THURSDAY, AUGUST 18, 1881

THE CENTRAL AFRICAN LAKES

To the Central African Lakes and Back: The Narrative of the Royal Geographical Society's East African Expedition, 1878–80. By Joseph Thomson, F.R.G.S. Two vols. (London: Sampson Low and Co., 1881.)

OUR readers must be familiar with the leading features of the remarkable Expedition which set out at the end of 1878 under the leadership of Mr. Keith Johnston, and returned in 1880 under that of Mr. Joseph Thomson. We have already told the story of the Expedition pretty fully, and Mr. Thomson himself described in these pages the main points in the geology of the Expedition which he traversed. Mr. Thomson was a very young and inexperienced man when, by the sad death of his accomplished chief, the command of the Expedition devolved upon him, just after it had got over the initial difficulties of the coast region and entered on the great tableland which occupies the greater part of the African area. Mr. Thomson showed himself at once equal to the emergency, and succeeded in winning a reputation that is likely to prove fruitful of good results both for himself and for African geology. He has been equally successful in telling the story of his journey. He writes in quiet and simple, but effective, style, his pages are full of incident and information of great interest, and he has a sense of humour and a love of fun which are not only of service to him under trying conditions, but which render his book light and pleasant reading. After seasoning themselves by a little trip to Usambarra, Messrs. Johnston and Thomson (the latter geologist to the Expedition), at the head of 150 men, left Dar-es-Salaam in May, 1879, for the north end of Lake Nyassa. A month later Mr. Johnston died at Behobeho, to the north of the Rufiji, just when the mountains that bound the plateau had been reached, through the usual difficulties attending African travel, partly arising from the nature of the country, partly from the natives, and partly from the men who formed the Expedition; but Mr. Thomson overcame them all with more than usual tact, and with unprecedented success. After a short rest at Lake Nyassa the route was resumed over the previously untraversed country between Nyassa and the south end of Lake Tanganyika. Leaving most of his men here, Mr. Thomson, with a small contingent, proceeded up the rugged west side of the lake to the famous river, about whose course Messrs. Cameron and Stanley gave such inconsistent accounts—the Lukuga. Mr. Thomson's observations on this river are of great value in connection with African hydrography. He found that Cameron and Stanley were both right. At Cameron's visit, what little current existed was towards the lake; Stanley, later on, found a distinct current setting from the lake, and prophesied that in a short time the barrier of vegetation across the river not far from its mouth would be carried away, and the Lukuga would carry the waters of Tanganyika in a full stream to the Lualaba-Congo. And this, Mr. Thomson found, had actually come to pass; he saw the Lukuga as a broad, swift effluent from Lake Tanganyika. When, however, he returned two months later, he found that the strength of the current had considerably decreased, as had also the volume of the river. Mr. Thomson discusses the interesting problem of the Tanganyika at some length, and with great intelligence, as well as with the knowledge of a trained and practical geologist. He refers to the facts observed by previous travellers as well as himself, as to the rise and fall of the level of the lake, the hydrography of the neighbouring country, &c., and concludes as follows:—

"With these facts before us the necessity of Tanganyika having a regular outlet is not so apparent as it seems to have been to Cameron. Neither need we, like Stanley, invoke the aid of great convulsions to account for the interruption or intermittency of the outflow. The phenomena are sufficiently accounted for by the facts I have enumerated, viz. (first), that since the Arabs settled at Ujiji till within the last five years there has been no very marked rise in the level of the lake; (second), that the rapid rise of the lake after that time was due to unusually wet seasons; (third), that the normal rainfall is less than fifty inches in the year; (fourth), that Tanganyika drains a remarkably small area of land, and has only a few insignificant rivers, torrents, and streams falling into it; and (fifth), that the volume of water passing out by the Lukuga is diminishing so rapidly as to be markedly noticeable in two months, even in the course of the rainy season, so that quite possibly the next traveller may arrive to find little or no water leaving the lake. These considerations, then, as well as all my inquiries and observations, lead me to conclude (first), that under normal circumstances the rainfall and evaporation nearly balance each other; (second), that many years ago a series of unusually dry seasons reduced the level of the lake below that of its outlet; (third), that it remained sufficiently long without circulation to become charged with salts, which have given the water a markedly peculiar and unpleasant taste, unlike that of ordinary fresh water, and also an exceptional power of corroding metal and leather; (fourth), that unusually wet seasons set in some five or six years ago, raising the level of the lake; (fifth), that it rose above its normal level, owing to the formation of a barrier in the bed of the Lukuga by rapid vegetable growth, and the depositing of alluvium by the small streams descending from the slopes on either side; and (sixth), that the lake having once overflowed the barrier, soon removed it entirely, thus regaining its original channel and level."

After a short stay with the hospitable and intelligent missionaries at Mtowa, to the north of the Lukuga outlet, and paying a visit to Ujiji, Mr. Thomson set out to make his way back through terrible Urua, where Cameron suffered so much. Here he had to undergo the worst reception he met with during his whole journey; every possible obstacle was placed in his way, every opportunity was taken to insult him and steal his goods, even the very blanket from beneath him when asleep; and it was only by the greatest self-restraint and tact that fighting was avoided and he and his men escaped with their lives. This they were only too glad to do, just before they reached the goal of the expedition, the river Lukuga. Returning to Mtowa, a passage was obtained in Mr. Hore's boat to the south end of the lake. Here Mr. Thomson picked up his men and set out on the return journey. He had intended to go east through Usanga and by the upper waters of the Rufiji, but as the natives were at war this would have been dangerous. So he went north by the east side of Tanganyika, taking occasion to have a good look at the mysterious Lake Hikwa, which he re-named Lake Leopold. Mr. Thomson did not succeed in reaching the actual shores of the lake.

"The point where we halted was upwards of 7000 feet above the sea, and from the fact that the River Mkafu, which flows into the lake, is only about 3000 feet, at a distance of sixty miles north, I infer that Lake Hikwa is not far from being on a level with Tanganyika. So steeply do the mountains descend, that from the place where we halted we could almost throw stones into the lake; only we lost sight of them before they reached the ground. The general altitude of the surrounding ranges must be quite 8000 to 9000 feet, and they extend in a quite unbroken line all round. At the north end I calculate the breadth of the lake at about twelve miles. Further south the breadth varies from fifteen to twenty miles. Longitudinally it lies north-north-east and south-south-west. Its length, from native report and from my proximity to it in passing between Nyassa and Tanganyika, I conclude to be certainly not less than sixty miles, probably seventy. Between the mountains and the shores there lies a narrow dark green strip of smooth land, apparently representing a once higher level. On this there are many villages, and the ground is highly cultivated. At the north end, as I have already stated, this strip broadens out into a marshy expanse, formed doubtless by the detritus of the River Mkafu."

Although the waters are almost certainly fresh, yet the lake seems to have no outlet. Without accident or obstacle to speak of, the Expedition, proceeding by Unyamyembe, reached Zanzibar, not much more than a year after it set out from Behobeho. Mr. Thomson, with good reason, congratulates himself that he never needed to fire a shot either in offence or defence, and that, besides the loss of Mr. Johnston, he left only one man behind him.

The Expedition is in many ways one of the most successful that ever entered Africa. Not only was it conducted with unusual efficiency, not only were the chiefs and people, with few exceptions, friendly throughout, but for the first time we have obtained trustworthy observations on the geology of the great lake region of Central Africa. The main conclusions reached by Mr. Thomson have already been described by himself in these pages. But he did not confine himself to geology. He gives us a fair idea of the general character of the country traversed, its mineral, vegetable, and animal productions, the characteristics and habits of the people, the nature of the work being done by missionaries, and the capacities of the country for industrial development. On the last point his views are far from being sanguine. He maintains that the resources of Central Africa have been greatly exaggerated, especially as to its minerals. We are inclined to think that on this point he has taken much too gloomy a view, and that, whatever may be the case with the region actually visited by him, there certainly appears to exist, in the districts traversed by Cameron, Livingstone, and more recently by Major Serpa Pinto, stores of iron and copper that may at a future time be turned to great industrial account. Young as Mr. Thomson is, we commend his remarks on missionary work to those whom it most intimately concerns; and we trust that his severe, but evidently just, criticisms on the conduct of the various Belgian expeditions in Africa will receive the attention they deserve from the management of the International African Association. In the Appendices are given notes on the natural history collections, and Mr. Thomson discusses the geology in detail, suggests that at one period the whole of the lake region of Central Africa must have been covered by the sea, the basin of Tanganyika, however, having been formed subsequently by a great fault or narrow depression of great though unknown depth.

Prefixed to the volumes are portraits of Mr. Johnston and Mr. Thomson, and appended a route map and an interesting geological chart.

OUR BOOK SHELF

Marine Algæ of New England and the Adjacent Coast. By Dr. W. J. Farlow. (Washington, 1881.)

THIS valuable essay on the "Marine Algæ of New England" is a reprint from the United States Fish Commission Report for 1879. It includes a list of all the species of sea-weeds, with the exception of the diatoms, which are known to occur on the coast of the United States, from New Jersey to Eastport, Me. Prof. Farlow gives in a compact and more or less popular form a description of the various orders and species, and he adds a short account of the general structure and classification of sea-weeds, so that all persons frequenting the coast of New England are thus furnished with a handy and compact manual of the subject. The fifteen excellent plates drawn by J. H. Blake and W. G. Farlow deserve a special notice, as they give details of structure which will enable the text to be understood by an intelligent student.

Since the appearance (1852-57) of Harvey's classic work on the North American Algæ, but few species have been added to the Flora. This is not perhaps so surprising as regards the Florideæ or Fucoids, to which Harvey paid so much attention; but as regards the unicellular or simple filamentous forms it is a cause of surprise, for Harvey never paid minute attention to these; and it may in part be accounted for that collections do not seem to have been made along the coast in spring. Prof. Farlow gives a most interesting sketch of the geographical distribution of the species met with. Cape Cod is, as was known to Harvey, the dividing line between a marked northern and southern flora, and subsequent observation shows that on the one hand the flora north of the Cape is more decidedly arctic than he supposed, and that on the other hand that south of the Cape is more decidedly that of warm seas. A good share of the commoner species are also natives of Great Britain, another large share are Scandinavian; but while this is the case the marine flora is also marked by the complete absence of many common British species. No members of the order Dictyotaceæ are to be found; no species of Cutleria or Tilopteris are to be met with. The species of Nitophyllum may be said to be wanting. That commonest of our red sea-weeds, *Plocamium coccineum*, is known as native by only one doubtful case. *Fucus canaliculatus*, *Himanthalia lorea* are quite wanting. The nearly ubiquitous *Codium tomentosum* has not yet been found. *Fucus serratus* is very rare, having only one locality recorded for it in the United States and one in Nova Scotia. *Gelidium corneum*, abundant in almost all parts of the world, is only occasionally found in New England, and then only in the starved form known as *G. crinale*.

Prefixed to the orders and genera will be found carefully-written diagnoses, and an artificial key to the genera is also added. The notes in smaller type which are given under the species often contain most valuable critical information, which will command the attention of all phycologists. To the critical students of our native species of algæ this little manual of the New England species will prove a most welcome volume. They will find in the chapter on the structure and classification facts that were not known in Harvey's day, and which, here collected for them within a brief space, they would otherwise have to search for in the writings of Thuret, Bornet, Janczweski, Rostefinski, Pringsheim, or Reinke.

The Berries and Heaths of Rannoch. (London: G. Bell and Sons, 1881.)

THE berry-bearing plants here described and delineated are eight, viz. *Vaccinium oxycoccos, V. Myrtillus, V. uliginosum, V. vitis Idæa, Arctostaphylos uva-ursi, A. alpina, Empetrum nigrum,* and *Rubus chamæmorus,* all of which do not, strictly speaking, come within the geographical limitation of the title-page. The heaths are three only in number, viz. the common *Erica cinerea* and *Tetralix,* and *Calluna vulgaris,* to which are added two other nearly allied species not actually found within the district, *Andromeda polifolia* and *Loiseleuria* (*Azalea*) *procumbens.* In the letterpress it is not to be expected that anything new could be added to what is already known about these plants; but in an appendix is given a list of the Gaelic names of the various species supplied by the editor of the *Scottish Naturalist.* The coloured plates are exceedingly good and characteristic; but surely it should have been stated that they are taken from Sowerby's "English Botany." The volume is a pretty one to lie on the drawing-room table. A. W. B.

Lehrbuch der Mineralogie. Von Dr. G. Tschermak. I. Lieferung. (Wien: Alfred Hölder, 1881.)

IT is with great pleasure that we have received this instalment of Prof. Tschermak's work, and also learnt from the publisher's introductory note that the rest of the book may be expected during the course of a year. The work is sketched somewhat on the lines of Naumann's well-known "Elemente der Mineralogie," but follows Miller's Mineralogy in the wider scope given to mineral physics. The present number is introductory, and treats of descriptive crystallography, crystal-structure, general mineral physics, and includes a considerable portion of mineral optics. In the crystallography the Millerian notation and the stereographic projection are employed, and the systems are developed from the principle of symmetry in a clear and simple manner. Prof. Tschermak has adopted the four-plane axial system in the rhombohedral system, which is sometimes designated the Bravais-Miller system. Possibly this may appear to non-mathematical students simpler, and may to a certain extent be more easily mastered, but we feel sure that in its practical application to crystallographic problems it does not possess either the elegance or conciseness of the three-plane axial system selected by Prof. Miller. We feel also that it is most unfair to Prof. Miller's memory to attach his name, even in a double-barrelled way, to a system which he steadily refused to adopt. The theories and facts of twin and mimetic crystals are carefully expounded. These constitute a branch of mineralogy which has become of the utmost importance since the application of the microscope in the investigation of the optic properties of minerals. Other sections, which are especially good, are those on mineral inclusions, on the hardness and etching of crystal faces. These contain a large amount of information which is rarely to be found except by a laborious search through scientific periodicals. The book is divided into sections, each dealing with its separate subject, and at the end of each section is a list of the more important literature of the subject. The work so far is excellent, and if, as we have every reason to expect, it be carried through in an equally satisfactory manner, we shall possess a text-book in keeping with the reputation of its author and worthy of the school to which he belongs. W. J. LEWIS

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.*]

Panizzi and the Royal Society

THE "Life of Panizzi" by his friend and colleague, Mr. Louis Fagan,[1] is marked by a tone of indiscriminate adulation which disfigures many specimens of modern biography. The hero is perfect, and they who think otherwise are dismissed with words of contempt, or are admonished to go and meditate on their wicked ways and then return in repentant mood to the community of hero-worshippers.

In the Royal Society's treatment of Panizzi, Mr. Fagan endeavours to justify another example of the wolf and the lamb, although it must be owned that in the pamphlets[2] from which the biographer quotes, the lamb's bleatings are sufficiently energetic to lead to the conclusion that he thought himself a match for the wicked wolf.

Mr. Fagan thinks it important "that Panizzi's stormy connection with the Royal Society should be fairly and impartially" stated; although how this can be done without hearing both sides he forgets to say; and yet he professes to give "the proper elucidation of the facts," "the whole circumstances of the case thoroughly weighed and dwelt upon"; how successfully he opposed "the force with which it was attempted to crush the evidence of his superior talent" (vol. i. p. 119), and although "thwarted and impeded at every step, Panizzi at last succeeded in once again proving that right can contend successfully with might" (vol. i. p. 130).

The reader will gain a very lop-sided idea of this quarrel if he trust to Mr. Fagan's account alone; and as in the reviews of this book no one has attempted to ascertain the truth of the matter (which indeed could not be done without access to the Royal Society's papers), I venture, as a member of the present Library Committee, to state the case from the other side, being naturally anxious to sustain the reputation, so unjustly assailed, of a former committee which contained the honoured names of Baily, Beaufort, Children, Greenough, Lubbock, Murchison, Peacock, Roget, and others.[3]

To make a long story short, it is sufficient to state that about the year 1832 the Royal Society wished to bring out a complete catalogue of the books, &c., in its library. As a preliminary step, a list of the mathematical books was compiled and set up in type as a specimen of the kind of work required. In the words of a Council minute, the sheets were "not designed for publication," they being "in a very rough and unfinished state."

In October, 1832, Dr. Roget meeting Mr. Panizzi at dinner, informed him of the Society's intention, and requested him to look over and revise the sheets in question, together with others that might afterwards be forthcoming. This was agreed to, and the first sheets were forwarded to Panizzi, who found so many errors in them that, as he informed Dr. Roget, "although I would never attempt to correct what had been already done, I was ready to undertake a new compilation."

Accordingly on October 16, 1832, the Library Committee resolved to recommend to the Council that Mr. Panizzi be engaged to make a new catalogue according to the mode to be agreed upon by the Committee, he to be paid 30*l.* for every thousand titles, the whole remuneration, however, not to exceed 500*l.*

[1] "The Life of Sir Anthony Panizzi, K.C.B." By Louis Fagan. Two vols., 8vo, 1880.

[2] "A Letter to H.R.H. the President of the Royal Society, on the New Catalogue of the Library of that Institution now in the Press." Pp. 56 and 3. Signed A. Panizzi, and dated January 28, 1837. The last three pages contain a postscript letter to the President, dated November 4, 1837, and a note in which it is stated that the pamphlet was not put into circulation until the latter date, in order that H.R.H. might have an opportunity of replying to it.

The President, not having availed himself of this opportunity, the second pamphlet was put forth. It is entitled "Observations on the Address by the President, and on the Statement by the Council to the Fellows of the Royal Society respecting Mr. Panizzi, read at the general meeting, November 30, 1837." Pp. 24. Dated December 22, 1837.

[3] Strictly speaking there were three committees, namely, one for the catalogue, a second for the library, and a third for deciding in doubtful cases under what division a book should be placed in the new catalogue.

Panizzi agreed to these terms, and offered "to wait on the Committee, as soon as convenient to them, to settle the manner in which they wish the work to be executed."

Now the whole gist of this quarrel consists in this, that the Library Committee naturally wished to control Mr. Panizzi in his mode of executing the work, while he refused to be controlled or interfered with in any manner. He even regarded as personal enemies all those who attempted so to interfere. He fancied that every one who differed from him was actuated by a sense of personal dislike. When he refused Dr. Roget's request to revise the sheets of the Catalogue, he says (p. 6): "I had no idea when I so candidly expressed my opinion that I was making a powerful and unrelenting enemy in one of the most influential officers of the Royal Society." At p. 51 he says: "so gratuitous an insult would never have been allowed had not Mr. Baily filled the chair at that meeting." And again (p. 5), "My statements will be received with derision by those who know that they may be unjust with impunity." At p. 18 he charges the Committee with "indelicate conduct," at p. 22 with "absurdity," at p. 25 such things were done "purposely to annoy me;" and again, "No suggestion of mine would ever be attended to by the Council." At p. 26 his work was regarded with "a malignant eye;" at p. 28 "The annoyance was incessant," "injurious and unjust;" at p. 33, "'treating me as if I were their servant," "unwarrantable liberty;" p. 38, "unjustly interfered with;" p. 41, "insulted with an order of submitting my work to revision. . . . I shall never consent for any one, be he who he may, to make any alterations in it." And when, on June 24, 1836, he was requested to attend the Library Committee on the following Monday at 4 p.m., he declined on the ground that "when I attended before I was not so well satisfied with my position as to wish to be in it again." At p. 54, when clamouring for payment of an unascertained balance which he claimed, he charges the Council with not meaning "to pay it unless they be compelled to it. . . . Possibly there is some legal means of obtaining redress; but in a country like this justice is not a luxury for a poor man to indulge in; and the Council, having at their disposal the funds of the Royal Society, can amuse themselves without personal trouble or loss with a law-suit which I have not the means of sustaining." Will it be believed, in the face of such language as this that Panizzi had already been paid the sum of 450*l.*, and his whole remuneration was not to exceed 500*l.*

In his second pamphlet (p. 18), after charging the Council with not meaning to act fairly, he hurls at it his "unmixed disgust and contempt." But I cannot help thinking that these vigorous epithets would have been more appropriate had they travelled the other way.

When requested to return the printer's revises, and he refused on the ground that they were his own property, together with the key of a drawer in one of the Royal Society's rooms, and he also refused, what wonder that, after so long a contest with this cantankerous man, the Council should have resolved on July 14, 1837, "that Mr. Panizzi be no longer employed in the formation of the Catalogue."

The reader may well exclaim by this time, What *is* all this hubbub about? Simply this: Mr. Panizzi insisted on adding to some of the items of the Catalogue original comments of his own, to which the Library Committee justly objected as committing the Society to opinions of doubtful value. Panizzi attached the greatest importance to these notes and comments. "The Committee, far from objecting to them, ought to have been thankful that I had taken the trouble of introducing them" (p. 31); and he proceeds to quote specimens illustrative of this part of his work. For example, he says: "To the 'Mémoires' of Charnières on the observations of the longitude, I added this note: 'All the author's additions and corrections carefully put in by J. B.' This note is on the title-page of this copy, and the volume is interspersed with alterations in manuscript. I suppose J. B. to mean James Bradley." Later on in the same page he adds: "The author's additions, if put in by Bradley, are, of course, of much more value than if written by any other J. B."

Now the book in question is only a single Mémoire of De Charnières, not a collection of "Mémoires," as described by Panizzi. Moreover, there are five reasons why the additions and corrections could not have been written in by Dr. Bradley.

1. He died five years before the memoir by Dr. Charnières was published. This may well excuse the other four reasons, but they are curious as illustrating the carelessness of a man who was convinced of his own infallibility.

2. The writing of the anonymous J. B. is small and neat: that of Bradley large and awkward. The Royal Society had in its possession manuscripts of Bradley and his signature, which could be seen by merely asking the assistant-secretary for them, and yet Panizzi did not submit the writing of J. B. to this simple test.

3. Bradley was not in the habit of writing in his books.

4. The so-called "additions and corrections" are simply the corrigenda collected into eight pages at the end of the book, and transferred in MSS. to the text, a fidgety piece of work, not likely to be undertaken by so busy a man as Bradley.

5. At the end of the book J. B. drops his incognito and appears as *J. Bevis*, a fact overlooked by Panizzi.

Other similar examples might be given, and indeed were submitted to the Fellows of the Royal Society at the time, in order to justify the resolution of the Library Committee "that all comments or notes expressing matters of opinion on the articles in the catalogue be omitted"; but the statement of them would occupy too much space, dealing as they do with details which unless given in full would not be understood.

Mr. Panizzi was undoubtedly a vigorous clever man; but in the matter of books, he, unfortunately for his own reputation, aspired to universal knowledge which belongs to no one. The gold of a universalist is apt to shrink down into dross when tested in the crucible of a specialist. Having occasion to consult a book by Gay-Lussac, and not finding it in the Catalogue of the British Museum Library, the attendant requested me to write the name and title on a slip and show it to Mr. Panizzi. No sooner had he glanced at the slip than he exclaimed "Ah! you have made a mistake: it is Guy-Lussac!" This readiness on all occasions to say something apparently to the purpose, may impress subordinates with a sense of power on the part of their chief, but to tell a chemist that Gay-Lussac is Guy-Lussac would be much the same as telling him that potash and soda are identical compounds. C. TOMLINSON

Highgate, N., August 2

The Oldest Fossil Insects

IN a paper on "The Devonian Insects of New Brunswick" (*Bull. Mus. Compar. Zoology*, 1881, vol. viii. No. 14) I have drawn attention to the fact that a fern on the same slab with Platephemera was determined in 1868 by Prof. Geinitz as *Pecopteris plumosa*, and therefore the slab considered by him as belonging to the Carboniferous. I believed that here an important gap was still to be filled, namely, the reliable determination of the fern, which is not mentioned in Mr. S. H. Scudder's monograph, nor in Principal Dawson's note on the geological relation of those insects, which closes Mr. Scudder's paper.

A paper by Mr. Dawson (*Canad. Naturalist*, 1881, vol. x. No. 2) is intended to fill this gap. The fern is after the study of the original specimen determined as *Pecopteris serrulata*, and said to be a common species in those beds. If I am not entirely mistaken it will be difficult to agree with Mr. Dawson's opinion (*l.c.* p. 2) "that doubts and suspicions thus cast on work carefully and exhaustively done should not seriously affect the minds of naturalists," as it happens that in his work of 1880 this common species is not quoted at all among the plants found in those beds, except in a note (p. 41) stating that in the beds 6 to 8 three or four other species occur, among them *probably P. serrulata*. Mr. Dawson quotes for the species the figures 207 to 209 in his Report of 1870, but I confess to be unable to recognise the Platephemera fern in those figures.

Prof. O. Heer has kindly drawn my attention to his "Flora Fossilis Arctica of Bear Island, Spitzbergen, 1871." He has given (pp. 14, 15) a detailed review of the fossil plants from St. John's, New Brunswick, and, as he still believes, has proven that those layers do not belong to the Devonian but to the Ursa stage of the Lower Carboniferous. This important and elaborate statement is disposed of by Mr. Dawson, as far as I know, only in his report, 1873, p. 8, in the following words:—"The so-called Ursa stage of Heer includes this (Lower Carboniferous), but he has united it with Devonian beds, so that the name cannot be used except for the local development of these beds at Bear Island."

It is true that Mr. Dawson, in the supplement to the third edition of the "Acadian Geology," 1878, p. 72, has tried to explain the different opinion of Prof. Heer by the earlier introduction of the Palæozoic flora in American formations. But this fact, known by every one, and of course by Prof. Heer, is not considered by him to be a sufficient objection to the statements given in the "Flora of Bear Island."

The paper of Prof. Heer states carefully and exhaustively the facts which induced him to consider those layers at St. John's as belonging to the Lower Carboniferous. Therefore naturalists will scarcely agree that such a statement, made by a prominent and acknowledged authority, can be cancelled by a simple negation not supported by facts. Till this is done in a reliable manner, those oldest insects will have to be considered as belonging to the Lower Carboniferous.

Cambridge, Mass., July 25 H. A. HAGEN

The True Coefficient of Mortality

THE very interesting and suggestive lecture of Alexander Buchan on "The Weather and Health of London" (NATURE, vol. xxiv. p. 143 *et seq.*) reminds me of the propriety of calling the attention of writers on "vital statistics" to a point in relation to the true method of discussing the mortuary data. The specific point to which attention is drawn is the necessity of estimating the relative tendency to special diseases by comparing the number of deaths from the given cause with the number of persons living at the ages embraced in the record; instead of making the comparison (as is usually done) with the total deaths from all causes, or with the total number living at all ages.

In like manner, in discussing the influence of age on the mortality from any given disease, it is very common to prepare tables of the number of deaths at each age, and in some instances these numbers have been assumed to represent the relative tendency to the disease at different ages. It is scarcely necessary to say that this is a very serious error, for it must be borne in mind that the number of persons living at different ages is very unequal. Indeed it is self-evident that the true coefficient of mortality for any given disease at any given age is expressed by the ratio of the number of deaths from the specified disease at the given age to the number of persons living at the same age: or, as it may be otherwise indicated, the number of deaths from the given disease at the given age per 1000 persons living at the same age.

In illustrating this point I shall select cancer, because, in relation to the influence of age, it furnishes an extreme case, and thus affords a glaring instance of the fallacy of taking any basis of comparison other than the number of persons living at each age. The mortuary records of the Department of Seine in France, during the eleven years, from 1830 to 1840 inclusive, furnish a total of 9118 deaths from cancer, 2163 males and 6955 females. The following table relating to the mean annual mortality from this disease among females will illustrate this point:—

Age. Years.	Number of females living.	Mean annual deaths from cancer among females.	Annual deaths from cancer in 1000 females living at all ages.	Annual deaths from cancer in 1000 females living at each age.
(1)	(2)	(3)	(4)	(5)
0 to 10	139,840	1·273	—	0·00910
10 „ 20	115,269	1·182	—	0·01026
20 „ 30	104,342	15·364	0·04196	0·14725
30 „ 40	73,203	74·727	0·20409	1·02081
40 „ 50	54,124	148·727	0·40619	2·74788
50 „ 60	36,800	147·273	0·40221	4·00198
60 „ 70	25,703	133·545	0·36472	5·19564
70 „ 80	12,852	83·364	0·22767	6·48659
80 „ 90	3680	24·818	0·06778	6·74408
90 „ 100	340	2·000	0·00546	5·88769
All ages.	566,153	632·273	—	1·11679

The foregoing table demonstrates the inaccuracy of the popular impression that the tendency to cancer attains its maximum between the ages of 35 and 50 years. The numbers in columns (3) and (4) might seem to support such an opinion; but, as we have seen, those in column (5) are evidently the true indices of the tendency to this disease at different ages; and it will be observed that the mortality goes on steadily augmenting with each succeeding decade of age up to 90 years. The fact likely to be most strongly impressed on the reader by the numbers in column (5) is the remarkable regularity of increase of the coefficient of mortality for cancer with advancing life among females after the age of 25 or 30 years. Between the ages of 25 and 75 the mortality increases nearly in arithmetical progression as the age advances in arithmetical progression, the average increment being about 1·30 per 1000 living at each age for each decade. Assuming this to be the law of mortality from cancer among females, it admits of very simple mathematical expression. Thus, let

A = the age at which liability to cancer begins.

A' = any age greater than A.

C = constant coefficient, variable according to country, state of civilisation, &c.

Then we have—Annual mortality per 1000 living at age $A' = C(A' - A)$.

In our table representing the mortality from cancer in the department of the Seine from 1830 to 1840 inclusive, the value of A may be taken = 25, and $C = 0{\cdot}13$; hence we have—Annual mortality per 1000 living at age $A' = 0{\cdot}13\,(A' - 25)$. Thus by the formula the mortality at 55 = 3·90, and column (5) gives 4·00 between 50 and 60; at 75, formula = 6·50; table = 6·49 between 70 and 80.

The mortality from cancer seems to be vastly smaller in England than it is in France, so that a less value must be given to the constant C. The foregoing formula represents the law of increasing mortality with advancing life in the simplest form, as a function of the age. This extreme simplicity is probably unique in the case of cancer, and seems to indicate that age is so far the controlling element in the development of this disease as to overpower all other causes. In the case of other diseases we cannot expect to escape the necessity of employing those exponential functions in investigating their laws of mortality, which are essential when a multiplicity of causes are in operation.

Many years ago the attention of the medical profession in this country was called to the fact that the available mortuary data were not discussed in a manner which revealed the true value of the facts contained in the numbers.[1] But there is reason to believe that Prof. Francis A. Walker, the intelligent superintendent of the census of the United States for 1880, will not overlook this point when he comes to the discussion of the mortuary statistics which have been collected.

Berkeley, California, July 7 JOHN LE CONTE

[Mr. Le Conte does not appear to have apprehended the point discussed in the lecture on "The Weather and Health of London"—that point in no part of the inquiry being the tendency to the disease at different ages, but the manner of the distribution of deaths in the case of each disease through the weeks of the year, with the view of arriving at some knowledge of the influence of season in determining that distribution. Only in one case, viz., in discussing the rates of the mortality from diarrhœa in several large towns, was a reference to population required, and in that case the curves were drawn, showing the weekly rate of mortality per 1000 of the population of the respective towns.—ALEXANDER BUCHAN.]

Bisected Humble Bees

I TOO have frequently observed humble bees lying dead or stupefied under lime-trees, sun-flowers, and some other plants, and once I saw a Staphylinus, commonly known as Black Cock-tail, or Devil's Coach-horse, nip a humble bee in two, and on passing that way later I found that it had cleared out the honey-bag and left the two halves of the bee on the path, as described by your correspondent. I have known boys catch humble bees and eat the honey in them; and probably many other animals have learned how to get at the sweet drop.

Trinity College, Cambridge THOS. MCK. HUGHES

AT your request for information on the above I beg to say that I have observed both the flycatchers alluded to by your correspondent, and also the little blue tit (*Parus cærulea*) attack the humble bees in the manner described, to extract the honey-bag. This attacking the bees is not, so far as my experience goes, a general characteristic of these birds, and what should lead them to it occasionally I cannot ascertain.

Exeter, August 15 EDWARD PARFITT

[1] *Vide* papers by the writer, entitled "Statistical Researches on Cancer," *Southern Med. and Surg. Journ.*, new series, vol. ii. pp. 257-293, May 1846. Also "Vital Statistics," illustrated by the "Laws of Mortality from Cancer," *Western Lancet*, vol. i. pp. 176-190, March, 1872 (San Francisco).

I NOTICE the same phenomenon here, under the sycamore trees, when they are in blossom, which your correspondent Mr. Masheder observed recently under his lime trees, namely, the heads and thoracic segments of severed humble bees lying on the ground, with legs and wings attached, still retaining their vitality in some cases, but without any trace of the abdominal segments, for the sake of whose contents, no doubt, the bees were destroyed. We have no fly-catchers here. I suspect the tom-tits, which are abundant in the vicinity of this wholesale apicide, but I have no direct evidence of their guilt. R. V. D.

Beragh, Co. Tyrone, August 15

Migration of the Wagtail

Apropos of recent letters on this subject in NATURE, permit me to note that on my voyage out to the East Indies in the month of October, 1878, on board the Dutch mail steamer *Celebes*, two wagtails alighted on the ship when not very far north of the equator (the ship's course being then from Aden to Padang in Sumatra). On observing them I pointed them out to a Dutch friend, who at once recognised them as *Kwikstails*. They were rather lively, and did not appear to us to be fatigued; after staying with us for some days they took their departure, but in what direction I had not the satisfaction of observing.

Without affirming positively, I believe the species was the *Motacilla alba*. HENRY FORBES

Sumatra, June

ITALIAN DEEP-SEA EXPLORATION IN THE MEDITERRANEAN

AFTER some delay, beyond our control, the war-steamer of the Italian Royal Navy *Washington*, Capt. G. B. Magnanghi, R.N., left Maddalena on the 2nd inst. on her thallassographic mission. Under the able direction of Capt. Magnaghi, two days were devoted to preliminary dredgings and trawlings in depths from 200 to 1000 metres, principally for testing our apparatus, which works admirably. On the 4th inst. (yesterday afternoon) we did our first deep-sea dredging in 3000 metres; the dredge came up empty, but I had the pleasure of securing, attached to the hempen tangles, a magnificent specimen of that strange blind Crustacean discovered by the *Challenger* in the North Atlantic, and named *Willemœsia leptodactyla;* it is no doubt one of the most characteristic forms of the deep-sea fauna, and its discovery in the Mediterranean is of very great importance and interest, as all students of thalassography will be fully aware, after what Dr. Carpenter has written on the biological conditions of the deeper parts of that sea. Our specimen of *Willemœsia* is slightly smaller than the one dredged by the *Challenger*, and figured in Sir Wyville Thomson's "Atlantic," vol. i. p. 189; but otherwise it differs only in one or two minor details, which may be sexual differences; it was dredged off the west coast of Sardinia.

On account of a slight mishap with our engine we have anchored at Asinara for a couple of days, but shall at once resume our work. HENRY H. GIGLIOLI

Asinara, Sardinia, August 5

KÖNIG'S WAVE-SIREN

EVERY musician is painfully familiar with the fact that two notes nearly, but not quite exactly, in unison with one another, produce, when sounded together, a throbbing sound commonly described as the phenomenon of "*beats.*" In the elementary theory of acoustics the cause of beats is shown to be the mutual *interference* of the two vibrations, one sound interfering with the other and silencing it, when one set of waves is half a vibration behind the other. Just as at certain points on the earth's surface there are no tides when a high tide and a low tide coming from different seas meet, so there is no sound when two sets of sound-waves meet in opposite phases. If the two notes differ just a little in pitch they will alternately reinforce and interfere with one another, and produce the throbbing sound of beats, the number of beats (or maxima of sound) per second being the same as the difference in the number of vibrations per second. If one tone makes m vibrations per second and the other n (a slightly smaller number, being a slightly flatter tone) there will be $m - n$ beats per second heard. If this number be not more than 3 or 4 per second the beats can easily be counted. When they get as rapid as 12 or 14 per second they come too fast to be counted, and are very harsh and grating. They are most disagreeable at about 33 per second; and if yet more rapid, are heard as a harsh, disagreeable, rattling sound quite different from a true note. Imperfect octaves and imperfect twelfths likewise cause beats; in fact there are beats heard for any imperfectly tuned consonance in which the frequency of the higher note is 1, 2, 3, 4, 5, . . . or any integer number of times that of the lower.

But along with the disagreeable and throbbing phenomenon of beats there arises another phenomenon when two notes not in unison with one another are simultaneously sounded. This is a low booming tone, to which musicians give the name of the "grave harmonic." If two stopped organ-pipes are brought to unison, and then one of them is sharpened by gradually pushing in its stopper, the beats are heard first slow, then fast, then unendurably rapid. But when they reach about twenty or thirty per second the low booming note begins, and rises gradually in pitch as the beats become too rapid to be discriminated. When the higher note has reached a point about half-way between unison and the octave note, the beats are practically imperceptible, and from this point the phenomena recur again, but in *inverted order*, the grave harmonic falls in pitch down to a low booming tone, while the beats begin again to be distinguishable, grow harsher, then become slower, until when the interval of the octave is reached they also disappear.

A great controversy with respect to these low tones of the grave harmonics has arisen in recent years, and though it smoulders from month to month, occasionally blazes up into vigorous flame. The controverted question is, What are these grave harmonics, and to what are they due? Also, What becomes of the *beats* when they occur so rapidly that the ear cannot distinguish them? The answer given by Dr. Thomas Young, and by Smith in his "Harmonics" (1749), was that the rapid beats actually passed into the grave harmonic, just as in the generation of any pure tone the separate vibrations (which, when very slow, are heard as separate sounds) blend into one continuous tone whose pitch depends upon their frequency. This view is maintained at the present day with great energy also by the famous acoustician Dr. Rudolph König of Paris. On the other hand, Helmholtz has emphatically maintained that the grave harmonic is not, and cannot be, thus accounted for, and has given very cogent reasons for thinking that it has another explanation; and in this view he is supported by Preyer, Lord Rayleigh, Ellis, Bosanquet, and all the best English physicists. Mere alternations of sound and silence, however rapidly they occur, cannot produce the same effect on the mechanism of the ear as a pure to-and-fro motion of the same periodic frequency. A tuning-fork which vibrates 100 times per second will give out waves which, falling on the ear, push the drumskin in, and draw it back that number of times per second. But a continuous tone interrupted 100 times per second by short periods of silence produces quite a different mechanical action on the mechanism of the ear. The writer of this article once tried to ascertain, by the experiment of rotating a vibrating tuning-fork upon its axis, whether the alternations of sound and silence which are observed as it is rotated would blend into a continuous tone; but no kind of blending took place. Another most conclusive proof that the beats and the beat-tones are distinct phenomena is that at a certain speed both can be heard going on simultaneously. Helmholtz gives to the grave harmonic the name of "*difference-tone*," because its number of vibrations exactly corresponds to the difference between the number of vibrations of the primaries. Two notes whose frequencies are respectively m per second and n per second will give rise to a difference-tone whose frequence is $m - n$ per second, which is, in fact, just the same number as the number of beats between the two. König uses a different name, and agreeably to his (and Young's) theory, calls these notes "*beat-notes*," and classifies them into two sets, *lower* and *upper*, the lower beat-note being that corresponding to the beats between the lower note and the one that is sharper than it, the higher beat-note being that corresponding to the beats between the higher note and the octave of the lower. For example, if the notes c' and d' are sounded together, their frequencies being in the ratio 8 : 9, there will be heard a beat-note whose frequency is relatively 1, or three octaves below the lower note. If c' and b' (a seventh) are sounded, their frequencies being in the ratio 8 : 15, there will be heard a beat-note of the upper series of relative frequency 1 (being the difference between 15 and 16), or also three octaves below the c'. So also the interval between c' and $\sharp f''$ (the twelfth-tone flattened by about a semitone, so as to make the ratio 8 : 23) will also give a beat-tone of relative frequency 1, being the difference between 23 and 24.

Now on Helmholtz's theory beats can only arise between vibrations so near together on the scale as to act on the same fibre of Corti in the ear (provided the vibrations be pure and free from upper partial tones), and they should therefore be audible not as *two tones* but as fluctuations in loudness of *one* tone. But when c' and d' are sounded we certainly hear *two separate* tones *plus* the low note which we call the grave harmonic. Helmholtz has therefore concluded that another explanation must be sought, and this he finds in a mathematical investigation of the resultant displacements due to super-

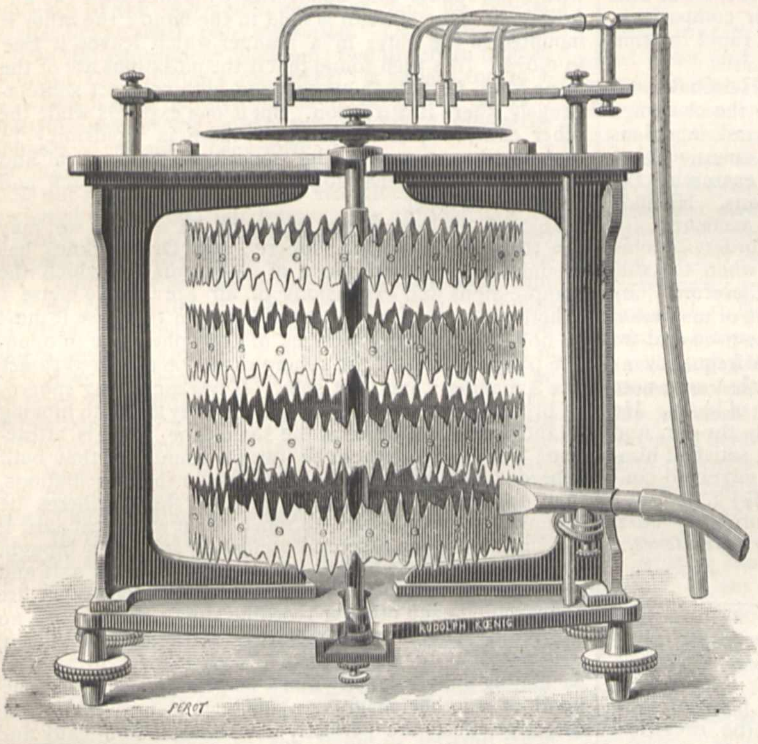

Fig. 1.

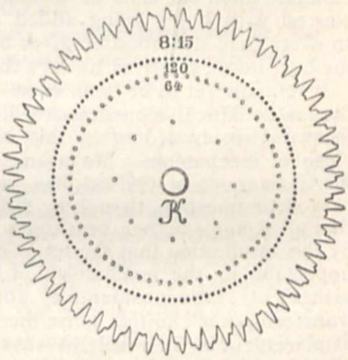

Fig. 2.

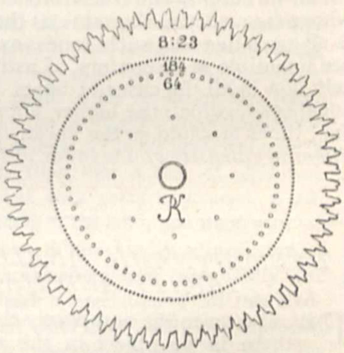

Fig. 3.

posing two tones, on the supposition that the vibrations of the primaries are so large that the moving forces are no longer simply proportional to the displacement, but are influenced by the squares or higher powers of them. He has shown that when this is the case combinational tones *must* arise whose frequencies correspond to the difference in the number of vibrations, and he further conjectures that to the dissymmetry of the drumskin and other vibrating parts of the ear is due the fact that the *squares* of the displacements can thus affect the resultant vibration. If so, all the combinational tones other than those of mistuned unisons must really arise in the ear itself and be *subjective* in character, as indeed Mr. Bosanquet, who has lately studied the matter most carefully, roundly declares.

Dr. König, however, undaunted by Helmholtz's reasonings, has returned to the contest with new weapons. He has repeated all his former experiments with new tuning-forks specially made of massive form, so as to be yet more perfect in tone, and finds his observations on beat-tones confirmed. He has further constructed a new instrument, the *wave-siren*, with which to establish his doctrine that *beats*, when too rapid to be heard separately, blend into a *beat-note*. In this instrument vibrations are set up in the air by blowing through a slit against the edges of a notched disk or rim which rotates rapidly upon an axis. In 1872 Dr. König constructed sirens on this principle, the indentations at the edges of the disks being simple harmonic curves, or "wave-forms," which therefore gave rise to simple tones. In the new wave-siren (Fig. 1) the indentations are determined in the following manner :—Two simple vibrations whose ratio is known are mechanically compounded together by machinery, and a resultant curve is obtained which *exactly* corresponds on a large scale to the resultant motion of the air when the two notes having this interval are sounded together. This compound curve is then set off very exactly round the periphery of a metal disk, and cut out in the metal with the utmost nicety. Fig. 2 shows the form of the curve (set out on the edge of a *flat* disk) for the interval of the seventh, the ratio being 8 : 15. When this disk is rotated rapidly, and wind is blown through a flat nozzle held with its opening radially at the edge, two notes are heard giving this exact interval. *If the vibration is slow, beats are heard: if the vibration be rapid, the beat-note is heard.* In order to compare these notes the more accurately with a true combinational tone, the same disk is pierced by three concentric rings of holes, one with 64, another with 120, giving the ratio 8 : 15, and another, with 8 holes only, corresponding to the number of beats between 120 and the octave of the 64 set (128), that is to say, to the upper beat-note of the interval 8 : 15. When air is blown through the rings of 64 and 120 holes in the rotating siren-disk, exactly the same notes and same beats or beat-notes are produced as by the wave-curve at the edge. Here there can surely be no partial tones present to complicate the phenomenon. For greater convenience in comparing several combinations, the wave-forms are cut upon cylindrical rims mounted upon one axis as in the first figure, a flat disk pierced with holes being added above for comparison. In every case slow rotation gives beats, and rapid rotation the beat-note exactly as König's theory requires.

It remains yet to be seen what answer Helmholtz and the mathematical acousticians will give to the challenge thrown down by König in this beautiful and ingenious piece of mechanism. Meantime we may mention that Mr. Bosanquet of Oxford has just been examining the very same question, though by different means. He finds that all König's higher beat-tones can be accounted for by the assumption that the terms of higher orders become important in the mechanism of the ear when the displacements are considerable, and that therefore "by transformation" in this sense the variations of maximum displacement in the resultant wave give rise to to-and-fro vibrations of simple form having the same frequency as these variations, and therefore evoke *in the ear* a note whose frequency is the same as the number of beats. He is also positive that such tones exist only in the ear, and are inaudible in resonators. Lastly, he has satisfied himself that in all the cases of beats between mistuned consonances in which the higher note is (nearly) 2, 3, 4, . . . &c., times as rapid as the lower, *the beat consists of variations of intensity of the* lower *of the two primary tones.*

S. P. T.

HYDRODYNAMIC ANALOGIES TO ELECTRICITY AND MAGNETISM

FROM a scientific and purely theoretical point of view there is no object in the whole of the Electrical Exhibition at Paris of greater interest than the remarkable collection of apparatus exhibited by Dr. C. A. Bjerknes of Christiania, and intended to show the fundamental phenomena of electricity and magnetism by the analogous ones of hydrodynamics. I will try to give a clear account of these experiments and the apparatus employed; but no description can convey any idea of the wonderful beauty of the actual experiments, whilst the mechanism itself is also of most exquisite construction. Every result which is thus shown by experiment had been previously predicted by Prof. Bjerknes as the result of his mathematical investigations.

It has long been known that if a tuning-fork be struck and held near to a light object like a balloon it attracts it. This is an old experiment, and the theory of it has been worked out more than once. Among others Sir William Thomson gave the theory in the *Philosophical Magazine* in 1867. In general words the explanation is that the air in the neighbourhood of the tuning-fork is rarefied by the agitation which it experiences. Consequently the pressure of the air is greater as the distance from the tuning-fork increases. Thus the pressure on the far side of the balloon is greater than that on the near side, and the balloon is attracted.

Dr. Bjerknes has followed out the theory of this action until he has succeeded in illustrating most of the fundamental phenomena of electricity and magnetism. He causes vibrations to take place in a trough of water about six inches deep. He uses a pair of cylinders fitted with pistons which are moved in and out by a gearing which regulates the length of stroke and also gives great rapidity. These cylinders simply act alternately as air-compressers and expanders, and they can be arranged so that both compress and both expand the air simultaneously, or in such a way that the one expands while the other compresses the air, and *vice versâ*. These cylinders are connected by thin india-rubber tubing and fine metal pipes to the various instruments. A very simple experiment consists in communicating pulsations to a pair of tambours, and observing their mutual actions. They consist each of a ring of metal faced at both sides with india-rubber and connected by a tube with the air-cylinders. One of them is held in the hand; the other is mounted in the water in a manner which leaves it free to move. It is then found that if the pulsations are of the same kind, *i.e.* if both expand and both contract simultaneously, there is attraction. But if one expands while the other contracts, and *vice versâ*, there is repulsion. In fact the phenomenon is the opposite of magnetical and electrical phenomena, for here like poles attract, and unlike poles repel.

Instead of having the pulsation of a drum we may use the oscillation of a sphere; and Dr. Bjerknes has mounted a beautiful piece of apparatus by which the compressions and expansions of air are used to cause a sphere to oscillate in the water. But in this case it must be noticed that opposite sides of the sphere are in opposite phases. In fact the sphere might be expected to act like a magnet; and so it does. If two oscillating spheres be brought near each other, then, if they are both moving to and from each other at the same time, there is attraction; but if one of them be turned round, so that both spheres move in the same direction in their oscillations, then there is repulsion. If one of these spheres be mounted so as to be free to move about a vertical axis, it is found that when a second oscillating sphere is brought near to it, the one which is free turns round its axis and sets itself so that both spheres in their oscillations are approaching each other or receding simultaneously. Two oscillating spheres, mounted at the extremities of an arm, with freedom to move, behave with respect to another oscillating sphere exactly like a magnet in the neighbourhood of another magnetic pole. I believe that these directive effects are perfectly new, both theoretically and experimentally. The professor mounts his rod with a sphere at each end in two ways: (1) so that the oscillations are along the arm, and (2) so that they are perpendicular. In all cases they behave as if each sphere was a little magnet with its axis lying along the direction of oscillation.

Dr. Bjerknes looks upon the water in his trough as being the analogue of Faraday's medium; and he looks upon these attractions and repulsions as being due, not to the action of one body on the other, but to the mutual action of one body and the water in contact with it. Viewed in this light, his first experiment is equivalent to saying that if a vibrating or oscillating body have its motions in the same direction as the water, the body moves away from the centre of disturbance, but if in the opposite direction, towards it. This idea gives us the analogy of dia- and para-magnetism. If, in the neighbourhood of a vibrating drum, we have a cork ball, retained under the water by a thread, the oscillations of the cork are greater than those of the water in contact with it, owing to its small mass, and are consequently *relatively* in the same direction. Accordingly we have repulsion, corresponding to diamagnetism. If, on the other hand, we hang in the water a ball which is heavier than water, its oscillations are not so great as that of the water in its vicinity, owing to its mass, and consequently the oscillations of the ball relatively to the water are in the opposite direction to those of the water itself, and there is attraction, corresponding to paramagnetism. A rod of cork and another of metal are suspended horizontally by threads in the trough. A vibrating drum is brought near to them; the cork rod sets itself equatorially, and the metal rod axially.

If a pellet of iron be floated by a cork on water and two similar poles (*e.g.* both north) be brought to its vicinity, one above and the other below the pellet, the latter cannot remain exactly in the centre, but will be repelled to a certain distance, beyond which however there is the usual attraction. The reason is that when the pellet is nearly in the line joining the two poles the north pole of the pellet (according to our supposition) is further from this line than the south one. The angle of action is less; so that although the north pole is further away, the horizontal component of the north pole repulsion may be greater than that of the south pole attraction. Dr. Bjerknes reproduces this experiment by causing two drums to pulsate in concord, the one above the other. A pellet fixed to a wire, which is attached by threads to two pieces of cork, is brought between the drums, and it is found impossible to cause it to remain in the centre.

Dr. Bjerknes conceived further the beautiful idea of tracing out the conditions of the vibrations of the water when acted on by pulsating drums. For this purpose he mounted a sphere or cylinder on a thin spring and fixed a fine paint-brush to the top of it. This is put into the water. The vibrations are in most cases so small that they could not be detected, but by regulating the pulsations so as to be isochronous with the vibrations of the spring, a powerful vibration can be set up. When this is done a glass plate mounted on four springs is lowered so as to touch the paint-brush, and the direction of a hydrodynamic line of force is depicted. Thus the whole field is explored and different diagrams are obtained according to the nature of the pulsations. Using two drums pulsating concordantly, we get a figure exactly like that produced by iron filings in a field of two similar magnetic poles. If the pulsations are discordant it is like the figure with two dissimilar poles. Three pulsating drums give a figure identical with that produced by three magnetic poles. The professor had previously calculated that the effects ought to be identical, and I think the same might have been gathered from the formulæ in Sir William Thomson's "Mathematical Theory of Magnetism," but this only enhances the beauty of the experimental confirmation.

Physicists have been in the habit of looking upon magnetism as some kind of molecular rotation. According to the present view it is a rectilinear motion. Physicists have been accustomed to look upon the conception of an isolated magnetic pole as an impossibility, but here, while the oscillating sphere represents a magnetic molecule with north and south poles, the pulsating drum represents an isolated pole. These are new conceptions to the physicist, let us see whither they lead us. The professor shows that if a rectilinear oscillation constitutes magnetism, a circular oscillation must signify an electric current, the axis of oscillation being the direction of the current. According to this view what would be the action of a ring through which a current is passing? If the ring were horizontal the inner parts of the ring would all rise together and all fall together, they would vibrate and produce the same effect as the rectilinear vibrations of a magnet. This is the analogue of the Amperian currents.

To illustrate the condition of the magnetic field in the neighbourhood of electric currents, Dr. Bjerknes mounted two wooden cylinders on vertical axes, connecting them by link-work, which enabled him to vibrate them in the same or opposite ways. To produce enough friction he was forced to employ syrup in place of water. The figures which are produced on the glass plate are in every case the same as those which are produced by iron filings in the neighbourhood of electric currents, including the case of currents going in parallel and in opposite directions.

The theory is carried out a step further to explain the attraction and subsequent repulsion after contact of an electrified and a neutral substance and the passage of a spark. But it is extremely speculative, and is not as yet experimentally illustrated, and I think that at present it is better to pass it by.

I believe that the professor will exhibit his experiments and give some account of his mathematical investigations, which have occupied his time for five years, to the Académie des Sciences this afternoon. His results have not been published before. GEORGE FORBES

Paris, August 15

NOTES

JOHN DUNCAN, the Alford weaver-botanist, has at last passed away, and his dust now lies under the earth whose beautiful children he knew and loved so well. He expired a little after noon on the 9th instant, in his eighty-seventh year, and was buried on the 16th in the old churchyard at Alford, in a selected spot, where a monument will soon be raised to his memory by the free-will offerings of those who admired his high character and pure-minded enthusiasm for science. The poor old man has not lived long to enjoy the comforts lately provided for him, but it is pleasant to think that this aged and unselfish student of nature passed the last days of his long and silent life in comparative affluence, and that he now rests in no pauper's grave. His life was so recently sketched in these pages (NATURE, vol. xxiii. p. 269) that it is unnecessary here again to rehearse it. In December last, when it was ascertained that, after an unusually laborious life, winning his daily bread by weaving, carried on till beyond his eighty-fifth year, he had through failing strength been at last reluctantly forced to fall on the parish for bare support, an appeal was made in his favour by Mr. Jolly, H.M. Inspector of Schools, in the newspaper press throughout the country, and in our own columns. The response was speedy and ample, so that in a very short time a sum of 326*l.* was spontaneously sent for his relief, with every expression of admiration and regret from all parts of the land, and from most of our most eminent scientific men, whose kindly appreciation of his scientific labours was not unfrequently very aptly and memorably put. His pride and appreciation of all this kindness were genuine, deep, and child-like, and were expressed not seldom in piquant and touching terms; so that his numerous friends have the great satisfaction of thinking, that by their means, though he has departed sooner than was anticipated, they have helped to comfort the evening of his days. His constitution was of the healthiest type, and his tenacity of life remarkable in a frame so exhausted, and he only passed away when the last particle of the expiring taper was slowly consumed. As already told in NATURE (vol. xxiv. p. 6), the money raised in John Duncan's behoof has been vested in seven trustees, under a trust-deed executed during his life. By its provisions his valuable books on botany and other sciences are bequeathed to the parish library of Alford for the use of the district; and all remaining funds are to be safely invested and the interest to be devoted for all time to the foundation of certain prizes, to be called by his name, for the promotion of the study of natural science, especially botany, amongst the children in certain parishes in and round the Vale of Alford. A memoir of the old man is now being written by Mr. Jolly, and will be anticipated with interest.

IN the death, on the 27th ult., of Mr. Hewett Cottrell Watson, at the age of seventy-seven, English botany has lost one of her most indefatigable workers. For the space of fifty years Mr. Watson has been a prolific writer on the geographical distribution of British plants, and on the distinguishing characters of the more "critical" species; and in these departments of botany he has left very few who can approach him in the extent and the accuracy of his knowledge. In addition to many smaller publications, and a vast number of contributions to periodical literature, the principal works with which his name will be associated are the "New Botanists' Guide" (1835–7), the "Cybele Britannica" (1847–59), and the numerous editions of the "London Catalogue of British Plants." His garden at Thames Ditton had long been an object of pilgrimage to botanists desirous of seeing growing specimens of rare or little-known species or varieties of British plants; and his judgment was the last appeal in questions of difficulty. In 1847 he spent three months investigating the flora of the Azores, which was then very little known, and added about 100 species to the flora of the Archipelago, many of which were new to science. Throughout life Mr. Watson was an ardent believer in phrenology; when a student at the University of Edinburgh he became acquainted with George and Andrew Combe; and was for a time editor of the *Phrenological Journal.*

PROF. RAOUL PICTET of Geneva, who has been giving his attention of late to marine architecture, announces, according to the *Times* correspondent, a discovery which, if his anticipations be realised, will effect a revolution in the art of shipbuilding and greatly augment the speed of sea-going and other ships. The discovery consists in a new method of construction and such an arrangement of the keel as will diminish the resistance of the water to the lowest possible point. Vessels built in the fashion devised by Prof. Pictet, instead of sinking their prows in the water as their speed increases, will rise out of the water the faster they go, in such a way that the only parts exposed to the friction of the water will be the sides of the hull and the neighbourhood of the wheel. In other words, ships thus constructed, instead of pushing their way through the water, will glide over it. According to the professor's calculations, in the accuracy of which he has the fullest confidence, steamers built after his design will attain a speed of from 50 to 60 kilometres the hour. A model steamer on the principle he has discovered is in course of construction at Geneva. The machinery has been ordered at Winterthur, and when ready the new vessel will make her trial trip on Lake Leman.

THE Electrical Exhibition, though now open to the public, seems far from being completely arranged. Our Paris correspondent writes that the English section was opened on Sunday, a result due to the personal exertions of the Earl of Crawford, the English Commissioner, which has caused great satisfaction. The organisation of the English section is highly approved. The evening sittings have been postponed for an indefinite number of days, owing to a series of mistakes in the engineering department. The electrical railway is not ready. In spite of these drawbacks the receipts of the first three days were from 4000 to 5000 francs each. On Sunday they were largely increased, although the fees were diminished by half. We hope shortly to refer to the Exhibition in detail. Independently of the Catalogue, the administration of the Exhibition has published a handbook on Electricity and its Applications, by Armingaud, Becquerel, Bert, Blanco, Breguet, Clerac, Deprez, Fontaine, Mascart, Reynaud, and others. *L'Électricité* has published a "Petit Vocabulaire raisonné" of every word used by electricians, with an introduction by W. de Fonvielle.

IT is announced that Miss Pogson, daughter of the Madras Government Astronomer, has been appointed Meteorological Reporter to the Government of that Presidency. Miss Pogson has for some years discharged with great ability the duties of Assistant Government Astronomer.

WE believe that the Royal Commission which has been constituted for the purpose of inquiring into and reporting upon the facilities for technical education in various countries is now practically complete. It will comprise Messrs. Samuelson, Slagg, Stevenson, and Woodall, the members respectively for Banbury, Manchester, South Shields, and Stoke-upon-Trent. Mr. Swire Smith of Huddersfield, Prof. Roscoe of Manchester, and Mr. Philip Magnus, the director of the City and Guilds of London Institute, have also accepted invitations, and Mr. G. R. Redgrave of the Science and Art Department will probably be selected to accompany the Commission as secretary. It is expected that the Commission will commence its travels about the middle of October.

THE British Association having decided to hold its annual meeting for 1882 at Southampton, a large and influential committee, including the Corporation and magistrates of the borough and the clergy and ministers of all denominations, has been appointed to make the necessary arrangements. A subscription and guarantee fund to cover the requisite expenses of the meeting has been commenced.

THE meeting of the International Congress at Bordeaux on the Phylloxera having been antedated to August 29, is now postponed, on account of the elections, till October 10.

THE Epping Forest and County of Essex Naturalists' Field Club held a Field Meeting at Chelmsford on Saturday, August 13, in conjunction with the subscribers to the "Essex and Chelmsford Museum." The Chelmsford Museum was visited under the guidance of the Rev. R. E. Bartlett, M.A., the hon. curator, and Mr. E. Durrant, the hon. secretary. After lunch the whole party proceeded in drags to Danbury Hill, the ancient camp of which was visited under the guidance of Mr. H. Corder; the company then assembled to hear an address by Prof. G. S. Boulger on "The Origin and Distribution of the British Flora." About six o'clock the party returned to Chelmsford to tea at the "Saracen's Head" Hotel, and an ordinary meeting of the Essex Club was held, the President, Mr. R. Meldola, occupying the chair. The President communicated on behalf of General Pitt-Rivers the report on the excavation of the ancient earthwork at Ambresbury Banks in Epping Forest. It appears that this investigation has been carried out with considerable success, a number of fragments of pottery of British construction having been found beneath the rampart on or near the old surface of the ground. The Club has thus so far settled the date of the camp by a single cutting, and the current theory that it was the work of the Romans must be abandoned. Although undoubtedly British, further excavations will be required before it can be decided whether it dates from before or after the Roman conquest. We are glad to see that the Great Eastern Railway Company has assisted the Club to a great extent by allowing the members to travel at greatly reduced fares on any of their lines within the County of Essex on the occasion of field or of ordinary meetings.

THE *Daily News* correspondent writes that the Swiss Seismological Commission, which, by the co-operation of its numerous members and correspondents, continues the work of simultaneous earthquake observation, has just issued a report on the earthquake of July 22. This shock was felt over a wide area. In France it extended over the departments of Drôme, Isère, Savoy, Upper Savoy, Saône et Loire, Ain, Jura, and Doubs. In Italy it affected chiefly the high valleys of North-Western Piedmont. In Switzerland the movement was observed in the cantons of Geneva, Vaud, Friburg, Neuchâtel, Solothurn, Basel, and the western districts of Aargau and Berne. From Valence to Basel, and from Chalons-sur-Saône to Suza and Zinzal, the region of disturbance included both sides of the Jura Mountains, besides traversing the great chain of the Alps. It affected an area 350 kilometres long and 250 kilometres wide, equal to 8000 square kilometres of surface. There were two very slight shocks on the evening of July 21, and a feeble shock at 12.10 on the morning of the 22nd. The principal shock, which took place at 2.48 a.m., was followed at 3.30 and 4.30 by two oscillations that were only just perceptible to the senses. The great shock consisted of two quakes and several smaller, but distinct, vibrations. In some localities as many as ten vibrations were counted. Relatively to its extent, the shock was intense; in the neighbourhood of Chambéry and Aix-les-Bains, chimneys fell and walls were fissured. In Switzerland the shock was stronger near the Jura than nearer the Alps, and especially strong at Geneva, in Vaud, and in Neuchâtel. Prof. Forel, who edits the report, remarks on the singular variations in the intensity and direction of the shock even in the same neighbourhood. These differences, which have been observed in previous earthquakes, are too great to be due solely to errors of observation. An earthquake is often more felt in one quarter of a town than in another; and as this variation is irregular, a locality that hardly feels a shock at all on one occasion feeling it on another, it cannot arise from differences in the density of the underlying strata. Prof. Forel offers no explanation of this phenomenon, albeit he thinks it ought to be explained, and craves for it the particular attention of his brother seismologists.

DR. K. VON FRITSCH of Halle discusses the subject of earthquakes in the last issue of the *Verhandlungen* of the Berlin Geographical Society. He maintains that the cause of earthquakes must be sought for at a rather small depth, the greatest depth ascertained not exceeding ten to fourteen miles, and usually far less, whilst rather feeble forces produce earthquakes which are felt at great distances. It is known that Krupp's hammer, which weighs 1000 centners. and falls from a height of three metres, produces sensible concussions on a surface of eight kilometres diameter; whilst the recent explosion of the Leimbach dynamite manufactory was felt at Halle and Merseburg, forty-one and forty-five kilometres distant. Whilst showing how easily concussions are produced by causes comparatively feeble, Dr. Fritsch points out how earthquakes might be and must be produced by the increase and decrease of volume of rocks under the influence of physical and chemical forces, and by concussions, by the opening of crevices in rocks, and by the subsidence of masses of rocks due to these agencies. Many schists are subjected, as is known, to extension, and when crevices arise the schists must enter into oscillations which must produce very varied phenomena, according to the direction and the force of the oscillations, much like to what we see in the oscillations of tuning-plates. Dr. Fritsch concludes by saying that future researches as to the causes of earthquakes ought to be directed especially to the study of the geotectonical conditions of the localities where they occur.

IN the course of the excavations for the new fort at Lier, in the neighbourhood of Antwerp, a number of bones of extinct animals, mammoth's teeth, and the almost complete skeleton of a rhinoceros have been dug up. It was in the same district that, in 1760, was found the immense skeleton of a mammoth, which has been preserved in the Natural History Museum at Brussels.

THE Faure accumulators have been tried again by the Paris Omnibus Company on a tramway with a carriage arranged for the purpose. The experiment is said to have been highly successful.

THE Committee formed some time ago for the exploration of the subsidences in Blackheath have published a report, in which, while giving an account of their proceedings, and the opinions of various geologists for the probable causes of the subsidences, they themselves have come to no definite conclusion.

A CONGRESS has been opened at Bordeaux on the education of the deaf and dumb. In connection therewith the *Journal Officiel* publishes a series of articles by M. Claveau, General Inspector of "Établissements de Bienfaisance," who tries to prove that the method of teaching the deaf and dumb how to speak was invented and practised by St. John of Beverley, Archbishop of York, in 865, and fully described by the Venerable Bede.

THE Meteorological "Centralanstalt" founded by the Swiss Naturalists' Society at Zurich has become a Government Institution by a decree of the Swiss Senate, and now bears the title "Swiss Meteorological Centralanstalt." Herr R. Billwiller has been appointed director, while the Swiss Home Secretary and a special Commission will superintend the Institution.

THE Royal University Bill (Ireland) on Tuesday night last was read a third time in the House of Commons, having been sent down from the House of Lords. It now only awaits the Royal Assent. The programme of the Natural Science course seems framed in accordance with modern views, and when the Scholarships and Exhibitions shall be finally settled by the Senate, we will probably refer again to the subject.

WE notice in the last number of the *Zeitschrift* of the Berlin Geographical Society (vol. xvi. fascicule 3) an interesting description of spring in Madagascar, from the pen of the late Herr J. M. Hildebrandt, who died on May 29 at Antananarivo. Spring arrives about the middle of November, when the cold south-eastern wind which blew throughout the winter, leaving its moisture on the eastern slopes of the highlands, covered with thick forests, and driving before it the savannah fires, gives place to the north-western wind which brings warmth and moisture. The revival of nature under the influence of this wind is well described by Herr Hildebrandt, and his paper contains valuable information as to the flora and fauna of Madagascar.

THE additions to the Zoological Society's Gardens during the past week include an Orange-winged Amazon (*Chrysotis amazonica*) from South America, presented by Mr. R. Seyd; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Sir Patrick Colquhoun; a Herring Gull (*Larus argentatus*), British, presented by Mr. E. A. Brown; a White-crested Touracou (*Corythaix albocristata*) from South Africa, presented by Capt. T. G. Steer; a Black-eared Marmoset (*Hapale penicillata*) from South-East Brazil, presented by Mrs. Alsop; an American Tapir, ♂ (*Tapirus terrestris*) from Trinidad, presented by Herr Fritz Zurcher; two West Indian Agutis (*Dasyprocta cristata*), three Garden's Night Herons (*Nycticorax gardeni*), and two Martinican Doves (*Zenaida martinicana*) from the Antilles, presented by Mr. H. T. Burford Hancock, F.Z.S.; two Stock Doves (*Columba œnas*), British, presented by Mr. A. E. C. Streatfield; two Topela Finches (*Munia topela*) from China, a Nutmeg Finch (*Munia punctularia*) from India, a Javan Nutmeg Finch (*Munia nisoria*) from Java, a Francis Eagle Owl (*Bubo pöensis*) from West Africa, two Aldrovandi's Skinks (*Plestiodon auratus*), and two Pantherine Toads (*Bufo pantherinus*) from North Africa, a Bay Antelope (*Cephalophus dorsalis*) and a Water Chevrotain (*Hyomoschus aquaticus*) from West Africa, purchased; and two Common Marmosets (*Hapale jacchus*) from Brazil, deposited. In the Insectarium may be seen full-fed larvæ, now spinning up, of the Atlas (*Attacus atlas*) and Ailanthus (*Attacus cynthia*) Silk-Moths, also freshly-hatched ones of the Marbled White Butterfly (*Arge galatea*) and Scarlet Tiger Moth (*Callimorpha dominula*). Amongst the aquatic forms examples of *Hydrous piceus* (the large Water-Beetle), *Pelobius hermanni*, *Notonecta glauca* and *Argyroneta aquatica* are at present exhibited.

GEOGRAPHICAL NOTES

THE current number of the Geographical Society's *Proceedings* gives the paper recently read by Mr. Whymper on some of the geographical results of his expedition among the the Ecuadorian Andes, with a diagram of his routes, while Mr. W. G. Lock supplies a contribution on Iceland, which is published at a convenient season for tourists. Mr. Lock's paper refers chiefly to the Askja volcano, the largest in the island, and is illustrated by a map of the east coast of Iceland. In the "Geographical Notes" a brief reference is made to this season's Arctic expeditions, and we are informed that Mr. Leigh Smith has lately sailed from Peterhead on his fifth Arctic expedition; and on reaching Franz-Josef Land he intends to construct a house and refuge at Eira Harbour, and afterwards to get as far north as possible. A very interesting account is given, from a letter recently sent home by M. de Brazza, of the results of his explorations and of the advantages of his route to Stanley Pool by the Ogowé as compared with Mr. Stanley's along the north bank of the Congo. After some news respecting Russian travellers an account is given of Messrs. Soltau's and Stevenson's journey from the Irawaddy to the Yangtsze, to which we recently referred. Under "Correspondence" is a letter from Major H. G. Raverty on the Dara'h of Nur, which does not leave a pleasant impression on the reader's mind.

LORD ABERDARE has finally accepted the office of representative of the British Government at the International Congress of Geographers at Venice, and he will of course act as chief delegate of the Geographical Society. The India Office and the Admiralty are sending maps, charts, &c., to the Exhibition, and the former will be represented by Lieut.-General Sir H. Thuillier, late Surveyor-General of India, and the latter by Sir F. J. Evans, Hydrographer of the Navy. It is probable that nothing further will be done to represent this country officially, as the Treasury sternly decline to furnish funds.

THE Italian North-African explorers, Massari and Matteucci, to whose journey we have repeatedly referred, instead of returning by Tripoli, as was expected, struck across the Continent and came out at the Gulf of Guinea. Only a few days ago they arrived at Liverpool, and it is sad to record that, after so successfully accomplishing an arduous work, Dr. Matteucci has succumbed to African fever. He died on the morning of his arrival in London last week; his body has been conveyed to his native city, Bologna. Matteucci was only twenty-nine years of age.

LETTERS from Zanzibar of the 1st ult. notify the arrival there of Mr. Thomson, the African traveller, whose services have been engaged by the Sultan to examine and report on the mineralogy of the mainland. It is his Highness's wish that Mr. Thomson's first surveys should be devoted to the discovery of coal mines, of which several are said to exist not far from the coast. His Highness writes that he intends sending the explorer shortly to Makindarry, which is to be the centre of his future operations.

LETTERS from the steamer *Oscar Dickson* have been received at Gothenburg. The steamer, as our readers will remember, was frozen in at the mouth of the Yenisei River in 72° lat. N., and between 76° and 77° long. E. The winter was successfully passed, the difficulties the crew experienced were great, however. The sun was below the horizon for seventy days, and the cold rose to – 41° C. During March and April enormous masses of snow fell, so that it covered the ice to a height of seven feet above the ship's deck; the thickness of the ice was seven and a half feet.

WHAT might have been the climate during the Glacial Period is the subject of an interesting paper published by Dr. Woeikoff in the last issue of the *Zeitschrift* of the Berlin Geographical Society (vol. xvi. fasc. 3). It is well established now that for the formation of glaciers, not only a sufficiently low temperature is necessary, but also a sufficient supply of moisture in the atmosphere. Thus, on the Woznesensky gold-mine, which lies at a height of 920 metres and has a mean temperature of – 9° Celsius, but a rather dry climate, we have no glaciers, nor in the Verkhoyansk Mountains, where the mean temperature is as low as – 15°·6, and the temperature of January is – 48°·6. To show these differences Dr. Woeikoff prepares a table of the temperatures at the lowest ends of glaciers, and we see from his figures that in Western Norway, at the end of the Jostedal glacier (400 metres high), the mean temperature is 4°·8 Cels., 5°·8 at the end of the Mont Blanc glaciers (1099 metres), 6°·8 at the Karakorum glaciers in Tibet (3012 metres), and even 7° on the western slope (212 metres) of the New Zealand highlands, and 10° on the eastern slope (835 metres). In other countries, as, for instance, on the Mounkou Sardyk Mountain, in Eastern Siberia (3270 metres), the mean temperature at the end of the glaciers is as low as – 10°·2, and – 2°·4 in the Daghestan Mountains of the Caucasus. Thus the difference of mean temperatures at the lower ends of glaciers reaches as much as fully 20°. Besides we see that, provided the quantity of rain and snow is great, glaciers descend as low as 212 metres above the sea-level in a country (New Zealand) which has the latitude of Nice and the mean temperature of Vienna and Brussels, that is, higher than that of Geneva, Odessa, and Astrakhan, whilst the average temperature of winter is higher there than that at Florence. Further, Dr. Woeikoff discusses the rather neglected influence of large masses of snow upon the temperature of a country during the summer, and by means of very interesting calculations he shows how much the temperature of summer in higher latitudes is below what it ought to be in consequence of heat received from the sun, and *vice versâ* during the winter, these differences being due on the one side to the refrigerating power of snow, and on the other side to the heating power of sea-currents. In a following paper he proposes to discuss the other causes which might have influenced the climate of different parts of the earth during the Glacial period.

WE notice in the *Verhandlungen* of the Berlin Geographical Society (vol. viii. fascicule 5) a full report on the surveys which were made in the Russian Empire, including Caucasus, Siberia, Turkestan, and the Orenburg military district, during the year 1880. This is translated from the official report published in the organ of the Ministry of War.

DURING the last session of the German Reichstag, Dr. Thilenius, Prof. Virchow, and Herr von Wedell Malchow presented a petition to the Government requesting the participation of Germany in the exploration of the Polar regions proposed by the late Karl Weyprecht in the interest of meteorology, geology, and other sciences. It is now announced that the German Government will probably soon take steps in this direction, and will first give its principal attention to securing the co-operation of other nations.

HEFT VIII. of Petermann's *Mittheilungen* begins with a paper by Dr. Danckelman on the Temperature Conditions of the Russian Empire, after Dr. Wild. The other papers are on M. Desiré Charnay's Expedition in Central America, Dr. I. B. Balfour's visit to Socotra, the Irawaddy above Bamo according to the data collected by the Indian Pundit in 1879–80 (with a map), and an article on the unfortunate Flatters Expedition by Dr. Rohlfs.

IN the *Bulletin* of the Antwerp Geographical Society (tome vi. 2e fasc.) is a paper of much interest by Dr. Delgeur on the Geographical Knowledge of the Ancient Egyptians.

MESSRS. LONGMANS AND STANFORD have published an enlarged edition of the Alpine Club Map of Switzerland. As it is issued in a number of separate sheets, it ought to prove useful to tourists.

THE Geographical Society of Lisbon has resolved to send an exploring party into the Sierra d'Estrella for scientific investigation. The mountain chain in question has never been scientifically explored.

IN 1879 Mr. L. Loth, a Government surveyor in Dutch Guiana, made a survey of a considerable portion of the River Saramaca, and his map of its course, on the scale of 1 : 400,000, together with an account of his expedition, has lately been published in the *Transactions* of the Amsterdam Geographical Society.

THE *Oesterreichische Monatsschrift für den Orient* of this month contains an interesting article on the new Conseil de Santé et d'Hygiène publique en Egypte by Prof. v. Sigmund, a well-known authority of the Medical High College of Vienna. Amongst various other papers we may mention an essay on Japanese paper manufacture by Dr. Rudel of Dresden, and an article on the wines of Cyprus by Dr. Richter of Larnaca.

SOLAR PHYSICS—THE CHEMISTRY OF THE SUN[1]

The Test supplied by Change of Refrangibility

WE have then got so far. Limiting our studies to iron we find that the prominence spectrum is made up of one set of lines seen in the terrestrial spectrum, and the spot spectrum made up of another set. And more than this, if we add the lines seen in the prominence and spot spectra together we do not then by any means make up the complete spectrum.

It is fair to ask the following question :—Have we any other means of establishing this extraordinary fact of the separation of the iron lines in spots and storms? We have. Reference has already been made to the change of refrangibility of the lines brought about by the change of velocity of movement of the various solar vapours. But if, as already hinted, the lines of iron behave to each other in precisely the same way as the lines of two perfectly distinct substances behave to each other; then if we observe changes of refrangibility in the iron lines, both in spots and flames, we should get the same differentiation as we have already got in the lines thickened or intensified in the spectra of spots and flames.

We will now see the results which have been obtained along this line of research, and it should be pointed out that it is not a method by which it is easy in a short time to accumulate a large number of observations, because metallic prominences are very rare except at the sun-spot maximum, and in the case of spots we not only want a spot, but we want that spot to be in a very considerable state of commotion, in order that the change of refrangibility may be obvious enough to enable us to record the phenomena.

So far as this inquiry has gone at present we have only observed the lines contorted in spots.

In the diagram (Fig. 37) the zig-zag lines indicate the iron lines which changed their refrangibility in a number of spots observed at the end of last year. The point is that, although we have a great many of the iron lines bent, twisted, contorted—with their refrangibility changed, yet some of the iron lines mixed with them give us no indication of movement. All these observations have been made upon lines seen at the same moment in the

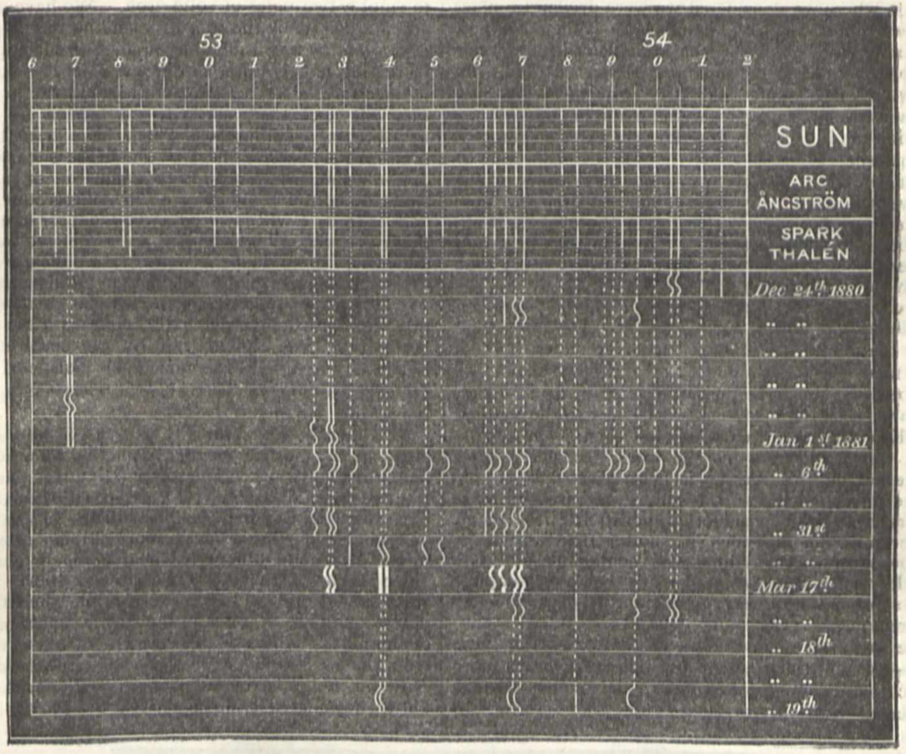

FIG. 37.—Different rates of motion registered by different iron lines.

same field of view. Observations of this nature exist twelve years old, but no importance can be attached to them, for the reason that the phenomenon was not understood, as I hope it is understood now, and precautions were not taken in the observations then made to show that no motion of the slit across the spot took place in the interval between the two observations. For, of course, it is not fair to compare a line which one sees in one part of the spectrum with a line seen in another, unless one is absolutely certain that the slit has not moved on the sun's image; because one-thousandth part of an inch on the sun's image means a good many miles on the sun. Referring to Fig. 37 we have, at wave-length 5366–70, three lines, two in motion, and one at rest, all belonging to a well-known group of iron lines. At a later date we have the line at 5382 at rest, while that at 5378 is in motion. Thus it will be seen that these points and others prove there is just as much individuality in the way in which the lines of iron change their refrangibility as there is in the way in which one particular line, and then another, is thickened in a sun-spot or brightened in a prominence; and if we go further we find this very interesting and additional fact, that the lines which are not contorted are in a great many cases precisely those lines which are seen in the flames, but not in the spots.

It is seen therefore that the evidence afforded by change of refrangibility is of like nature to that afforded by the thickening of lines in spots and brightening of lines in flames.

The explanation which lies on the surface is that the vapours in the flames produce one set of lines in one place or at a certain temperature, and the vapours in the spots produce another.

Sometimes these vapours are mixed up by up-or-down rushes, and sometimes therefore the lines are common.

Bearing of these Observations on the Origin of the Fraunhofer Lines

At the end of the last lecture it was pointed out that the observations we are now discussing seem to indicate that in time we may be able to say that the absorption to which any particular Fraunhofer line is due takes place in a certain region of the solar atmosphere, whereas formerly we could only say that it was produced by absorption *somewhere*.

[1] Lectures in the Course on Solar Physics at South Kensington (see p. 150). Revised from shorthand notes. Continued from p. 324.

The time has now come, I think, to go into this question in more detail.

Let us consider the maps. Of the 96 iron lines in that first region which we considered only 4 are seen in the flames; 92 of those therefore must not be looked for in the flame region, for the reason that twelve years of patient work have not divulged their existence. Again of these same 96 lines only about 32 are seen in the spots at all extensively affected. It is useless therefore to look for the remaining 60 lines or thereabouts in the same spot region, for the reason that they have been looked for for a long time without being seen equally widened.

Of course it must be remembered that these changes are due to *change of intensity*, and that other lines may be there of an intensity so low that they have escaped the keen eyes of those anxious to chronicle them. Still it will be acknowledged, I think, that the method of treatment I have adopted is the best open to us, and is a fair one on the whole.

The facts being so, it looks really as if the origin of the mass of the absorption to which the Fraunhofer spectrum of iron is due is to be sought in a region of the solar atmosphere much nearer to the place assigned to it by Kirchhoff originally than to that lower region where we considered we were driven to place it when the new method was first established. When the new method had been working for some considerable time observers recorded hydrogen with magnesium underlying it, and with sodium underlying that. And since they were metals of low atomic weight and vapour density we were justified in considering them as occupying the highest levels—the very extreme limit of the solar atmosphere.

It was therefore fair to argue that if the substances of the lowest atomic weights were really close to the photosphere, those of highest atomic weights were really in the photosphere itself, and therefore, being in the photosphere, the absorption by means of which we were able to determine their existence really took place in or near the photosphere.

This later work, I think, seems to show that that view requires reconsideration; and it may well be that subsequent work will show that those Fraunhofer lines, which we do not trace in flames and which we do not trace in the spots, are probably absorbed in a cooler, higher region of the atmosphere, much more nearly occupying the place assigned to the general atmosphere by Kirchhoff than that which has been given to it by later observers. If we accept this the work becomes a little plainer, and the reason that we get such an excessively simple spectrum in the lower reaches of the sun is because the more complex vapours exist at a considerable elevation above them, and as the interior of the sun must be hotter than any of its envelopes, no cold substances—nothing approaching the solid state which we have learnt for many years gives us the most complete spectrum of the substance—nothing approaching a solid can enter those charmed regions.

Therefore we are also again driven to the view that these cooler vapours—vapours much nearer the solid state, much more condensed, much more complex than those which can exist alone in the hottest layer—probably originate the great mass of absorption; that is, many lines not traced either in spots or flames are produced in the higher regions.

If this be so, the Fraunhofer spectrum is really not the spectrum of any particular part of the sun; but because it contains lines thickened in the spots, lines brightened in the flames, and other lines about which we know nothing, it must really be the summation of the absorption of the different strata which compose the solar atmosphere; so that chemically the solar atmosphere, with regard to the iron spectrum, gets more and more complex every mile we go upwards. Of course, too, if this is good for iron it is good for every other substance which we believe to exist in, or to have some connection with, the solar atmosphere.

Further Test supplied by this View

If this be so we really can go on with our tests; we can bring the laboratory into the field, and we must learn in our laboratory experiments to make abstraction of those lines which are due to the more complex masses reduced by the transcendental temperature which we employ, if there is any truth in the view that I am bringing before you. In a laboratory experiment, for instance, when we want to observe the vapour of iron we have to employ two poles of solid iron. We have no means, such as are afforded us by the sun, of shielding the precise part we want to observe by a considerable number of envelopes of gradually-increasing temperature, so that even if we can get the highest temperature in the laboratory this result of the highest temperature will be cloaked, masked, and hidden by all those results, by all those simplifications which have been brought about to produce that precise effect of the highest temperature. So that the only thing we can do is to watch the intensities of the lines when we considerably change our temperature. I am speaking now of iron. I will show by and by that for some other substances there is a method which enables us to get over this excessive difficulty, for no doubt a very great difficulty it is; but in the case of iron, that really is the only thing that remains to us. Fig. 38 will give an idea of the way in which we may be misled if we do not examine our light source with the greatest care. It is engraved from a photograph of the spectrum taken between two poles of a Siemens machine, moistened with a salt of calcium, an image of the vertical poles having been thrown on the vertical slit.

It is seen how wonderfully we get the simplifications brought about by the electric current, depicting themselves in two perfectly distinct ways. The lower part gives the spectrum of the positive pole, and the upper part of the spectrum of the negative pole. In the first place it will be seen that there is no axis of symmetry for these lines; some of them elongate considerably in one direction; others of them elongate considerably in the other; some of them are exceedingly short, and only appear close to that region of the negative pole where the lines broaden; others again are brighter in the region much nearer the middle of the field. Others of the lines start from a region far removed from the arc; others again seem to start almost in the arc itself. Now this not only reminds one of what one sees in a solar storm, but it shows us most distinctly that even in the electric arc, when we have had time to study it sufficiently, these very simplifications which we have been so long in search of may be recognised eventually and permanently recorded.

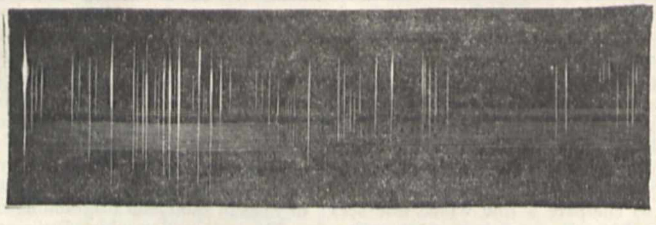

FIG. 38.—Photograph of the spectrum of the poles, showing that the lines start and end in different levels.

Tests supplied by the Variations between Solar and Terrestrial Spectra

Attention has been called to Kirchhoff's statement that the existence of the terrestrial elements in the sun is established by the fact of the coincidence of wave-length and *intensity* between the lines visible in our laboratories and the lines recorded as existing in the solar spectrum.

We have now arrived at a point when we can discuss this with advantage.

I propose to show first that the statement is not true; and, secondly, how the tests supplied by the variations from terrestrial spectra can be explained on, and bring most valuable confirmation to, my view. We are now able to say that at least two causes are at work, and they will require to be discussed separately.

But first as to the facts. We have already seen what enormous differences there are in the spectrum of calcium under different conditions. In the diagram of the calcium spectrum (Fig. 28) we saw that H and K, the most important lines in the sun, are really thin lines at the temperature of the electric arc, but that they kept intensifying and were rendered visible almost alone, when, instead of using the electric arc we used an induced current of considerable tension. But when we pass from the case of calcium, which occupied the attention of solar observers several years ago, to other elements, and when we go still more into the minute anatomy of the thing, we find that the further we go the less final is the statement that the matching in intensity of the lines is perfect.

Nor is this all. *Not only is the matching less perfect in intensity, but whole reaches of lines in various spectra are left out which cannot be accounted for on the long and short principle.* It has been before pointed out that of the 26 lines of aluminium, 2 only being left in the solar spectrum is easily explained, because the 24 dropped were short lines. But when we come to other elements, we find of adjacent lines—lines of equal length, and which, so far as we can gather, ought to be equally represented in the sun—one is absent, and one is present, probably with more intensity than it would seem to deserve from its behaviour among other lines of the spectrum. A table will best exhibit the sort of variation that crops up and insists on being recorded when the solar spectrum is photographed in anything like the detail which it absolutely demands. The method of recording will be at once understood.

Metal.	Wave-length.	Intensity in sun. 1=darkest.	Intensity in photograph. 1=brightest.	Intensity. Thalèn. 1=brightest.	
Mn	4083·0	4	2	5	
	4083·5	3	2	3	
Fe	4197·5	1	2	–	
	4198·1	3	2	–	Stronger line.
Co	4118·0	2	1	–	
	4120·5	4	1	–	Stronger line.
Ni	4458·6	2	2	–	
	4647·8	3	2	5	
Cr	4344·4	4	3	2	
	4351·8	3	3	2	
Mo	4706·5	3	1	4	
	4757·5	0	1	4	
W	4842·0	5	1	1	
	4887·5	3	2	2	
Ti	3980·8	2	1	–	
	3989·25	1	1	–	
Zn	4679·5	3	1	1	
	4721·4	4	1	1	
Pt	4442·0	4	2	4	
	4551·8	3	2	2	
Pd	3893·0	1	1	–	
	3958·0	3	1	–	Stronger line.
Zr	3957·22	2	1	–	
	3990·45	3	1	–	Stronger line.
Di	3989·65	0	1	–	
	3993·98	3	1	–	
Rb	4201·0	0	1	2	Stronger line.
	4215·5	3	1	0	

Now if Kirchhoff's view be anything like a representation of the whole truth there ought not to be any difference between these intensities; the line least intense in the photograph ought to be least intense in Thalèn's tables, and if it existed in the sun at all, it ought to be least intense amongst the Fraunhoferic lines, but as a matter of fact, there is an absolute inversion. The cobalt line 4120·5 is four times as intense in the sun as in the photograph; in the titanium line 3989·25 the intensities are equal; while in tungsten 4842·0 they are inverted, being represented as of minimum intensity in the sun, and maximum by Thalèn and in the photograph. In the sun one of the lines of iron is given as of first, and the other as of third intensity, while in the photograph they are both of second order. Again, in didymium we get a first order line recorded in the photograph which is absent from the sun altogether, whereas another line of the first order near it is there as a line of small intensity; so also in rubidium, and so we might go on. Indeed it is evident that the moment we go into minute details in this work we find that the general statement requires a very considerable amount of modification. And in addition to that too, there comes the question, how on this theory of the identity of the nature of the substances in the earth and the sun, are we to account for the bright lines seen in the sun itself—for the bright lines seen in the photosphere, to say nothing of those seen in the chromosphere — which have no corresponding Fraunhofer lines at all—lines so numerous that in a prominence of moderate complexity we may say that half the lines are absolutely unknown to us? Now when the other lines observable under these conditions—lines which we can get accurately, are lines known to us (we are dealing with the product of the very highest temperatures which we can command) we are justified, I think, in imagining that these lines which we do not get at, are lines which we could get at if we could proceed a little further. They elude our grasp; we know nothing about them; we put a query against them all because we cannot get at that stage of temperature at which they are produced.

There is one very beautiful case of this kind that comes out from Tacchini's observations (Fig. 39). From the beginning of February, 1872, Tacchini had observed the two iron lines 4922·5, 5016·5, when suddenly the whole rhythm of his observation was broken, and at the end of December, 1872, these iron lines ceased to be visible in the flames altogether.

On no one occasion after this for some time was either of these iron lines observable, but from January to September, 1873, he saw two lines of wave-lengths, 4943 and 5031, about which absolutely nothing whatever is known; so that it really is, I think, a perfectly justifiable suggestion that these lines are the spectrum of a substance which exists in the flames which is produced at a much higher temperature than that needed to give us those other forms of "iron" which produce the lines in the spots.

That is a suggestion which is obvious from a reference to the maps, and if it is correct we must acknowledge that when the sun was in that intense state of quiescence that there were no downward currents—nothing to bring the cooler vapours from the higher regions of the sun down to obstruct the general tenour of the solar way in the flame region, that at last, in consequence of this wonderful tranquillity, even the iron lines—the only two lines which indicate the presence of iron in the flames—faded away because iron, as we know it, faded away. There is no other explanation that I know of. In addition to those two lines we have two other lines about which we know nothing, except that they are probably due to a temperature which we cannot approach.

Special Test with regard to Iron

Part of the work which has been undertaken in connection with this special branch of the investigation, has been a careful inquiry into the changes brought about in the spectrum of iron by exposing it to as widely different temperatures as possible. The research is a very laborious one, and it may be some day we shall get a very much better record than that which my assistants and myself have produced; what we have been able to do we have done over the region of the spectrum which we have already worked over in the spots and flames.

In different horizons we have recorded the results observed when we use either the arc or the coil, or the oxyhydrogen flame or the Bessemer flame or some other light-source, and we vary in each case, as far as can be, the temperature employed. For instance, when we use the quantity coil we use a big jar, a little jar, and no jar at all; and the same with the intensity coil. Now if this map is carefully studied,[1] it will be seen that the intensity of the lines is very considerably changed when we pass from one set of observations taken under one set of conditions, to another set taken under other conditions. It is not a mere question of dropping out the lines when we pass from the temperature of the arc to the temperature of the coil, but it really is a considerable intensification of certain of the lines under certain conditions. There are three conditions under which we get the two lines 5339 and 5340, and they are not seen afterwards. The line 5433 is seen rather faint in the sun and very strong in the Bessemer flame. 5197·5 is very faint in the sun, but its intensity is doubled and even trebled with certain conditions of the quantity coil. I have introduced these facts to point a remark about Kirchhoff's statement; when Kirchhoff made that statement he was amply justified by the science of the time. He was familiar naturally with the spectrum of iron, which he had studied in his own laboratory, and other good observations of the spectrum of iron had been recorded in his time. But, with observations like these before one, which

[1] The map is too large and too detailed to be reproduced here.

one must take into account; it is too *coarse* a statement—I do not use the word in any offensive sense—to say that the iron lines in the sun correspond with the iron lines seen on the earth. Which iron lines—which of these horizons—are to be taken? It will be seen in a moment, if there are differences between these horizons, that if we take any one, we throw all the others out of court, and we have no right to do that; so that statement about the coincidence in the intensity could not be made with the facts now at our disposal. Any one wishing to make that statement would have to go over that work, and he would, following it honestly, I believe, find that the statement was true in no instance whatever. Fig. 40, which is an engraving from a photograph, will show the kind of difference one gets, even when one deals with the electric arc, which undoubtedly gives an iron spectrum which is the nearest approximation to the Fraunhoferic spectrum. The lines at wave-lengths 4325·0, 4300·7, 4271·0 are three of the strongest iron lines in the arc spectrum, and those at 4071·0, 4063·0, 4045·0 are also strong iron lines, though less strong than the others. Now it will be seen that in the solar spectrum the last three are much more important, much thicker, and much darker than the first, so that here is an absolute inversion in the thickness of the lines. I

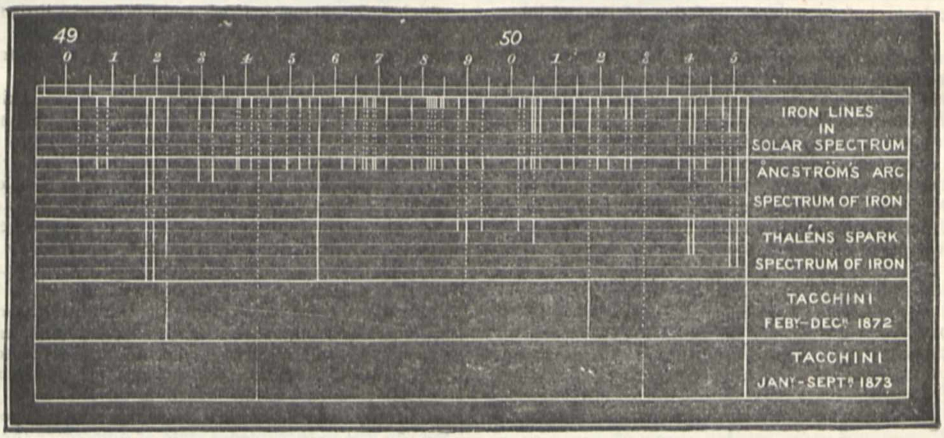

FIG. 39.

appeal to the photograph because there is no partiality about it; it has no view, no anxiety therefore to intensify one particular part of it at the expense of the other. This photograph is referred to only as the exemplar of many similar reversals which we see whenever such observations are made.

Let us now take some iron lines which have been studied in spots and storms, and consider the differences in their intensity among the Fraunhofer lines. We may also note the changes brought about in our laboratories.

The diagram (Fig. 41) gives the main results in a convenient manner. It does not profess to go over the whole ground, but I think it will enable me to point out the way in which the phenomena observed on the sun are re-echoed and endorsed by the work which has been done in the laboratory,

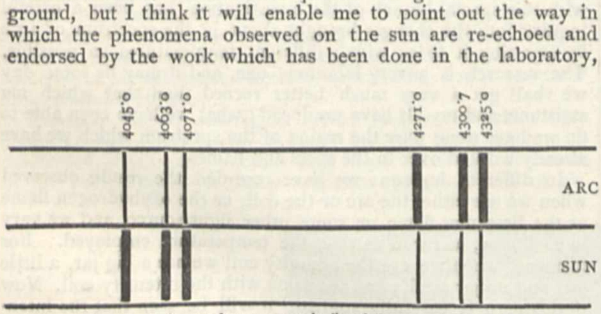

FIG. 40.—Anomalous reversals (iron) from a photograph.

and how severe the tests applied have been, and how well the view has borne the strain.

The diagram refers to three lines visible in the first map—three lines that in an instrument of ordinary dispersion might easily be mistaken for a single line in the sun. We have, as before, the intensities among the Fraunhofer lines recorded in the upper part of the diagram; we then go to our photographs of the arc, and find that the line at 4923·2 is entirely absent. We then pass on to the quantity coil, which gives us the three lines; but there is a difference between the intensities of the lines as seen in the quantity coil with a jar, and the lines seen in the sun, 4918 being thinner than in the sun. If we take the jar out of circuit 4923·2 almost disappears, and we get very nearly the same result as we get from the arc. We then try the intensity coil, which is supposed to give us an equivalent or higher temperature than the quantity coil does. What do we find there? That 4923·2 is enormously expanded and developed, apparently at the expense of 4918, which becomes thin. Taking the jar out, we come back to a result which is very much like the solar spectrum, with the difference, however, that 4918 is somewhat less intense than in the sun. Then come the facts which have already been brought forward throughout with special reference to these particular lines, that the two lines which are seen alone in the arc are seen alone in the spots, or at all events in 73 spots out of 100; and the other line which is so enormously

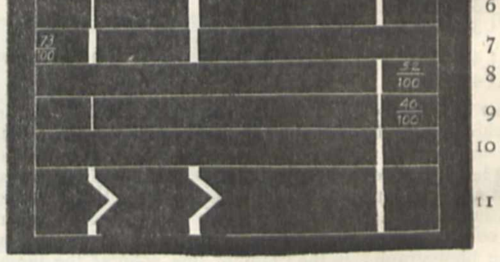

FIG. 41.—Diagram showing the behaviour of three iron lines under different conditions, solar and terrestrial. 1, solar spectrum; 2, arc; 3, quantity coil with jar; 4, quantity coil without jar; 5, intensity coil with jar; 6, intensity coil without jar; 7, spots observed at Kensington; 8, Prominences observed by Tacchini; 9, prominences observed by Young; 10, reversed in penumbra of spot observed on August 5, 1872, by Young; 11, motion indicated by change of refrangibility.

expanded when we use the highest temperature is seen alone in 52 out of 100 prominences by Tacchini. Again, further connecting this diagram with the last one, we have found in several cases when a change of refrangibility has been observed in the iron lines in the spots visible on the sun that the two lines 4918 and

4919·8 have been affected, while 4923·2 has remained at rest. That will give an idea of the way in which we really do find the laboratory work and the observatory work, each coming to the rescue of the other, each helping us to understand something which, without the other record, would be excessively difficult.

Tests supplied by the Absence of Lines from the Solar Spectrum

It is my conviction that many lines of the different chemical substances are absent from the solar spectrum when that absence cannot be attributed to anything depending upon reduction in the quantity of the substance present. In connection with this point there is an experiment to which attention may now be directed, because it is an attempt to imitate solar conditions somewhat; so that the inquiry is rendered possible as to whether these lines may not owe their absence from the Fraunhofer lines to their being the product of a very low temperature, a temperature which we cannot expect to find in the sun in any regions where the pressure would be sufficient to enable any absorption phenomena to take place. The point of the experiment is this: There are bodies which we can render incandescent at low temperatures. For iron, as we have already seen, we have to use a coil, but such substances as magnesium, sodium, lithium and the like can be volatilised at the temperature of the Bunsen flame, and at that temperature we get a certain spectrum from them. Now a great many different spectra have been recorded by different observers for these bodies, and the question was, could we get any independent method of determining which lines were really due to high and which to low temperatures. Now it is generally conceded that the temperature of the Bunsen flame is lower than the temperature of an induction spark; and we have an arrangement by which we can pass a spark between horizontal platinum poles through a flame in which the substance to be experimented on is volatilised. In this way we can see what change in the spectrum is introduced by the passage from the temperature of the flame to the temperature of the spark. We can fill the flame with the vapour, say of sodium, and observe its spectrum; then when the flame is nicely charged in the region between the two poles, we can pass a spark through it, and by throwing the image of the spark upon the slit of the spectroscope we can first of all get a spectrum of the flame, and then the spectrum also of that particular part of the flame through which the spark is passing. Now we really have got a good deal of light from that method of observation. In the case of magnesium, for instance, the change is very striking (see Fig. 42).

The flame gives us a spectrum in which are seen two lines corresponding with the two least refrangible members (b_1 and b_2) of a very prominent group of lines in the green part of the solar spectrum, and associated with these is a less refrangible line unrepresented in the sun, the whole forming a wide triplet. On passing the spark this last line is very greatly enfeebled, if not abolished altogether, for the very obvious reason that the molecule which gives rise to it is dissociated more rapidly at the temperature of the spark than it is at the temperature of the flame, and as that line dies out another solar line (b_4) appears, the three forming a triplet similar to, but narrower than, that

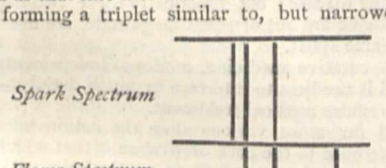

FIG. 42.—Flame and spark spectra of magnesium.

produced in the flame alone. Kirchhoff showed that potassium was not present in the sun, the line upon which he worked being the red line which is seen when potassium is thrown into a flame. The fact that we get that red line in the flame shows that it is a line produced by a low temperature; the molecule which produces the vibration therefore may probably be one which is produced at a low temperature. But when we pass a spark through a vapour giving us that red line we do not increase, but rather reduce, the intensity of the line, and we bring a great many lines into prominence which were not seen before, and those lines, I believe we are justified in saying, do exist among the Fraunhofer lines. In the same way we can colour the flame red with lithium, but the red line of lithium is not in the sun; but by passing a spark through lithium vapour we can intensify the line in the yellow and the line in the blue; and the line in the blue is undoubtedly among the Fraunhofer lines. Therefore it appears that we really can account for a great many of these variations in the solar spectrum by simply assuming that those lines which are absent represent molecular groupings so complex that there is no part of the sun where their absorption could be visibly produced, cold enough to allow them to exist.

Test supplied by the Lines strengthened in Spots and Flames and those seen in the Spectra of Two or more Substances

It has already been pointed out that these lines, which have been called basic lines, have been tested in two ways. In the first place, a list of lines had been prepared from Ångström's tables and Thalèn's tables, and then they had been discussed in connection with the bright lines seen by Young in his observations on Mount Sherman. The result was striking, inasmuch as of the 345 lines which were included by Young, only a small number of which were seen in spots and storms, 15 of the lines which were recorded as common to two substances by Thalèn, had been seen almost without exception, the only exception being in the case of some of the spots. The attack was then varied by taking 100 observations of sun-spots at Kensington, determining, without any reference to the basic nature of the lines at all, the 12 most widened lines in each spot which it was possible to observe; and then taking, side by side with these observations of the spots, 100 observations of flames from the rich store which has been recorded by Prof. Tacchini of Palermo. Then again, without reference to the basic character of the lines, to plot the lines down in each flame day by day.

As a reminder we may again refer to the diagram already given (Fig. 36). It will be remembered that the result was a very remarkable one. We found the lines of iron (we limited ourselves to iron) seen in the spots were few in number; that the lines of iron seen in the flames were still fewer in number, and moreover that the lines seen in the flames were not the lines seen in the spots. That was a result which might have been considered as very extraordinary if we had brought to it no other considerations than those with which we were conversant ten years ago when the work began.

What we have to do now, then, is to find what has been the result of this inquiry with regard to the basic nature of these lines. Have we, as a matter of fact, or have we not, in these most widened lines in spots, and the most brightened lines in flames, picked out those lines which are common to two substances. The facts are these:—We have, in the first horizon of the lower part of the accompanying map, the lines recorded by Ångström in his first memoir as common to two substances; the names of the two substances being given below. In the fourth horizon we have the observations of Thalèn made a few years after the observations of Ångström. And in passing from Ångström to Thalèn we pass from the temperature of the arc to the temperature of the induction coil. Now it will be seen that Thalèn also gives us lines in some cases agreeing with Ångström's, in other cases extending the information given by him, and in order to make this work as complete as possible we have gone over this region with the arc as Ångström did, and with the induction coil as Thalèn did, only we have had the advantage probably of using a more powerful coil. In fact we have used two coils—one so arranged as to give us the maximum effect of tension, and the other the maximum effect of quantity. In the first place it will be seen there is a general agreement between the observations—an agreement marred only in appearance here and there by the fact that in some cases the lines are so near the position of air lines that it has been impossible to make the observation absolutely complete. In other cases the appearance of imperfection arises from the fact that lines which are not seen at the temperature of the arc begin to make their appearance at the temperature of the coil; so that in a case like *that* at wave-length 5017, for instance, where Ångström gives no line as common to two substances, yet Thalèn does. We find that both are right; that at the temperature of the electric arc that line does not appear in one substance or the other, while at the temperature both of the quantity and the intensity induction coil the line is certainly there. In Fig. 36 A represents Ångström's work, T Thalèn's, and L Q and L I my own work with the quantity and intensity coil.

What then is the total result? It is this—that every important line in the spots, every important line in the storms, has been picked up by this method, and in fact *the map of basic lines along this region is practically a map of the lines widened in spots and present in storms, and nothing else.* Now it may be said that result is interesting, and perhaps important, but that it deals only with a very limited part of the inquiry. That is perfectly true.

The spectrum of iron, and the spectra of other substances have however been attacked in other regions. It is unnecessary to go into many details, but the general result is the same; in other regions we have as in the old region an almost perfect coincidence between the lines most widened in spots, and the lines regarded as basic by previous observers.

So much then for the result in the case of iron, to which, although we have not absolutely limited our attention, we have to a very large extent confined it. This result may be expressed in rather a different way, and it will then be easy to see the extraordinary parallelism which goes on between two perfectly distinct sets of facts, first, the statement of the spectroscope that such and such a line is seen in the spectra of two or more substances, and then the statement of the telescope with the attached spectroscope that such and such a line is seen widened in spots or brightened in flames. Here we have the numbers for the two regions which I have already discussed the region from F to *b*, and from *b* towards D.

Iron

	F–*b*	*b*–548
Total number of lines	96	67
Number in spots and flames	38	41
Basic lines	15	17
,, seen in ,, ,,	14	15
,, not seen ,, ,,	1	2

The total number of iron lines in the first case is 96. Of those 96 lines only 38, or less than half, are found in the spots and flames. When we go into the lower regions of the solar atmosphere, we leave in fact more than half of the iron lines on one side. Of these 96 lines 15 are found by other observers as well as myself to be common to two or more substances. Now comes the question, what is the behaviour of these common lines with reference to spots and storms? The table shows that among the lines seen in spots and storms fourteen of these basic lines are seen. It must be remembered that our records only give us day by day the results of the 12 most widened lines, and not of all the lines widened. In the next region the number of iron lines is somewhat less—67; 41 of these, or more than half, are picked out by spots and storms; Seventeen are basic; of the 17 basic ones 15 are seen in the spots and storms, and only two are lines that are not seen.

Now, we will turn to another substance, nickel, and there we see very much the same kind of thing at work. In nickel for the region F to *b* we have 20 lines recorded by Thalén.

Nickel.

	F-*b*
Total number of lines	20
Number in spots and flames	3
Basic	5
,, Seen	3
,, Not seen	2

Of these 20, 17 are dropped, abolished, when we come to observe the bright lines and the widened lines of nickel in the spots and storms—the 20 comes down to 3. Among the 20 lines 5 are found to be common to two substances. Of those 3 are seen in the flames, that is to say, every line of nickel seen in a spot or flame is common to two substances, and only 2 are visible in the 20 lines not affected by spots and storms. This is all the work of this nature which we need now consider, but it is not all the work that has been done. Neither my assistants nor myself, I am sure, have spared our attempts, nor ourselves, for the matter of that, in trying to get at the bottom of this matter, and the facts which have been here brought forward are typical of a much larger number of facts which have been observed. In the case of every part of the spectrum, in the case of every substance, the verdict is the same. We have the fact, that two things are going on exactly parallel to each other—first that some lines are common to two substances; next that the lines common to two substances are seen almost exclusively alone, both in the sun's spots and in the sun's flames. So that in addition to the fact that the hottest regions of the sun seem to simplify the spectra of the substances enormously, we have this result that the simpler the spectrum becomes, the more complex becomes the origin of the lines; by which I mean that in the ordinary solar spectrum there are a great many lines due to iron, and to nothing else; but the moment we come to the simpler spectrum yielded to us in the spots or the flames, then we have no more right to say that those lines belong to iron than that they belong to titanium, cerium, nickel, and other substances with which those lines are generally observed to be basic.

This, then, is a help towards the demonstration of the view which was first announced in the year 1874, that the line-spectra of bodies (we are dealing almost exclusively with line-spectra) are not produced by the vibration of similar molecules, but they probably represent to us the vibrations of a great number of simplifications brought about by the temperature employed to produce the incandescence of the vapours.

Can we go further than this? Here we must confess both our imperfect instrumental and mental means. We cannot talk of *absolute* coincidence because the next application of greater instrumental appliances may show a want of coincidence. On the other hand there may be reasons about which we know at present absolutely nothing which should make absolute coincidence impossible under the circumstances stated. The lines of the finer constituents of matter may be liable to the same process of shifting as that at work in compound bodies when the associated molecules are changed; but however this may be the fact remains, whatever the explanation may be, that the lines of the elementary bodies mass themselves in those parts of the spectrum occupied by the prominent lines in solar spots and storms.

J. NORMAN LOCKYER

(*To be continued.*)

STATE MEDICINE[1]

FIRST: a few words on what may be called the general theory of our subject-matter. The term "State Medicine" corresponds to the supposition that, in certain cases, the Body-Politic will concern itself with the health-interests of the people —will act, or command, or deliberate, or inquire, with a view to the cure or the prevention of disease. Before any such supposition can be effectively realised, the science of medicine—that is to say, the exact knowledge of means by which disease may be prevented or cured—must have reached a certain stage of development; and unless the science be supposed common to all persons in the State, the existence of State medicine supposes a special class of persons whom the unskilled general public can identify as presumably possessing the required knowledge. Thus, given the class of experts to supply the required exact knowledge, the Body-Politic undertakes that, within the limits of its own constitutional analogies, it will make the knowledge useful to the community.

I have intimated that in State Medicine (just as in private medicine) the medical function may be exercised either in curing or in preventing disease; but practically these two departments of State Medicine are not of equal magnitude, nor are dealt with in quite the same spirit.

As regards curative medicine, modern Governments have in general found it needless to interfere in much detail in favour of persons who require medical treatment.

Larger and far more various than the action taken by the State with reference to the cure of disease is that which it takes in regard of prevention; and it is particularly of preventive medicine that I propose to speak. In its legal aspect it is represented by a considerable mass of statutes (nearly all of them enacted within the last thirty-five years), and by an army of administrative authorities and officers appointed to give effect to those enactments. I need not describe in detail the laws and administrative machinery to which I refer, but I may remind you of the largeness and variety of the scope, even by quoting only the terms in which I was able, twelve years ago, to speak of the public health law of England: "It would, I think, be difficult to over-estimate, in one most important point of view, the progress which, during the last few years, has been made in sanitary legislation. The principles now affirmed in our statute-book are such as, if carried into full effect, would soon reduce to quite an insignificant amount our present very large proportions

[1] An Address delivered at the opening of the Section of Public Medicine, in the International Medical Association, by John Simon, C.B., F.R.S., D.C.L., LL.D.

of preventible disease. It is the almost completely expressed intention of our law that all such states of property and all such modes of personal action or inaction as may be of danger to the public health should be brought within scope of summary procedure and prevention. Large powers have been given to local authorities, and obligation expressly imposed on them, as regards their respective districts, to suppress all kinds of nuisance, and to provide all such works and establishments as the public health primarily requires; while auxiliary powers have been given, for more or less optional exercise, in matters deemed of less than primary importance to health; as for baths and wash-houses, common lodging-houses, labourers' lodging-houses, recreation-grounds, disinfection-places, hospitals, dead-houses, burial-grounds, &c. And in the interests of health the State has not only, as above, limited the freedom of persons and property in certain common respects, it has also intervened in many special relations. It has interfered between parent and child, not only in imposing limitation on industrial uses of children, but also to the extent of requiring that children shall not be left unvaccinated. It has interfered between employer and employed, to the extent of insisting, in the interest of the latter, that certain sanitary claims shall be fulfilled in all places of industrial occupation. It has interfered between vendor and purchaser; has put restrictions on the sale and purchase of poisons; has prohibited in certain cases certain commercial supplies of water; and has made it a public offence to sell adulterated food or drink or medicine, or to offer for sale any meat unfit for human food. Its care for the treatment of disease has not been unconditionally limited to treating at the public expense such sickness as may accompany destitution; it has provided that, in any sort of epidemic emergency, organised medical assistance, not peculiarly for paupers, may be required of local authorities; and, in the same spirit, it requires that vaccination at the public cost shall be given gratuitously to every claimant. The above survey might easily be extended by referring to statutes which are only of partial, or indirect, or subordinate interest to human health; but, such as it is, it shows beyond question that the Legislature regards the health of the people as an interest not less national than personal, and has intended to guard it with all practicable securities against trespasses, casualties, neglects, and frauds."[1] At the time when that description was written I unfortunately had to confess that the intentions of the Legislature were not carried into effect; for that the then existing laws (especially in respect of the local authorities which should give effect to them) were in a state of almost chaotic confusion and unworkability; but since that time an entirely new constitution of local authorities has been made, some thousands of additional officers have been appointed, and the general fabric of the law has been consolidated, and its powers in some respects extended and made more stringent, with a view to the better prevention of disease, so far as legal powers and facilities can attain that object.

Such being the very large contribution which the Body-Politic makes to the purposes of State Medicine in this country, let us next see how we of the medical profession stand in respect of the scientific contribution which we distinctively owe to the same great object.

In preventive, just as in curative, medicine, it occasionally happens that consequences more or less valuable result from some mere chance-hit of discovery; but except so far as this may sometimes (and but very rarely) happen, disease can only be prevented by those who have knowledge of its *causes*—knowledge which does not deserve to be called knowledge, unless in proportion as it is *conclusive and exact;* and thoroughly to investigate the causes and their mode of operation is the quite indispensable first step towards any scientific study of prevention. Essentially we know how to prevent, by having first learnt exactly how to cause. Therefore it is that preventive medicine has had almost no development until within these later times. The germinal thought of it may be traced in even the first days of our profession. The spirit in regard of which Hippocrates has been aptly called the Father of Medicine—the scientific spirit of observation and experiment, as distinguished from the spirit of priestcraft, was one which his medical writings equally showed in their preventive as in their curative relations; and when he, some twenty-three centuries ago, expounded to his contemporaries that pathology is a branch of the science of nature—that causes of disease are to be found in physical accidents of air and earth and water, and in quantities and qualities of food, and in personal habits of life, he (not without risk of being denounced for impiety) virtually proclaimed for all time the first principle of preventive medicine, and indicated to his followers a new line of departure for those who would most largely benefit mankind. His followers, however, have had their work to do. True knowledge of morbific causes could only come by very slow degrees, and as part of the development with which the physical and biological sciences have, little by little, with the labour of ages, been building themselves up; and so no wonder that, despite the lapse of time, even the most advanced of nations are hitherto but beginning to take true measure of the help which preventive medicine can render them.

Now what is the nature of that *study of causes* through which we may gradually arrive at counter-causing or prevention?

Addressing a skilled audience, I shall utter what to them is the merest commonplace when I say that, in the physical and biological sciences we acknowledge no other study of causes than that which consists in *experiment.* And the study of morbific causes is no exception to that rule: it is solely by means of experiment that we can hope so to learn the causes of disease as to become possessed of resources for preventing disease.

The experiments which give us our teaching with regard to the causes of disease are of two sorts: on the one hand we have the carefully pre-arranged and comparatively few experiments which are done by us in our pathological laboratories, and for the most part on other animals than man; on the other hand, we have the experiments which accident does for us, and, above all, the incalculably large amount of crude experiment which is popularly done by man on man under our present ordinary conditions of social life, and which gives us its results for our interpretation.

When I say that experiments of those two sorts are the sources from which we learn to know the causes of disease, I of course do not mean that the mental process by which an experiment becomes instructive to us is the same in regard of the two sorts of experiment. On the contrary, the ætiological problem (so long as it is a problem) is approached in the two cases from two opposite points of view; and the dynamical continuity of relation, which we call cause and effect, is traced, in the one case, from the one pole, and in the other case, from the other pole of the relation. In the one case, starting with knowledge of our own deliberately-prepared *cause*, our question is, What will be its effect? In the other case, starting from a certain *effect* presented to us, our question is, What has been its cause? But in the second case, just as in the first, when the question is answered, when the problem is solved, when the relation of cause-and-effect has been made clear, we recognise that the conjuring-power which has brought us our new knowledge is the power of a *performed experiment.*

Let me illustrate my argument by showing you the two processes at work in identical provinces of subject-matter.—What are the classical experiments to which we habitually refer when we think of guarding against the dangers of Asiatic cholera? On the one side there are the well-known *scientific* infection-experiments of Prof. Thiersch, and others following him, performed on a certain number of mice. On the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848-9 and 1853-4, were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies which supplied those districts with water. Both the professor and the companies gave us valuable experimental teaching as to the manner in which cholera is spread. I need not state at length the facts of those experiments, probably known to all here, but may rather justify my parallel by referring to an ætiological question which will presently be discussed in our section.

It concerns the *causation of tubercle*—the most fatal by far of all the diseases to which the population of this country is subject. On that subject, for the last sixteen years, we have had a new era of knowledge. It was the great merit of a Frenchman, M. Villemin, that he, in 1865, first made us fully aware that tubercle is an infectious disease. He did this by certain *laboratory experiments* performed on other animals than man. He found that general and fatal tubercular infection of the animal was produced when he inoculated it subcutaneously with a little crude tubercular matter from the human subject. That first laboratory investigation of the subject has been followed most extensively by others; and the further experiments, while

[1] "Eleventh Report of the Medical Officer of the Privy Council," 1869, pp. 20, 21.

entirely confirming M. Villemin's discovery, have shown that subcutaneous inoculation is not the only mode by which the tubercular infection can be propagated. Dr. Tappeiner and others have shown that the same effect is produced on the animal if tubercular matter (such as the sputa of phthisical patients) be diffused in spray in the air which the animal breathes; and Prof. Gerlach of Hanover showed twelve years ago with regard to the bovine variety of tubercular disease (the perlsucht of the Germans), that its infection can be freely introduced through the stomach if bits of tubercular organs be given in the food, or if the healthy animal be fed with milk from the animal which has tubercle. That the communicability of tubercle from animal to animal is also being tested to an immense extent by *popular experiments* on the human subject, is what a moment's reflection will tell; and from that wide field of experiment I select one instance for illustration. I have every reason to believe that Prof. Gerlach's experiments on the communicability of tubercle by means of milk are very extensively parodied by commercial experiments on the human subject. I learn, on what I believe to be the highest authority in this country, that tubercle (in different degrees) is a malady which abounds among our cows; and that so long as the cow continues to give milk, no particular scruple seems to prevent a distribution of that milk for popular use. To the persons who consume that milk an important question as to the causability of tubercle is put in an experimental form. Whether they will become infected with tubercle is a question which the individual consumers do not stand forward to answer for themselves, like the animals of the laboratory experiments: but Dr. Creighton's lately-published book, entitled, "Bovine Tuberculosis in Man," and a paper in which I am glad to say he brings under notice of our section the very remarkable series of facts on which he grounds that startling title, seem to suggest a first instalment of answer in accordance with Prof. Gerlach's experimental finding.

The two sorts of experiment—the scientific and the popular—differ, as I have noted, in this particular: that the popular experiment is almost always done on man; the scientific almost always on some other animal. It is true that many memorable cases are on record, where members of our profession have deliberately given up their own persons to be experimented on by themselves or others for the better settlement of some question as to a process of disease; have deliberately tried, for instance, whether, in this way or in that, they could infect themselves with the poison of plague or of cholera; and as regards one such case which is in my mind, I think it not unlikely that the illustrious life of John Hunter was shortened by the experiments which he did on himself with the ignoble poison of syphilis. There have been cases, too, where criminals have been allowed to purchase exemption from capital or other punishment at the cost of allowing some painful or dangerous experiment to be performed on themselves. And cases are not absolutely unknown where unconsenting human beings have been subjected to that sort of experiment. But waiving such exceptions, the rule is, as I have said, that scientific experiments relating to causes of disease are performed on some animal which common opinion estimates as of lower importance than man. Now, as between man and brute, I would not wish to draw any distinction which persons outside this room might find invidious; but, assuming for the moment that man and brute are of exactly equal value, I would submit that, when the life of either man or brute is to be made merely instrumental to the establishment of a scientific truth, the use of the life should be economical. Let me, in that point of view, invite you to compare, or rather to contrast with one another, those two sorts of experiment from which we have to get our knowledge of the causes of disease. The commercial experiments which illustrated the dangerousness of sewage-polluted water-supplies cost many thousands of human lives; the scientific experiments which with infinitely more exactitude justified a presumption of dangerousness, cost the lives of a few dozen mice. So, again, with experiments as to the causation of tubercle:—judging from the information which I quoted to you, I should suppose that the human beings whose milk-supply on any given day includes milk from tubercular cows might be counted, in this country, in tens of thousands; but the scientific experiments which justify us in declaring such milk-supply to be highly dangerous to those who receive it were conclusive when they amounted to half-a-dozen. So far, then, as regards the mere getting of experimental knowledge, we must not, with a view to economy of life, be referred to popular, rather than scientific, experiment. And in the same point of view, it perhaps also deserves consideration that the popular experiments, though done on so large a scale, very often have in them sources of ambiguity which lessen their usefulness for teaching.

Let me now briefly refer to the fact that, during the last quarter of a century, all practical medicine (curative as well as preventive) has been undergoing a process of transfiguration under the influence of laboratory experiments on living things. The progress which has been made from conditions of vagueness to conditions of exactitude has, in many respects, been greater in these twenty-five years than in the twenty-five centuries which preceded them; and with this increase of insight, due almost entirely to scientific experiment, the practical resources of our art, for present and future good to the world, have had, or will have, commensurate increase. Especially in those parts of pathology which make the foundation of preventive medicine, scientific experiment in these years has been opening larger and larger vistas of hope; and more and more clearly, as year succeeds year, we see that the time in which we are is fuller of practical promise than any of the ages which have preceded it. Of course, I cannot illustrate this at length, but some little attempt at illustration I would fain make.

First, let us glance at our map. When we generalise very broadly the various causes of death (so far as hitherto intelligible to us) we see them as under two great heads, respectively autopathic and exopathic. On the one hand, there is the original and inherited condition under which to every man born there is normally assigned eventual old age and death, so that, sooner or later, he "runs down" like the wound-up watch with its ended chain; and, as morbidities under this type, there are those various original peculiarities of constitution which make certain individual tenures of life shorter than the average, and kill by way of premature old age of the entire body, or (more generally) by quasi-senile failure of particular organs. On the other hand, as a second great mass of death-causing influence, we see the various interferences which come from outside; acts of mechanical violence, for instance, and all the many varieties of external morbific influence which can prevent the individual life from completing its normal course.

As regards cases of the first class—cases where the original conditions of life and development are such as to involve premature death (which in any such case will commonly show itself as a fault in particular lines of hereditary succession)—the problem for preventive medicine to solve is, by what cross-breeding or other treatment we may convert a short-lived and morbid into a long-lived and healthy stock; and this, at least as regards the human race, has, I regret to say, hardly yet become a practical question. But, as regards cases of the second class, evidently the various extrinsic interferences which shorten life have to be avoided or resisted, each according to its kind; and here it is that the scientific experimenters of late years have been giving us almost daily increments of knowledge.

Two early instances, vastly important in themselves, though of a comparatively crude kind, I have already mentioned; and I now wish to glance at some illustrations of the immense scope and the marvellous exactitude of the newer work.

The invaluable studies of M. Pasteur, beginning in the facts of fermentation and putrefaction, and thence extending to the facts of infectious disease in the animal body, where M. Chauveau's demonstration of the particulate nature of certain contagia came to assist them—they, I say, partly in themselves, and partly in respect of kindred labours which they have excited others to undertake, have introduced us to a new world of strange knowledge. We have learnt, as regards those diseases of the animal body which are due to various kinds of external cause, that probably all the most largely fatal of them (impossible yet to say how many) represent but one single kind of cause, and respectively depend on invasions of the animal body by some rapidly self-multiplying form of alien life. This doctrine, which scientific experiment initiated, has, for the last dozen years, been extending and confirming itself by further experiment. As soon as the doctrine began to seem probable, science saw that, should it prove true, it must have the most important corollaries. If the cause of an infecting human disease is a self-multiplying germ from the outside world, the habits of that living enemy of ours can be studied in its outside relations. It becomes an object of common natural history, it has biological affinities and analogies. We can cultivate it in test-tubes in our laboratories, as the gardener would cultivate a rose or an apple, and we can see what agrees and what disagrees with its life. And then, as the next and immeasurably most important stage, where nothing but experiment on the living body will help us, we can try whether perhaps any of our modifications of its life have made it of weaker power in relation to the living bodies which it invades, or whether, through our more intimate knowledge of its vital affinities, we can artificially give to bodies which it would invade, a partial or complete protection against it. Such, at first blush, were the obvious possibilities of research which the new doctrine of infectious disease suggested to the mind of the pathologist; and never since the profession of medicine has existed, had a field of such promise been before it. The promise has not been belied. A host of diseases has been worked at in such lines as I just now indicated, and with many of them important progress has been made.

It would be impossible for me even to name a twentieth part of the investigations which have been more or less successful. As regards some which have most struck me, I pass with but a word Dr. Klein's investigation of the pneumo-enteritis of swine; Prof. Cohn's and Dr. Koch's and Dr. Buchner's respective contributions to the natural history of the anthrax bacillus; Dr. Bollinger's recognition of the microphytic origin of an important cancroid disease of horned cattle, with Dr. Johne's illustration of the inoculability of this disease; the research by Drs. Klebs and Tommasi-Crudeli into the intimate cause of marsh-malaria; and, not least, the demonstration (as it appears to be) which Dr. Grawitz has recently published, that some of the commonest and most innocent of our domestic microphytes can be changed by artificial culture into agents of deadly infectiveness. I pass these and others, in order that I may more particularly speak of some which have already shown themselves practically useful; for in respect of some of them the time has already come when abstract scientific knowledge is passing into preventive and curative knowledge.

First, and not in a spirit of national partiality, I will mention the application which M. Pasteur's doctrine has received at the hands of Mr. Lister, with regard to the antiseptic treatment of wounds: an application which, enforced and illustrated at every turn by Mr. Lister's own eminent skill as an experimentalist, has been confirmation as well as application of the parent doctrine; and the beneficent uses of which, in giving comparative safety to the most formidable surgical operations, and in immensely facilitating recovery from the most dangerous forms of local injury, are recognised—I think I may say, by the grateful common consent of our profession in all countries, to be among the highest triumphs of preventive medicine.

Secondly, out of the experimental studies of anthrax—chiefly out of those of Dr. Sanderson and Mr. Duguid in this country, and those of Dr. Buchner in Germany and M. Toussaint in France, has grown a knowledge of various ways in which the contagion of that dreadful disease can be so mitigated that an animal inoculated with it, instead of incurring almost certain death, shall have no serious illness; and the further knowledge has been gained that the animal submitted to that artificial procedure is thereby more or less secured against subsequent liability to the disease. In other words, with regard to that disease, an infliction which sometimes spreads to man from his domestic animals, and one which in some parts of Europe is of serious consequence to agricultural interests, as well as to animal life, the later experimenters—of whom I may particularly name M. Toussaint and our countryman, Prof. Greenfield, seem to be giving to the animal kingdom, and to the farmers, the same sort of boon as that which Jenner gave to mankind when he taught men the use of vaccination. Quite recently, our great leader, M. Pasteur, seems to have made, by new experiments, still further progress in the mitigation of anthrax.

Thirdly, a similar discovery has been made by M. Pasteur, with reference to the contagium of a very fatal poultry disease, known by the name of fowl's cholera; he has learnt to mitigate that contagium to a degree, in which, if fowls be inoculated with it, they will suffer no serious ailment; and he has found that fowls so inoculated (or, as he, in honour of Jenner, would say, "fowls so vaccinated") are proof against future attacks of the disease.

Fourthly, Prof. Semmer of Dorpat, through experiments done under his direction by Dr. Krajewski, has made a similar discovery in regard of the infection of septicæmia; has found, namely, that by treatment like that with which M. Toussaint mitigates the contagium of splenic fever, he can bring the most virulent septic contagium into a state in which it shall be mild enough to serve for harmless inoculations; which inoculations, when performed, shall be protective against future infections.

Finally, in a different direction of experimental work, let me name the recent most admirable research which Dr. Schüller of Greifswald has made, nominally in respect of certain surgical affections of joints, but in reality extending to the inmost pathology and therapeutics of all tubercular and scrofulous affections. A knowledge of the fatal infectiveness of crude tubercular matter had been given (as I before said) by Villemin and those who followed him; and Prof. Klebs, four years ago, declared the infective quality to be due to the presence of a microphyte (micrococcus), which he had succeeded in separating from the rest of the matter by successive acts of cultivation in fluids of inorganic origin. Dr. Schüller solidly settles, and widely extends, that teaching. According to his apparently quite unquestionable observations and experiments, the micrococcus which characterises tubercle characterises also certain affections popularly called "scrofulous"—namely, "scrofulous" synovial membrane, "scrofulous" lymph-glands, and lupus: so that these diseases may be defined as essentially tubercular, and that inoculation with matter from any of them, or with a cultivation-fluid in which the micrococcus from any of them has been cultivated, will infect with general tuberculosis. The rapid multiplication of the tubercle-micrococcus in the blood and tissues of any inoculated animal can be verified both by microscopical observation, and by inoculative experiment; and an extremely interesting part of the research, in explanation of certain of our human joint-diseases, is the demonstration that if in the inoculated animal a joint is experimentally injured, that joint at once becomes a place of preferential resort to the micrococcus which is multiplying in the blood, and becomes consequently a special or exclusive seat of characteristic tubercular changes. Even thus far the practical interest of Dr. Schüller's book is such as it would not be easy to overstate, but still greater interest attaches to the last chapter of the book, in which, confidently resting on the pathological facts which I have quoted, he makes proposals for the treatment of tubercle on the basis of its microphytic origin, and shows the successful result of such treatment as he has hitherto tried, from that basis, on animals artificially infected by him.

I venture to say that in the records of human industry it would be impossible to point to work of more promise to the world than these various contributions to the knowledge of disease, and of its cure and prevention; and they are contributions which from the nature of the case have come, and could only have come, from the performance of experiments on living animals.

At this most productive epoch in the growth of medical science, our English studies have been interrupted. An Act of Parliament, passed five years ago under the title of the Cruelty to Animals Act, has made it difficult or impossible for scientific observers any longer to follow in this country any such courses of experiment as those which of late years, at the cost of relatively insignificant quantities of brute suffering, have tended to create an infinity of new resources of relief for the sufferings both of brute and man. The Act does not in express terms interdict all performance of such courses of experiment: it nominally allows them to be done under a variety of limited licences which may be granted by a Principal Secretary of State; but the limitations under which these licences are granted, and the trouble, delay, and friction which necessarily to some extent, and, in fact, often to an intolerable extent, attend the obtaining of any one of them, are practically little better than prohibition.

The Act apparently contemplates, as the chief subjects of its operation, an imaginary class of unqualified persons, who, with no legitimate relation to scientific research, would, under pretence of such research, torture, and (it is supposed) take pleasure in torturing, live animals; and against this devilish class of persons the Act is very indulgently framed: for, instead of expressly refusing licence to unqualified persons, and perhaps hinting to such of them as would do wilful cruelty under pretence of study that the lash and the treadmill are for such scoundrels—instead of this, I say, the Act virtually confounds together that imaginary class of unqualified and cruel persons, and, on the other hand, our professional class of *bonâ fide* scientific investigators, on whom the progress of medicine depends, and whose names are sufficient security for their conduct. What is counted good for the one class is also counted good for the other. The law will trust no licensed experimenter farther than it can provide for his being minutely watched and regulated by the Secretary of State: and in respect of the details of experimental procedure, the supervision of that high political officer is substituted for the discretion and conscience of the scientific investigator.

Consider for a moment what this means in regard of the members of our profession whom it affects. Contrast with it the almost unbounded trust with which the world, from time immemorial, has regarded the character of our profession. Consider the relation of inmost confidence in which members of our profession in every corner of the kingdom are admitted to share in the sanctities and tendernesses of domestic life. Consider our immense daily responsibilities of human life and death. Consider that there is not a member of our profession to whom the law does not allow discretion that, in certain difficulties of child-birth, he shall judge whether he will kill the child to save the mother. And in contrast with all this, is it to be seriously maintained that society cannot trust us with dogs and cats? that our foremost workers—(for it is essentially they who are affected)—cannot be trusted to behave honestly towards their brute fellow-creatures, unless they be regulated and inspected under a special law in much the same prevenient spirit as if they were prostitutes under the Contagious Diseases Act?

I have reason to believe that, if that Act continues on the statute-book, one of two results will follow. Experiments, indispensably necessary for the growth of medical science in relation to the cure and prevention of disease, will cease, or almost cease, to be done in this country; or, as the alternative to this, persons who desire to advance the science of their profession, will be tempted to clandestinely ignore the law and to run their chance, if the worst comes to the worst, of having to try conclusions with the common informer.

Let me illustrate this by two personal references: I have already mentioned Prof. Lister as an experimenter, whose name is now classical wherever science has reached, and whose work has been of signal advantage to mankind. Last autumn Mr. Lister wished to do some experiments in extension of the particular branch of knowledge with which his name is identified, and at a point which he considered of extreme importance in surgical pathology. He found he must either abandon his investigation or must conduct it in a foreign country, and in his zeal for science he chose the latter course. His experiments (which had to be on large animals) were done at the Veterinary College of Toulouse; and in stating this fact in a letter, from which I quote, Mr. Lister added that "even with reference to small animals, the working of the Act is so vexatious as to be practically prohibitory of experiments by a private practitioner like myself, unless he chooses to incur the risk of transgressing the law." A second name which I have mentioned is that of Prof. Greenfield, who has so highly distinguished himself in developing, by means of experiments, the preventive medicine of splenic fever. Dr. Greenfield, in order to perform his inoculation-experiments, had of course to become a licence-holder under the Act; and his experience of the hindrances which attach to that position is expressed to me in the following terms: "It is my deliberate conviction, as a result of my experience, that these hindrances and obstacles are so numerous and so great as to constitute a most serious bar to the investigation of disease, and even of such remedial measures as would by common consent be for the direct benefit of the animals experimented upon. When to this is added all the annoyance and opprobrium which are the lot of investigators, it is to be wondered at that any one should submit to be licensed." Dr. Greenfield's experimental operations consisted only in inoculating the virus of animal diseases, and he says: "I have not been engaged in other investigations for the simple reason that, with the present restrictions and the difficulty in obtaining a licence, I regard it as almost hopeless to attempt any useful work of the kind in this country."

As I feel sure that the Act must at no distant time be reconsidered by the Legislature, and as I also very strongly feel that, quite apart from any question of legal enactments, there is the question of moral right or wrong to be considered in the matter, I would beg you to allow me to make my own public confession of faith (from which I dare say yours will not much differ) in that extremely important matter of controversy.

The question being whether medical science can rightly use living animals as subjects for experiments which may be painful, and even, in exceptional cases, very painful to them, the answer may be sought in either of two directions: 1. What says the voice of the experimenter's own conscience? and 2. What says the standard of common contemporary conduct in analogous cases?

As regards the first, if I may take the liberty of expressing my own feeling, I would say this. I do not in any degree regard it as matter of indifference that, in certain cases, by my own hand or by that of some one acting for me, I must inflict death or pain on any living thing. I, on the contrary, think of it with true compunction; but I think of it as good or bad according to the end which it subserves. Where I see my way to acquire, at that painful cost, the kind of exact knowledge which, either in itself or in contribution to our common stock, will promote the cure or prevention of disease in the race to which the animal belongs, or in the animal kingdom generally, or (above all) in the race of man, I no more flinch from what then seems to me a professional duty, though a painful one, than I would, in the days before chloroform, have shrunk from the cries of a child whom I had to cut for stone. If, in a case of the latter sort, the surgeon nerves himself to his work by the conviction of an indispensable usefulness in what he has to do, so does the pathologist in his, and surely in a much larger sense. The agitated parent of the child might sometimes be tempted to say: "Forbear giving this cruel pain; let the poor little sufferer die"; but the surgeon's reply would have comforted her. And so with the physiological experimenter: except that he, instead of looking at one individual life to be saved, is looking at a race or at many races, and reflects how, in respect of some grievous physical misery, the whole of them, in all their multitudinous successions, may be redeemed through the suffering of the few. This is my personal view of the abstract right or wrong in the question. I state it because, in matters of right and wrong, no man ought to shelter himself behind authority. I believe I may add that if it, or something very like it, had not for centuries been the general view of the medical profession, our professional knowledge would probably be standing in this present age about where it stood in the days of the Plantagenets. Of Harvey and Hunter and Bele, we well know that such was the view on which they acted in rendering their immortal services to mankind; and I am not aware that any man, whose opinion really counts in matters of medical science, would express any material dissent from it.

The second standard to which I referred was that of the common conduct of men in *analogous* cases. I pointedly say "analogous," rather than "similar," because common life does not in fact give cases which, properly speaking, are "similar" to ours. But what, I ask, is the common *principle* of behaviour of civilised man towards the so-called lower animals? He in every respect subordinates their lives to his own. If he thinks he can get an advantage to himself by killing or painfully mutilating an animal, he does so with apparently no hesitation. See, for one instance, the sexual mutilations which are inflicted on all but a small minority of most kinds of domestic animals, and, as regards some kinds, on many of the females. When I appeared as a witness six years ago before the Royal Commission which was considering the question of our experiments, I particularly endeavoured to draw their attention to this view of the case; and in one of my answers (No. 1491) I entered on it more fully than would be suitable to the present occasion.

Thus, either way, whether I look to what I may call the general conscience of the medical profession, or look to the principles by which men in general govern their conduct towards the brute creation, nowhere do I see fair ground on which exception from outside can be taken to a limited, a strictly economical, use of animal life for purposes of scientific experiment.

No doubt there can be found, outside the medical profession, excellent persons, and plenty of them, whose first inclination would be to dissent from that position of ours; and some such persons have (as I think, hastily) given public utterance to such first impressions, and done their best to promote legislation against us. Among names which I see identified with opinions different from ours, are some for which I have deep respect. Particularly of one such man, whom I have the honour to know, I think it may be truly said that his own whole life has been one of practical beneficence, and I would not willingly incur the censure of any such man. But even to him I would fearlessly say, that I think he has not done justice to the case of our profession. To him, and such as him, I would confidently appeal to reconsider their first impressions. On him, and such as him, I would urge that the practice of scientific experimentation on living animals is but an infinitesimally small application of the licence which common life claims for itself in regard of animals; and I would challenge such men to examine, with strict impartiality, what are their own responsibilities, direct and indirect, in regard of the infliction of pain on living animals.

I protest against any man's applying to this extremely important question a purely arbitrary standard of right or wrong. Those who pronounce judgment on their neighbours must be prepared to state the principle on which they judge. "Compound for sins you are inclined to, by damning those you have no mind to," is the Pharisee's easy-going formula. Where would life be if that were generally accepted? Suppose a *genus* of action; let men draw an arbitrary line across it—a line prescribed by no better rule than that which governed the lady's dislike to Dr. Fell; let them affix a nickname of praise to all on one side of the line, and a nickname of dispraise to all on the other: truly we should thus have the readiest of royal roads to unlimited mutual persecution.

And I protest against a standard of right and wrong being fixed for us on grounds which are merely sentimental. In certain circles of society, at the present time, æsthetics count for all in all; and an emotion against what they are pleased to call "vivisection" answers their purpose of the moment as well as any other little emotion. With such sections of society, our profession cannot seriously argue. Our own verb of life is ἐργάζεσθαι, not αἰσθάνεσθαι. We have to think of usefulness to man. And to us, according to our standard of right and wrong, perhaps those lackadaisical æsthetics may seem but a feeble form of sensuality.

Of the mere screamers and agitation-mongers who, happy in their hysterics or their hire, go about day by day calumniating our profession and trying to stir up against it the prejudices and passions of the ignorant, I have only to express my contempt.

I regret to have had to speak at so much length of the heavy cloud which at present hangs over the study of scientific medicine in England, and which, in my opinion, is likely to be of specially disastrous effect on the progress of preventive medicine. As a very old public servant in that cause, I should indeed grieve to see it brought to a stand-still for want of the scientific nurture which, in truth, is its very basis of life; and, speaking publicly of the danger on this occasion, I have hoped that the occasion may give importance to what I say.

And now, gentlemen, from contemplating that cloud, which happily is but local, and which perhaps may be but temporary, I gladly turn to skies which have no cloud. If there exist in the social organism any function whatsoever for which development and eventual triumph may be foretold, surely it is that of State Medicine. Of the two great factors concerned in it—the two strong powers which within our own time have converged to make it the reality which it is—the growth of science on the one hand, and the growing stress of common humanity on the other, neither one is likely to fail. Of our science it is needless to say that it will grow. To the science of nature indeed is allotted that one incomparable human day which knows no sunset. In the pure light of its ever-present daybreak, individual workers will pass away, generations will change, but the studies of Nature, and, above all, the gathering of such knowledge as can lessen man's physical difficulties and sufferings, will surely grow from age to age, and, as on Proserpina's sacred tree, one golden fruit will follow another: "simili frondescet virga metallo." And no less also in the other direction, the auguries are wholly for our cause. Popular education is gradually making its way, and it will grow to be a force on our side. Masses of mankind that now have to be humbly pleaded for by others, will then be strong to speak for themselves. Physical interests, now but little understood, will then be within grasp of all men's apprehension. Not only will health be recognised at its true value, and its elementary requirements be regarded, but also the frauds and villanies which are now committed against it will have become intelligible to the common mind; and the workman of the future will strike against being cheated in health as he would now strike against being cheated in wages. As such times come to the world, the science and the profession which care for man as man will get to be better appreciated than now. And in proportion as an educated people grows to become Body-Politic, State Medicine will be seen to represent the true ideal of Government-action which sets its standard of success in the "greatest happiness of the greatest number."

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1881.—The observations of this body in both hemispheres from its discovery on May 22 by Mr. Tebbutt at Windsor, N.S.W., to the end of last month, are closely represented by a parabolic orbit. The intensity of light is now rapidly going off, and if any decided deviation from the parabola is established it can only be through the later observations in these latitudes. It is therefore important for the theory of the comet that the larger instruments in our observatories should be brought to bear upon the accurate determinations of position, and that this should be continued as long as practicable. The following ephemeris for Greenwich midnight is calculated from elements, which are likely to give the comet's places pretty closely:—

	Right Ascension. h. m. s.	Declination. ° ′	Log. Distance from Earth.	Log. Distance from Sun.
August 20	14 31 0	+77 19·6	0·1206	0·1501
22	38 10	77 3·0		
24	45 22	76 47·1	0·1376	0·1672
26	52 36	76 31·9		
28	14 59 52	76 17·2	0·1532	0·1837
30	15 7 10	76 3·2		
Sept. 1	14 31	75 49·2	0·1676	0·1995
3	21 54	75 35·7		
5	29 18	75 22·4	0·1810	0·2148
7	36 46	75 9·3		
9	44 16	74 56·3	0·1934	0·2295
11	51 51	74 43·4		
13	15 59 29	74 30·8	0·2050	0·2436
15	16 7 11	74 18·1		
17	14 57	74 5·4	0·2160	0·2572
19	22 47	73 52·6		
21	30 42	73 39·7	0·2264	0·2704
23	16 38 41	+73 26·7		

The intensity of light on September 23 will be only one-third of that on August 20.

Dr. B. A. Gould has published in pamphlet-form an account of the Cordoba observations of this comet, with particular reference to his observations of June 11, to which we referred last week. We give his conclusions respecting the object seen that evening in his own words:—"La latitud considerable presta poca probabilidad á la hipótesis de que esta estrella haya sido un planeta interior. El movimiento relativo en declinacion, y la falta de cualquier objeto visible de la misma clase en la vecindad del cometa el dia siguiente, no parecen admitir la suposicion que el cometa se hubiera dividido como el de Biela. El brillo que se necesitaba, para que fuese visible la estrella en aquel momento y aquella posicion, indica una magnitud no inferior á la tercera.

"Esta observacion tambien tiene que esperar su solucion en lo futuro, y tal vez solo despues de muchos años."

SCHÄBERLE'S COMET.—According to M. Bigourdan's elements, the position of this comet at Berlin midnight on August 23 will be in R.A. 11h. 42·5m., Decl. +40° 34′, and at the same hour on August 25 in R.A. 12h. 16·0m., Decl. +34° 14′, and the intensity of light will be at a maximum between these dates. It may be observable in the other hemisphere for some weeks after perihelion passage.

THE COMPANION OF SIRIUS.—Prof. Colbert of the Dearborn Observatory, Chicago, has calculated the following orbit of the companion to Sirius:—Apastron passage, 1867·0, position of node, 42°·4; node to periastron in the direction of the star's (retrograde) motion, 133°; inclination, 57°·1; eccentricity, 0·58; semi-axis major, 8″·41; period, 49·6 years. These elements give, for 1881·2: angle of position, 45°·6; distance, 9″·9; and for 1882·2, position 43°·1; distance, 9″·5. For 1890·2 the position is 322°·2; distance, 2″·2; and the distance is near its minimum.

SCIENTIFIC SERIALS

The Journal of Anatomy and Physiology, vol. xv. Part IV. July, 1881, contains: On the ovary in incipient cystic disease, by Dr. V. D. Harris and A. Doran (Plate 23).—The anatomy of the Koala (*Phascolarctos cinereus*), by Dr. A. H. Young.—On the lymphatics of the pancreas, by Drs. George and F. Elizabeth Hoggan (Plate 24).—A case of primary cancer of the femur, by R. Maguire (Plate 25).—A case of chronic lobar pneumonia, by

Thomas Harris.—A contribution to the pathological anatomy of primary lateral sclerosis (sclerosis of the pyramidal tracts), by Dr. Dreschfeld (Plate 26).—On the form and proportions of a fœtal Indian elephant, by Prof. Turner (Plate 27).—On the femoral artery in apes, by Dr. J. Macdonald Brown.—The brain and nervous system: a summary and a review, by Robert Garner.—Index to vol. xv.

THE recent numbers of *Trimen's Journal of Botany* (217–223) contain quite the average number of articles of interest, relating both to British and to foreign botany.—H. and J. Groves describe *Chara obtusa*, a species new to Britain.—Dr. Vines summarises the existing literature on the very difficult and intricate subject of the morphology of the Scorpioid Cyme, referring especially to the confusion resulting from the difference in the use between Continental and English writers of the terms "helicoid" and "scorpioid."—Prof. Dickson gives a very interesting account of the morphology of the pitcher of *Cephalotus follicularis*, illustrated by two plates. His conclusions on its structure are thus summarised:—"1. That the pitcher results from a calceolate pouching of the leaf-blade from the upper surface. 2. That the apex of the leaf is on the far side of the pitcher-orifice from the main axis and from the lid, and is probably represented by the tip of the middle dorsal wing. 3. That the pitcher-lid represents an outgrowth or excrescence from the upper leaf-surface." Mr. C. B. Clarke and Dr. Hance continue their descriptive papers, the former relating chiefly to Indian, the latter to Chinese botany, and including the description of many new species. Among the more important of Mr. Clarke's contributions is a complete review of the order Commelinaceæ, in which are comprised 307 species (including the common *Tradescantia virginica*, or Virginian spider-wort of our gardens), distributed over twenty-six genera. These are placed by Mr. Clarke in three tribes—the Pollieæ (26 species), Commelineæ (144 species), and Tradescantieæ, which includes nineteen out of the twenty-six genera.

THOUGH the recent numbers (41, 42) of the *Scottish Naturalist* contain no article calling for special remark, this quarterly fully maintains the character which it has already acquired under the editorship of Dr. Buchanan White.

THE *American Naturalist*, July, 1881, contains: On the origin and descent of the human brain, by S. V. Clevenger.—On the eastern snow bird, by Samuel Lockwood.—On bacteria as a cause of disease in plants, by T. J. Burrill.—Record of American carcinology for 1880, by J. S. Kingsley.—Aboriginal stone-drilling, by Charles Rau.—On the effects of impacts and strains on the feet of mammalia, by E. D. Cope.

IN the last number of the *Bulletin* of the Torrey Botanical Club which has reached us are two interesting articles. Mr. F. Wolle described several freshwater algæ new to the flora of the United States, several of them being previously undescribed. These are *Synechococcus racemosus*, *Calothrix Hosfordii*, and *C. laculosa*. Prof. C. E. Bessey describes and figures a simple dendrometer for measuring the height of trees.

Annalen der Physik und Chemie, No. 6.—Determination of the specific gravity of distilled mercury at 0°, and the disturbing reactive dilatations of the glass therewith connected, by P. Volkmann.—Researches on sound-strength, by A. Oberbeck.—On the quantity of electricity furnished by an influence machine of the second kind, and its relation to moisture, by E. Riecké.—On the distribution of electricity on the surface of moved conductors, by H. R. Hertz.—On Herr Exner's experiments with regard to the theory of Volta's fundamental experiments, by V. A. Julius.—The determination of the transference-numbers of the ions for lithium and carbonic acid compounds, by J. Kuschel.—On the galvanic behaviour of carbon, by H. Muraoka.—Remarks on Herr Warburg's paper, on some actions of magnetic coercitive force, by C. Fromme.—The intensity of the horizontal terrestrial magnetic force for Göttingen in 1880, with the secular variation of the same, by K. Schering.—On a new volumenometer, by A. Paalzow.—On the oxygen spectrum, by A. Paalzow and H. W. Vogel.—The photometry of the Fraunhofer lines, by K. Vierordt.—A polarisation-apparatus of platino-cyanide of magnesium, by E. Lommel.—On the law of dispersion, by the same.—Researches on the height of the atmosphere, &c. (continued), by A. Ritter.—On the absolute size of gas-molecules, by E. Dorn.—Remarks on Herr Besselhagen's paper on a new form of the Töpler mercury air-pump, by F. Neesen.

THE *Nuovo Giornale Botanico Italiano* for April contains an article by A. Piccone on the cause of the disease which was so destructive to the chestnut trees in the province of Genoa in the year 1880, and which was previously obscure. He claims to have established that it is due to the attacks of a parasitic fungus, *Septoria castaneæ*, which attacks the branches and leaves, but the reproductive organs of which he has been unable to detect. Its extraordinary development during that year appeared to be due to the rainy character of the summer. Fitzgerald and Bottini's article on the bryology of the valleys of the rivers Secchio and Magra is accompanied by a valuable coloured map showing the nature of the soil in the district bordering the western coast of the peninsula stretching from near the Gulf of Spezzia to Lucca.

Zeitschrift für wissenschaftliche Zoologie, Bd. 35, Heft 4, June 14, 1881, contains: On the minute structure of the stigmata in the insects, by O. Krancher (plates 28 and 29).—A revision of the species of Holothuroids described by Prof. Brandt from Mertens' collection, by Dr. Hubert Ludwig.—Contribution to a knowledge of the Hydrachnid genus Midea of Bruzelius, by F. Könike (plate 30).—A revision of the Hydrachnids described by H. Lebert from the Lake of Geneva, by F. Könike.—Contribution to a knowledge of the Psorospermeæ of fish, by Prof. O. Bütschli (plate 31).—Studies on the Bopyridæ, by Prof. R. Kossmann (plates 32 to 35).

Revue Internationale des Sciences Biologiques, June 15.—Examination of vision from the stand-point of general medicine (concluded), by M. Charpentier.—Insane conceptions, their mechanism and diagnostic character, by M. Spitzka.

Revue Internationale des Sciences biologiques, July, 1881, contains:—Metallotherapy, by Dr. L. H. Petit.—Protoplasm considered as the basis of animal and vegetable life, by Prof. Hanstein.—The multiplication, colonisation, and encystment of the rhizopods, a review by Dr. Bütschli.—On the coloration of living protoplasm by Bismarck Brown, by L. F. Henneguy.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, August 3.—R. Meldola, F.C.S., vice-president, in the chair.—Miss E. A. Ormerod exhibited some *Coleoptera* and *Hemiptera* from Port Elizabeth, South Africa.—Mr. E. A. Fitch exhibited an ear of wheat infested by *Siphonophora granarii*, every specimen of which was attacked by a parasite belonging to the genera *Allotria* or *Aphidius*.—Papers read: Mr. A. H. Swinton, on the oviposition of *Iodis vernaria*.—Prof. Westwood, description of a new genus of Hymenopterous insects (*Dyscolesthes*) from Chili.—Mr. A. G. Butler, descriptions of new genera and species of *Lepidoptera* from Japan.—Mr. R. Trimen, on some new species of *Rhopalocera* from Southern Africa.—Mr. C. O. Waterhouse, descriptions of new Longicorn *Coleoptera* from India.—Mr. W. L. Distant, descriptions of some new neotropical *Pentatomidæ* and *Coreidæ*, and of the female of *Morpho Adonis*, Cram.

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