual action of Jupiter and Saturn, which forms the foundation of the theories of these two planets.

These theories are extremely complicated, and I shall endeavour briefly to point out, and to explain as far as I can without the introduction of algebraical symbols, the nature of the peculiar difficulties which M. Leverrier has had to encounter in their treatment, and which he has so successfully overcome. These difficulties either do not present themselves at all, or do so in a very minor degree in the theories of the smaller planets.

First, then, the masses of Jupiter and Saturn are far larger than those of the inferior planets, the mass of Jupiter being more than 300 times and that of Saturn being nearly 100 times greater than the mass of the earth. For this reason it is necessary to develop the infinite series in which the perturbations are expressed to a much greater extent when we are dealing with Jupiter and Saturn than when we are concerned with the mutual disturbances of the inferior planets. Also Jupiter and Saturn are so far removed from these latter planets that the disturbances which they produce in the motion of these planets are extremely small, in spite of the large masses of the disturbing bodies.

But the great magnitude of the disturbing masses is far from being the only reason why the theory of the mutual disturbances of Jupiter and Saturn is so complicated.

Another cause which aggravates the effect of the former is the near approach to commensurability in the mean motions.

Twice the mean motion of Jupiter differs very little from five times that of Saturn. In other words, five periods of Jupiter occupy nearly the same time as two of Saturn, so that if at a given time the planets were in conjunction at certain points in their orbits, then after three synodic periods they would be again in conjunction at points not far removed from their positions at starting. Hence, whatever uncompensated perturbations may have been produced in the motions of the two planets during these three synodic periods will be very nearly repeated in the next three synodic periods, and again in the next three, and so on.

Hence the disturbances will go on accumulating in the same direction during many revolutions of the two planets, and will become very important. The inequalities of long period thus arising will affect all the elements of the orbits of the two planets; but the most important are those which affect the mean longitudes of the bodies, since these are proportional to the square of the period of the inequalities, whereas the inequalities affecting the other elements are proportional to the period itself.

The principal terms of the inequalities of mean longitude are of the third order, if we consider the excentricities of the orbits and their mutual inclination to be small quantities of the first order.

Terms of the same period, however, and those far more numerous and more complicated in expression, occur among those of the fifth and of the seventh order of small quantities, and M. Leverrier has included these terms also in his approximations.

But the circumstance which contributes in the highest degree to cause the superior complexity of the theories of the larger planets is the necessity, in their case, of taking into account the terms which depend on the squares and higher powers of the disturbing forces.

I will endeavour to point out the nature of these terms and the manner in which they arise.

By the theory of the variation of elements we are able to express at any given time the rate of variation of any one of the elements in terms of the mean longitudes and the elements of the orbits of the disturbed and the several disturbing bodies. If this rate of variation were given in terms of the time and known quantities, we should at once find the value of the element for any given time by a simple integration. But this is not the case.

The method of variation of elements gives us not a solution, but merely a transformation of our original differential equations of motion. The rates of variation are given in terms of the unknown elements themselves; and in order to find the elements from the equations so formed, we must employ repeated approximations.

Let us consider this matter a little more particularly.

The terms which express the rate of variation of any element may be divided into two classes—

1. Those which involve the mean longitudes of one or both of the planets concerned, as well as the elements of their orbits.

2. Those which involve the elements only.

The first are called periodic terms, since they pass from positive to negative, and *vice versâ*, in periods comparable with those of the planets themselves. The second are called secular terms, and vary very slowly, since the elements on which they depend do so. Each of the terms in the expression of the rate of variation of any element will involve the mass of one of the disturbing bodies as a factor. Hence, if all these masses be very small, all the periodic inequalities of the elements will be likewise very small, and we shall obtain a value of the rate of variation which is very near the truth if we substitute for the complete value of any element its value when cleared of periodic inequalities. Then the periodic inequalities in the element under consideration may be found by direct integration, supposing the elements to be constant in the terms to be integrated, and the mean longitudes only to vary. Also the secular variation of the element considered, that is, the rate of variation of the element when cleared of periodic inequalities, will be given by the secular terms taken alone. If the disturbing masses, however, are not very small, this process is not sufficiently accurate, and the periodic inequalities thus found can only be regarded as a first approximation to the true values. In order to find more correct values, we must substitute for the elements in the second member of the equation their secular parts augmented by the approximate periodic inequalities before found.

Now, if in any periodic term we increase any element by a periodic inequality depending on a different argument, that is, involving different multiples of the mean longitudes, the result will evidently be to introduce new periodic terms which will involve the square of one of the masses or the product of two of them as a factor. Similarly, if in any periodic term any element be increased by a periodic inequality depending on the same argument, the result will also introduce new terms of the second order which do not involve the mean longitudes, and which therefore constitute new secular terms. These will be particularly important if the inequality in question be one of long period. Also in the secular terms the result of increasing any element by a periodic inequality will be to introduce a new periodic term depending on the same argument. Lastly, it should be remarked that in finding the periodic inequalities of any element by integration of the corresponding differential equation, we must take into account the secular variations of the elements which were neglected in the first approximation. The new terms thus introduced, like the others which we have just described, will evidently be of the second order with respect to the masses.

If the disturbing masses be large, as in the case of the mutual disturbances of Jupiter and Saturn, it may be necessary to proceed to a further approximation, and thus to obtain new terms, both periodic and secular, which involve the cubes and products of three dimensions of the masses. The number of combinations of terms which give rise to these terms of the second and third orders is practically unlimited, and the art of the calculator consists in selecting those combinations only which lead to sensible results. This is the chief cause of the great complexity of the theories of the larger planets, and more especially of those of Jupiter and Saturn.

M. Leverrier lays it down as the indispensable condition of all progress that we should be able to compare the whole of the observations of a planet with one and the same theory, however great may be the length of time over which the observations extend. In order to satisfy this condition, he develops the whole of his formulæ algebraically, leaving in a general symbolical form all the elements which vary with the time, such as the excentricities, the inclinations, and the longitudes of the perihelia and nodes. He treats in the same way the masses which are not yet sufficiently known.

All the work is given in full detail, and is divided as far as possible into parts independent of each other, so that any part may be readily verified. All the terms which are taken into account are clearly defined, so that if it should ever be necessary to carry on the approximations still further, it will be easy to do so without having to begin the investigation afresh. The whole work is presented with such clearness and method as to make it an admirable model for all similar researches.

After the development of the disturbing functions, and the formation of the differential equations on which the variations of the elements depend, the first step to be taken is to determine by integration of these equations the periodic inequalities of the elements of the orbits of Jupiter and Saturn which are of the first order with respect to the masses. As we have already said, the expressions of these periodic variations of the elements are given with such generality that, in order to obtain their numerical values at any epoch whatever, it is sufficient to substitute the secular values of the elements at that epoch. The calculation of the various terms under this general form is very laborious, and it requires great and sustained attention in order to avoid any error or omission of importance. On the other hand, by substituting from the beginning the numerical values of the elements at a given epoch, the calculation is rendered much shorter and admits much more readily of verification; but the result thus obtained only holds good for the given epoch, and is thus entirely wanting in generality.

In the determination of the long inequalities of Jupiter and Saturn, the approximation is carried to terms which are of the seventh degree with respect to the excentricities and the mutual inclination of the orbits. In the next place the terms of the first order in the secular variations of the elements of the orbits are determined. After this the periodic inequalities of the second order with respect to the masses are considered. These are determined in the same form as the terms of the first order, in order that their expressions may hold good for any epoch whatever. The formulæ relating to these terms are necessarily very complicated. The coefficient belonging to a given argument depends, in general, on a great number of terms which are classed methodically. Next are determined the terms of the second order in the secular variations of the elements of the orbits. Afterwards, M. Leverrier takes into account the influence of the secular inequalities on the values of the integrals on which the periodic inequalities depend. The last part of this chapter is devoted to the completion of the differential expressions of the secular inequalities by the determination of certain secular terms in the rates of variation of the excentricities and the longitudes of the perihelia, which are of the third and fourth orders with respect to the masses.

(*To be continued.*)

NOTES

We record with sincere regret the death of Prof. Alphonse Oppenheim, at Hastings, on the 17th inst.; he died by his own hand through grief at the death of his wife. Prof. Oppenheim is well known for his numerous researches in organic chemistry. Formerly one of the professors of chemistry at the University of Berlin, he only a few months ago, as we recorded at the time, had accepted the chair of chemistry at the University of Münster, in Westphalia. Prof. Oppenheim was a frequent contributor to this journal, and was much esteemed by a large circle of friends in England.

The death is announced, on the 17th inst., at the age of seventy-seven, of Mr. W. H. Fox Talbot, F.R.S., the inventor of the photographic process known as Talbotype, a name latterly merged in the general name photography. Mr. Talbot was a man of varied attainments and manifold work. He was educated at Harrow and Cambridge, where he distinguished himself as a Greek scholar. He took a delight in chemistry, and it was in 1833 that he seems to have conceived the idea of inventing some process by which the beautiful pictures exhibited in a camera lucida could be impressed and rendered permanent. He and Daguerre seem to have brought their several processes to a satisfactory result almost simultaneously, though Daguerre was the first to announce his process, in 1839. Mr. Talbot lost no time in communicating to the Royal Society the details of his own process, though it was not till 1840 that he made the discovery which "laid the foundation of the photographic art in its present form." In 1842 Mr. Talbot was presented with the gold medal of the Royal Society. He did not patent his discovery, but on account of its great value and many uses, freely gave the benefit of it to the public. In subsequent years Mr. Talbot published various modifications and applications of his process, but latterly he had turned his attention to quite a different field, publishing various works on antiquarian, classical, and linguistic subjects.

WE have received from our correspondent a full report of the meeting of the German Association at Munich, with important addresses by Prof. Haeckel, "On the Evolution Theory at the Present Day," and by Prof. Nägeli, on the "Limits of Natural Knowledge." Pressure on our space compels us to defer this report till next week.

PROF. J. E. HILGARD, assistant in charge of the United States Coast Survey, has been offered the directorship of the new International Bureau of Weights and Measures in Paris. Prof. Hilgard is one of the excursion party which includes Sir J. D. Hooker, Prof. Asa Gray, and Dr. Hayden.

THE second biennial meeting of the International Congress of Americanists for the discussion of all matters relating to American archæology, philology, ethnology, and pre-Columbian antiquities generally, was held at Luxemburg on September 10–13. There was a numerous attendance of delegates from all parts of the world. Many papers were contributed to the Congress. Dr. Leemans, Prof. Leon de Rosny, Abbé Pipart, and M. Madier de Montjan, read papers upon primitive American civilisation, and especially picture-writing and hieroglyphics. Several Americans sent communications relative to the mound-builders of the Mississippi valley and the Pueblo Indians of New Mexico; amongst these Messrs. Gillman, of Detroit, Michigan (so well known for his discoveries amongst the burial mounds there), Force, of Cincinnati, and Moody, of Illinois, may be specially mentioned. Dr. Rink contributed a valuable paper upon the primitive habitat of the Esquimaux, maintaining, in opposition to the usual belief, that they came from the interior of America. Messrs. Hyde Clarke and F. A. Allen, of London, contributed essays upon the wider aspect of the question, seeking to trace the civilisation of the primitive races of the New World to a fountain-head in Asia. A valuable paper from M. Lucien Adam furnished a detailed analysis of the grammar of sixteen Indian nations ranging from Lake Athabasca to the Llanos of Brazil. It was resolved by the Congress to memorialise the South American Governments to take steps to preserve authentic records of the language and customs of all small Indian tribes likely soon to become extinct. Throughout the Congress great interest was manifested by the inhabitants in the important subjects discussed, and the visitors were most hospitably entertained by the burgomaster and municipality at a final banquet on Thursday, the 13th, upon which occasion congratulatory telegrams were received from the King of the Netherlands and from Prince Henry, the Governor of the Grand Duchy. It has been decided to hold the third Congress in 1879, at Brussels, when it is hoped the attractiveness and convenience of the locality will induce a larger attendance. The proceedings will be published as soon as possible, and are expected to fill three volumes of 600 pages each.

THE value of the work accomplished by Mr. Stanley, who reports himself on August 10 from Emboma, near the mouth of the Congo, will be universally acknowledged, and there can be but one opinion as to the rank he will hold among geographical explorers. He has solved one of the few great geographical problems which remained for solution, and has performed a feat which baffled even Livingstone's patient genius. Both Livingstone and Cameron had to turn away from Nyangwe on the Lualaba, in Manyuema, foiled in their desire to descend the mysterious river; had Stanley been equally scrupulous no doubt the also would have had to submit to defeat. Determined, however, to trace the course of the river or meet with the fate of Park, he tells us that partly by marching along the banks, partly by sailing down the river, he traced the course of the Lualaba, changing its name "scores of times," almost direct north from Nyangwe to 2° N. lat., where it turns north-west, then west, then south-west, until as it approaches the Atlantic coast it becomes known as the Kwango or Zaire. Many cataracts had to be passed, and at one of the last of them the remaining one of the Pocock brothers was drowned. The breadth of the stream, Mr. Stanley states, varies from two to ten miles, and in some parts is choked with islands. If we consider Webb's Lualaba as the main stream, then its origin must be regarded as the Chambeze rising to the west of Lake Nyassa, and under many names flowing thence through Lake Bangweola, northwards through Lake Moero, Kowamba, and the reported Kamolondo by Nyangwe to at least 2° N. lat., and thence south-west to the Atlantic Ocean—a course with all its windings, not far short of 3,000 miles. Its basin will thus be included between 32° E. and the west coast of Africa, and 12° S. and 2° N. lat. Its affluents are many, some of them very large. There is the Western Lualaba with its many tributaries, probably the Casai, also with numerous affluents, and very possibly even the Ogovai may be an offshoot from the lower Congo. Between 26° and 17° E. the river has an uninterrupted course, descending thence by about thirty falls and rapids to the great river between the falls of Yellala and the Atlantic. Livingstone heard of a large lake with many islands many miles to the north of Nyangwe, and this may simply be one of the ten-mile wide stretches referred to by Mr. Stanley. Further details will be anxiously looked for, but with our present information we must regard the Congo as one of the largest and most important rivers on the globe. It seems clear that Livingstone was mistaken in connecting the Lualaba with the Nile system. The conduct of Mr. Stanley's expedition it is not our business to criticise; but it seems clear that unless we were prepared to wait for an indefinite period the solution of this important problem and the opening up of undiscovered Africa to commerce and science and civilisation, some pioneer must sooner or later have forced his way through the tribes along the route taken by Stanley. This addition to knowledge has been achieved with much suffering and loss of life, though it seems probable that the many "battles" reported to have been fought may turn out to have been exaggerated in their details. Mr. Stanley was to proceed from Emboma to Cabinda, and thence to St. Paul de Loanda, so that we may soon expect to be able to welcome him home.

IN speaking of the famine in Madras the *Times* Madras correspondent, under date August 29, writes as follows:—"I have not seen Mr. Pogson, the Government astronomer, very lately, but I am informed that he has indicated to the Government the probability of the coming north-east monsoon being a failure also, as the intensity of the solar heat continues unabated. If this be so it is quite impossible to say what the subsequent months will bring forth. The possibility of a great catastrophe such as the failure of seasonal rains at the end of two seasons of scarcity and famine is too horrible to contemplate; but it is in accordance with the history of former famines and the conclusions of scientific men, that rainy seasons in the tropics should be abnormal under the influence of the intense solar heat and the absence of 'spots' on the sun." It is a pity that positive statements like this should be published without reference to any data on which they are based. Had observations on the monsoons been carefully made, tabulated, and worked out for many years past, it would be possible to predict with something like certainty the character of the coming monsoon.

DR. MATTHEWS DUNCAN, of Edinburgh, is to succeed Dr. Greenhalgh at St. Bartholomew's.

DR. WILLIAM STIRLING has been appointed to the chair of physiology in the University of Aberdeen.

THE *American Journal of Pure and Applied Mathematics*, the New York *Nation* states, will appear quarterly, beginning with January, 1878. The form will be quarto, and 384 pages will constitute a volume. The associate-editor in charge is Dr. W. E. Story, Johns Hopkins University, Baltimore.

AT the Social Science Congress which has been meeting in Aberdeen during the past week, there were very few papers of strictly scientific interest. Among papers in the Educational Section was one by Prof. Bain on Competitive Examination for Public Appointments. In their choice of subjects the Civil Service Commissioners had, he remarked, been guided by the received branches of education in the college and schools, but after an inquiry into the essential nature of the subjects, he arrived at the conclusion that the sciences and not the languages were the proper subjects for competition. Other languages than our own were only of secondary utility. He expressed surprise at our intense conservatism in the matter of languages. There were according to him three great regions of study that should be fairly represented by every successful candidate—first, the sciences as a whole; secondly, English composition; and thirdly, institutions and history, with perhaps literature. These he would fix as a minimum. Sir Alexander Grant, principal of the Edinburgh University, read a paper on the Best Means of Securing a High Standard of Education. He considered a revision of the code, in order to remove the inequality in which classics and mathematics stood in relation to science in the "specific subjects," and a reconstruction of the normal school system to be necessary. Dr. Brown, of Haddington, read a paper in which he advocated the establishment of schools of forestry in Great Britain, in view of the fact that all candidates for admission to the department of the Indian Civil Service which had to deal with this matter, had to pass an examination which they at present could only qualify themselves for by going to France or Germany for the instruction. Something of this kind was being attempted in connection with the botanic gardens of the Edinburgh University, where ground had now been acquired for an arboretum.

SOME of our readers may like to know that, as might have been expected, the three rhinoceroses now exhibited in the Alexandra Park are specimens of the African Black Rhinoceros (*Rhinoceros bicornis*). This species is extremely uncommon in menageries, and we have heard of no other in this country except the fine adult male now living in the Zoological Society's Gardens in Regent's Park. The three specimens above referred to are all young, a pair being about eighteen months old, and the other a male not more than a year old. In the larger specimens the posterior horn is much smaller than that upon the nose, whilst in the young male its existence is only indicated by a slight rugosity. The late development of the posterior horn is of particular interest, as it shows that the growth of this dermal appendage is a secondary phenomenon, which makes it not surprising that there may be causes which result in it attaining a greater size than usual, as it does in the so-called distinct species, *R. keitloa*, in which the only characterising feature is its large posterior horn.

IT is perhaps a fortunate thing that our great politicians, like the Chancellor of the Exchequer and Mr. John Bright, are beginning to concern themselves in their public addresses with science as well as art. With reference to Mr. Bright's recent address, as the *Times* remarks, if his hearers complain tha they have not been told much about either science or art, we can only say that we agree with them, and that we deplore our common loss. In the coming time it is to be hoped that public speakers, like Mr. Bright, will know better what science really is than they seem to do now.

IT is stated that the Italian Government has authorised two officers of the Royal Navy to take part in the Polar expedition which the Swedish Government is fitting out.

THE *Gaulois* states that M. Duruof, the balloonist, has been engaged by the Russian Government to organise an aëronautical service for the Danube army.

THE last field meeting of the Woolhope Naturalists' Field Club for the year will be held at Hereford, for a foray among the funguses, on Thursday, October 4. M. Maxime Cornu, of Paris, is expected to be present. An exhibition of funguses, apples, and pears will be held in the museum room at the Free Library. The fungus foray will be made on the Whitfield Lawns, by the kind permission of the Rev. Archer Clive. Carriages will leave the Free Library at 10 A.M., to return there by 3.30. A meeting of the members will be held on the return, in the Woolhope Room, for the election of officers for the ensuing year, and for the transaction of the ordinary business of the club. After dinner, or in the course of the evening, the following among other papers will be given:—A Report on the Progress of Mycology during the Year, by Dr. Bull; a Report on the Progress of "The Herefordshire Pomona," by the Rev. C. H. Bulmer; "On a Fossil Fungus (*Pythium*) with Zoospores *in situ*, belonging to the Palæozoic Epoch," by Worthington G. Smith, F.L.S.; and if time permit, a paper "On the Mosses of Herefordshire," by the Rev. Augustin Ley.

AT a meeting of the Linnean Society of New South Wales, on March 26, 1877, Mr. E. P. Ramsay read a "Note of a Species of Echidna (*Tachyglossus*) from Port Moresby, New Guinea," in which he described a fine and apparently full-grown male Echidna from that locality, applying to it the specific name *lawesi*, after its discoverer, Mr. Lawes, who had given the specimen to the Museum at Sydney. Mr. Ramsay's description has been published in the *Proceedings* of the above-named Society, and is accompanied by a plate representing the head and forepart of the animal and one of the hind feet, of the natural size. Unfortunately no diagnosis is given whereby the differences between this New Guinean form and the two long-known species of Australia and Tasmania are made plain; but as that gentleman is doubtless familiar with both of them, we may take his word for it that *Tachyglossus lawesi* is a good and distinct species. Its distinctness from the other New Guinean form, *T. bruijni*, is manifest.

IT has been proposed by a correspondent of the *New York Tribune* to give the names of Romulus and Remus to the two satellites of Mars.

WE understand that the Council of the Working Men's College, Great Ormond Street, have arranged for the ensuing session a series of lectures in connection with the Science and Art Department upon Human Physiology. The lectures will be delivered on Friday evenings by Mr. Thomas Dunman, and will commence on October 5.

AT the meeting of the Birmingham Natural History Society on the 18th inst. Mr. W. R. Hughes, F.L.S., gave some account of the recent dredging excursion of the Society to Arran. He described how the idea of such an excursion took shape, and gave an interesting account of the numerous finds of the party, mainly in Lamlash Bay, where, of course, it was not to be expected that anything new was to be found. Still, many of th forms obtained were of great interest, and the members present gained much solid instruction by being able to examine specimens fresh from their native habitat. Other societies would do well to imitate this enterprising Birmingham association; indeed it might not be a bad idea for several societies to club together and carry out a similar excursion on a more extended scale. Dr. Marshall described the echinoderms, molluscs, annelids, and crustaceans taken.

PROF. PALMIERI has noted for the present year great anomalies of temperature. The degree of heat observed at the Vesuvius Observatory is unprecedented, having reached 34° C., and the mercury has fallen as low as – 7° C. This low temperature has never been reached once before, even in January and February, in the twenty-five years during which the observatory has been established.

THE Emperor of Brazil has formed a commission charged with the determination of geographical positions in the empire, and the first work of this commission is just published. It contains an account of the determination of the longitude and latitude of Barra de Pirahy. Geodesic operations are continued for localities situated on the prolongation of the Santos railway, and also on the parallel (10° in length) destined to join Rio to the great meridian of the empire, which will be measured by the commission.

Die Natur of September 17 contains an interesting collection of some of the myths and stories which constitute the folk-lore of the Australian aborigines.

IN the Anthropological Section of the Havre meeting of the French Association M. Gustave Lagneau exhibited an ethnographic map of France, on which he has attempted to indicate, in accordance with historical and ethnographical data, the division, juxtaposition, superposition and mixture of the various ethnical elements which have contributed to the formation of the present population of the country.

OF the many natural history societies in the United States but one, so far as is known, is composed almost entirely of Germans, the proceedings of which are published in the German language. This is the Naturhistorisches Verein, of Milwaukee, Wisconsin, of which the annual report for 1876–77 has just been published. This society is organised in five sections—zoology, botany, mineralogy, geology, and ethnology—holds regular meetings, and has quite a large active membership.

A GENERAL inventory has been taken by the French ministry of all the public libraries of France. More than 200 towns have been found to possess each a library numbering from 10,000 to 20,000 volumes.

A SWEDISH paper just received publishes an interesting article under the heading, "Why is the Climate of Europe growing Colder?" The article states that in the Bay of Komenok, near Koma, in Greenland, fossil and very characteristic remains of palm and other trees have been discovered lately, which tend to show that in these parts formerly a rich vegetation must have existed. But the ice period of geologists arrived, and, as a consequence of the decreasing temperature, this fine vegetation was covered with ice and snow. This sinking in the temperature, which moved in a southerly direction, as can be proved by geological data, *i.e.*, the discovery of fossil plants of certain species, seems to be going on in our days also. During the last few years the ice has increased far towards the south; thus between Greenland and the Arctic Sea colossal masses of ice have accumulated. On European coasts navigators now frequently find ice in latitudes where it never existed before during the summer months, and the cold reigning upon the Scandinavian peninsula this summer results from the masses of ice which are floating in the region where the Gulf Stream bends towards our coasts. This is a repetition of the observations made in the cold summer of 1865. The unaccustomed vicinity of these masses of ice has rendered the climate of Iceland so cold that corn no longer ripens there, and the Icelanders, in fear of a coming famine and icy climate, begin to found a new home in North America.

PROF. NORDENSKJÖLD'S voyages seem to have been of service in opening up a sea-route to Siberia for commerce. A vessel belonging to M. Sidoroff, Capt. Schwanenberg, arrived at Vardö on September 16, after a passage of twenty-one days from the mouth of the Yenisei; and the steamer *Trazer*, belonging to M. Sibiriakoff, Capt. Dahlmann, which sailed from Bremen on July 28 for the mouth of the Yenisei, returned to Hammerfest on September 24.

A FIRE in Washington has destroyed the greater part of the Patent Office Museum, with thousands of patent models, many of great value.

WE notice among Messrs. Churchill's announcements for the forthcoming season: "A Handbook of Analysis of Water, Air, and Food, for the Medical Officer of Health," by Cornelius B. Fox, M.D., M.R.C.P., Medical Officer of Health for Central, East and South Essex; "Parasites: an Introduction to the Study of the Entozoa of Man and Animals, including some Account of the Ectozoa," by T. Spencer Cobbold, M.D., F.R.S., F.L.S., Professor of Helminthology in the Royal Veterinary College; and a "Student's Guide to the Anatomy of the Joints," by Henry Morris, M.A., M.B., F.R.C.S., Assistant-Surgeon to and Lecturer on Anatomy at the Middlesex Hospital.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseoviridis*) from West Africa, a Nisnas Monkey (*Cercopithecus pyrrhonotus*) from Nubia, presented by Mr. W. D. James; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. W. W. Stead; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. J. F. Greenwood; a Capybara (*Hydrochœrus capybara*) from South America, presented by Mr. W. Smith; a Peregrine Falcon (*Falco peregrinus*), European, an African Buzzard (*Buteo tachardus*) from Africa, presented by the Rev. W. Willimott; a West African Python (*Python sebæ*), a Royal Python (*Python regius*) from West Africa, presented by Mr. J. J. Kendall; a Goffin's Cockatoo (*Cacatua goffini*) from the Fiji Isles, an Ariel Toucan (*Ramphastos ariel*), a Maximilian's Aracari (*Pteroglossus wiedi*), two Blue-bearded Jays (*Cyanocorax cyanopogon*), two West Indian Rails (*Aramides cayennensis*) from South America, deposited; two Upland Geese (*Bernicla magellanica*) from the Straits of Magellan, three Andean Geese (*Bernicla melanoptera*), two Slaty Coots (*Fulica ardesiaca*) from Peru, purchased; an Axis Deer (*Cervus axis*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), born in the Gardens.

THE DIRECT PROCESS IN THE PRODUCTION OF IRON AND STEEL[1]

IN mixing comparatively rich iron ore in powder, with about twenty-five per cent. of its weight of pounded coal, and in exposing this mixture for some hours to the heat of a common stove or of a smith's fire, metallic iron is formed, which, on being heated to the welding point, on the same smith's hearth, may be forged into a horse-shoe of excellent quality. The admixture with the ore of some fluxing materials, such as lime or clay, will, in most cases, be of advantage to rid the iron of adherent slag.

The simplicity of this process is such that it naturally preceded the elaborate processes now in use for the production of iron and steel upon a gigantic scale, nor can it surprise us to find that attempts have been made from time to time down to the present day, to revert to the ancient and more simple method. It can be shown that iron produced by direct process is almost chemically pure, although the ores and reducing agent employed may have contained a considerable percentage of phosphorus,

[1] Some Further Remarks regarding the Production of Iron and Steel by Direct Process. Paper read at the Newcastle Meeting of the Iron and Steel Institute, by C. William Siemens, D.C.L., F.R.S., President.

sulphur, and silicon, and that, if freed from its adherent slag, it furnishes a material superior in quality and commercial value to the ordinary iron of commerce.

The practical objections to the direct process, as practised in former days, and as still used to a limited extent in the United States of America and in some European countries, are that—

1. Very rich ores only are applicable, of which about one-half is converted into iron, the remainder being lost in forming slag.

2. The fuel used is charcoal, of which between three and four tons are used in producing one ton of hammered blooms.

3. Expenditure of labour is great, being at the rate of thirty-three men, working twelve hours, in producing one ton of metal (see Percy). Iron produced by direct process in the Catelon forge is therefore expensive iron, and could not compete with iron produced by modern processes except for special purposes, such as furnishing melting material for the tool steel melter.

But, it may be asked, could the advantages of the direct process not be combined with those of modern appliances for the production of pure and intense heats, and for dealing with materials in large masses, without expenditure of manual labour, and cannot chemistry help us to larger yields and the faculty of using comparatively poor and impure ores?

A careful consideration of these questions led me to the conclusion, some years ago, that here was a promising field for the experimental metallurgist, and that I possessed some advantage over others in the use of the regenerative gas furnace as a means of producing the requisite quality of heat without the use of charcoal and blowing apparatus. I engaged, accordingly, upon a series of experimental researches at my sample steel works, at Birmingham, and, in 1873, I had the honour of submitting the first fruits of these inquiries to the Iron and Steel Institute, in a paper entitled "On the Manufacture of Iron and Steel by Direct Process." Encouraged by the results I had then obtained, I ventured with some others upon some larger applications, the principal one of which has been one at Towcester, in Northamptonshire.

Viewed by the light of present experience, it would have been wiser to have fixed upon another locality with fuel, skilled labour, and better ores within easy reach; but in extenuation of the error committed, it may be urged that the site was fixed by force of circumstances rather than by selection, the chief temptation being an ample supply of small Northamptonshire ore at a very low cost. It was, however, soon discovered that this ore, although capable of producing iron of good quality, was too poor and irregular in quality to yield commercial results unless it was mixed with an equal weight of rich ore, such as pottery mine, Spanish ore, or Rollscale, all of which, as well as the fuel, are expensive at Towcester, owing to high rates of carriage. It is in consequence of these untoward circumstances that the works at Towcester have not been completed by the addition of rolling mills, the intention being to transfer the special machinery ultimately to existing ironworks when the process has been sufficiently matured for that purpose.

The Towcester Works were visited, in the autumn of last year, by two eminent metallurgists, Professors von Tunner, of Leoban, and Akermann, of Sweden, who have published the results of their observations in separate reports.[1] The results noted down by Mr. von Tunner are referred to by our past-president, Mr. I. Lowthian Bell in his paper on the "Separation of Carbon, &c.," which was read in March last, and will be discussed at the Newcastle meeting. The criticisms contained in these publications are conceived in the fairest possible spirit, and form indeed a most valuable record of the progress achieved up to that time, but they furnish me with an inducement to break silence sooner than I had intended, regarding the further progress which has been effected, and the conclusions I am disposed to draw from past experience regarding the direct process of the future.

The leading idea which guided me in these was to operate upon such mixtures of ores, fluxes, and reducing agents as would, under the influence of intense heat, resolve themselves forthwith into metallic iron and a fluid cinder, differing essentially from the methods pursued by Chenat, Guilt, Blair, and others, who prepare spongy metal in the first place by a slow process which is condensed into malleable iron or steel by after-processes, but assimilating to some extent to the process first proposed by Mr. Wm. Clay. In my paper of 1873 I described two modes of effecting my purpose, the one by means of a stationary, and the other by means of a rotative furnace chamber, the former being applicable chiefly where comparatively rich ores are available, and the latter for such poorer ores as occur near Towcester.

[1] Das Eisenhüttenwesen von L. Ritter von Tunner, Wien, 1870.

At the Towcester Works three rotative furnaces have been erected, two of them with working drums seven feet in diameter and nine feet in length, and the third of smaller dimensions. The gas flame both enters and passes away from the back end of the furnace, leaving the front end available for the furnace door, which is stationary. The ends of the furnace chamber are lined with Bauxite bricks, and the circumference with ferrous oxides, resulting from a mixture of furnace cinder enriched with roll scale or calcined blackband in lumps. About 30 cwt. of ore mixed with about 9 cwt. of small coal having been charged into the furnace, it is made to rotate slowly for about two and a half hours, by which time the reduction of the metal should be completed, and a fluxed slag be formed of the earthy constituents containing a considerable percentage of ferrous oxide. The slag having been tapped, the heat of the furnace and the speed of rotation are increased to facilitate the formation of balls, which are in due course taken and treated in the manner to be presently described.

These balls contain on an average seventy per cent. metallic iron and thirty per cent. of cinder, and upon careful analysis it is found that the particles of iron, if entirely separated from the slag, are pure metal, although the slag may contain as much as six per cent. and more of phosphoric acid, and from one to two per cent. of sulphur. In shingling those balls in the usual manner the bulk of the cinder is removed, but a sufficient residue remains to impart to the fracture a dark appearance without a sign of crystalline fracture. The metal shows in being worked what appears to be red shortness, but what should be termed slag shortness. In repiling and reheating this iron several times this defective appearance is gradually removed, and crystalline iron of great purity and toughness is produced, but a more ready mode of treatment was suggested by Mr. Samuel Lloyd, one of my co-directors in the Towcester Company, in reverting to the ancient refinery or charcoal hearth. The balls as they came from the rotator are placed under the shingling hammer and beaten out into flat cakes not exceeding an inch in thickness. These are cut by shears into pieces of suitable size and formed into blooms of about 2 cwt. each, which are consolidated under a shingling hammer and rolled into bars.

The bars have been sold in Staffordshire and Sheffield at prices varying from 7*l.* to 9*l.* per ton, being deemed equal to Swedish bar as regards toughness and purity.

It may therefore be asserted as a matter of fact that iron and steel of very high quality may be produced from ores not superior than Cleveland ores by direct process, but the question remains at what cost this conversion can be effected. The experimental works at Towcester are, unfortunately, not sufficiently complete to furnish more than the elements upon which the question of cost may be determined, the principal reasons being that the one reheating furnace and a 30 cwt. hammer at the works are not sufficient to deal with the iron produced by the three rotators, that the iron has to be finished at a rolling-mill elsewhere, and that transports weigh heavily upon the cost of production. The principal factor in the calculation of cost is unquestionably the rotator. [A table furnishes the working result of eighteen consecutive charges as taken from the charge-book.] The mixture of ore consisted for each charge of 12 cwt. of Towcester ore (containing about 38 per cent. metallic iron) mixed with 8 cwt. of calcined Great Fenton ore, 1 cwt. of tap cinder, 1 cwt. of limestone, and 6½ cwt. of small coal. The time occupied for each charge was three hours fifty-seven minutes, or say four hours, and the yield of hammered blooms was on an average 6 cwt. 2 qrs. 13 lbs., whereas the metal contained in each charge amounted (by estimate) to 9 cwt., showing a loss of 25 per cwt. This loss is, however, partially recovered in using a portion of the cinder again in succeeding charges, but the proportion of cinder that may be used again with impunity depends upon the amount of impurities, namely, of phosphorus, sulphur, and alumina contained in the ore. The coal used in the producers amounted to two tons per ton of hammered blooms produced, and in pricing the materials used and labour engaged upon the work, the table—prepared by the manager at the works—gives 3*l.* 8*s.* as the cost per ton of hammered blooms. To this must be added for repairs and general expenses, and the cost of rolling the hammered blooms into bars, which in the case of Towcester practice are very heavy, but of which an experienced iron-master would form his own estimate. The cost of working the metal in the hollow fires is also not included, and this may be taken to add from 25*s.* to 30*s.* to the ton. The refined iron so produced will, therefore, cost from 5*l.* 5*s.* to 5*l.* 10*s.* per ton.

Other tables give the analysis of irons produced from various descriptions of ores, and Kirkaldy's tests of the mechanical properties of the iron; but it should be understood that these tests were taken with a view rather to test various modes of manufacture than to show high results. Only a small proportion of the samples had been subjected to the refinery process, and the variable percentage of phosphorus may be taken really as indicative of the extent to which the cinder had been removed from the metal.

Another table gives the analysis of slags produced in the process. These are, no doubt, rich in iron, but it must be remembered that in the case of comparatively pure ore they can be used almost entirely in succeeding charges, and that in the case of ores containing much sulphur and phosphorus they are the recipients of those impurities—in the same way as the puddling cinder carries off the same impurities in the puddling furnace—and thus serve a useful end.

If rich ores, such as hematites, are available, it is more advantageous to use a stationary furnace and to modify the process as follows:—

A mixture of pulverulent ore mixed with a suitable proportion of fluxing materials and reducing agent is prepared, and from four to five tons of it is charged from a charging platform into the heated chamber to the depth of some twelve to fifteen inches. But before charging the mixture some coke dust or anthracite powder is spread over the bottom and sides of the chamber to protect the silica lining of the same. The heat of the furnace is thereupon raised to a full welding heat, care being taken that the flame is as little oxidising as possible. The result is a powerful superficial action upon the mixture or batch, causing simultaneous reduction of the ore and fusion of the earthy constituents. In the course of two hours a thick skin of malleable iron is formed all over the surface of the mixture, which, on being withdrawn by means of hooks, is consolidated and cleared of cinder under a hammer, and rolled out in the same heat into rough sheets or bars, to be cut up and finished in the refinery furnace or charcoal hearth. One skin being removed, the furnace is closed again, and in the course of an hour and a half another skin is formed, which, in its turn, is removed and shingled, and so on until, after three or four removals, the furnace charge is nearly exhausted. A fresh charge is then added, and the same operation continued. Once every twelve hours the furnace should, however, be cleared entirely, and the furnace lining be repaired all round.

The shingled metal so produced forms an excellent melting material for the open-hearth or Siemens-Martin process; but if ores both rich and free from sulphur and phosphorus are used, together with roll and hammer scale, which forms an admirable admixture, I simplify the process still further in causing the fusion to take place in the reducing furnace.

The furnace having been charged with say five tons of batch, the heat is allowed to ply on it for four or five hours, when about two tons of hematite pig iron are charged upon the surface by preference in a heated condition. The pig metal on melting constitutes a bath on the surface of the thick metallic skin previously formed, and gradually dissolves it on the surface while it is forming afresh below, and in the course of from three to four hours the whole of the materials charged are rendered fluid, consisting of a metallic bath with a small percentage of carbon, covered with a glassy slag containing about 15 per cent. only of metallic iron. The carbon of the bath is thereupon brought down to the desired point of only about 1 per cent. of carbon and spiegeleisen or ferro-manganese is added, and the metal tapped in the usual manner. By these means the direct process of making cast steel is carried to a further limit than I have been able to accomplish before, and no difficulty has presented itself in carrying it into effect. The steel so produced is equal in quality to that produced by the open hearth process as now practised. If light scrap, such as iron and steel turnings or sheerings, are available, these may be mixed with advantage with the batch to increase the yield of metal.

These are, in short, the more recent improvements in the direct process of producing iron and steel which I have been able to effect, and which I should have been glad to lay before the Iron and Steel Institute in a more complete form than I am able to do at the present time.

THE AMERICAN ASSOCIATION AT NASHVILLE

As we have said already, while the Nashville Meeting of the American Association could not be called a brilliant one, most of the papers read were of substantial importance, and show that a large amount of valuable scientific work is being carried on in the United States. The number of visitors does not appear to have been up to the usual mark, mainly, we believe, on account of the great heat which prevailed at Nashville, but among those present were many of the most prominent men of science in America. The reception by the authorities of the State and city was all that could be desired, and the arrangements as to excursions, entertainments, and public lectures were in every way satisfactory.

The Western Union Telegraph Company, which has a Telegraphic Station in the building where the Association met, tendered the use of its wires free for all members so far as related to domestic affairs.

It is customary at the meetings of the American Association for each of the vice-presidents to give a public lecture; we give a long abstract of the lecture by Prof. O. C. Marsh, the importance of which cannot be overrated. We have already referred briefly to Prof. Pickering's paper on the Endowment of Research. The first obstacle encountered, he said, was the opinion widely maintained, even by scientific men, that the original research of a country was natural, and that it was useless to try to force it. We might as well say that music and art were natural growths. What should we have of ancient art were it not for the encouragement of many ancient rulers? In later days how would art and literature have thrived had it not been for the support of the public in purchasing books, &c. With the man of science it was different. There was generally little or no pecuniary reward for his success. The consequence was he was obliged to engage in some other occupation, generally teaching, which still allowed a little time for research. If these same men were allowed to devote their entire energies to investigation, and were aided by the necessary appliances, far more would be accomplished. The solution of the matter was organisation, the carrying out of a plan by which researches should be rendered as systematic as the process of mechanical arts. They had first the munificent bequest of one of the first presidents of the Association. The income of the Bache fund amounted to 2,000 dols. or 3,000 dols. Second was the Rumford fund, originally intended for giving medals in light and heat, but now largely applied to aiding investigation in these sciences. Besides these were many indirect aids. The paper then gave a plan of an institution for making researches: First, a president; second, a corps of investigators of acknowledged scientific ability; third, a large corps of assistants, whose duty it should be to carry out work laid out for them; fourth, workmen, such as mechanics. He then went on to describe a building such as would be as perfect as possible for the institution. It was useless to hope for architectural beauty, as the effect would be spoiled by attachments made to the exterior. No more common mistake was made than in wasting money which should be used for equipment. They had too many colleges with far too little endowment. Such an institution, added to a college, would prove of great advantage.

At a general evening meeting, Prof. Newcomb (president) spoke at some length, extemporaneously, on the two recent important discoveries made by American men of science, viz., the existence of oxygen in the sun, by Prof. Draper, and the satellites of Mars, by Prof. Hall. At the same evening meeting Prof. A. R. Grote, of Buffalo, read a sketch of a scheme for an international scientific service formed by the union of the various civilised governments and national scientific societies, for the carrying out of such scientific work as all the world is interested in. Under the auspices of such an association "all extra-limital, astronomical, geographical, and biological expeditions would be fitted out and directed to those places which would be most fruitful for the particular purpose."

Of the papers read in the various sections we are able, at present, to give little else but the titles. In Section A, which includes Mathematics, Astronomy, Physics, Chemistry, and Mineralogy, the following, among other papers were read:—*On a New Type of Steam Engine theoretically capable of utilising the full Mechanical Equivalent of Heat Energy, and on some points of Theory indicating its Practicability*, by Prof. R. H. Thurston; *Mechanics of the Flight of Birds*, by Mr. A. C. Campbell. An interesting paper in this section by Prof. Forshey, treated of *The Physics of the Gulf of Mexico and of its Principal Affluent the Mississippi*; the author brought together many important data concerning what he styled "the cis-Atlantic Mediterranean." Another paper in this section by Prof. Mendenhall, was *On Measurement of the Wave-length of the Blue Line of the Indium Spectrum.*

Among papers in Section B (Geology, Zoology, Botany, and Anthropology) we notice the following as likely to prove of importance:—*The Structure of Eruptive Mountains*, by Prof. Powell; *On Sex in Flowers*, by Mr. Thos. Meehan; *On the Original Connection of the Eastern and Western Coalfields of the Ohio Valley*, *On the Continuation of the Fields of the Alleghany Chain to the North of the Delaware River*, and *On the Geographical and Geological Distribution of the Genus Beatricea, and of certain other Fossil Corals in the Rocks of the Cincinnati Group*, all by Prof. Shaler; *On the Classification of the Extinct Fishes of the Lower Types*, and *On the Origin of Structural Variation*, by Prof. Cope; *Notes on the Geology of the Rocky Mountains*, by Prof. Sterry Hunt; *Some Popular Errors concerning the North American Indians*, by Capt. Powell. In a paper by the same author, *On Overplacement*, he asserted that the effects of glacial action had been greatly over-estimated in the western country, and that the "overplacement" in the Mississippi Valley was due rather to the erosion of the atmosphere, the rains of centuries, and the rivers. A curious paper in this section was by a lady, Mrs. H. K. Ingram, *On Atmospheric Concussion as a Means of Disinfection*, in which she confidently advanced the idea, based on the germ-theory of disease, that by means of concussion produced by gunpowder explosion or other effective method, cholera and other epidemic diseases could be effectually prevented or dissipated. In a paper by Lieut.-Col. Mallery, the author held that the Indians are not passing away; there are now in existence, he stated, 300,000 Indians, of whom 50,000 are Sioux. Instead of decreasing with advancing civilisation, they are steadily increasing, and Col. Mallery believes that the native population of America, north of Mexico, at the time of its discovery, has been widely over-estimated. Capt. Powell agreed with Col. Mallery, and stated his conviction that at the time of the discovery of America there were not more than 500,000 natives north of Mexico, while now in the States, Canada, and Alaska there are about 400,000. As president of the Sub-section of Anthropology, Prof. Daniel Wilson gave an interesting address on *Races in America*, presenting a *résumé* of the various theories that had been advanced with respect to American ethnology and the peopling of America, and giving some wise advice as to how future researches ought to be conducted. Another anthropological paper was on the *Origin of the Japanese*, by a native of Tokio, Shuje Isawa, in which the author came to the conclusion that the present Japanese are descended from Hindoo conquerors.

No paper of general importance seems to have been read in permanent Sub-section C (Chemistry), all of them, judging from the titles, being on points mainly of manufacturing interest.

It was decided that the next meeting should be held at St. Louis, and at the closing meeting an Education Committee was appointed with a view to the introduction of science into the schools of the country. Another committee was appointed to report annually on the relations of science to the industrial arts, and the following important resolution was passed in reference to the Signal Service Weather Reports:—

"Resolved, that this Association most respectfully asks the attention of Congress and the country to the great advances in the science of meteorology and in the art of weather prediction, which might be hoped for if the meteorological observations now taken by the Army Signal Office, under the direction of the Secretary of War, were made the subject of special research and discussion by scientific experts.

"Resolved, further, that a committee of five members or fellows be appointed by the President to represent this Association before Congress as petitioners for such permanent and liberal organisation of the meteorological service, that the valuable material collected by it may be utilised in the manner here suggested."

INTRODUCTION AND SUCCESSION OF VERTEBRATE LIFE IN AMERICA[1]

II.

THE reptiles most characteristic of our American cretaceous strata are the *Mosasauria*, a group with very few representatives in other parts of the world. In our cretaceous seas they ruled supreme, as their numbers, size, and carnivorous habits enabled them to easily vanquish all rivals. Some were at least sixty feet in length, and the smallest ten or twelve. In the inland cretaceous sea, from which the Rocky Mountains were beginning to emerge, these ancient "sea serpents" abounded; and many were entombed in its muddy bottom. On one occasion, as I rode through a valley washed out of this old ocean bed, I saw no less than seven different skeletons of these monsters in sight at once. The mosasaurs were essentially swimming lizards, with four well-developed paddles, and they had little affinity with modern serpents, to which they have been compared.

The *Crocodilia* are abundant in rocks of cretaceous age in America, and two distinct types are represented. The tertiary marine beds of the Atlantic coast contain comparatively few crocodilian remains, and all are of modern types, the genus *Gavialis* having one eocene species, and the alligator being represented only in the latest deposits.

It is worthy of special mention in this connection that no true *Lacertilia*, or lizards, and no *Ophidia*, or serpents, have yet been detected in American cretaceous beds; although their remains, if present, would hardly have escaped observation in the regions explored. The former will doubtless be found, as several species occur in the mesozoic of Europe, and perhaps the latter, although the ophidians are apparently a more modern type. In the eocene lake-basins of Western America, remains of lizards are very numerous, and indicate species much larger than any existing to-day.

The first American serpents, so far as now known, appear in the eocene, which contains also the oldest European species.

The *Pterosauria*, or flying lizards, are among the most interesting reptiles of mesozoic time, and many of them left their remains in the soft sediments of our inland cretaceous sea. These were veritable dragons, having a spread of wings of from ten to twenty-five feet.

The strange reptiles known as *Dinosauria*, which, as we have seen, were numerous during the deposition of our triassic shales and sandstones, have not yet been found in American Jurassic, but were well represented here throughout the cretaceous, and at its close became extinct. These animals possess a peculiar interest to the anatomist, since, although reptilian in all their main characters, they show clear affinities with the birds, and have some features which may point to mammals. The cretaceous dinosaurs were all of large size, and most of them walked on the hind feet alone, like modern struthious birds. Near the base of our cretaceous formation in beds which I regard as the equivalent of the European Wealden, the most gigantic forms of this order yet discovered have recently been brought to light One of these monsters (*Titanosaurus montanus*) from Colorado, is by far the largest land animal yet discovered, its dimensions being greater than was supposed possible in an animal that lived and moved upon the land. It was some fifty or sixty feet in length, and, when erect, at least thirty feet in height. It doubtless fed upon the foliage of the mountain forests, portions of which are preserved with its remains. With *Titanosaurus* the bones of smaller dinosaurs, one (*Nanosaurus*) not larger than a cat, as well as those of crocodiles and turtles, are not uncommon. The recent discovery of these interesting remains, many and various, in strata that had long been pronounced by professional explorers barren of vertebrate fossils, should teach caution to those who decline to accept the imperfection of our knowledge to-day as a fair plea for the supposed absence of intermediate forms.

In the marine cretaceous beds of the west only a single dinosaur (*Hadrosaurus agilis*) has been found, but in the higher fresh-water beds which mark the close of this formation their remains are numerous, and indicate several well-marked species, if not genera.

The first appearance of birds in America, according to our present knowledge, was during the cretaceous period, although many announcements have been made of their existence in preceding epochs. The evidence of their presence in the trias, based on footprints and other impressions is at present, as we have seen, without value, although we may confidently await their discovery there if not in older formations. *Archæopteryx*, from the European Jura, the oldest bird known, and now fortunately represented by more than a single specimen, clearly indicates a much higher antiquity for the class. The earliest American forms at present known are the *Odontornithes*, or birds with teeth, which have been exhumed within the last few years

[1] Abstract of a lecture delivered at the Nashville meeting of the American Association, August 30, by Prof. O. C. Marsh Continued from p. 450.

from the chalk of Kansas. The two genera *Hesperornis* and *Ichthyornis* are types of distinct orders, and differ from each other and from *Archæopteryx* much more than do any existing birds among themselves, thus showing that birds are now a closed type, and that the key to the history of the class must be sought for in the distant past.

In *Hesperornis* we have a large aquatic bird, nearly six feet in length, with a strange combination of characters. The jaws are provided with teeth, set in grooves; the wings were rudimentary, and useless, while the legs were very similar to those of modern diving birds. This last feature was merely an adaptation, as the more important characters are struthious, showing that *Hesperornis* was essentially a carnivorous swimming ostrich. *Ichthyornis*, a small flying bird, was stranger still, as the teeth were in sockets, and the vertebræ biconcave, as in fishes and a few reptiles. *Apatornis* and all other allied forms occur in the same beds, and probably all were provided with teeth. It is strange that the companions of these ancient toothed birds should have been pterodactyls without teeth. In the later cretaceous beds of the Atlantic coast various remains of aquatic birds have been found, but all are apparently distinct from those of the west.

During the tertiary period birds were numerous in this country, and all yet discovered appear to have belonged to modern types.

It is now generally admitted by biologists who have made a study of the vertebrates, that birds have come down to us through the dinosaurs, and the close affinity of the latter with recent struthious birds will hardly be questioned. The case amounts almost to a demonstration, if we compare, with dinosaurs, their contemporaries, the mesozoic birds. The classes of birds and reptiles, as now living, are separated by a gulf so profound, that a few years since it was cited by the opponents of evolution as the most important break in the animal series, and one which that doctrine could not bridge over. Since then, as Huxley has clearly shown, this gap has been virtually filled by the discovery of bird-like reptiles and reptilian birds. *Compsognathus* and *Archæopteryx* of the old world, and *Ichthyornis* and *Hesperornis* of the new, are the stepping-stones by which the evolutionist of to-day leads the doubting brother across the shallow remnant of th gulf once thought impassable.

It remains now to consider the highest group of the animal kingdom, the class *Mammalia*, which includes Man. Of the existence of this class before the trias we have no evidence, either in this country or in the old world, and it is a significant fact that at essentially the same horizon in each hemisphere, similar low forms of mammals make their appearance. Although only a few incomplete specimens have been discovered, they are characteristic and well preserved, and all are apparently marsupials, the lowest mammalian group which we know in this country, living or fossil. The American triassic mammals are known at present only from two small lower jaws, on which is based the genus *Dromotherium*, supposed to be related to the insect-eating *Myrmecobius*, now living in Australia.

Although the Jura of Europe has yielded other similar mammals, we have as yet none of this class from that formation; while, from rocks of cretaceous age, no mammals are known in any part of the world.

In the lowest tertiary beds of this country a rich mammalian fauna suddenly makes its appearance, and from that time through the age of mammals to the present, America has been constantly occupied by this type of life in the greatest diversity of form. Fortunately, a nearly continuous record of this life, as preserved, is now accessible to us, and ensures great additions to our knowledge of the genealogy of mammals, and perhaps the solution of more profound problems.

The boundary line between the cretaceous and tertiary in the region of the Rocky Mountains has been much in dispute during the last few years, mainly in consequence of the uncertain geological bearings of the fossil plants found near this horizon. The accompanying invertebrate fossils have thrown little light on the question, which is essentially whether the great lignite series of the West is uppermost cretaceous, or lowest eocene. The evidence of the numerous vertebrate remains is, in my judgment, decisive, and in favour of the former view.

This brings up an important point in palæontology, one to which my attention was drawn several years since, namely, the comparative value of different groups of fossils in marking geological time. In examining the subject with some care, I found that for this purpose plants, as their nature indicates, are most unsatisfactory witnesses; that invertebrate animals are much better; and that vertebrates afford the most reliable evidence of climatic and other geological changes. The sub-divisions of the latter group, moreover, and in fact all forms of animal life, are of value in this respect, mainly according to the perfection of their organisation, or zoological rank. Fishes, for example, are but slightly affected by changes that would destroy reptiles or birds, and the higher mammals succumb under influences that the lower forms pass through in safety. The more special applications of this general law, and its value in geology, will readily suggest themselves.

The evidence offered by fossil remains is, in the light of this law, conclusive, that the line, if line there be, separating our cretaceous from the tertiary, must at present be drawn where the dinosaurs and other mesozoic vertebrates disappear, and are replaced by the mammals, henceforth the dominant type.

It is frequently asserted, and very generally believed, that the large number of huge *Edentata* which lived in North America during the post-pliocene, were the results of an extensive migration from South America soon after the elevation of the Isthmus of Panama, near the close of the tertiary. No conclusive proof of such migration has been offered, and the evidence it seems to me, so far as we now have it, is directly opposed to this view. No undoubted tertiary edentates have yet been discovered in South America, while we have at least two species in our miocene, and during the deposition of our lower pliocene large individuals of this group were not uncommon as far north as the forty-third parallel of latitude, on both sides of the Rocky Mountains. In view of these facts and others which I shall lay before you, it seems more natural to conclude from our present knowledge that the migration which no doubt took place was from north to south. The edentates finding thus in South America a congenial home flourished greatly for a time, and, although the larger forms are now all extinct, diminutive representatives of the group still inhabit the same region.

The ungulates are the most abundant mammals in the tertiary, and the most important, since they include a great variety of types, some of which we can trace through their various changes down to the modified forms that represent them to-day. Of the various divisions in this comprehensive group, the perissodactyle, or odd-toed ungulates, are evidently the oldest, and throughout the eocene are the prevailing forms. Although all of the perissodactyles of the earlier tertiary are more or less generalised, they are still quite distinct from the artiodactyles, even at the base of the eocene. One family, however, the *Coryphodontidæ*, which is well represented at this horizon, both in America and Europe, although essentially *Perissodactyle*, possesses some characters which point to a primitive ungulate type from which the present orders have been evolved. Among these characters are the diminutive brain, which in size and form approaches that of the reptiles, and also the five-toed feet from which all the various forms of the mammalian foot have been derived. Of this family, only a single genus, *Coryphodon* (*Bathmodon*), is known, but there were several distinct species. They were the largest mammals of the lower eocene, some exceeding in size the existing tapirs.

In the middle eocene, west of the Rocky Mountains, a remarkable group of ungulates makes its appearance. These animals nearly equalled the elephant in size, but had shorter limbs. The skull was armed with two or three pairs of horn-cores, and with enormous canine tusks. The brain was proportionally smaller than in any other land mammal. The feet had five toes, and resembled in their general structure those of *Coryphodon*, thus indicating some affinity with that genus. These mammals resemble in some respects the perissodactyles, and in others the proboscidians, yet differ so widely from any known ungulates, recent or fossil, that they must be regarded as forming a distinct order, the *Dinocerata*.

Besides these peculiar mammals which are extinct, and mainly of interest to the biologist, there were others in the early tertiary which remind us of those at present living around us. When a student in Germany some twelve years ago, I heard a world-renowned professor of zoology gravely inform his pupils that the horse was a gift of the old world to the new, and was entirely unknown in America until introduced by the Spaniards. After the lecture I asked him whether no earlier remains of horses had been found on this continent, and was told in reply that the reports to that effect were too unsatisfactory to be presented as facts in science. This remark led me, on my return, to examine the subject myself, and I have since unearthed, with my own hands, not less than thirty distinct species of the horse tribe, in the tertiary deposits of the west alone; and it is now, I think, generally admitted that America is, after all, the true home of the horse.

I can offer you no better illustration than this of the advance vertebrate palæontology has made during the last decade, or of the important contributions to this progress which our Rocky Mountain region has supplied.

The oldest representative of the horse at present known is the diminutive *Eohippus* from the lower eocene. Several species have been found, all about the size of a fox. Like most of the early mammals, these ungulates had fourty-four teeth, the molars with short crowns, and quite distinct in form from the premolars. The ulna and the fibula were entire and distinct, and there were four well-developed toes and a rudiment of another on the fore-feet, and three toes behind. In the structure of the feet and in the teeth, the *Eohippus* indicates unmistakably that the direct ancestral line to the modern horse has already separated from the other perissodactyles. In the next higher division of the eocene another genus (*Orohippus*) makes its appearance, replacing *Eohippus*, and showing a greater, although still distant, resemblance to the equine type. The rudimentary first digit of the fore-foot has disappeared, and the last premolar has gone over to the molar series. *Orohippus* was but little larger than *Eohippus*, and in most other respects very similar. Several species have been found in the same horizon with *Dinoceras*, and others lived during the upper eocene with *Diplacodon*, but none later.

Near the base of the miocene, in the brontotherium beds, we find a third closely-allied genus, *Mesohippus*, which is about as large as a sheep, and one stage nearer the horse. There are only three toes and a rudimentary splint bone on the fore-feet, and three toes behind. Two of the premolar teeth are quite like the molars. The ulna is no longer distinct, or the fibula entire, and other characters show clearly that the transition is advancing. In the upper miocene *Mesohippus* is not found, but in its place a fourth form, *Miohippus*, continues the line. This genus is near the *Anchitherium* of Europe, but presents several important differences. The three toes in each foot are more nearly of a size, and a rudiment of the fifth metacarpal bone is retained. All the known species of this genus are larger than those of *Mesohippus*, and none pass above the miocene.

The genus, *Protohippus* of the lower pliocene, is yet more equine, and some of its species equalled the ass in size. There are still three toes on each foot, but only the middle one, corresponding to the single toe of the horse, comes to the ground. This genus resembles most nearly the *Hipparion* of Europe. In the pliocene we have the last stage of the series before reaching the horse, in the genus *Pliohippus*, which has lost the small hooflets, and in other respects is very equine. Only in the upper pliocene does the true *Equus* appear and complete the genealogy of the horse, which in the post-tertiary roamed over the whole of North and South America and soon after became extinct. This occurred long before the discovery of the Continent by Europeans, and no satisfactory reason for the extinction has yet been given. Besides the characters I have mentioned there are many others in the skeleton, skull, teeth, and brain of the forty or more intermediate species, which show that the transition from the eocene *Eohippus* to the modern *Equus* has taken place in the order indicated, and I believe the specimens now at New-Haven will demonstrate the fact to any anatomist. They certainly carried prompt conviction to the first of anatomists who was the honoured guest of the Association a year ago, whose genius had already indicated the later genealogy of the horse in Europe, and whose own researches so well qualified him to appreciate the evidence here laid before him. Did time permit I might give you at least a probable explanation of this marvellous change, but justice to the comrades of the horse in his long struggle for existence demands that some notice of their efforts should be placed on record.

(*To be continued.*)

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, September 17.—M. Peligot in the chair.—The president requested the meeting to designate one of their fellows to represent the Academy in the annual public meeting of the five academies, which will take place on October 25.—M. Tresca then, in the name of M. Leverrier, presented to the Academy vol. viii. of the "Atlas Météorologique de l'Observatoire de Paris pour l'an 1876."—A note by M. Faye, on the atlas of the superior movements of the atmosphere, by M. H. Hildebrandsson. The author bases his work on the observation of cirrhus clouds.—A note by M. G. de Saporta on the discovery of fossil plants in the tertiary strata in the vicinity of the North Pole.—On an erratic block of granite situated in the neighbourhood of Geneva, by M. de Marignac. It appears that the block in question is a mass of about 300 cubic metres of granite, and that the prefect of the Department, Haute Savoie, has given permission to a Railway Company to take possession of it and to cut it to pieces. M. de Marignac, who is the owner of the ground upon which it lies, now recommends the preservation of the block and offers it to the Academy together with the area it lies upon, under the sole condition that it shall be preserved. M. Dumas spoke in favour of M. de Marignac's proposition.—On the spontaneous disappearance of phylloxera, by H. Marès.—M. P. de Tchihatcheff then presented to the Academy his translation of M. Grisebach's work, "The Vegetation of the Globe," and made some remarks on the same.—M. Alluard read a memoir on a new condensation-hygrometer, invented by himself.—A letter from M. E. Stephan announcing the discovery of a new comet by M. Coggia was read. (Of this we gave the details in the Astronomical Column of our last number, p. 442.) The letter further contained details of an observation of one of the satellites of Mars, by M. Borrelly, made at Marseilles.—M. Leverrier transmitted to the Academy details of MM. Paul and Prosper Henry's observation of the same satellite, made with the equatorial in the garden of the Paris Observatory.—M. P. H. Boutigny pointed out that in a passage in his work, "Études sur les corps à l'état sphéroïdal," published some thirty years ago, he expressed his belief in the existence of satellites of Mars and pronounced the hope of their future discovery.—New researches on the ammoniacal fermentation of urine and spontaneous generation, by MM. P. Cazeneuve and Ch. Livon.—On the physiological action of salicylate of soda, by MM. Bochefontaine and Chabbert.—A note by M. V. Duram on a luminous meteor observed on September 11 at Boën (Loire), and on a shock of earthquake felt at the same place on September 12. The meteor was of unusual brilliancy; it appeared in the east of the sky at 7.45 P.M.; its elevation above the horizon was but small; it left a long curved trail, and its appearance was marked by a slight detonation; the direction of its path was from north to south. The shock of earthquake was felt at 6h. 52m. true time, and lasted several seconds.—M. Faye then drew the attention of the Academy to a memoir just published by M. P. de Saint Robert, on the spherical movement of the pendulum, with regard to the resistance of the air and the rotation of the earth.

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