THURSDAY, FEBRUARY 21, 1895.

SIR ANDREW CROMBIE RAMSAY.

Memoir of Sir Andrew Crombie Ramsay. By Sir Archibald Geikie. (London: Macmillan and Co., 1895.)

THE memoir of Sir Andrew Crombie Ramsay, by Sir Archibald Geikie, is a valuable addition to literature and to science. We will refer our readers to the book itself to learn when Sir Andrew was born and when he died: what support the theories of heredity obtain from the scientific tendencies and noble courage of his parents and fore-elders: who were the teachers and friends, and what were the surroundings of his early years: what, in fact, made Ramsay such as we find him in later life—a prominent person in whatever circumstances he was thrown, whether in the small society of a provincial town or in scientific gatherings in London—a welcome guest at every table, a critic whom any author bolstering up wild theories with bad evidence would fear to have to reckon with. "Of an eminently social temperament, he made acquaintances easily wherever he went, and these chance acquaintanceships sometimes ripened into lifelong friendships. In one family circle we find him reading aloud Shakespeare, or Scottish ballads, or a good novel; in another, he takes part, heart and soul, in singing glees and madrigals; in a third, he joins in dancing and all kinds of merriment." "English literature was to him a vast and exhaustless garden, full of alleys green and sunny arbours, where from boyhood he had been wont to spend many a delightful hour. When he found among his colleagues one whose talk was not always of stones, but who had ranged like himself far and wide in literary fields, he opened out his inner soul, and his conversation glowed with an animation and power, as well as a gleeful exuberance, which astonished and charmed his companion." And "though he was not in any sense an antiquary, he knew a good deal about the history of architecture, and took a keen delight in visiting ruins and trying to form a mental picture of what they must have been before the gnawing tooth of time had dismantled them. Whatever, indeed, linked him with the past had a charm for him. He never willingly missed an opportunity of seeing a ruined castle or keep, a mouldering abbey, a grass-grown encampment, or a lonely cairn. If tradition or song invested any spot with a living interest, he would not consider his geological inspection complete if it had not included a visit to that site." With his quick sympathy and conversational powers, with his wide knowledge of nature, as well as of history and her monuments, no wonder that he was welcome in any society of intelligence and culture. He would unbend in congenial and sympathetic company, like a strong man who had been out in the cold and had at last gained a warm fireside, but he always made room for others round the fire. His impulsive and generous nature was, however, tempered with native judgment and caution. He soon gauged the character of those with whom he was thrown, and quickly estimated the amount of receptivity and the temper of an audience.

These qualities and acquirements with a strong and clear memory gave him much wealth of illustration in scientific discussion and in lectures. But his power lay more in the lucid exposition of his subject, the keen insight into the real points at issue, and the clearness with which he brought forward his array of facts and arguments. After one of his lectures at the Royal Institution, Faraday ran up to him, shook him by both hands, and asked "where did you learn to lecture?"

In the memoir will be found an account of his appointment to the chair of Geology at University College, from which he was always spoken of as Prof. Ramsay; also of his lectures there and elsewhere, and of his various other literary work.

In 1841 he was appointed Assistant Geologist on the Government Survey, under Sir Henry De la Beche, and throughout his forty years of service he was more and more identified with all its active work, either as himself taking part in the mapping, or as superintending others, though it was not until he had been appointed Director-General, in succession to Sir Roderick Murchison, that he officially represented the Survey to the public and the Government. An account of his life is therefore the history of the Survey, and no one is more competent than our author to tell the story.

The great difference between the work which has to be done by the Geological Survey and by amateurs is this. The amateur may examine a district in more or less detail, may pick out the points of interest, or the facts which illustrate some theory which he is developing, but, where he sees nothing, he may say nothing about it. The Geological Surveyor, on the other hand, must colour all his map, and sometimes has to put in by inference whole formations of which he has seen nothing whatever.

> "He spends the day, map in hand, over the ground assigned to him for survey. Every exposure of rock is noted by him on his map or in his note-book, with all the needful details. Each stream is followed step by step up to its source; each hill-side and ravine is traversed from end to end; each quarry, sometimes each ditch, and even the very furrows and turned-up soil of a ploughed field are scrutinised in turn. . . . He is brought into every variety of scenery, and is compelled by his very duties to study these varieties and make use of them in his daily work."

But even after all this careful search it often happens that he can find no direct evidence of the solid rocks below the soil and subsoils. He has to plot sections and see what, with the known thicknesses and dips, should be found there. He has to examine the surrounding country to see what occurs in the same relative position elsewhere, and whether any lines of disturbance which might complicate the results are running into the area respecting which, with little evidence, he must make up his mind and say something. The survey work is thus a grand training in the faculty of keeping one's wits about one, and of detecting sources of error. The habit of mind thus formed is often exhibited in the criticism of amateur work by trained survey men, and many can recall Ramsay's indignant protests against impossible or exaggerated sections when appealed to as evidence in support of any view.

He had a wonderful "eye for a country," to use one of his own favourite expressions. He rapidly took in the lie and relation to one another of the rock masses. To this any geologist who has worked with him in the field can testify, and this is what lends its value to his classical memoir on the geology of North Wales, and to the maps and sections of which it is explanatory. But when he had sketched out the outlines of the history of the ancient volcanoes of that area, and had noted the choked craters now exposed by the denudation of the overlying masses of lava and cinder and mud, and when he had described the isolated portions of the volcanic and marine deposits which, building up mountains round the ancient roots of the volcano, still remained the record of great sheets that once spread continuously far and wide over the whole area; when he had done all this, he turned to another aspect of the question, and sought a clear answer to the inquiry how much rock, which we know surely once covered this area, has been removed by denudation? and we find in the same memoir sections illustrating the conclusions at which he had arrived.

For when "he had traced out the structure of a complicated geological region, and was able to show what should have been the form of the surface had it depended on geological structure," he was then "in a position to demonstrate how much material had been removed by denudation," namely, all that was above what he called the *Plain of Marine Denudation*, that is the old base level of ancient erosion, down to which all the agents of waste—rain, rivers, ice, and sea—had reduced the uplifted land; or perhaps, as we should now say, giving greater prominence to subaërial action, to the level at which the sea had arrested the work of the various agents that were reducing all dry lands to sea level.

Few men's work illustrates better than Ramsay's the place and value of a good "working hypothesis" in some kinds of higher scientific research. Imaginative and fertile in suggestion, no one was more sorry when further observations did not clearly support his first impressions, and he tried and tried and tried again to make it fit; yet he bowed always with deference to established evidence and logical consequence.

Besides his regular survey work, which was itself full of new observations and original treatment, and besides many papers giving the results of his researches on special points, he from time to time plunged into more speculative questions, and advanced some theory in explanation of the larger phenomena, especially those connected with surface configuration. For instance, reflecting on the great quantity of fine mud, "flour of rock," carried down by glaciers, and observing that ice was not, like water, restricted in its flow to continuous downward slopes, and holding that ice charged with stones would grind away more rock where the pressure was greater or the rock softer, he propounded the theory of the glacial erosion of rock basins.

His explanation is probably true in some cases, but he gave it a wider application than has been borne out by subsequent investigations.

Unfortunately he adduced as his first example the Lake of Geneva, a basin to which even those who agree with him upon the general probability of there being glacially eroded hollows such as his theory requires, would not now be prepared to apply it.

The public will read with profit and pleasure the biography of such a striking personality by a graceful and accomplished writer, who knew all about the man and his work, and had the skill to select with judgment and the good sense to keep the whole within the modest bounds of one volume of large print. Ramsay's many friends will love to have the record of his struggles and his triumphs, so many of which are told in his own words. Every survey man, not only of Great Britain, but throughout the world, will turn to this account of the commencement and growth of the geological survey of Great Britain, and cannot fail to profit by the insight it gives into the methods, life, difficulties, and results of that important branch of the public service.

So easily does the story run, that we cannot say whether the general reader, or the scientific student, will best appreciate this sketch of the progress of geological research through the most active and interesting half-century of its history.

OUR BOOK SHELF.

Harvard College by an Oxonian. By George Birkbeck Hill, D.C.L. (London: Macmillan and Co., 1894.)

DR BIRKBECK HILL spent two months in 1893 in Cambridge, Massachusetts, and has compiled this little volume giving some account of the history of the celebrated college and university of Harvard. So far as Dr. Hill relies upon previous publications, his account is accurate, but his own observations and impressions are —as is very natural—often quite erroneous. Scant justice is done to the important and costly arrangements for the study of the various branches of the natural sciences which exist either at or in connection with the Massachusetts university. Dr. Hill is not fitted by his own education and experience to report on these matters, nor, indeed, can much value be attached to his somewhat antiquated standpoint as a critic or observer of university institutions. He contrasts Oxford and Harvard at every step, but he fails to give any picture or presentation of the real characteristics of the student's life at Harvard. He does not sufficiently emphasise the fact that the undergraduate at Harvard enjoys the immense benefit of true *university* education, at the hands of distinguished professors, with freedom and independence in regard to his choice and method of study, and as to such personal details of life as board and lodging; whereas the Oxford undergraduate is treated throughout his career as a goose to be nursed, monopolised and plucked by college ushers, who (owing to the system under which they are appointed) are, as a rule, as little capable of good teaching as they are of managing the domestic and disciplinary details of the college-boarding-houses. Dr. Hill notes that the rage for athletics is almost as serious an injury to study at Harvard as it is at Oxford. L.

Tableau Métrique de Logarithmes By C. Dumesnil. (Paris: Librairie Hachette and Co., 1894.)

THE use of logarithms for calculations is, as every one knows, a great saving of labour and time, and what otherwise would be complicated pieces of work are reduced to simple computations. The facility of working depends, after some time, on the good or bad arrangement of the tables, but instances often occur where much time is lost by having to turn pages backward and forward. For the case of logarithms to five places of decimals, M. Dumesnil has devised a means of eliminating altogether the use of tables, by adopting a series of scales neatly printed on stout sheets. From these scales the divisions are so arranged that all the logarithms of the numbers from 1 to 10,000 can be easily read off, and *vice versâ*. The scales, or tables as they are called, are published in two qualities. The cheaper are neatly and clearly printed, and on a smaller scale than the other; but, on the whole, we recommend the more expensive sheets, as the numbering is more easy to follow (coloured numbers being used), and the divisions are more legible on them.

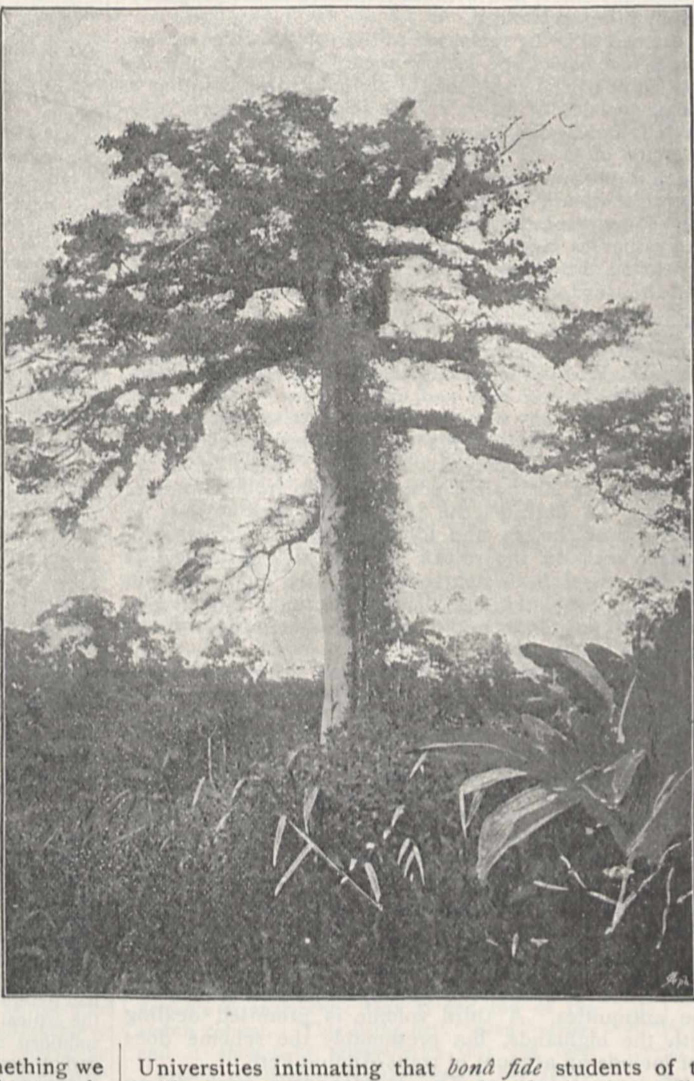
To understand the method of working the tables is a matter of only a few minutes' attention, and when grasped, either the logarithm of a given number, or the number from a given logarithm, can be read off without the least hesitation. The book of instructions, which is separate from the tables, contains all that the user of the tables can require. The explanations are full and concise, and the worked-out examples will prove an excellent help in acquiring the methods of solution.

In the Guiana Forest. Studies of Nature in Relation to the Struggle for Life. By James Rodway, F.L.S. With an Introduction by Grant Allen. Illustrated. (London: T. Fisher Unwin, 1894.)

WE have read Mr. Rodway's book with a good deal of pleasure. Such a subject as the struggle for existence amongst the animals and plants of the Tropics, could not fail to be full of interest when dealt with by an enthusiastic lover of nature. For it is in the Tropics that nature's principal workshops are situated, and no naturalist can afford, nowadays, to neglect that essential element in a liberal biological education—a visit to these regions. There the struggle for life is no longer, as in our own climates, a cold-blooded process which only a trained eye can follow, but a fiercely active competition for the means of subsistence which is everywhere apparent in every detail of the structure of the individual and of the economy of the species. The "heartless vegetable" amid such surroundings seems no longer a reality, but the cold figment of a northern imagination.

Mr. Rodway describes the vegetation of the forest, the swamp, the sand-reef, and the sea-shore, and each is sketched in vigorous outline. One cannot, however, avoid wishing that the author had been contented to give his impressions of the actual facts, without indulging in metaphysical moralising which is not scientific, and is not always common sense. Occasionally, too, he gives way to great prolixity, as, for instance, in an excursus on the interdependence of animals and plants, which is all very like something we have heard before, only that Mr. Rodway's story is longer. The author is on dangerous ground when he ventures to dip into the philosophy of natural selection, or to deal with problems of variation.

But notwithstanding these defects, the book is worth reading. It is well illustrated, and contains a large amount of really interesting observation which may stimulate the general reader, and which will recall many a half-forgotten scene to those who have themselves been travellers. The accompanying illustration, for which we are indebted to the publisher, shows a silk cotton-tree crowded with epiphytes.



Lehrbuch der Experimentalphysik. Von A. Wüllner. Band i. 5te Aufl. (Leipzig: Teubner, 1895.)

STUDENTS who wonder when we are to have an English treatise on experimental physics worthy of comparison with this well-known German one (and the French text-books of Jamin and Violle), will find food for reflection in the fact that thirteen years have elapsed since the last edition of Wüllner (the fourth) was published. Much the same thing holds good of other German scientific treatises, notwithstanding that publishing firms of repute in the Fatherland do not consider it beneath their dignity to quicken the sale of the remnant of an edition by having notices posted up in the Universities intimating that *bonâ fide* students of the subject can secure copies at a reduced price by application through the University Professor.

In the present case the interval has been well employed. The first volume has grown to 1000 pages—or 150 more than in the fourth edition—and the most important researches published up to the end of 1892 have been incorporated. In the last edition the article on internal friction contained simply a discussion of torsional vibrations and logarithmic decrement, followed by a page in which mention was made of the researches of Schmidt and others, with a statement of the conclusion arrived at, viz. that in such elastic oscillations the motion is opposed by an internal resistance or friction which is measured by the logarithmic decrement and is distinct from the elastic after-effect. In the new edition six pages are devoted to Boltzmann's theory of internal friction of solid bodies, according to which the damping of oscillations is regarded as due to the elastic after-effect, the result of which is that the force urging the wire towards its position of equilibrium is not proportional to the displacement. In the fourth edition there was little about "Hardness," beyond a list of the substances forming Moh's scale; indeed, the whole article on the different kinds of *Festigkeit* was rather unsatisfactory. Now the researches of Hertz and the experiments of Auerbach have made it possible to measure the hardness of a substance absolutely and without reference to the properties of any other substance.

Instead of being relegated to the volume on heat, as in some text-books, the kinetic theory of gases is here described at considerable length in the section treating of the properties of gases. This section has been overhauled, and Stefan's theory of gaseous diffusion is given in place of O. E. Meyer's. Prof. Wüllner attaches so much importance to recent developments of the electromagnetic theory of light, that he has determined to alter the sequence of the subjects in his treatise, so that light and radiation may come at the end. In the edition now appearing, the second volume will therefore treat of heat, and the third of electricity. *pv.*

Peru. Beobachtungen und Studien über das Land und seine Bewohner während eines 25-jährigen Aufenthalts. Von E. W. Middendorf. (Berlin: Robert Oppenheim, 1893-94.)

DR. MIDDENDORF commences his treatise on Peru by a long preface detailing the circumstances which led him to choose that country for his home. Starting as the surgeon of an Australian emigrant ship from Hamburg in 1854, he narrates the incidents of the voyage, an epidemic of cholera and other irrelevant accidents included, until on the return journey, sick of the sea, he left his vessel in Valparaiso, and after some further wandering took up his residence in Peru, and was led by degrees to pay closer and closer attention to the land and people of his choice.

The work is planned in three parts. The first volume deals nominally with Lima, the capital of the republic, but is really much wider in its scope, commencing with the history of Peru, and after describing the town with its streets and public buildings in somewhat tedious detail, proceeding to discuss the general institutions of the country from the standpoint of the capital. People, church, government, law, education, commerce, transport, and charities are all discussed; and at the end two chapters describe the municipal markets, slaughter-houses, water-works, bull-fighting arenas, theatres, and other places of amusement. The second volume describes the coast lands of Peru northward and southward of the capital, the towns and seaports, the provinces now ceded to Chile, the railway communications, and the antiquities. A third volume is promised dealing with the highlands, but presumably the scheme does not include an account of trans-Andine Peru.

We cannot look on Dr. Middendorf's work as an exhaustive or even a comprehensive work on Peru. It is indeed a book for the general reader rather than the student. Abounding as it does in personal reminiscences, and written in an easy conversational style, it should do much to further the knowledge of the country it describes amongst German-speaking people. But it is, we fear, too diffuse and bulky to serve this purpose with the same degree of satisfaction that a smaller work might have secured, and it lacks firm and wide generalisations which could present a clear picture of the land and people as a whole.

LETTERS TO THE EDITOR.

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The Liquefaction of Gases.

A CAREFUL study of his letter in NATURE of February 14 shows that Prof. Dewar makes four claims.

(1) He claims that in his lecture in 1884 he employed an apparatus "more readily and quickly handled" than that of Messrs. Wroblewski and Olszewski, in the illustrations he gave of the work of these investigators.

(2) He claims that in his lecture in 1884 he used liquid nitrous oxide for the production of liquid oxygen.

(3) He claims that in 1886 he liquefied oxygen by passing the gas through a long copper coil surrounded by liquid ethylene, and that his apparatus made it possible to transfer the liquid oxygen to a glass vessel wherein it could be used as a cooling agent.

(4) He claims that, in conjunction with Prof. Liveing, he determined the refractive index of liquid oxygen before the publication of Prof. Olszewski's first paper on the subject.

I shall consider these claims in order.

(1) The apparatus used by Messrs. Wroblewski and Olszewski in 1883 is fully described and figured in *Wiedemann's Annalen* for that year (xx., 243). The gas to be liquefied was strongly compressed in a glass tube, imbedded in an iron cylinder, and connected by a capillary tube with a glass vessel, which in turn was connected with a store of liquid ethylene, previously cooled by passing through a narrow copper tube two metres long immersed in a bath of solid carbon dioxide; the glass vessel was also connected with a double oscillating Bianchi air-pump. The liquefied gas was collected in the glass vessel, and its properties were examined. A comparison of the apparatus of the Polish Professors with the brief description in the *Proceedings of the Royal Institution* of the apparatus used by Prof. Dewar in 1884 "for the purpose of lecture demonstration" will, I think, show that I stated the matter fairly in my former letter by saying that Prof. Dewar "describes and figures an apparatus which is a slightly modified form of that of the Polish Professors, which in turn was derived from the apparatus of Cailletet."

(2) If anything was gained by using liquid nitrous oxide for the liquefaction of oxygen, why has not Prof. Dewar continued to use this refrigerator?

Prof. Dewar claims to have devised a method for liquefying oxygen which was so much inferior to that brought into practice by Wroblewski and Olszewski that it was abandoned almost as soon as it was devised. I suppose his claim may be admitted here.

(3) Granting that the substitution of a long copper coil for a strong glass tube was an improvement, it must not be forgotten that the apparatus of Messrs. Wroblewski and Olszewski, described in 1883, enabled the liquefied gas to be collected in a vessel wherein its properties not only might be, but were, studied. That this apparatus was an efficient instrument is shown by the facts that before the year (1886) in which the copper coil method was described, Profs. Wroblewski and Olszewski had liquefied oxygen, nitrogen, and carbon monoxide: Prof. Olszewski had liquefied air, nitric oxide, and marsh gas; had determined the boiling points, at atmospheric pressure, and the critical temperatures and pressures, of these six gases; had solidified nitrogen, chlorine, hydrogen chloride, nitric oxide, carbon monoxide, and marsh gas; and had made careful determinations of the behaviour of hydrogen at $-211°$ and $-220°$. But with his copper coil 45 feet long Prof. Dewar did nothing; for, so far as can be judged from his published papers, he did not initiate experiments on the properties of liquefied oxygen, nitrogen, or air, until 1891-92. I think we may conclude that the apparatus of the Polish Professors was better adapted for accurate work than "that made wholly of metal" to which Prof. Dewar refers. If Prof. Dewar's apparatus of 1886 was "safer and better" than "that used in Cracow in 1890"—as he says it was—why was not this safer and better apparatus turned to some scientific use before a description appeared of the apparatus of Prof. Olszewski, in which the gases were liquefied in a steel cylinder and then transferred to

a glass vessel? In his letter of February 14, Prof. Dewar tells us that by means of the copper coil apparatus of 1886 "liquid oxygen was made and decanted or transferred from the vessel in which it was liquefied to another by means of a valve, and thereby rendered capable of use as a cooling agent." Why, then, did he not use the liquid oxygen as a cooling agent? It is strange that seven years after this, when Prof. Dewar wished to "prosecute research at temperatures approaching the zero of absolute temperature," he says he had "no recorded experience to guide [him] in conducting such investigations." Had he forgotten his own apparatus with its copper coil 45 feet long?

Prof. Dewar's apparatus of 1886 must have been peculiar; in one sentence in his letter of February 14, its deviser speaks of it as "entirely different in type from the crude plan Olszewski adopted in 1890," and in another sentence he assures us it was "practically identical in principle with that used in Cracow in 1890."

The apparatus of 1886 succeeded, in Prof. Dewar's hands, in producing solid oxygen; why have we heard nothing of that solid oxygen? Determinations of some of the constants of that substance would be interesting and important.

I must beg attention to an instance of Prof. Dewar's boldness. In 1883 Messrs. Wroblewski and Olszewski liquefied oxygen, and other gases, in a strong glass tube; in 1884 Prof. Dewar repeated some of the experiments of the Polish Professors, using, as they had done, a strong glass tube, but adapting some parts of their apparatus so as to make it "for the purpose of lecture demonstration . . . more readily and quickly handled"; in 1890 Prof. Olszewski described an improved and enlarged instrument, based on that described by his colleague and himself in 1883, wherein the liquefaction was effected in a steel cylinder. Yet, in his letter to NATURE of February 14, Prof. Dewar says "replace the glass tube in *my* apparatus of 1884 by a small steel cylinder, and attach to its lower end a narrow copper tube with a stopcock, and the Olszewski apparatus of 1890 is produced." (The italics are mine.)

(4) As regards Prof. Dewar's joint claim to priority in the determination of the refractive index of liquid oxygen, I admit he is right and I was wrong. I was misled by the date at the beginning of the paper which contains the earliest measurements by Messrs. Olszewski and Witkowski of the refractive index of liquid oxygen. The paper is headed *Extrait du Bulletin de l'Académie des Sciences de Cracovie, Octobre* 1891. At the end of the paper is the date 15 *Juillet* 1892; this, and the footnote, quoted by Prof. Dewar, make it evident that 1891 at the beginning of the paper is a printer's error for 1892. I am sorry I made this mistake. Nevertheless, considering that Olszewski's quantitative experiments on the optical properties of liquid oxygen were begun in 1887 (*Wied. Ann.* xxxiii., 570), and were continued in January 1891 (*ibid.* xlii., 633), I hold I was justified in referring to Prof. Dewar's experiments on the optical properties of liquid oxygen as "mainly repetitions of the work of Olszewski and Witkowski."

Towards the end of his letter Prof. Dewar refers to a quotation I made from one of his lectures ("Having no recorded experience to guide us . . ."); he follows my quotation (of which he gives only a part) by another to the effect that "the necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids, like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination." I had expressed admiration "of the skill which produced" Prof. Dewar's new vacuum receivers. Prof. Dewar now tells us that when he prefaced his account of the making of these receivers by the sweeping assertion that he had no experience to guide him in conducting investigations at very low temperatures, he did not mean what he said; it was merely his way of saying that the vacuum receivers he was about to describe had not been made before. It was not I who made, it was Prof. Dewar himself who has made, a "glaring misrepresentation of the meaning" of his own words.

Prof. Dewar lays no stress, in his letter, on his own experiments on chemical action, phosphorescence, &c., at very low temperatures. It is these experiments, I suppose, that are referred to when he speaks of "throwing his bread upon the waters"; or does he abandon them as trifling, in that strange sentence—"Have I ever suggested that Prof. Olszewski was anticipated, or attempted to raise any question of priority?"?

Prof. Dewar hides the essential questions in a mist of words. If he has made marked improvements in the methods of liquefying and manipulating the more permanent gases (besides his invention of vacuum receivers); if he has conducted original, accurate, and thorough investigations into the properties of the liquefied gases, where are the accounts of this work to be found? Every student of the subject knows he can lay his finger on the work of Olszewski, and also on that of his deceased colleague Wroblewski; and he knows that work to be thorough, accurate, and important. But where can there be found some distinct account of the scientific work in connection with the liquefaction of the more permanent gases that is attributed to the Fullerian Professor at the Royal Institution?

I asked, and I still ask, for an "instant and serious consideration" of the whole matter brought forward in these letters by "those who are truly interested in the advance of science, and are jealous of the good name of the scientific men of this country."

M. M. PATTISON MUIR.

Cambridge, February 17.

Argon.

THE beautiful research of Lord Rayleigh and Prof. Ramsay has proved up to the hilt that in argon they have discovered an unknown gas which is remarkable for some of its physical properties, and especially for its extraordinary chemical inertness.

Among the interesting questions which have been raised, but not fully solved, are: Is argon an elementary body? If so, how does it stand related to the other elements?

There are some who advocate its being an allotropic form of a previously known element, just as ozone is allotropic oxygen; but it has not yet been produced from, or resolved into, nitrogen or any other element; and, until that should happen, it has the full right to take rank as a new element.

At first sight the fact of its giving two different line spectra under different circumstances might favour the idea of its being resolved into two bodies; but the fact that these two very compound line spectra have twenty-six lines in common, which are not all of them among the strongest lines, appears to me an argument for the fundamental unity of the substance.

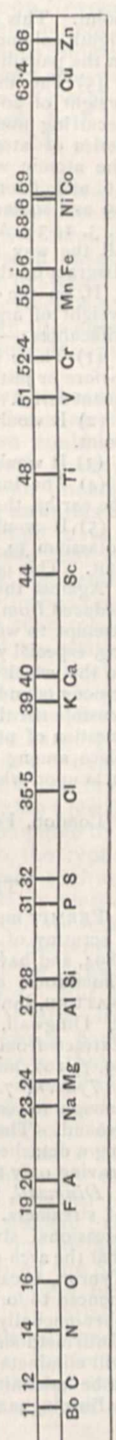
If argon be an element, what is its place in Mendeléeff's table? This is at present agitating the minds of chemists and physicists. The specific gravity of the gas would lead to an atomic weight of close upon 20; but it is argued from the velocity of sound through it that it is a monatomic gas, and therefore close upon 40. Now, if 20 be the atomic weight, it will fall in admirably with the periodic law; if 40, it will be perfectly inconsistent with it. The argument in favour of 20 is at least five-fold:—

(1) It will fill an existing vacancy in Mendeléeff's arrangement; that at the top of the eighth column. This is the first "even" series; and it should be remembered that the later "even" series in that last column are represented by the iron, palladium and platinum groups of metals; the "uneven" series being as yet unrepresented.

(2) It will follow the periodic law in regard to its melting point. That should be very low, like nitrogen, oxygen and fluorine; and so it is.

(3) It will follow the law in regard to its atomic volume. That is small; and so it should be by analogy.

(4) A great characteristic of argon is its not forming stable compounds with other bodies, at any rate at a temperature high above its boiling



point. This property characterises the other occupants of the eighth column; to some extent in the iron group, and certainly in the palladium and platinum groups.

(5) The introduction of a new element with the atomic weight of 20 (not 21 or 22), will extend the range of certain recurring numbers which appear near the beginning of the series of atomic weights. In the existing arrangement of the atomic weights it will be observed that between oxygen 16 and fluorine 19 there are three units; between fluorine 19 and sodium 23 there are four; then the numbers run 1, 3, 1, 3, 1, 3½. Adding argon at 20 the series becomes symmetrical all the way from oxygen to chlorine, as will be seen in the diagram on the previous page.

If, on the other hand, we were to adopt 40 as the atomic weight of argon, we should meet with the following serious difficulties:—

(1) There is no room for it. To place another figure just before or just after calcium would disarrange the whole subsequent series.

(2) It would break the periodic law in regard to its melting point.

(3) It would break the law in regard to its atomic volume.

(4) The inactive argon would be associated with metals of the earths, the compounds of which are remarkably stable.

(5) It would bring the atomic weight of three elements, potassium 39, calcium and argon each about 39·9, within one unit. This never occurs elsewhere in Mendeléeff's table.

Against these considerations there is the forcible argument deduced from the ratio of the specific heats of argon. I will not attempt to weigh the respective merits of these lines of reasoning, especially in the absence of the details of the experiments on the velocity of sound, and until we have some knowledge of the compounds of argon. Trustworthy conclusions will not be possible till this further information is obtained. It is not a question of physics *versus* chemistry, for the true theory of its place among the elements must be able to coordinate all the facts upon which both the chemist and the physicist rely.

J. H. Gladstone.

London, February 8.

The Aurora of November 23, 1894.

Permit me to call attention to a significant fact disclosed by a scrutiny of the observations of the aurora of November 23, 1894, and having an important bearing in discussing the auroral dimensions, and which appears to have escaped notice (see Nature, January 10). I refer to the *invisibility* of the objects at Dingwall, to the observers at Tynron, Dumfriesshire. Extracts from the synchronous accounts in Nature, November 29, p. 107, and January 10, p. 246, will prove this statement.

Tynron, 7.30.—Luminous mist in the northern sky, strong enough to cast shadows on the shining surface of the wet ground. The mist moved from the horizon to the zenith, forming a detached luminous belt in patches, disappearing at 8.30, leaving only the light in the north.

Dingwall, 7.30.—Sky covered in all directions by a canopy of streamers. At the same time the arch disappeared, and occasional streamers up to eight o'clock. It is not possible that the arch seen at Dingwall could be the same as the one at Tynron, because the former had vanished when the latter commenced to form, whilst there is a total absence of streamers, and phenomenally brilliant mist not recognised at the other place. Until methods of observation and analysis can be introduced that will eliminate the errors of identification, the solution is likely to be indefinitely postponed.

W. H. Wood.

Birmingham, February 3.

Making the necessary allowances for increased apparent luminosities of bright streaks, or of layers of light in the atmosphere, by the foreshortening effects of end-on, and of edge-presentations, the observations at Dingwall and in Dumfriesshire of the aurora of November 23 last, scarcely seem to recount very much which was not at the two places, at least partially, a fairly comparable and nearly contemporaneous description of the same phenomena. The first-formed light-band of the glow was very strong at Dingwall from the east to west, a little southward from the zenith, until 7.30 p.m., when with the usual drift of such displays to southwards, it became less prominent there than the approaching canopy of streamers which supervened, drifting up from the north-eastern sky in rear of it. But it assumed at the same time increasing prominency in Dumfriesshire (150 miles south of Dingwall), where between 7.30 p.m. and 8 p.m., it passed overhead in the shape of detached patches of light, a form which the belt was also seen to assume and to break up into, in a slow extinction stage at Slough, in that interval. The display of streamers rising from a large tract of light-mist approaching Dingwall from the northward and increasing constantly in lustre at 7.30 p.m., did not extend far south of Dingwall before it faded out soon afterwards; and being (as it was seen at Slough) a dense local discharge of them, its corona of brightly-foreshortened beams overhead would naturally be a very impressive sight at Dingwall, although from a position 150 miles distant in Dumfriesshire, the broadside aspect of the short outbreak, seen from afar, instead of from underneath, would only have the appearance of a sheaf of coloured light projected up from the usual flat streamer-base, neither very wide nor extraordinarily lofty, but of the massive berg-like form, which was its description at Slough, not unfrequently noticeable in rather strong auroras; and it may even have been quite easily hidden from view entirely at Tynron, although a clear horizon in the north near Slough allowed its observation there, by trees or by other obstructions in the landscape.

If Mr. Wood can happily devise a practical means of perfectly recording the times and descriptions of *all* the rapidly changing features of an aurora, and the shifting variations of its misty light-glows, he could no doubt achieve results from observations which would be no less a benefit to astronomers and terrestrial magneticians, than an exact continuous registration of cloud phenomena would be of welcome interest to meteorologists and terrestrial electricians; but it needs no great familiarity with auroral exhibitions to be quite certainly assured that results of records even so elaborate as those might be, would never be found to confute, but only to confirm the generally accepted view of the really cosmical heights and dimensions of all truly auroral lights and corruscations; of all such lights, that is to say, as show in their spectra the yellowish auroral line, or some of the other well-recognised spectral indices of the aurora.

A. S. Herschel.

The American Association.

At the Brooklyn meeting of the American Association for the Advancement of Science last summer, it was decided to meet this year at San Francisco, provided reasonable rates of fare could be secured from the trans-continental routes, as it was supposed could be done. Prof. Joseph Le Conte for three consecutive years had crossed the continent, laden with earnest and cordial invitations from the Universities and Scientific Societies of California, and the Common Council of San Francisco, to hold the meeting for 1895 in that city. The short-sighted policy of the railroads, however, refused to grant any concessions; and it has at last been decided to meet at Springfield, Mass., August 29—September 4. The meetings of affiliated societies will begin on Monday, August 26—rather later than the usual time of meeting.

Springfield is a small city, located in the heart of New England. It is the seat of the principal arsenal of the United States; and, while not a University city itself, it is within two or three hours' ride of nearly, or quite, a score of institutions of learning of the highest grade, including the two oldest and most powerful Universities in America, Yale and Harvard. This will be the second meeting at Springfield, the first having been held in 1859.

The Association is incorporated by the State of Massachusetts, and its office and museum are at Salem in that State; but no meeting has been held in New England since that at Boston in 1880, the most brilliant in all the history of the Association. The return to New England after this longest absence, gives unusual interest to the approaching meeting.

Wm. H. Hale.

Brooklyn, New York, February 2.

Earthquake in Norway.

About midnight on the 4th and 5th of this month, a fairly strong earthquake occurred in the southern part of Norway. The greatest disturbance was felt in the environs of the town of Aalesund, upon the west coast (about 60° 30′ lat. N.). From there the shocks extended to the region of the Trondhjems fjord, the Swedish border, and the Christiania fjord, and Bergen, but the extreme south-west part of the country seems to have been undisturbed. The earthquake is also reported from Fünen, in Denmark. The movement proceeded in about seven minutes from the west coast to Christiania (at Christiansund 11h. 38m. Christiania time, 11h. 15m. Greenwich time). It is interesting to notice that the earthquake resembles one which occurred on March 9, 1866, and was felt across the North Sea at the lighthouse of Fluggarrock, on the Shetland Islands. As I am engaged in collecting data about the earthquake of this month, I should be glad to know whether it was observed in the British Isles.

Christiania, February 11. HANS REUSCH.

"The Black-veined White Butterfly."

MY experience of this species in England enables me to support Mr. W. Warde Fowler's opinion (NATURE, February 14, p. 367) as to the preference of the species for open ground. I met with it in abundance in the New Forest in 1866, 1868, 1869, and 1870. It rarely occurred in or near dense woods, but preferred the open heaths and wastes of the Forest, where thistles were plentiful. In 1867 I found the species swarming, about midsummer, in hay fields on hill-sides in Monmouthshire. There were a few small orchards, but not much wood, in the neighbourhood. For a detailed account of the former distribution of *Aporia cratægi* in this country, I would refer Mr. Warde Fowler to my article on the subject in the *Entomologists' Monthly Magazine* for March 1887. H. GOSS.

Surbiton Hill, March 16.

The Zodiacal Light.

AT the present moment—7 p.m. February 16—the zodiacal light is more distinct than I ever remember to have seen it in England. The middle of the base is about 2° to the northward of the point where the sun set, and the axis is directed towards the Pleiades, and can be traced as far as the middle of Aries. The afternoon has been remarkably clear, and it is now a brilliant starlight evening. J. P. MACLEAR.

Cranleigh, Surrey, February 16.

OYSTERS AND TYPHOID.

THE statements that have recently appeared, both in the general and in the medical press, concerning the communication of typhoid fever through the agency of oysters when eaten raw, make it desirable to review some of the data on which the suspicion in question is based. For many years past it has been a matter of assumption, when typhoid fever has followed, within some ten to fifteen days, on the consumption of raw oysters, and when no obvious cause for the disease could be detected, that the oysters stood to the fever in the relation of cause; and this attitude received no inconsiderable impetus when, a few years ago, a member of our Royal family sickened of typhoid fever under circumstances that were suggestive of oysters as the vehicle of the disease. Then again, it must be admitted that it has been a matter of no very uncommon experience amongst medical men to have to treat typhoid fever in patients who, at an antecedent date corresponding with the incubation period of typhoid fever, had indulged in an oyster supper after leaving some place of entertainment. And the suspicion has been confirmed, in some cases, when it has been ascertained that another member of the same party, having nothing but the oyster supper, in common with the sufferer referred to, has also had typhoid fever about the same date, or had suffered from vomiting and other symptoms the day after the consumption of the oysters. The assumption in cases of this latter class has been, that the specific poison of typhoid fever was, with other matter that had become objectionable to the system, got rid of by the attack of sickness. A case generally illustrative of this class of occurrence was recently recorded in the *British Medical Journal*. Four friends had an oyster supper on November 5. Two of them lived not far apart, but the others had nothing in common as regards residence or anything else. On November 23 three of them sickened, and they were, later on, all found to have typhoid fever. One of the patients, during convalescence, disclosed both his profession and his views by re-naming his malady "bivalvular disease."

Amongst leading medical men who have adopted the view that oysters are a source of typhoid fever, we may name Sir William Broadbent, who early this year announced that from time to time he had seen cases of typhoid fever "apparently attributable to oysters," but that during the course of last autumn the evidence as to the communication of the infection through this agency has been of such a character as to produce "conviction" in his mind.

This naturally leads us to ask how the oyster becomes the vehicle of such a disease; and the evidence already forthcoming on this point is such that we could only wonder if typhoid fever were not occasionally conveyed to those who eat this favourite mollusc in an uncooked form. Investigation of some of the river estuaries and other places where oysters are cultivated and prepared for market would almost lead us to believe that conditions favourable to typhoid fever were deliberately chosen for the purpose. Indeed, it is notorious that a number of our British oyster-beds are in such relation to sewer outfalls, that the oysters must of necessity be bathed in a solution of sea-water and sewage at every tide. According to a commissioner appointed to inquire into this matter by the *British Medical Journal*, a well-known Essex oyster fishery has "a sewer discharging between oyster-beds on either side"; and at a "health-resort" (!) on the same coast, it is a common practice to moor the oyster-boxes to a pier or groyne, within a few feet of which the evidences of sewage are too palpable to be specified. In both the places referred to, the typhoid fever poison, which it is known finds access to drains, had had ample chance of fouling the sewers in question.

It has been alleged, on the evidence of certain recent bacteriological investigations as regards the contents of London sewers, that the organism producing typhoid fever cannot live and multiply in sewers. But the organism has been found in sewers; it also lives in sea-water; and the fact remains that sewage bathes our oysters during cultivation to an extent that is essentially disagreeable, and that ought not to take place; and, also, that typhoid fever follows the use of oysters so cultivated. It may also be alleged, as is done by certain oyster-growers, that sewage is fatal to the oyster itself. In answer to this, we can only say that such evidence as we have obtained, as to some of our oyster-beds, is absolutely opposed to this statement; and not only so, but we know of more than one instance where the oysters are deliberately brought from the beds to fatten in still nearer proximity to outfall sewers for a week or more preliminary to their sale. In brief, if sewage and noxious micro-organisms can be retained in the beard and other portions of the oyster, or in the "juice," which is so much relished, everything seems contrived to secure such retention of filth at some of our oyster fisheries.

Doubtless the same applies to many foreign oyster-beds. Indeed, the recent experience embodied in a report by Prof. W. N. Conn, as to an epidemic of typhoid fever amongst the students of a college at Middletown, Connecticut, not only supplies convincing evidence of this, but it affords the most connected and complete proof of "oyster-typhoid" as yet published. Quite an epidemic of typhoid fever occurred amongst the students of certain fraternities, and amongst a number of their friends who had joined them at their "initiation suppers," but who had subsequently returned to their distant homes, where they sickened. The incidence of the epidemic was on those fraternities only who had included raw oysters in their *menu*; and even amongst these some marked escapes were in persons who, for one and another reason, had not consumed oysters. The suspected oysters came from Long Island Sound, where they had been put to "fatten" in a fresh-water estuary within 400 feet of a sewer known to have been receiving typhoid material. The last piece of evidence bearing upon this subject comes from an official source. It is announced that, on the strength of a report by the Medical Officer of the Local Government Board, as to the diffusion of cholera in England during 1893, which report is now passing through the press, an inquiry has been commenced into the circumstances under which oysters are cultivated and stored round our coasts. The reference is clearly to the serious outbreak of cholera at Grimsby and Cleethorpes, and to the diffusion of the epidemic from those places, whence a large distribution of oysters and other shellfish is constantly in progress.

Whatever be the outcome of the inquiry which has been instituted, it is certain that two questions will come to the fore: (1) the need for control over our oyster-beds, and (2) the desirability or not of allowing crude sewage to be discharged direct into the sea, or into tidal estuaries.

NOTES.

SIR HENRY ROSCOE has been made Chairman of the Select Committee of the House of Commons appointed to enquire whether any, and what, changes in the present system of weights and measures should be adopted.

WE regret to announce that Mr. John Whitaker Hulke, F.R.S., president of the Royal College of Surgeons of England, died on Tuesday, from broncho-pneumonia. An obituary notice in the *Times* furnishes us with the following particulars with regard to his career. Mr. Hulke was born in 1830, and was the elder son of a well-known and highly-esteemed surgeon at Deal, where his family had been settled for several generations. He was educated at King's College School, and subsequently spent two years in Germany, where he thoroughly acquired the language. After a varied experience as surgeon to the hospital at Smyrna, during the Crimean War, and in King's College Hospital, he migrated to Middlesex Hospital. In 1859 he received the Jacksonian prize of the Royal College of Surgeons for his Essay on Diseases of the Retina, and soon afterwards he brought out a treatise on the ophthalmoscope, then a novelty in eye-practice. This led to his being regarded mainly as an ophthalmic surgeon; but he contributed to general surgery in the Medico-Chirurgical *Transactions*, and joined Mr. Holmes in editing the third edition of his "System of Surgery." He was elected a fellow of the Royal Society in 1867. In 1876 he was appointed an examiner in anatomy and physiology at the College of Surgeons; and in 1880 became a member of the Court of Examiners, an office which he held for ten years. In 1881 he was elected a member of the Council; and, after twice serving the office of vice-president, he became president in 1893, and has died in office. He had been president of the Pathological and Ophthalmological Societies, and at the time of his death was president of the Clinical Society and librarian of the Royal Medical and Chirurgical Society. Mr. Hulke was, however, much more than an accomplished surgeon. He was a good comparative anatomist, botanist, and geologist; and was at one time president of the Geological Society of which he was elected the Treasurer on February 15. He was an artist in water colour, and was able both to model in clay and to carve in marble. His loss is a real one to the medical profession, in which he was esteemed as a man of the highest probity and sagacity.

WE notice the death, at the age of eighty-eight, of a gifted mathematician, the Rev. T. P. Kirkman. He was elected a Fellow of the Royal Society in 1857.

THE following deaths have occurred among scientific men abroad:—Dr. Gerhard Krüss, Extraordinary Professor of Chemistry in the University of Munich. M. Jules Regnauld, Professor of the Paris Faculty of Medicine, at the advanced age of ninety. The Rev. J. Owen Dorsey, a well-known ethnologist, at Washington, February 5. Prof. Dorsey had been connected with the Bureau of Ethnology, since 1877. He was the president of the Anthropological Section of the American Association for the Advancement of Science in 1893. We also have to record the death, at the early age of forty-five, on January 28, of Dr. F. Schmitz, Professor of Botany at Greifswald. For many years past, Dr. Schmitz had turned his attention chiefly to the study of the Algæ, and especially of the red sea-weeds or Florideæ, to our knowledge of the life-history of which he had made substantial additions. He published, in the year 1877, an account of the formation of auxospores in the diatoms, and, in 1879, a description of the green Algæ of the Gulf of Athens.

PROF. L. GUIGNARD, President of the Botanical Society of France, has been elected to succeed the late Prof. Duchartre in the Section de Botanique of the Paris Academy of Sciences.

LORD RAYLEIGH will deliver a course of six experimental lectures on "Waves and Vibrations," at the Royal Institution, on Saturdays, March 2, 9, 16, 23, 30, and April 6. He will also deliver the Friday evening discourse on April 5, when his subject will be "Argon, the New Constituent of the Atmosphere."

A NEW thallium mineral has just been described, under the name of Lorandite, by Prof. Krenner, of Buda-Pesth. The new mineral occurs sparingly, in association with realgar, at Allchar in Macedonia. It is found as transparent crystals belonging to the Monosymmetric system, and having the form of plates or short prisms; its colour varies from cochineal-red to kermesite-red. The mineral proves on analysis to correspond to the formula $TlAsS_2$, and contains 59·5 per cent. of thallium.

WE have received from the Russian Chemical Society a pamphlet, devoted to the description of the new chemical laboratory which has been erected at the St. Petersburg University. The laboratory has been built in accordance with the requirements of modern scientific investigation, and has cost £32,720. All branches of research have separate large halls, special rooms being allotted to physical chemistry and accurate physical measurements. Although the laboratory is behind many of the largest laboratories of West Europe, it has the advantages of perfect arrangements for each separate worker, and it decidedly has no rivals for the perfection of ventilation. The total amount of warm air supplied to all the halls of the building attains 823,000 cubic feet per hour, so that the air will be totally changed from one to five times per hour in each separate hall.

THE Russian Geographical Society awarded, at its meeting of January 30, the Constantine medal to S. N. Nikitin for his numerous works on the geology of Russia; the Count Lütke medal to P. K. Zalesskiy for geodetical work in Turkestan; the great gold medal, to N. A. Karysheff for his work, "The Land rented by the Peasants"; and the Prjevalsky premium, of £60, to V. A. Obrucheff for his last journey in Turkestan and Central Asia. Small gold medals were awarded to the French geodesist, M. Defforges, and the Austrian geodesist, Baron Sterneck, for their pendulum observations in Russia, and to M. Sieroszewski for his MS. on the Yakutes; and the great silver medal of Prjevalsky's name to Baron Toll and Lieutenant Shileiko, for their last journey to Arctic Siberia. Eleven silver medals were also awarded for minor works.

SEVERE frost has continued in nearly all parts of the British Islands, but in England and Ireland the cold was rather less intense last week. The frost has now continued for over four weeks, and although the period is shorter than in some other frosts during the present century, the intense cold experienced has been seldom equalled. There were no instances of temperatures below the zero, as in the preceding week, but readings of 20°, and even 30°, below the freezing point have been recorded. At Greenwich the mean temperature since the commencement of the frost is 28°, and the mean of the night readings is only 22°. Very little snow has fallen in any part of the British Islands, but the ground is still covered with snow from the fall during the earlier part of the frost. The type of weather has undergone a very considerable change, and the last few days have been much less cold, although frost occurs each night. The European anticyclone appears to have thoroughly given way, and in the course of two or three days the barometer has fallen to the extent of nearly an inch over Scandinavia. An anticyclone is, however, situated over the British Islands, and while this continues frost is still likely to be experienced.

AT the meeting of the Anthropological Institute on February 12, Mr. Brabrook drew attention to the work of the Ethnographic Survey Committee of the British Association, on which the Institute was represented by Mr. Francis Galton, Dr. Garson, and himself. He said the committee had been successful in obtaining a long list of places suitable for survey, and had prepared an octavo pamphlet, of twelve pages only, which gave comprehensive instructions to those who were willing to engage in it. What the committee now desired was to increase the number of observers, and he appealed to the members of the Institute for assistance in this respect. The committee were especially anxious to induce medical men to interest themselves in obtaining the necessary physical measurements in suitable places. For this purpose, they would be glad to furnish instruments, and render any other assistance that might be necessary and practicable. All their experience had shown them how valuable the results of their work would be likely to be, and how desirable it was that it should be proceeded with without delay.

THE magnanimous spirit which the Academy of Natural Sciences of Philadelphia has shown towards the circular sent out by the Royal Society, asking for co-operation and suggestions in making a subject-catalogue of scientific papers, is an indication of the unity of the interests of scientific societies. A report has been adopted by the Academy to the following effect :—(1) That a catalogue of scientific papers as proposed by the Royal Society is desirable, and that international co-operation should be engaged in its preparation. (2) That in order to secure uniformity in all parts of such a catalogue, a central bureau, as suggested by the Committee of the Royal Society, appears to be necessary, rather than that separate portions of the catalogue should be prepared by various institutions, such central bureau to be under the direction of the Royal Society, from which the proposition emanates; all publications of societies and monographs to be sent to such central bureau; the expenses to be met by returns from the sale of copies of the catalogue. (3) That such a catalogue should be classified, and should be issued at least once a year, each volume to be provided with an alphabetical index. (4) That the scope of such a classified catalogue should embrace the various yearly bibliographies of special sciences now issued. (5) That whenever translations or summaries are believed to be desirable, English should be made the basis of the catalogue.

THE fragility of human promises is proverbial. The following dialogue, which took place in the House of Commons on Monday, furnishes another illustration of this quality :—Mr. Bartley asked the Chancellor of the Exchequer whether work was resumed last autumn at South Kensington Museum, as promised by him, so as to leave everything ready for the commencement of those buildings as soon as the money was voted by the House of Commons; and what sum was to be taken in the coming year's Estimates for the completion of those buildings in accordance with the pledge of the present President of the Local Government Board in March, 1894, on behalf of the Government. The Chancellor of the Exchequer said the demands upon the Estimates under other heads had proved so heavy that it had been found necessary to restrict the expenditure on bricks and mortar to the lowest possible point, and he feared that it would not be possible to undertake any large expenditure in connection with South Kensington Museum at present. Mr. Bartley: May I ask the right hon. gentleman whether it is not the fact that last year a certain estimate was allowed to pass on the distinct pledge from the Government that this year there would be a vote for this building? The Chancellor of the Exchequer: It was intended to do so, but, as we were reminded the other night, we ought to cut our coat according to our cloth, and the hon. member must allow me to know what our cloth is.

THE anniversary meeting of the Geological Society was held at Burlington House, on Friday, February 15, when the medals and funds were awarded as follows:—The Wollaston Medal, to Sir Archibald Geikie, F.R.S.; the Murchison Medal, to Prof. G. Lindström, ; the Lyell Medal, to Prof. J. F. Blake; the Bigsby Medal, to Mr. C. D. Walcott; the balance of the proceeds of the Wollaston Fund, to Mr. W. W. Watts; that of the Murchison Fund, to Mr. A. C. Seward; a moiety of the balance of the proceeds of the Lyell Fund, to Mr. P. F. Kendall; and the remaining moiety to Mr. B. Harrison. The President delivered his annual address, the subject bearing on the Palæozoic Crustacea. The following is a list of the officers and Council elected at the meeting for the ensuing year. (The names in italics represent new officers and members of Council.) President: Dr. H. Woodward, F.R.S. Vice-Presidents: Prof. A. H. Green, F.R.S., *W. H. Hudleston*, F.R.S., R. Lydekker, F.R.S., *Lieut-General C. A. McMahon.* Secretaries: J. E. Marr, F.R.S., J. J. H. Teall, F.R.S. Foreign Secretary: J. W. Hulke, F.R.S. Treasurer: *Dr. W. T. Blanford*, F.R.S. Members of Council: H. Bauerman, Dr. W. T. Blanford, F.R.S., *Prof. W. Boyd Dawkins*, F.R.S., Sir John Evans, K.C.B., F.R.S., Prof. A. H. Green, F.R.S., Dr. J. W. Gregory, *R. S. Herries*, Dr. G. J. Hinde, T. V. Holmes, W. H. Hudleston, F.R.S., J. W. Hulke, F.R.S., Prof. J. W. Judd, F.R.S., R. Lydekker, F.R.S., Lieut.-General C. A. McMahon, J. E. Marr, F.R.S., *H. A. Miers*, *E. T. Newton*, F.R.S., F. Rutley, J. J. H. Teall, F.R.S., *W. Whitaker*, F.R.S., Rev. H. H. Winwood, Dr. H. Woodward, F.R.S., H. B. Woodward.

THE first scientific account of the Chilo-Argentine earthquake, which has reached Europe, is probably that written by M. A. F. Noguès (*Comptes rendus*, vol. cxx. 1895, pp. 167–170). The earthquake was remarkable for its intensity and long duration, the amplitude of the oscillations, and the absence of subterranean sounds. The epicentral zone is elliptical in form, its longer axis being directed nearly north and south, and passing by Rioja, San Juan, and Mendoza. The first two of these towns were severely damaged, and a large number of persons in them were killed and wounded. The boundary of the disturbed area is as yet undetermined, but it must have included a great part of the Argentine Republic and of the north of Chili. Within the epicentral area, the duration of the shock was fifty-five seconds; at Santiago Observatory, where it was registered by a seismograph, the duration was 1m. 40s., and the amplitude of the oscillations 2·5 c.m.

THE *Revue Scientifique* contains a full discussion of the causes of mountain sickness, by M. H. Kronecker, who was sent to investigate the conditions under which the proposed railway to the top of the Jungfrau could be worked without endangering human life. M. Kronecker and six other persons from Berne were carried from Zermatt to a point near the summit of the Breithorn, 3750 m. above sea-level. It was proposed to reach the summit itself, but it was impossible to proceed without additional carriers, the work being much more laborious at high altitudes. But at the altitude reached, all the symptoms of mountain sickness had shown themselves—acceleration of the pulse and of respiration, desire for rest, even after a very slight effort, and headache. M. Kronecker arrived at some interesting conclusions regarding mountain sickness. It sets in at altitudes varying with different persons. Beyond 3000 metres it attacks all persons as soon as they indulge in the least muscular effort, but children and very old people are much less subject to it than others. It also varies with the character of the mountains, being usually less serious on isolated peaks. Persons in good health can stand passive transport to about 4000 m. without inconvenience, but they should not remain more than two or three hours at the top. A prolonged sojourn may be disastrous in its effects upon health, as it proved to be in the case of Dr. Jacottet. For the purposes of the railway, M. Kronecker recommends that all guards and other officials should be carefully selected and, if possible, acclimatised or frequently changed between the stations. Finally, the summit station should be arranged so that no further ascent whatever is necessary to get the full benefit of the view.

AT a recent meeting of the Société Française de Physique, M. de Kowalski read a paper on the conditions necessary for the production of kathode rays. Starting from an experiment due to Goldstein, in which in a vacuum tube having a constriction at its middle, it is found that the kathode rays are formed not only at the negative electrode, but also at the constriction, the author has made several experiments, using tubes of different shapes. He finds that wherever the electric discharge is sufficiently dense, as near the electrodes or in a capillary tube joining the two parts of a vacuum tube, kathode rays are produced. These rays are propagated in straight lines, are deviated by a magnet, and produce a bright patch where they strike the glass. The author has also succeeded in obtaining kathode rays in a tube without electrodes. This tube had somewhat the shape of an elongated hour glass, and was placed alongside a discharger, through which "Tesla currents" were passed. Under these circumstances kathode rays are produced at either end of the capillary tube forming the central part of the vacuum tube. M. de Kowalski concludes from his experiments (1): that the production of kathode rays is not connected with the disintegration of metallic electrodes in a rarefied gas; (2) that these rays are produced wherever the density of the discharge is sufficiently great; (3) that the direction of the rays is the same as that of the lines of flow of the current at the point where they are produced, and that they are propagated in the opposite direction to that in which positive electricity is supposed to flow.

AT the same meeting at which the above paper was read, M. Curie described some experiments he had made to see whether light rays were deviated by a magnetic field in the same manner as kathode rays. He has obtained no deviation, although his apparatus permitted him to pass the light rays for a distance of 20 cm. in a field having an intensity of 14,000 units, the direction of propagation of the light being perpendicular to the lines of force of the magnetic field. The experiment was made in air, as well as in carbon bisulphide in which sulphur had been dissolved. The author, although he does not think the above experiments are conclusive proof that kathode rays are radiation of a different nature from that which constitutes light, yet thinks they tend to show that there is some difference. Furthermore, if the kathode rays are analogous to light rays, it is difficult to explain the absence of double refraction when a magnetic field acts on the kathode rays.

THE triple number, pp. 10–12, of the *Bulletin* of the Botanical Department, Jamaica, is entirely occupied by a list of the more interesting trees and shrubs, 109 in number, grown in the Botanic Garden at Castleton, near Kingston.

IN the most recent part of the *Records of the Botanical Survey of India*, the Indian Government publishes a report of a Botanical tour in Kashmir, by Mr. J. F. Duthie, director of the Botanical Department of Northern India, accompanied by a map.

THE *Bulletin* of the Royal Gardens, Kew, No. 96, for December, 1894, contains an interesting article on "Cultural Industries in Dominica," an island which, since the abandonment of the coffee plantations, which were at one time its staple industry, is very far from yielding the economical products which might be expected from its climate and the fertility of its soil. A brief account is given of the four Botanic Stations in the Leeward Islands, those of Antigua, St. Kitts, Dominica, and Montserrat.

THE 1895 *Annuaire* of the Royal Observatory at Brussels, edited by M. F. Folie, has been received. The *Annuaire* is is second only to that published by the French Bureau des Longitudes. It comprises ephemerides containing the principal astronomical data for the current year; geographical, meteorological, and other statistics; physical constants; and several articles, among them being three by M. Folie, on diurnal and annual aberration.

THE Smithsonian Report for 1893 has recently been issued. The report comprises a selection of miscellaneous memoirs embracing a considerable range of scientific investigation and discussion. This collection of reprints and translations, running into very nearly seven hundred pages, contains articles of interest to workers in all branches of science. Among the contributions to the Report is a monograph on "North American Bows, Arrows, and Quivers," by Dr. O. T. Mason. The paper, which is illustrated by fifty-seven plates, deals with the types of bows, arrows, and quivers of the North American aborigines, with incidental references to similar forms found elsewhere.

MESSRS. J. AND A. CHURCHILL have nearly ready the second volume of "Chemical Technology," edited by Mr. C. E. Groves, F.R.S., and Mr. William Thorp. This volume is devoted to lighting by candles and oils, and is illustrated by about 350 figures; the section on fats and oils being written by Mr. William Y. Dent; stearine, by Mr. John McArthur; candle manufacture, by Messrs. L. Field and M. A. Field; the petroleum industry, by Mr. Boverton Redwood; lamps, by Mr. Boverton Redwood; and miners' safety lamps, by Messrs. Boverton Redwood and D. A. Louis. The third volume of the same work, containing gas lighting, by Mr. Charles Hunt, of Birmingham, and electric lighting, by Prof. Garnett, of the Technical Board of the London County Council, is in a very forward state.

MESSRS. MACMILLAN AND CO. will publish early in March the treatise on "Bessel Functions and their Applications to Physics," by Profs. Gray and Mathews, which has already been announced. The work, after an introductory chapter on the problems which gave rise to the functions, deals with their properties, expansions involving them, semi-convergent expansions, Fourier-Bessel expansions, complex theory, definite integrals, and the relation of the functions to spherical harmonics. The physical applications embrace flow of heat and electricity, vibrations of membranes, hydrodynamics, electrical waves along wires, diffraction, and a number of miscellaneous problems. Numerical tables are appended, and also the Tables of Functions with imaginary argument, which have been prepared by Prof. A. Lodge for the British Association. A note containing useful formulæ for the calculation of the roots of Bessel Functions and others related to them, has been added from a manuscript placed at the disposal of the authors by Prof. J. McMahon.

WE have received Parts iv. and v. of *Indian Meteorological Memoirs* (vol. v.), containing the discussion of hourly observations made at Allahabad and Lucknow, forming a portion of the harmonic analysis of the observations recorded at twenty-five observatories in India, since the year 1873. The investigation is carried out in a most thorough manner, and there can be no doubt that when the work is complete, and the results correlated, much light will be thrown upon the laws which regulate atmospheric movements in those parts. In any case, the discussion will take rank amongst the most important of the kind hitherto undertaken by any country.

A COMPOUND of grape sugar with one of the acid radicle derivatives of hydrazine recently prepared by Herr Struve, a pupil of Prof. Curtius, is described by Dr. Wolff, of Berlin, in the current *Berichte* of the German Chemical Society, and, on account of its properties and mode of formation, appears likely to be of considerable service in the commercial extraction of pure dextrose from syrupy mixtures. It is first shown that the sugars of the aldose type react with the acidylhydrazides to produce an aldose-acidylhydrazide by direct addition with elimination of a molecule of water. Thus, in the case of dextrose and benzhydrazide the following equation represents the change:

$$C_6H_5.C : O.NH.NH_2 + O : C_6H_{12}O_5 = C_6H_5.C.O.NH.N.C_6H_{12}O_5 + H_2O.$$

When dextrose and benzhydrazide are digested with 96 per cent. alcohol for five or six hours in a flask provided with an upright condenser, the new compound is produced in solution in the alcohol, and upon subsequent evaporation it separates in the form of acicular crystals, which can readily be purified by recrystallisation from alcohol. The crystals melt at 171–172° with partial decomposition. As lævulose does not react with benzhydrazide, pure dextrose can readily be isolated from the mixture of lævulose and dextrose in ordinary invert sugar by utilising the above reaction. The invert sugar is evaporated to a thick syrup, and the latter digested with alcohol and benzhydrazide for about six hours in a reflux still. The alcoholic solution is then evaporated over a steam-bath almost to dryness, the lævulose is extracted by washing with a minimum of alcohol, and excess of benzhydrazide removed by means of ether. Pure dextrose-benzhydrazide is then obtained in good crystals by two recrystallisations from alcohol. The recovery of dextrose from this compound with benzhydrazide is a very simple matter, for the compound is immediately broken up by boiling water into dextrose and benzhydrazide. It is found convenient in practice, however, to remove one of the products of the dissociation, the benzhydrazide, by precipitation as the insoluble benzal-benzhydrazide by means of benzaldehyde, which is found to be a most valuable reagent for the purpose. After filtration the liquid is evaporated to dryness, dissolved in cold water, whereby any traces of unprecipitated benzal-benzhydrazide are left behind, and again evaporated to dryness. The last traces of impurities, chiefly benzaldehyde and benzoic acid, are finally removed by dissolving the dextrose in alcohol and precipitating with ether. The dextrose eventually obtained by evaporation of the clear solution is quite pure. Dr. Wolff lastly states that the above is a general process for separating aldoses from mixtures of sugars, and he is experimenting with a view to its adoption on the large scale.

THE additions to the Zoological Society's Gardens during the past week include a Snowy Owl (*Nyctea scandiaca*) from Norway, presented by Miss Wright; a Dunlin (*Tringa alpina*), British, purchased.

OUR ASTRONOMICAL COLUMN.

MARS IN 1894.—Signor G. Schiaparelli remarks, in No. 3271 of the *Astronomische Nachrichten*, that the unfavourable state of the atmosphere during the opposition of Mars last year rendered magnifications beyond 200 impossible except in rare instances. Speaking generally, he says that the "seas" were less pronounced than in 1877, and the "canals" were better visible in 1894, and seen in greater numbers. Some of the largest ones showed faint traces of doubling, but, with the magnifying powers used, nothing could be made out with certainty on this point. The southern pole cap became invisible in the 18-inch Milan refractor at the end of October. On October 8 it had already become very faint. The total disappearance cannot have been later than October 29, *i.e.* on the 59th day after the southern solstice of the planet. This is unusually early. In 1877–78 it was well seen as late as 98 days after the solstice. In the present case it is pretty certain that the whole of the southern pole cap was melted. A great change was also observed in the isthmus or peninsula of Hesperia, which separates the Mare Tyrrhenum from the Mare Cimmerium. It was apparently separated into two unequal portions by a newly formed channel. The Mare Sirenum, which in October, 1892, had been separated into two parts, was in October, 1894, seen to have resumed its ordinary aspect. But on November 21 the separation had reappeared. "This fact," says Schiaparelli, "and other analogous ones which I have observed in previous oppositions, lead to the conclusion that the abnormal changes in the markings of Mars do not take place by chance and without regularity, but that the same variation may reappear, with the same aspect, even after a long interval of time. The form and extent of such changes is determined by some element which is stable, or at least periodic."

NOVEL METHODS IN PHOTOMETRY.—The determination of the times of exposure of a photographic plate which are required to produce the same density of film when exposed to different light sources, forms the basis of the methods recently adopted by Dr. Janssen for investigating the brightnesses of the heavenly bodies (*Bull. Mens. Soc. Ast. de France*, February). In the case of stars, the plate is placed a little within the principal focus of a telescope, so that a disc, or "stellar circle," replaces the almost point-like image ordinarily obtained; a series of exposures is made on one star, and another series on the star to be compared with it; the two images of the same density are thus identified, and the photographic brightnesses of the two stars are inversely as the durations of the corresponding exposures. To compare the light of a star with the sun, an opaque screen, pierced with holes of the same size as the stellar circles, is placed in front of the photographic plate, and these holes admit sunlight to the plate at the moment a triangular aperture in another metal plate is passed over them on releasing a spring; in this way a series of circles of increasing intensity is impressed on the plate, and can be compared directly with the stellar circles.

In its application to nebulæ, Dr. Janssen's method promises to be of great value. On the same plate which has been exposed to a nebula, a series of "stellar circles" is formed by directing the instrument to a star in the neighbourhood which shows no signs of variability. In the future, when one wishes to obtain a photograph of the nebula which will be strictly comparable to one taken previously, it will only be necessary by means of stellar circles to determine the exact exposure which should be given.

From an inquiry into the photographic luminosity of the tail of comet *b* 1881, Dr. Janssen finds that the intensity decreased in a ratio between the fourth and sixth power of the distance from the nucleus.

ATMOSPHERIC DISPERSION.—The fact that the image of a star as seen in a telescope is drawn out into a short vertical spectrum, with the red end uppermost, was noticed as long ago as 1729 by Bouguer, and the effect of the difference in colours of stars upon refraction appears to have been indicated in a general way by Lee in 1815. Even in small instruments this atmospheric spectrum is very noticeable in the case of bright stars at low altitudes, but, if necessary, it can be corrected by means of a thin prism placed in front of the eyepiece, or by employing an eyepiece of the form devised by the late Sir George Airy. In a recent paper (*Monthly Notices R.A.S.*, January), Dr. Rambaut points out the importance, in these days of extreme accuracy, of introducing a correction for atmospheric dispersion—according to the varying colours of stars—more especially in connection with observations of two stars in close proximity, as in measures of double stars, and observations for parallax. The claims of this hitherto rather neglected factor appear to be fully substantiated by a series of measures at different hour angles of β Cygni, in which double star the colours of the components are strongly contrasted; they "show clearly a systematic difference affecting the distance between them, of the sort, and in the direction, that theoretical considerations indicate." Dr. Rambaut also shows that the systematic differences depending upon hour angle in the measures for the parallax of α Centauri by Drs. Gill and Elkin, which they corrected by empirical formulæ, are due to a difference in the mean refrangibility of the light of the star and of the comparison stars. Further confirmation is derived from a re-discussion of the Dunsink observations on the parallax of 61 Cygni, and the resulting value is corrected from 0″·465 to 0″·400.

An ingenious method of measuring atmospheric dispersion has been devised by M. Prosper Henry, and values determined for different colours (NATURE, vol. xliii. p. 400).

THE SUN'S PLACE IN NATURE.[1]

I.

I AM anxious to give in these lectures a statement, as clearly and as judicially as I can, of the discussions which have been going on since these results were published, to show what holes have been picked in the new views, and what new truths may be gathered from the new work which has now been brought to bear upon the old, so that as a result the place I have given to the sun among its fellow stars may be justified or withdrawn. These lectures will be different from the former ones, inasmuch as I then attempted to give you a piece of quiet history of several regions of fact and knowledge which had been well surveyed and mapped, and had become part and parcel of the common property of mankind. But now I shall have, in considering the discussion, rather to take you with me into the forefront of those who are fighting the battles on the confines of the unknown. I have to bring you news from the front, something like that which we are promised to-morrow or the next day from Port Arthur. I have to show how the battle is waging, who has lost, what positions have been occupied, and what things new and true and beautiful have been wrested from the unknown region; and I am the more anxious to do that because it enables me to bring before you the enormous advantages under which such work is now carried on; advantages in that now, when any question is put to any part of the heavens, we know that there are many good workers employed under the best possible conditions to get the particular information that we want; besides these advantages, in every branch of inquiry we find advances gigantic, marvellous, almost beyond belief.

I am sorry to say that in this work the centre of gravity of the activity has left our country and has gone out West. We have to look to our American cousins for a great deal that we want to know in these matters, for the reason that now they not only have the biggest telescopes, and most skilled observers, but also they have been wiser than we—they have occupied high points on the earth's surface, and thus got rid of the atmospheric difficulties under which we suffer in England, and especially in London.

[1] Revised from shorthand notes of a course of lectures to working men at the Museum of Practical Geology during November and December, 1894. (Continued from page 377.)

Let me bring before you one of the most perfect pieces of workmanship in the world constructed to investigate the phenomena of the heavens. It is a photograph of the Lick Observatory, situated at an elevation of 4000 feet on Mount Hamilton. Mr. Lick, the founder, was a very ambitious man. He was, I believe, an hotel-keeper at San Francisco, but however that may be, he has made his name immortal by helping on the progress of mankind. I wish we had some hotels like the San Francisco hotel in this country, and some Mr. Licks, because then some Englishman might immortalise himself in the same way. This, then, is the magnificent locality in which a great deal of the work that I shall have to refer to has been done. The principal instrument of this great Observatory is a refracting telescope having an object-glass three feet in diameter, and a tube fifty-four feet in length. This is practically the most important telescope in the world at the present moment, and to give you an idea of the wonderfully

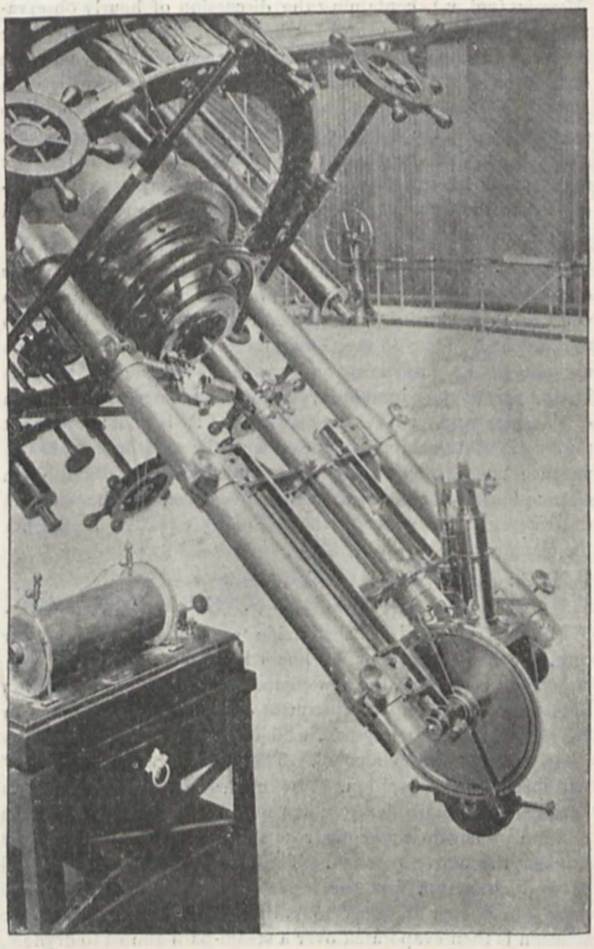


FIG. 4.—Spectroscope attached to the eye-end of the Lick Telescope.

broad way in which the authorities have gone to work, I need only state the following fact. Some of you who have been in an observatory may remember that it has sometimes been very difficult to get the observatory chair at the right height, or in the right position, for observing a star or any celestial body with any comfort. The Americans get over this by simply raising the floor. By means of hydraulics the enormous floor, some 80 feet in diameter, is moved up and down with the chair. The importance of spectroscopic work has not been lost sight of in the equipment of the Observatory, and a very powerful spectroscope can be used in conjunction with the great equatorial for observing or photographing the spectra of the various celestial bodies (Fig. 4).

One of the most important telescopes in England at present is Dr. Roberts' reflector, with which several majestic representations of the heavenly bodies have been produced; these I shall have to show you at one time or another in relation to different branches of our subject. In this instrument (Fig. 5) a reflecting telescope of 20 inches aperture is combined with a refractor of 7 inches aperture. The refractor is used as a guiding telescope, and ensures that the images of the stars and nebulæ fall on the same part of the photographic plate which is being exposed in the reflecting telescope throughout the whole

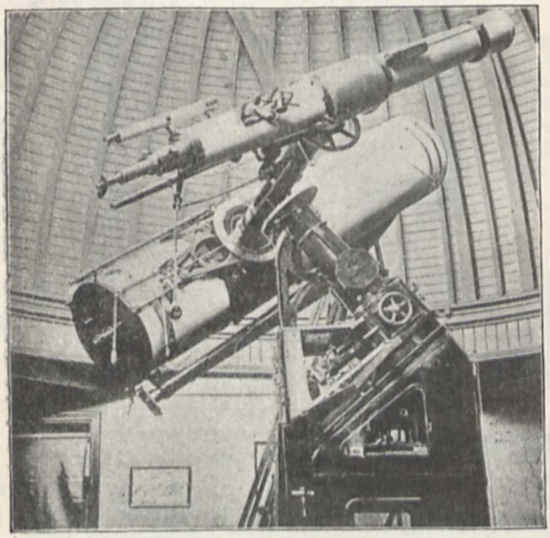


FIG. 5.—Dr. Roberts' twin telescope.

time of the exposure. Even with the best driving clocks, such a guiding telescope cannot be dispensed with when the exposures are prolonged for the number of hours necessary in some cases.

First for the nebulæ. What is the difference, written down in this way, between a nebula and a star cluster? A comparison of Figs. 6 and 7 will at once show that in the case of a star cluster we have to deal with a collection of separate stars, while in the case of a nebula there is a filmy sort of

FIG. 6.—The Cluster 15 M Libræ, from a photograph by Dr. Roberts.

luminosity which is quite distinct from the neighbouring stars. Here and there the nebulosity is suddenly brightened, but, as I shall show hereafter, these condensations are not to be regarded as stars in the ordinary sense. Here is the nebula of Orion, which we owe to that wonderful telescope of Dr. Roberts. Several of you may have seen the nebula of Orion with a telescope, but you have never seen it exactly like this. You get here the idea which gave rise to the old notion of a candle shining through horn; this nebula is the one which, on account of its brightness, spectroscopically gives us most easily indications of those bright lines which for ever set at rest the idea that we are dealing with solid or liquid bodies.

At the beginning of the present century it was found that in

FIG. 7.—The spiral nebula in Canes Venatici, from a photograph by Dr. Roberts.

order to get the spectra of stars the best thing to do was to put a prism outside the telescope, and to let the light enter the telescope and be brought to a focus after it had passed through the prism; and it is a most unfortunate thing, that the

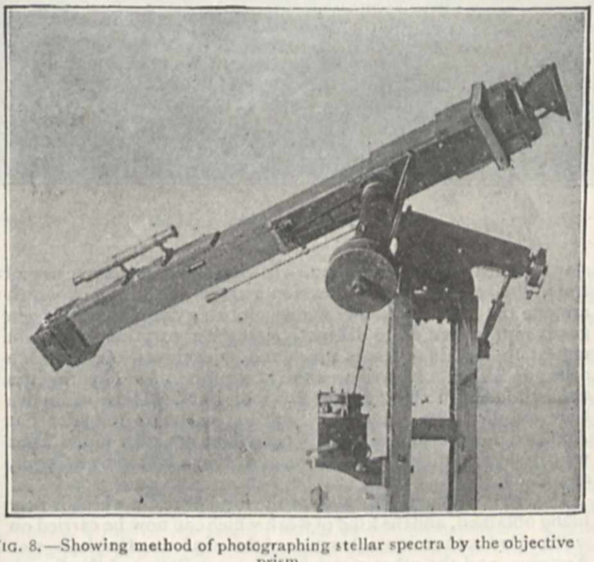


FIG. 8.—Showing method of photographing stellar spectra by the objective prism.

neglect of the application of this principle has landed us probably in a delay of fifteen or twenty years in gathering knowledge on this subject. The whole credit of reviving this idea is due to Prof. Pickering, of the Harvard College, who since its application has been able, with the aid of the large funds that he has at his disposal, and the magnificent help which he has accumulated round him, to obtain practically the spectra of all the stars down to the fifth or sixth magnitude in both hemispheres. In a few years' time we shall be able to work on the spectra of all the stars in both hemispheres, just as well as we can at present deal with their magnitudes and positions by the star charts.

An instrument of this kind (Figs. 8 and 9), having an aperture of only 6 inches, has been in use at Kensington for some time, and some of the results which have been obtained by its aid are shown in Figs. 3, 10, and 11. They are absolutely untouched photographs.

Without going into minute differences, we can, and, if we are wise, we shall deal first of all with the larger differences presented by the various classes of stars which people space. Here we have photographs of stars of different classes (Fig. 3). You will understand from these photographs how perfectly justified Rutherfurd and others have been in attempting to classify the

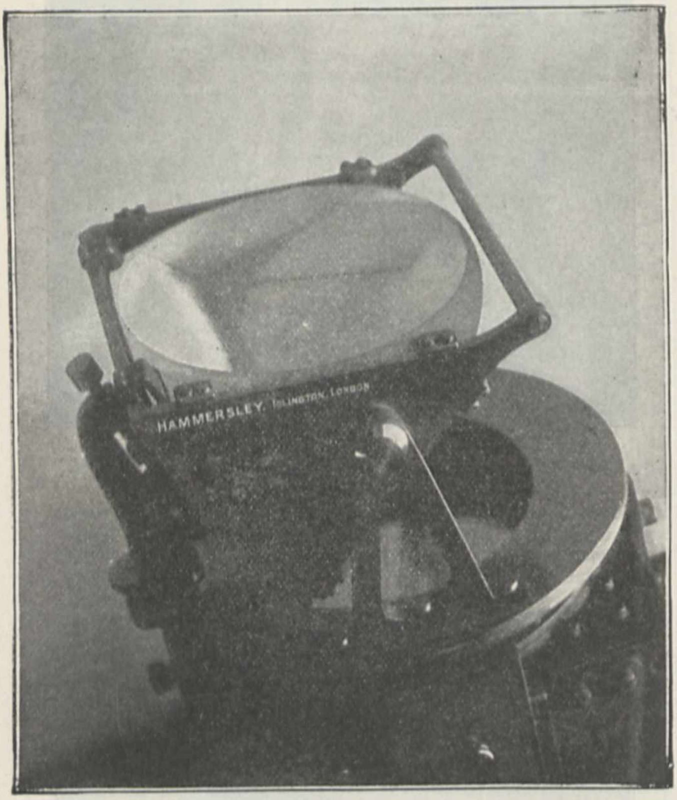


FIG. 9.—Details of objective prism.

stars by means of their spectra. In Sirius we get one class of stars, distinguished by the development of certain lines, which are due to the absorption of hydrogen. In α Cygni the hydrogen gas is represented quite distinctly, but the absorption there with regard to certain lines is much more developed than in such a star as Sirius. In Arcturus the absorption of the hydrogen is almost hidden in an enormous mass of lines. Here again we have another class, and it is not too early to remark that Arcturus in its spectrum exactly resembles our own sun. Thus we can say the sun is like Arcturus, not like α Herculis, α Cygni, and so on.

Fig. 10 is an exemplification of the kind of result which is now being obtained, and the kind of work which can now be carried on with regard to the minute structure of these spectra. One is the star Arcturus, and the other a star in the constellation of the Swan. You see at once that, if it is a question of attempting to determine whether stars are like each other, or whether they are unlike each other, and the points in which they differ, with the resources of modern science at the present moment—small

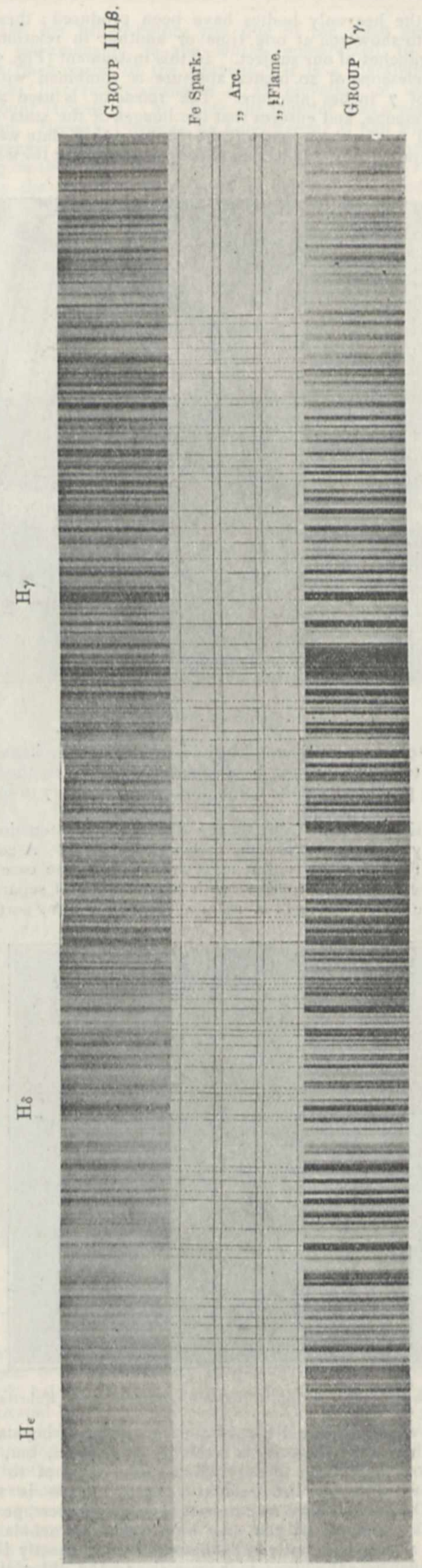


FIG. 10.—Spectrum of γ Cygni (1) compared with that of Arcturus (2).—From photographs taken at Kensington.

as those resources are in some instances—we have really an opportunity of doing a considerable amount of work. The particular bit of work which is represented in this diagram is an inquiry showing how the iron lines observed in the spectra are represented in these different stars; we can see whether the condition of the iron vapour is the same in a star like Arcturus as it is in a star like γ Cygni. You will see that from this point of view there is a great difference in the atmospheres of these two stars.

The definition of the negatives obtained by means of the objective prism is of such excellence that they may be almost indefinitely enlarged, and this gives them a special value when we come to investigate the smaller differences between stars which have more or less resemblance to each other. Practically, we are able to dispense with elaborate micrometric measurements, and by placing the enlargements alongside each other, to see at a glance which lines agree in position and which are different.

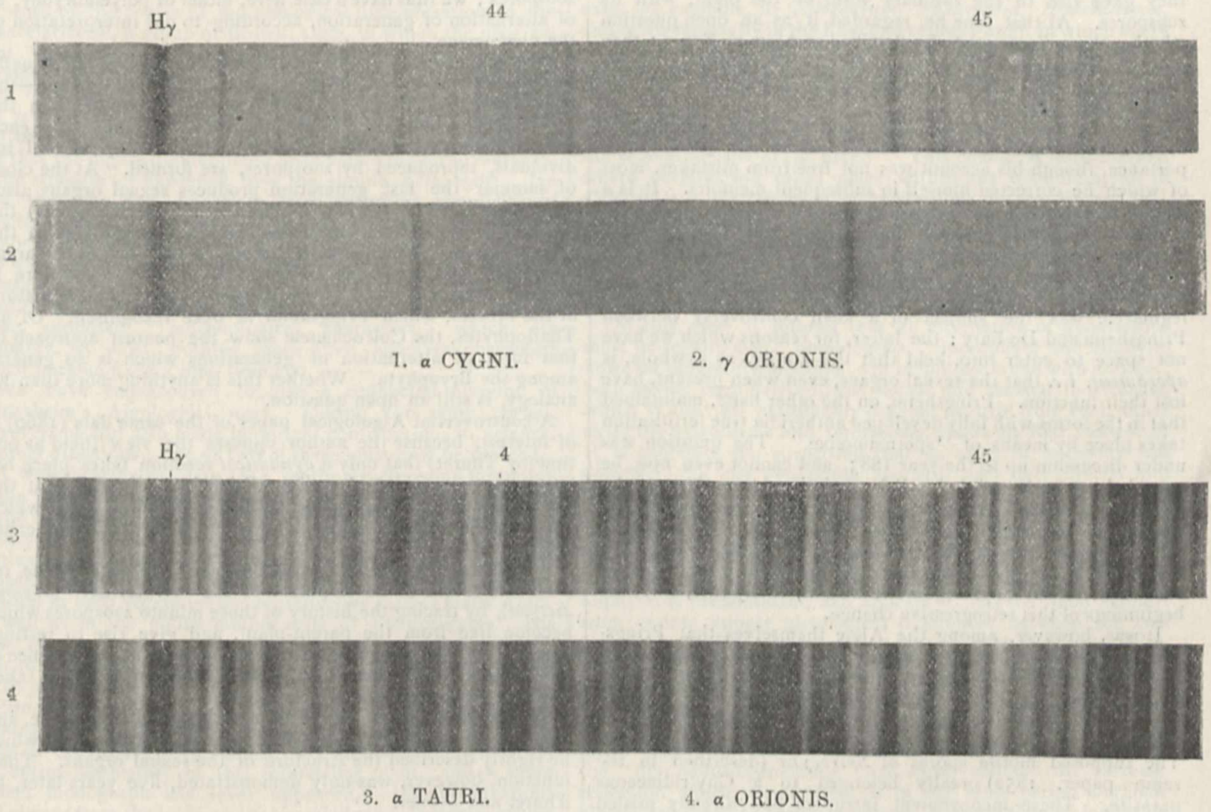


FIG. 11.—Portions of stellar spectra, greatly enlarged.

The spectra represented in Fig. 11 have been enlarged twelve times from the original negatives, and on this scale the whole of the visible spectrum would be more than a yard in length. Here we observe, in the first place, the immense difference between two groups of stars, one in which the lines are very numerous, the other in which the number of lines is relatively small. We also have an opportunity of studying the more minute differences between stars like α Tauri and those like α Orionis, and between stars like α Cygni and those like γ Orionis. In some cases, differences which might easily be overlooked altogether in an examination of the negatives alone, become apparent when the scale is enlarged in this way.

Thus, by means of the aids which have been placed at our disposal, by the recent improved condition of our stock in trade, and the wonderful diligence and skill of observers, chiefly in America, who have taken up the new work, we are now in a very much better position than we have ever been before to investigate this subject. J. NORMAN LOCKYER.

(*To be continued.*)

NATHANAEL PRINGSHEIM.

BOTANISTS throughout the world will have heard, with deep regret, of the death of Prof. Pringsheim on October 6, of last year. His name is inseparably associated with the modern progress of the science, and there must be many, who, like the writer of this notice, can trace their first interest in scientific botany, in no small degree, to the fascination of Pringsheim's discoveries. Pringsheim was born in Silesia, in 1823. His career, though an active one, was unusually free from official cares. Except during four years, when he was Professor at Jena, he does not appear to have held any teaching post of importance. During the greater part of his scientific life, his work was carried on in a private laboratory, founded by himself at Berlin, and devoted entirely to the researches of original workers.

Pringsheim was founder and editor, from 1858 to the time of his death, of the famous *Jahrbücher für Wissenschaftliche Botanik*, and was President of the German Botanical Society from its first origin. He was among the many distinguished foreign men of science who attended the meeting of the British Association at Manchester, in 1887.

The scientific activity of Pringsheim extended over a period of fully forty years (1848–1888), a time which covers what was perhaps the most brilliant epoch in the history of botany. His work began in the days when the science of histology was being built up on the basis of the cell-theory of Schleiden and Schwann; he himself contributed essentially to its construction. Pringsheim's greatest services to science, however, were in the department of the morphology and life-history of the lower plants, a line of research in which he was unsurpassed. Comparatively late in life his attention became directed to physiology, but in this direction his success was less conspicuous.

Pringsheim's earliest contribution to science was his Latin dissertation, "De formâ et incremento stratorum crassiorum in plantarum cellulâ" (Linnæa, 1848), in which he discusses a question much agitated at that time, whether the increase in thickness of the cell-wall takes place from without or from within. He decides, chiefly from observations on the cells of the testa in seeds, in favour of apposition from the interior, a conclusion which no doubt holds good for the great majority of cases, though the researches of Strasburger have shown that this is not a universal rule.

Not long afterwards, Pringsheim made his first investigation of a question which continued to occupy him, at intervals, for more than thirty years, namely, the development of the Saprolegniaceæ, a family which lies on the confines of the Algæ and the Fungi. At that time the delimitation of genera and species among these plants had fallen into great confusion. Pringsheim's memoir of 1851 bears the title, "On the development of *Achlya prolifera*," but its subject was really a *Saprolegnia*. The zoospores of this family had long been known, and their resting-spores had already been observed by Schleiden and Nägeli. Pringsheim, however, was the first to prove that the two kinds of spore are produced by the same species; he watched the germination of the resting-spores, and saw that they gave rise to the ordinary form of the plant, with its zoospores. At that time he regarded it as an open question whether these plants are to be classified as Algæ or Fungi; they are now assigned to the latter class, but placed near its lower limit, where there has not been much divergence from the original algal stock.

Six years later, Pringsheim discovered the sexual reproduction of the Saprolegniaceæ, an observation of the greatest importance, though his account was not free from mistakes, most of which he corrected himself in subsequent memoirs. It is a remarkable fact that in certain species of the family some individuals are entirely without male organs, and produce their oöspores parthenogenetically. In others the antheridia are present, but there is no penetration of the oögonium. The question whether actual fertilisation takes place in *any* Saprolegniaceæ was the subject of a keen controversy between Pringsheim and De Bary; the latter, for reasons which we have not space to enter into, held that the family, as a whole, is *apogamous*, *i.e.* that the sexual organs, even when present, have lost their function. Pringsheim, on the other hand, maintained that in the forms with fully developed antheridia true fertilisation takes place by means of "spermamœbæ." The question was under discussion up to the year 1883, and cannot even now be regarded as finally settled. Pringsheim's observations of the supposed amœboid male cells are not very convincing, but neither is the negative evidence against the occurrence of fertilisation in the family decisive. The higher Fungi are characterised, as most botanists now agree, by a total loss of sexuality; in the Saprolegniaceæ we can, at any rate, trace the beginnings of this retrogressive change.

It was, however, among the Algæ themselves that Pringsheim's chief triumphs were won. His earliest purely Algological paper was on the germination of the resting-spores of *Spirogyra*, in which he first demonstrated the interesting fact that the embryonic plant presents a distinction of apex and base, which is quite lost in the later stages of its growth. The supposed motile spores of *Spirogyra* (described in the same paper, 1852) really belonged to a Chytridiaceous parasite. These inconvenient intruders had already misled Pringsheim in some of his earlier observations on Saprolegniaceæ. In the same year, he made out the reproduction in an interesting little Alga (*Cœlastrum*) allied to the "Water-net."

Pringsheim's really fundamental investigations of the Algæ begin, however, with the year 1855, when he communicated to the Berlin Academy a memoir on the fertilisation and germination of the Algæ, and on the nature of sexuality. The chief point of this paper is the demonstration of the sexual reproduction of *Vaucheria*. Vaucher himself had discovered the "anthers" at the beginning of the century; it was reserved for Pringsheim to find the spermatozoids, and to observe the act of fertilisation. The proof of such an advanced mode of reproduction in an unicellular (or rather non-cellular) plant, was a startling discovery, and provoked some opposition among those who had not the skill to repeat Pringsheim's observations. This paper further contains researches on the fertilisation of *Fucus*, and on the sexual organs of Florideæ (which of course was only fully understood at a later time), and preliminary observations on the remarkable life-history of *Œdogonium* and *Bulbochæte*.

These latter discoveries were more fully announced in the next year (1856), and two years later the whole history was told in a magnificent memoir, published in the first number of the *Jahrbücher für Wissenschaftliche Botanik*. This work is a model for all such investigations, and has left very little for subsequent observers to do, as regards these plants, which in some respects present unique peculiarities. The Œdogoniaceæ possess (in addition to the asexual zoospores) highly differentiated ova and spermatozoids. Many species are monœcious, and a few diœcious; most, however, present a remarkable form of dimorphism. Certain cells produce ova; others (usually in the same plant) give rise to small zoospores, which become free, attach themselves to the oögonia, or to adjacent cells, and there germinate into dwarf male plants. Each of these dwarfs consists of a single vegetative cell and an antheridium, in which two or more spermatozoids are produced, by which fertilisation is effected.

Another interesting point discovered by Pringsheim, is the germination of the sexually produced resting-spores, which do not grow directly into new plants, but give rise, each, to four zoospores; we thus have a case here, either of polyembryony, or of alternation of generation, according to our interpretation of the phenomena.

Two years later (in 1860), Pringsheim published an equally important memoir on another group of freshwater Algæ—the Coleochæteæ. These curious little plants, which grow on the larger water-weeds, go through one cycle of development in each year. During the earlier part of the season, only asexual individuals, reproduced by zoospores, are formed. At the close of summer the last generation produces sexual organs also. After fertilisation (which is effected by spermatozoids) the oöspore passes into its winter rest, and then germinates in the spring, giving rise, not to a normal plant, but to a parenchymatous, fruit-like body, in each cell of which a zoospore is formed. There is thus an evident analogy with the life history of the Mosses, which Pringsheim at once recognised. Of all Thallophytes, the Coleochæteæ show the nearest approach to that form of alternation of generations which is so general among the Bryophyta. Whether this is anything more than an analogy, is still an open question.

A controversial Algological paper of the same date (1860) is of interest, because the author opposes the view (held at one time by Thuret) that only a *dynamical* reaction takes place between spermatozoid and ovum. Pringsheim showed that the two sexual cells undergo an actual material fusion, a fact which lies at the root of all sound views as to the nature of the sexual process.

A year later, Pringsheim added to our knowledge of the remarkable course of development of the "Water-net" (*Hydrodictyon*), by tracing the history of those minute zoospores which become free from the parent-plant, and give rise to resting-spores. In this case, however, he overlooked the occurrence of conjugation, which, according to a subsequent observer, takes place between these small motile cells.

In 1862, Pringsheim turned his attention to marine Algæ, and published some observations on the Red Seaweeds, in which he rightly described the structure of the sexual organs. Their function, however, was only demonstrated, five years later, by Thuret and Bornet.

To the year 1863 belongs an important memoir on the Characeæ, a group of plants, which, though so popular as text-book types, is still a mystery as regards its relationships. Pringsheim's observations were chiefly in the proembryo and allied structures. He established the existence of a striking correspondence between these organs and the protonema of the Mosses.

After an interval of six years, he returned, in 1869, to the investigation of the Algæ, and made known perhaps the most important of all his Algological discoveries, that of the conjugation of zoospores, a process which he regarded as the primitive form of sexual reproduction in plants. This striking discovery was first made in *Pandorina*, one of the Volvocineæ, a family which is often claimed by the zoologists, but which, in its reproductive phenomena, betrays the closest affinity to undoubted plants. The motile cells, which conjugate in pairs, sometimes differ considerably in size, while in other cases they are almost exactly similar. The product of their union is a resting-spore, which on germination ultimately, though not directly, gives rise to a new colony. Pringsheim observed the details of the fusion of the sexual cells, and found that they first become united by their anterior ciliated ends, which consist of clear protoplasm. He compared this part of the conjugating cell to the "receptive spot" of the ovum in the higher Algæ, to the canal-cells of archegoniate plants, and to the synergidæ of Angiosperms.

The conjugation of motile cells has since been observed in many other families of Algæ, of the most diverse affinities, and in several of these families we can now trace the advance from this primitive fusion of like cells, to the union of a differentiated ovum and spermatozoid. Pringsheim's discovery has thus proved to have all the far-reaching significance which his scientific insight attributed to it, from the first. It has now become customary to speak of active conjugating cells as "planogametæ," a word which is useful, as indicating their sexual function, but which must not be allowed to disguise their complete homology with the asexual zoospores.

In 1871, he discovered the male plants, and the presumable sexual cells of *Bryopsis*, a marine Alga, allied to *Vaucheria*; the process of fertilisation however, has not yet been observed.

Pringsheim's latest contribution to Algology was his interesting work on the course of morphological differentiation in the Sphacelariaceæ, a group of Brown Algæ, in which the progress from filamentous to cormophytic structure can be traced through a very complete series. His views on evolution were akin to those of Nägeli (*cf.* NATURE, vol. xliv. p. 582). He saw clearly the evidence of descent, but could not admit that natural selection is a sufficient explanation. It seemed to him that the highly differentiated *Sphacelaria* has no advantages, in the struggle for existence, over the simple *Ectocarpus*. He held that the first deviations from primitive simplicity of structure, are of a "purely morphological nature," and have no demonstrable relations to any physiological functions.

Views of this kind are widely spread among German naturalists. The greater, however, our knowledge of the conditions of life in plants becomes, the less room is left for these supposed "morphological characters," which are probably, at most, nothing more than adaptive characters which became fixed a long time ago.

We have endeavoured to give a connected account of Pringsheim's Algological work. During the same period, however, he had also made important contributions to science, in other directions; one or two of these must now be noticed.

A work bearing the date 1854, on the structure and formation of the vegetable cell, belongs to the period when the doctrine of the multiplication of cells by division had just gained the victory over the erroneous theory of free-cell-formation, so pertinaciously defended by Schleiden. Pringsheim's researches afforded valuable support to the new views. He observed cell-division in both vegetative and reproductive cells of many Algæ, as well as in the pollen-mother-cells of Phanerogams. His view of the formation of the cell-wall comes wonderfully near to the conception of this process, which we have attained at the present day.

Pringsheim's work on the morphology and development of *Salvinia* (1863) is his most important contribution to our knowledge of the higher Cryptogams. He completely worked out the entire life-history of this interesting plant, and a more perfect monograph has never appeared. His observations will always form the basis of our knowledge of *Salvinia*, which is one of the most highly modified forms of Pteridophyta; they also throw great light on the embryology and development of the class as a whole.

Pringsheim, in 1876, crowned his long series of morphological investigations by a remarkable essay on the alternation of generations in the Thallophytes, and its relation to that in the Mosses. In the opinion of the writer of this article, this is, from a theoretical point of view, his most important work.

The essay was suggested by the experiments of its author on the sprouting of Moss-fruits. These experiments were undertaken by Pringsheim with the express object of determining whether the Moss-seta could be induced to subserve vegetative propagation in the same way as the Moss-stem, which it so closely resembles anatomically. He found that the seta of *Bryum* and *Hypnum*, when divided, and kept in moist air, developed a normal protonema, from which true Moss-plants arose. In other words, he succeeded in producing the sexual from the asexual generation, without the intervention of spores, thus establishing the first case of *apospory*, our knowledge of which has since been so much extended. In the meantime the converse phenomenon of *apogamy* had been demonstrated by De Bary and Farlow, who showed that the asexual generation, in certain Ferns, may arise from the sexual prothallus, without the intervention of the sexual organs. These facts really formed the groundwork of Pringsheim's theory of alternation.

His leading idea is that the successive generations among the Thallophytes are to be sought in the free sexual and asexual forms of the plant, and not in the plant itself on the one hand, and its fruit on the other. To take only one example: in the case of the Florideæ, Pringsheim regarded the tetrasporic and the sexual individuals as constituting the alternate generations; the sexually produced crystocarp he interpreted, not as a distinct generation, but as a case of polyembryony. Having expanded this view, he proceeds to face the difficulty, how it is to be reconciled with the life-history of the Mosses (in the widest sense), in which the alternation is between vegetative plant and fruit.

His reply is very ingenious: in the germination of the sexually produced spore of many Thallophytes the young plant suppresses more or less completely its vegetative stage, and proceeds at once to the formation of asexual spores; this is the case, for example, in *Œdogonium*, *Sphæroplea*, *Hydrodictyon*, and more especially in the Phycomycetous Fungi, in which every transition can be traced between "sporangial" and "mycelial" germination of the sexually produced spore. Pringsheim goes on to say: "It may therefore hold good, as a general experience among Thallophytes, that the *first* neutral generation hurries by a short road to spore-formation, suppressing more or less the vegetative part of the plant." This change is most marked where germination takes place within the oögonium, as in *Coleochæte*. "Alternation of generations in the Mosses thus appears as a contracted form of that in Thallophytes; in the former the neutral generations are reduced to a single one, which remains in unbroken connection with the sexual plant. There is therefore no reason for comparing the sporogonium of a Moss with the fruits of the Thallophytes," and so on.

On this view it follows that all alternation of generations must be regarded as "homologous." Sporophyte and oöphyte, however differently modified, are homologous one with another, both having been derived from sexual and asexual individuals, which at one time presumably differed as little from each other as do the cystocarpic and tetrasporic plants in Red Seaweeds. It is therefore not surprising that in cases of apogamy and apospory, the one generation may still pass over directly into the other.

These views have been much criticised, and the case is still *sub judice*. In England the opposite theory—that the alternation in the higher Cryptogams is "antithetic," the sporophyte being an intercalated generation, not homologous with the oöphyte, is predominant, and appears to receive some support from certain minute histological differences between the two generations. Pringsheim's interpretation of the facts has, however, some advantages, perhaps the most conspicuous among which is that it would enable us to understand the existence of the immense and unbridged gulf which separates the sporophyte of the Muscineæ from that of the Vascular Cryptogams. The latter might well have been derived from ancestors, in which the "first neutral generation" had never suffered the extreme reduction which characterise it in the Moss series, but had always retained its vegetative organs. It would then no longer be necessary to endeavour to force the plant of a Fern or a Horsetail to fit into the Procrustean limits of a Moss-sporogonium.

We have endeavoured to give some idea of Pringsheim's varied activity in the field of morphology, which he cultivated with unsurpassed success. It remains to say a few words regarding his physiological investigations, though these are far from possessing the same importance.

So far as we have been able to find, Pringsheim's first entry upon the physiological domain dates from the year 1875, when he published papers on the spectrum of chlorophyll, and on the modifications of that pigment. His *début* as a physiologist thus almost exactly coincides with the climax of his brilliant career as a morphologist. It was not till 1879, however, that he became prominent in physiological questions. In that year he produced his first paper on the action of light and the function of chlorophyll. He investigated the action of light of great intensity on vegetable tissues, and observed the consequent destruction of the chlorophyll, in green tissues, and the disorganisation of the protoplasmic structures, both of which he attributed to excessive oxidation. The great conclusion at which he arrived, is that "the function of chlorophyll, by means of its powerful absorption, especially of the so-called chemical rays, is to limit the intensity of respiration, and to serve as a regulator of that process." He then proceeds further, and states that "the fact that only *green* parts of plants evolve oxygen, finds its sufficient explanation in the diminution of the amount of respiration, owing to the presence of the chlorophyll screen." Pringsheim thus regarded assimilation as a function of the protoplasm alone, with which the chlorophyll has nothing to do, except in a purely negative way, by keeping off rays which would induce the opposite process of oxidation.

In the same year, Pringsheim announced his discovery of a body which he called Hypochlorin. This he was able to demonstrate in the chlorophyll corpuscles by the aid of certain reagents, notably dilute hydrochloric acid. He regarded hypochlorin as the first and most constant product of assimilation.

Both of Pringsheim's conclusions were sharply attacked, and an extensive controversial literature soon grew up. As regards the hypochlorin, it seems certain that Pringsheim was mistaken in the importance he attributed to this body. It has been clearly shown by Arthur Mayer, and others, that hypochlorin is not a product of assimilation, but is derived from the chlorophyll itself, by the action of the reagents employed.

The other theory, that of the screen-action of chlorophyll, has not met with much favour from physiologists, and it is clear that this is, at any rate, not its only function. The investigations of Timiriazeff and Engelmann have proved that certain of the rays absorbed by the chlorophyll (principally those in the red) are just the rays most active in the decomposition of carbon dioxide. The chlorophyll, in fact, is not merely a screen, but is also a light-trap, which catches and detains those rays which are most effective in the assimilative work of the plasted. On the other hand, recent investigations as to the action of light on protoplasm have shown that many pigments (such as those of spores and pollen-grains) are useful as screens, and have rendered it probable that this may be a function (though not the most important one) of chlorophyll also. The discovery, by Winogradsky, that certain Bacteria can assimilate carbon from inorganic carbonates, without chlorophyll, and in the absence of light, is a striking confirmation of the view that the seat of assimilation is the protoplasm itself, and to this extent Pringsheim's opinion is completely justified.

It would be unfair to deny considerable credit to Pringsheim for the boldness and originality of his physiological theories, and the energy with which he supported them. Yet it must be admitted that his views on these subjects were one-sided, and not characterised by the same sober judgment which distinguishes his morphological researches. Entering, in mature life, upon an unaccustomed field of investigation, he failed to add greatly to his previous reputation; and though much of his physiological work is suggestive, it has not given us, as he intended it should, a consistent theory of assimilation.

Passing over some of the physiological papers of secondary importance, we come to Pringsheim's last work (1888), which treats of the origin of calcareous incrustations on water-plants. He explains this process by the removal of carbon dioxide from calcium bicarbonate during assimilation, causing the precipitation of the insoluble calcium carbonate. This theory is supported by the interesting observation that the incrustation only takes place in the light; but it may be doubted whether the explanation given is sufficient.

We have not attempted to record all Pringsheim's researches, but enough has perhaps been said to give some idea of his life's work.

In addition to his original investigations, Pringsheim rendered a great service to science by means of his magnificent Year-book for Scientific Botany, which shares, with the botanical portion of the French *Annales des Sciences Naturelles*, the honour of constituting the finest serial record of morphological and physiological botany. It is satisfactory to hear that, since the death of the founder, the editorship of this great publication has been undertaken by two such distinguished botanists as Profs. Strasburger and Pfeffer.

By the death of Pringsheim we have lost another of the leaders who guided scientific botany through the period of its most vigorous growth. Very few of his generation now remain; they have left worthy successors behind them, but the work of Pringsheim, in the field of morphology, will not soon be rivalled.

D. H. SCOTT.

THE ANTITOXIC SERUM TREATMENT OF DIPHTHERIA.[1]

THE subject with which we shall deal to-night, though at first sight of interest to the physician only, has been so fully discussed in the public prints, and has been so bitterly and irrationally opposed on the one hand (perhaps also unreasonably beplauded on the other), that those who take even a general interest in the public health, or who are wishful to obtain some insight into the practical and scientific aspects of a new system of treatment, may well be interested to know something of what is being so freely discussed in the columns of our daily newspapers. Beyond this, however, many take a more personal interest in a method of treatment which holds out promise of help in the cure or amelioration of the symptoms and conditions met with in diphtheria a disease which, very justly, is looked upon as one of the most dangerous with which the physician has to deal. To begin with, I should like to make a frank confession. With that conservatism which is met with even in the most radical of natures, many, of whom I was one, felt disposed to treat antitoxic serum as belonging to the same group of substances as tuberculin, around which was constructed a theory of which the laboratory experimental basis, though apparently fair and firm, was as yet insufficient for the support of the structure of therapeutic treatment that was afterwards raised upon it. I followed the earlier experiments on this new method with great attention; I carefully analysed the principles on which the method was founded, and then with some misgivings watched the gradual development of the treatment as applied to actual cases of diphtheria. I was inclined to receive the statistics with great reserve, as I felt that this new method, like all new methods of treatment, might be making cures in the minds of the observer, and not on the bodies of the patients. Now, however, I am convinced that whatever justification my incredulity may have had from the consideration of previous experiments, none could be claimed in connection with the experiments that were carried out in the investigation of this special subject, and I am thoroughly satisfied that, although the antitoxic serum treatment may not come up to the expectations of all the rash writers on the subject—for many people seem to think that it should be a specific against diphtheria in all its stages—it promises, and this promise has in part been redeemed, to diminish the diphtheria case mortality in a very remarkable manner.

What is Diphtheria?

It is primarily an inflammation of the mucous membrane (the moist skin) of the tonsils, of the soft palate, of the upper part of the gullet, and of the upper part of the windpipe. During the course of this inflammation, which appears to be set up by the action of a special bacillus, there are usually thrown out some of the fluid elements of the blood and some of the white cells that float in the blood; these coagulate and form a soft toughish layer or film which offers an excellent food and resting place for this bacillus, which under such favourable conditions secretes or manufactures a most virulent poison. This poison is rapidly absorbed into the blood and is carried to various parts of the body; its effects are evident at first only on the nervous system, but afterwards on the muscles.

The Bacillus of Diphtheria.

First as to the bacillus. In 1875 Klebs described a short bacillus which he found on the surface of the greyish leather-like diphtheritic false membrane or film. Following up these observations, Loeffler traced a definite etiological relationship between this bacillus and diphtheria. First he obtained pure cultures of the bacillus by growing it on solidified blood serum, or on a mixture of three parts of blood serum and one part of neutralised beef bouillon containing extract of beef, 1 per cent. of peptone, 0·5 per cent. of common salt, and 1 per cent. of grape sugar. This organism may be readily detached from the surface of the false membrane by pressing firmly but gently with a little bit of cotton wadding twisted round the end of an iron wire or an ordinary penholder. When stained and examined under the microscope the diphtheria bacilli are found to be small rods from 3 to 6 μ (1 μ = $\frac{1}{25000}$th of an inch) in length, fairly plump, straight, or slightly curved, sometimes wedge-shaped or pointed, but usually somewhat enlarged and rounded at the ends, where also in stained

[1] A lecture delivered at the Royal Institution, on Friday, February 8, by Dr. G. Sims Woodhead.

specimens the protoplasm is more deeply tinted than in the centre. This organism grows singly or in groups, or felted together to form a net-work; it may occur in irregular masses of considerable size. When these bacilli have been growing for some time on an artificial nutrient medium, they appear to be segmented, the stained material accumulating in small round nodules placed at intervals within a kind of membrane which is only very delicately tinted. During the past five weeks I have examined about 600 specimens taken from the throats of diphtheria patients, and I may say that in nearly every case where the disease has been diagnosed by the physician in charge, as being one of diphtheria, these typical bacilli have been found, whilst in those cases in which there was any doubt as to the nature of the disease, similar bacilli were found in some, but not in others.

This is of importance, because we shall find that this bacillus gives us the substance with which animals are rendered immune to the attacks of the bacillus itself, these immune animals in turn supplying the antitoxic serum. To prove that this bacillus is really the cause of the disease, Loeffler, in an elaborate series of experiments, inoculated the pure cultures of the bacillus grown on artificially prepared media, into animals; he was thus able to set up characteristic lesions, especially if he took the preliminary precaution to abrade slightly the mucous membrane, thus as it were ploughing the ground before scattering the seed. On such abraded surfaces the bacilli grew very luxuriantly, and false membranes were produced; in these lesions the bacilli could afterwards be found and again separated in pure cultures, whilst the characteristic toxic symptoms of diphtheria were in each case experimented upon, repeated with the utmost fidelity. Loeffler also pointed out a most important fact in connection with the presence of the organism in the body. He found that it was strictly confined to the local wounds or lesions in the throat and posterior part of the nose, and he was also able to prove that in this position these organisms commenced to manufacture most virulent poisons, which, unlike the bacilli, can become diffused throughout the body. Klein and Sydney Martin in this country have both made very valuable contributions to our knowledge, the former concerning the bacteriology of the disease, the latter in regard to the chemical action on the tissues of the toxic or poisonous products of the bacillus.

The Toxines of Diphtheria.

Martin found that after the poison formed in the throat has made its way into the internal organs of the body it undergoes certain changes; it is broken down into somewhat less poisonous compounds, but these, accumulating at certain points, act especially on the nerves and muscles. It appears then that we have to deal with two sets of poisons: a very virulent poison formed by the bacilli directly from the fibrin and albuminoids of the fluids of the blood, exuded on the surface of the mucous membrane; and secondly, a less poisonous series which appear to accumulate especially in the spleen. So long as these poisons remain in the body we have the general fever, rise of temperature, and altered conditions of circulation (as evidenced by the pulse), so characteristic of the disease. At a later stage, sometimes after all the primary symptoms of diphtheria have passed away, there are often met with what are called post-diphtheritic paralyses, which are due apparently to alterations in the nerves going to muscles, especially those going to the delicate muscles of the soft palate and around the opening into the windpipe, though other groups of nerves and muscles may be similarly affected. These post-diphtheritic paralyses may be due then to the action either of the virulent poison (ferment) formed in the membrane, or of the somewhat less poisonous but more stable toxines that are formed in the later stages of the disease. Through the kindness of Dr. Martin I am enabled to show you figures of nerves and muscles, the degeneration of which is due to the action of these poisonous substances. It is here unnecessary to enter into any detail as to the minute changes that take place in the nerve and muscle fibres, but on comparison of the affected nerve fibres with a healthy nerve fibre, it is evident that we have here grave structural alterations which must interfere most materially with the power of the nerve to conduct nerve impressions from the spinal cord to the muscle. The outer part or sheath of the nerve is in some places entirely wanting, whilst in other cases the axis cylinder or core of the nerve is either greatly attenuated or entirely absent. The poison in these cases has set up changes by which the communicating paths between the muscles and the spinal cord and brain have become thoroughly disorganised. The muscles, too, instead of being formed of cleanly striated fibres, have this striation greatly obscured, first by a kind of cloudy or ground-glass look, and later by the appearance of a number of strongly refractile granules. These, when stained with osmic acid, become black, from which we argue that they are composed of fat, and it is said that the muscle has undergone a fatty degeneration, the muscular protoplasm being partially converted into fat; ultimately the striation may be almost lost. In a case of diphtheria, then, the following stages may be traced: a sore throat (often simple enough to begin with), by which the mucous membrane is prepared for the reception of the diphtheria bacillus. The diphtheria bacillus becomes implanted on this surface, gives rise to an acute inflammatory condition, and, subsisting on the inflammatory exudation, sets up a local manufactory of a most virulent poison. This poison, absorbed into the circulation, at once acts on the nervous system, although a certain proportion seems to be broken down into more stable, but less virulent, poison, which remains in the body, and may continue to act for a considerable time on the nerves and muscles.

Immunity against Diphtheria.

Whilst these poisons are attacking the more highly organised, and therefore less stable tissues, they are stirring up or stimulating the other tissues of the body to resist their invasion and action. If this were not the case, any one attacked by diphtheria must eventually succumb to the disease; but we know a considerable proportion of the cases of diphtheria recover even when no treatment at all is resorted to. Whatever may be the exact explanation of this recovery, we know that it depends upon the power of certain cells in the body to accommodate themselves to the presence of the toxines, and to go on doing their work of scavengering and of removing foreign substances from the body even under what originally were adverse conditions; during this process the cells become so profoundly and permanently altered that the patient is for some time protected against further attacks of the same disease. It was originally maintained that this alteration was entirely confined to the cells, but it is now generally accepted that these cells form or secrete substances which, thrown into the blood, either act directly upon the toxines so as to interfere with their activity, or so react upon the cells that they are able to continue their work in the presence of the toxine. At all events, a certain immunity against the disease is acquired. Upon these various theories is based the *rationale* of the antitoxic serum treatment of diphtheria. Ferran claims to have been the first to obtain such a condition of immunity against diphtheria in animals; shortly afterwards, Fraenkel in Germany obtained similar results. Seeing that this immunity depends upon an alteration in the composition of the serum, should it not be possible, argued Prof. Behring, to take the serum of an immunised animal and transfer it to a patient suffering from diphtheria, so as to help the tissues and cells of the patient to cope with the toxic products of the diphtheria bacillus during the earlier stages of the disease, inducing, as it were, a kind of artificial immunity to help the patient over the acute period of the attack when the poisons, though most virulent, are most unstable, and when the tissues have not yet become acclimatised to the presence of the toxic products of the bacillus; when, in fact, they are paralysed and are able to do little to protect themselves. Behring so followed up this idea, that he was able to initiate a system of treatment which promises to revolutionise our therapeutic methods in the treatment of certain specific infective diseases.

The Production of Antitoxic Serum.

Working on the fact that an animal might be rendered more and more insusceptible to the action of the toxic products of bacteria, Behring found that he might proceed in either of two ways. He might make an artificial wound with a needle, and introduce weakened bacilli into the animal, the weakened bacilli then growing but feebly and producing a modified toxine. After the effects of the first dose had passed off, he was enabled to increase the dose and to use more active bacilli, injecting them first into the tissues and eventually directly into the circulation, with the result that enormous doses of virulent diphtheria bacilli might ultimately be introduced without giving rise to more local swelling or general febrile disturbance than was first noticed when the small dose of modified bacilli was introduced. Such a method as this, however, was attended with considerable drawbacks, as it was almost impossible to gauge, at all accurately, the number and strength of the bacilli. Not so, however, with the products of the micro-organisms, the activity of which could of course be more accurately measured, and the dose more accurately graduated. The bacilli might multiply and continue their action on the tissues, but the poisons when injected alone would not alter in quantity or activity. As may be readily imagined, the fluid constituents of the blood can only contain those substances that are introduced into it from without, either through the vital activity of the cells of the body, the products of which must be thrown into this fluid before they can be excreted, or through artificial injection. The antitoxic substances then found in the blood of an immunised animal, must in the case of natural immunity following an attack of diphtheria be the result of the activity of the tissue cells, especially of the connective tissue and white blood cell groups which have been "stimulated" by the toxines introduced from without, from the false membrance in the throat. Where it is desired to produce an artificial immunity, and an "artificial" antitoxic serum, the cells are stimulated by the introduction into the body of artificially prepared toxine. The cells acted upon by the toxine elaborate the protective fluid, which is thrown into and accumulates in the blood. This substance may act in one of several, or even in several ways. (1) It may directly antagonise the diphtheria toxine, and may thus prevent the paralysing action of these poisons on the scavengering cells or phagocytes, as they are called; these, left free to perform their proper functions, can deal with the foreign elements that have got into the blood, and also with the bacilli at the seat of the local attack, for, as has been pointed out by several foreign observers, and by Ruffer in this country, immediately beneath the layer of bacilli in the false membrane there is usually a very considerable accumulation of leucocytes, especially in those cases in which recovery ultimately takes place. (2) The antitoxic substances may act on the bacilli, inhibiting their growth and interfering with their power of producing toxines. This of course can only be a local action should it play any part in the process. (3) These substances may act directly on the cells of the blood lymph and tissues, so stimulating and strengthening them that they are able to perform those functions above mentioned. It is at present difficult to state which of these processes is the one, or the most important, in protecting or curing the patient, and it may be that all play a part. It may be that the tissue cells, when acted upon by the specific diphtheria poison, become so modified that they are enabled to produce or secrete a substance which directly antagonises the action of that poison. This substance, thrown into the blood, remains there for some time, rapidly accumulates as larger and larger doses of the poison are thrown in, neutralising the poison, whose power of doing damage to the tissues is thus held in check, but remaining for some time after the toxine has disappeared; or this antitoxic substance, reacting upon the cells, may render them less susceptible to the action of the toxine.

The earlier immunising experiments were naturally performed upon the smaller animals, such as rabbits. Then Behring used sheep, and after various other animals had been tried, the horse was selected by Roux and Nocard as perhaps the best of all animals from which to obtain antitoxic serum. In the first place, he is comparatively insusceptible to the action of the diphtheria bacillus—even considerable doses of living bacilli may be injected under the skin without producing anything more than a slight local swelling and a rise of temperature. It has also been found that horse serum, when injected, produces little or no change in the healthy human subject—that is, the serum seems to mix perfectly well with human blood plasma, and there is comparatively little danger of the extra serum being excreted by the kidneys in the form of albumen. This is a most important point, and one that no doubt influenced Roux and Nocard in their selection of the horse as an animal from which to obtain immunised serum. Beyond this, however, the blood, when drawn from the vessels, separates very perfectly into two portions—a firm clot, which if the blood be caught in a cylindrical glass jar, forms a kind of column in the centre, and a clear straw-coloured serum which accumulates around the clot, and forms a layer often several inches deep above it. This serum contains the antitoxic substances. Lastly, considerable quantities of blood can be obtained from such a large animal as the horse, and if he be well fed, groomed and exercised, the process of bleeding may be repeated pretty frequently without causing any inconvenience to the animal; in fact, as one observer said, he stands bleeding as well as did our forefathers, who thought as little of being bled as we think of going to Aix or Buxton.

Let us now turn for a moment to the method of treating the horses that we wish to render immune, in order that they may supply the antitoxic serum that is to be used for the treatment of cases of diphtheria. Roux's method, which is that that has been most carefully described, and which is the one used in this country first by Dr. Ruffer at the British Institute of Preventive Medicine, and then by Prof. McFadyean at the Royal Veterinary College, consists in introducing diphtheria toxine of a given strength in gradually increasing doses, until the blood of the animal so infected is found to contain a sufficient quantity of the antitoxine.

Preparation of Diphtheria Toxine.

The toxine with which the animal is to be injected is prepared as follows:—A broth is prepared by soaking a pound of finely-minced beef in water. This is allowed to stand for twenty-four hours in the cold. To the fluid expressed from the meat fibres at the end of that time is added ½ per cent. of common salt and 2 per cent. of peptone (meat artificially digested by pepsine). This broth is then rendered faintly alkaline by the addition of soda salts or caustic soda. This is done because it is found that the diphtheria bacillus cannot grow at all vigorously, or form its poisons rapidly in an acid solution, and such poison as is formed is neutralised, or is unable to act in the presence of even a faint trace of acid. It is found that even in Roux's solution, which is always faintly alkaline to begin with, an acid reaction soon appears, but, after about ten days, this is replaced by an alkaline reaction, and as soon as this takes place, the growth of the bacilli takes on new activity, the quantity of toxine is increased, and it becomes much more virulent. Roux found that he obtained his most virulent toxines after a month's growth. If the growth is allowed to go on longer than this, a process of oxidation appears to take place, and I have found that the toxine from a culture carried on for two months had already lost much of its toxic activity. It should be noted that a virulent bacillus should always be taken in the first instance, otherwise the results may be very disappointing.

This nutrient material is placed in a layer of not more than half an inch thick in a flat-bottomed flask, which is plugged with cotton wadding, and then closed with an indiarubber cork or cap. Through this composite plug three tubes are passed into the flask; the two lateral tubes are bent at right angles, both inside and outside the flask; whilst the centre tube is fitted with a small thistle-head, which may be plugged with cotton wadding, and then closed with an indiarubber cap. The outlets of the lateral tubes are also plugged with cotton wadding, and the whole apparatus is kept for an hour or two in steam maintained at a temperature of 100° C. (Flasks so treated may be preserved for years without any change, beyond some slight evaporation, taking place in the broth.) A small quantity of a pure broth culture of the virulent diphtheria bacillus is now drawn into a long thin pipette, the indiarubber cap and the cotton wadding plug are removed from the thistle-head, and the contents of the pipette are introduced; the pipette is withdrawn, the cotton wadding is replaced, the indiarubber cap is fitted in position, and the flask is placed in an incubator which is maintained at the temperature of the body (98·4° F., or 38°·2° C.). As soon as the growth is well started (usually at the end of about twenty-four hours), a current of moist air is made to pass continuously over the surface of this cultivating fluid, the air being first warmed and saturated with moisture, in order as far as possible to prevent evaporation. A fine flocculent deposit soon makes its appearance on the bottom of the vessel, the supernatant fluid remaining clear. This deposit increases in thickness, much more luxuriant growth going on after the first ten days. Toxine is formed by the diphtheria bacilli so long as they can grow freely—that is, so long as they can obtain sufficient nutrient material from the fluid and from the air that is continually passing over the surface. At the end of a month, if all these precautions are taken, the toxine should be of such a strength that 1/10th of a c.c. (about two or three drops) injected into a guinea-pig weighing 500 grammes (over 17 ounces) will kill it within forty-eight hours. The strength of the toxine or poison may be a little greater or a little less than this, but it is a comparatively easy matter to measure the strength, and therefore to graduate the dose to be used in immunising the horse. This only holds good, however, if the active diphtheria bacilli are removed or destroyed; these, if left in the fluid, would be a complicating and inconstant factor in the equation. In order to kill these bacilli the Germans recommend the addition of ½ per cent. of carbolic acid to the culture; the dead bacilli falling to the bottom, leave a perfectly clear supernatant fluid. The French, on the other hand, recommend the separation of the bacilli from the fluid by means of a Pasteur-Chamberland filter. By this means a clear virulent poison which does not contain any diphtheria bacilli is obtained. With this fluid a horse with a good constitution, and which has been proved to be free from tubercle and glanders, is injected under the skin of the side of the neck in front of the shoulder. Small doses are first injected, either pure or with the addition of 1/3 of the volume of weak solution of iodine of potassium. If the fluid is of full strength, only about 2 c.c. can be given at the first injection. This is followed within twenty-four hours by a local swelling at the seat of injection, about the size of the palm of the hand, and the temperature may rise 1° or 2° F. (½° to 1° C.), otherwise the general health of the horse does not seem to suffer. He eats well, and unless regularly exercised may become very lively; of this we have had ample evidence during the recent frost and snow, when it has been unsafe to give much exercise to horses that are not very sound in limb, and as a result they have been very fresh indeed. As soon as the swelling has disappeared and the temperature has receded to the original level, a somewhat larger dose is given; the same process is repeated time after time (the dose being gradually increased to bring about the same amount of swelling and rise of temperature) for about three months, or until such time as the requisite amount of immunity is acquired, *i.e.* until the antitoxic action of the blood is sufficiently marked. That there is a gradually increasing immunity is evidenced by the fact that enormously large doses of the toxine in the later stages of the treatment produce even less local and constitutional disturbance than was observed after the first few injections of comparatively small quantities.

The blood is now drawn off from the jugular vein of the immunised horse by means of a metal cannula or tube to which is attached an indiarubber tube; these are first thoroughly boiled, in order that no living micro-organisms of any kind may remain on or in them, and the skin of the horse is carefully cleansed with antiseptic lotions. The indiarubber tube leads the blood into a flask or vessel which has also been carefully sterilised, and provided with a well-fitting cotton-wadding plug or glass-stopper. The vessel when filled is placed in an ice-safe until the solid part, the clot, is completely separated from the fluid—the serum. From each gallon of blood about one quart of serum is expressed, though this varies considerably in different cases, and according to the time that the separation is allowed to continue (24 to 48 hours). This serum, a limpid straw-coloured fluid, is carefully decanted under strict antiseptic precautions, and, mixed with carbolic acid or camphor, is stored in small phials, each of which contains about a sufficient quantity for the treatment of a single patient. In the Pasteur Institute, and in the British Institute of Preventive Medicine, the antitoxic serum is apparently brought up to such a strength that $\frac{1}{100}$ of a c.c. injected into a medium-sized guinea-pig (500 grammes, or over 17 ounces) will protect it against an injection twenty-four hours later of ½ c.c. of a culture of living diphtheria bacilli strong enough, if given by itself, to kill the guinea-pig in twenty-four hours. It is usually recommended that 20 c.c. of this serum should be given at the first dose, and that if necessary a second 10 c.c. should be given half an hour later. The method of testing the strength of the serum adopted by the Germans is that devised by Ehrlich, who takes ten times the lethal dose of diphtheria toxine, and in a test-tube adds a definite and known quantity of the blood to be tested. This mixture is then injected into a guinea-pig, and if the antitoxic power of the blood has been gauged aright, the animal does not suffer in the slightest degree from what under ordinary circumstances would kill ten guinea-pigs. The addition of less or weaker serum, or of more toxine would leave the mixture still toxic.

In order to obtain a definite standard with which to compare the antitoxic power of any serum, and to determine the dose of such serum, Behring and Ehrlich have described what they term a normal antitoxic serum—that is, a serum of such a strength that $\frac{1}{10}$th of a c.c. added to ten times the lethal dose of diphtheria toxine is exactly sufficient to render it innocuous. 1 c.c. of such normal serum contains one "immunisation unit," and should be sufficient, when added to a hundred times the lethal dose and injected, to render it innocuous. In horses wholly immunised the serum may be fifty or even a hundred times as active as the normal serum above mentioned and the dose to be given varies according to the number of immunisation units in any sample. It is not here necessary to go into the question of dose, but it may be stated that 500 of these immunisation units are usually necessary to produce the desired effects in cases of diphtheria, though in some cases still larger quantities have to be used. Behring now supplies four strengths of the serum, the weakest (marked with a yellow label) is sent out for injection of cases where the disease has not already been contracted. The next (marked with a green label) is of a strength of 600 antitoxine units, and is given to those cases in which the treatment is commenced at the very outset of the disease—that is, when the first symptoms of diphtheria manifest themselves. The next stronger antitoxic serum (white label) equals 1000 antitoxine units, and is used for cases somewhat more advanced in which the prognosis is at all grave; whilst in still graver cases, and where the symptoms have been developed for some considerable time, it is often necessary to give a serum of 1500 units; this is marked with a red label, and is of course highly concentrated in order that the size of the dose may not be unduly increased. In place of No. 1, healthy children and adults who are exposed to diphtheritic infection may receive a quarter of the dose of the green label flask, which Behring considers will protect against diphtheria with very great certainty. Although these general directions are laid down, it is strongly insisted upon by Behring, Kossel, Roux, and in fact by all those who have had experience of antitoxic serum, that the dose must vary according to the severity of the disease, so that much must be left to the discretion of the medical practitioner in charge of the patient. The great error into which those who first use this agent fall, is the administration of far too small a dose, especially in the case of children, for whom the dose is nearly as large as it is for adults. For this reason some of the statistics published in this country and abroad are far too unfavourable to the method. The great drawback with this method is that the dose necessary to be injected is so large; but in the loose tissue of the side of the chest, the back, or the buttock, immediately under the skin, the fluid soon disappears. It is hoped that before long, however, the active principle may be separated, and so obtained in smaller bulk.

So far we have dealt principally with the antitoxic serum as prepared by Behring and Roux and by Roux's method, which is certainly attended with comparatively few difficulties; these, however, have the disadvantage that they take from three to six months to give the desired results. In order to do away with this disadvantage, Klein has carried out a series of experiments in which he has been able to obtain serum of considerable activity in as short a period as twenty-three days. Instead of introducing the poison only, he adopts the plan used by Behring and Roux in their earlier experiments, of injecting living bacilli which have lost a certain degree of their activity, using for this purpose old cultures. He afterwards introduces toxine along with more virulent bacilli, and thus obtains in the animal such a degree of immunity that it is enabled to withstand or to react very slightly to more than a fatal dose of diphtheria bacilli. By the third week the animal will bear the injection of large quantities of virulent bacilli, and by the end of twenty-three or twenty-six days the serum has acquired such antitoxic properties that 1 c.c. of it will protect 40 to 80 guinea-pigs against a lethal dose of living diphtheria bacilli. It is difficult to compare these results with Roux's and Behring's, but Klein's serum has been used with marked success in certain cases of diphtheria. It appears to have a special power of causing the membrane to clear away, and so to remove the manufactory of the poison, as on this membrane the diphtheria bacilli accumulate. This method is mentioned as one that may be used especially where it is desired to obtain antitoxic serum quickly.

Smyrnow has suggested quite a different method of preparing antitoxine. Under Nencki's advice he passed electric currents through the serum of animals, and was thus able to endow it with a certain immunising power. But he was still more successful in obtaining powerful antitoxine by electrolysing diphtheria bouillon cultures; curiously enough, the more virulent the culture the more powerful was the antitoxic substance he obtained. When this antitoxic substance was injected into a rabbit, which twenty-four hours before had received about ½ c.c. of a two or three days old diphtheria bouillon culture, there was a rapid rise of temperature followed by marked improvement in the condition of the animal. This observer believes that antitoxine can be obtained by this method that will be much more suitable for the treatment of the human subject than those obtained by the ordinary methods. His experiments, however, are far too few to carry any great weight, though they open up a most interesting field for future investigation.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The extension of the buildings of the Cavendish Laboratory is about to be undertaken, at an expense of over £4000. About half the cost will be met from the accumulated fees of students working in the laboratory.

Mr. E. Hamilton Acton, Fellow and Lecturer in Chemistry of St. John's College, died suddenly, from heart disease, on Friday night. Mr. Acton was only in his thirty-third year; but he had earned a considerable reputation as a chemist, and his researches in vegetable chemistry, in particular, were of importance He was an able and successful teacher. His funeral on Tuesday was attended by some hundreds of the junior members of the University, and by representatives of all the scientific departments.

MR. ROBERT PERKINS, of Jesus College, Oxford, leaves England next week for Honolulu to resume his investigations on behalf of the Joint Committee appointed by the Royal Society and the British Association for the zoological exploration of the Sandwich Islands. The large collections he made there during his former stay (March 1892 to September 1894) have been submitted to various specialists, with results that show him to be an indefatigable observer in all branches of terrestrial zoology; and, since his return to England last autumn, he has been busily engaged in discovering what has yet to be done to complete our knowledge of the indigenous Fauna which is so rapidly disappearing.

THE County Councils of Northumberland and Durham are truly advancing technical education by affording assistance to Dr. W. Somerville, Professor of Agriculture in the Durham College of Science, to carry out extensive manurial trials. The experiments were begun in 1892, on nine farms in Northumberland; in 1893, when Durham joined in the work, the number of farms rose to twenty-six; while during 1894, the trials were made at no less than forty-three different centres in the two counties. The investigations must have a not inconsiderable influence upon the prosperity of the agriculture of the district to which they refer.

THE Technical Instruction Committee of the Essex County Council, with a view to promoting the spread of scientific knowledge among those engaged on the coast in the fishing industries, started a modest biological station at Brightlingsea last year, and, under the superintendence of Mr. J. T. Cunningham, a number of specimens were collected for the purposes of demonstration. Some experiments on the continental method of growing oysters were also commenced, but, owing to the unfavourable character of the season, the results could not be carried very far. We are glad to learn, however, that the Committee, in conjunction with the Borough Council of Colchester, propose to carry on the work of the station, and that the Fishmongers' Company have also shown their appreciation of the movement by giving a grant of £50 per annum for three years.

THE Manchester Town Council have accepted a tender for the erection of a technical school at a cost of £140,000.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xvii. No. i. (January, 1895).—Sur une transformation de mouvements, P. Appell, is a treatment of Elliot's problem (*Comptes rendus*, 1893), and of a question solved by Mestschersky, in the *Bulletin des Sciences Math.* 1894, as a particular case of transformation of movements.—An extract from a letter addressed to Dr. Craig by M. Hermite, gives the result of an investigation of the asymptotic value of log Γ(a) when a is large.—On the first and second logarithmic derivatives of hyperelliptic σ functions, by Oskar Bolza, opens with a statement of certain well-known theorems of the theory of elliptic functions, and then extends these and some allied theorems to hyperelliptic functions.—Sur la definition de la limite d'une fonction. Exercice de logique mathématique, by Prof. Peano. The definition is one previously used by the writer, and also by two or three previous writers. It is practically given by Abel (*Works* ii. p. 199), in the form. "Pour qu'une série Σu_n soit convergente, il faut que *la plus petite des limites* de nu_n soit zéro." The same general idea of a limit is given in Cauchy's "Cours d'Analyse algébrique" (1821, p. 13), "quelquefois . . . une expression converge à-la-fois vers plusieurs limites différentes les unes des autres." Prof. Peano works on this definition, and demonstrates at some length its principal properties. To this end he employs la logique mathématique, "Cette science s'est rapidement développée de nos jours, et on l'a appliquée dans plusieurs travaux."—Dr. E. McClintock contributes an article on theorems in the calculus of enlargement (a paper read before the American Mathematical Society, August 14, 1894). It is an interesting sequel to his essay on the calculus of enlargement (vol. ii. pp. 101-161).—In his note on Foucault's pendulum, Mr. Chessin considers the motion of a physical pendulum on the surface of the earth, taking into account the rotation of the earth about its axis. The initial velocity relatively to the earth of the pendulum is supposed equal to zero, as in Foucault's experiment. Hence he retains the name of "Foucault's pendulum," although oscillations of any finite amplitude are considered. The portrait which is given with this number is that of M. E. Picard.

Wiedemann's Annalen der Physik und Chemie, No. 2.—Fluorescence of solutions, by O. Knoblauch. There is a constant ratio between the intensity of the fluorescence and the existing light, even when the intensity of the latter is altered in the ratio of 1 to 6400. The author proves experimentally and theoretically that the effect upon the various fluorescent bodies of varying the solvent is very different.—The potential gradient in the positive portion of the glow discharge, by A. Herz. The potential gradient in the positive unstratified glow discharge of a vacuum tube decreases as the current increases, and also as the diameter of the tube is increased; but it increases with the pressure, though not as rapidly.—Unipolar induction, by Ernst Lecher. The author discusses the different aspects of the question whether, when a cylindrical magnet rotates about its axis, the lines of force due to it are stationary, or rotate with the magnet. The former was Faraday's original view, the latter has been maintained by Tolver Preston and others. After showing that all the experiments hitherto quoted as decisive one way or the other may be equally well interpreted on either assumption, he describes some test experiments which show that the lines of force stand still while the magnet rotates.—Electric dispersion, by P. Drude. A method is described for investigating the relation between the dielectric constant of a substance and the period of the electric waves traversing it, or what may be described as the electric dispersion of the substance. If the dielectric constant decreases as the period increases, there will be normal, if it increases, anomalous dispersion. For alcohol the dispersion was found to be normal, and of the same order of magnitude as its optical dispersion. Water showed abnormal dispersion with the large wave-lengths used, whereas ebonite showed no perceptible dispersion.—Effect of kathode rays upon some salts, by E. Goldstein. Lithium chloride, when exposed to Kathode rays, assumes a heliotrope or dark violet colour, which it retains for some time in a sealed tube. Chlorides and other haloid salts of potassium and sodium show similar effects. The colours are very superficial, and disappear on heating, or by the action of moisture.

SOCIETIES AND ACADEMIES.

LONDON

Geological Society, January 23.—Dr. Henry Woodward, F.R.S., President, in the chair.—Carrock Fell: a study in the variation of igneous rock-masses. Part ii. The Carrock Fell Granophyre. Part iii. The Grainsgill Greisen, by Alfred Harker. The augite-granophyre of Carrock Fell was first described in its normal development, special attention being drawn to the various types of micrographic intergrowths which it exhibits. The variation of the rock was next examined, and, in particular, a curious basic modification which occurs near its junction with the gabbro described in a former paper. The granophyre here passes into a coarse type rich in augite, iron ores, and apatite, its silica-percentage falling to as low as 58. The author attributed this to the acid magma having incorporated in itself portions of the highly basic margin of the gabbro. The latter rock seems to have been fused or dissolved by the magma, with the exception of certain of its more refractory minerals which survive in the modified marginal part of the granophyre. The latter part of the paper dealt with a remarkable quartz-mica rock found on the north side of the Skiddaw granite. It differs in some respects from the Cornish greisens, and resembles in its mode of occurrence certain pegmatites in the Scottish Highlands. The author considered the rock to have been extruded from the granite in connection with the post-Silurian crust-movements of the district, while its composition has probably been further modified by subsequent chemical changes.—The geology of the country around Fishguard (Pembrokeshire), by F. R. Cowper Reed.—The tract of country forming the subject of this communication occupies the northern part of Pembrokeshire, from Newport to Strumble Head.—On the mean radial variation of the globe, by J. Logan Lobley. The author submitted considerations (chiefly derived from the characters of the earlier sediments) which led him to suppose that crust-folds have not been produced by continuous contraction of the earth, and that the planetary heat and mean radius of the earth have been practically invariable during the period which has elapsed since Cambrian times.

Zoological Society, February 5.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—A communication was read from Dr. E. A. Goeldi, in which he described the breeding habits of some tree-frogs observed by him in the Province Rio Janeiro. *Hyla faber*, Wied, constructs nests of mud on the shallow borders of ponds, wherein the young are protected from enemies whilst in the larval state. *Hyla goeldii*, Boulenger, dispenses with the metamorphoses, which are hurried through within the eggs, these being carried by the female on her back. *Hyla nebulosa*, Spix, deposits its eggs in a slimy mass attached to withered banana-leaves, the young remaining in this sort of nest until in the perfect, air-breathing condition.—Mr. Edgar A. Smith gave an account of a collection of land-shells made principally by Mr. A. Everett at Sarawak, British North Borneo, Palawan, and other neighbouring islands.—Mr. Oldfield Thomas read a paper upon the long-lost mammal *Putorius africanus*, Desm., and its occurrence in Malta.—Mr. F. E. Beddard, F.R.S., read a paper on the visceral anatomy of the tree-kangaroo (*Dendrolagus bennettii*), and pointed out the structure of the brain and other organs.

CAMBRIDGE.

Philosophical Society, January 28.—Prof. J. J. Thomson, President, in the chair.—The following resolutions were proposed by Prof. Sir. G. G. Stokes, seconded by Mr. Glazebrook, and passed unanimously: (1) "That the Cambridge Philosophical Society desires to express its sense of the great loss sustained by the University and the Society by the death of Prof. Cayley; whose eminence conferred honour on the Society which reckoned him amongst its Presidents, and whose simple and earnest character was an example to all and endeared him to those who knew him." (2) "That the Society do now adjourn without transacting the business of the meeting, as a mark of respect to Prof. Cayley."

PARIS.

Academy of Sciences, February 11.—M. Marey in the chair.—M. Faye gave a short account of the contents of volumes iv. and v. of the *Annales de l'Observatoire de Nice*.—On the presence and distribution of alumina in plants, by MM. Berthelot and G. André. Alumina is found in quantities comparable with the amounts of other bases present, mostly in the roots of plants having deep and abundant roots.—Preparation and properties of titanium, by M. Henri Moissan. Titanium has been prepared in the electric furnace. It requires the most intense heat capable of production, and even then contains at least 2 per cent. of carbon. It forms friable masses, having a bright white fracture. It is harder than quartz and steel. Its specific gravity is 4·87. The chemical properties are given in detail. Its silicide and boride are as hard as the diamond. The carbide, TiC, and nitride, Ti_2N_2, are also fully described.—On some derivatives of phenolphthaleïn, by MM. A. Haller and A. Guyot.—M. Guignard was elected Member of the Botany Section.—Report on a work by M. E. Hardy relative to the application of sound vibrations to the analysis of two gases of different densities. By means of an instrument (termed a Formènophone) consisting essentially of two pipes tuned to give the same note, and blown with air and with air containing a lighter gas, the quantity of lighter gas present may be ascertained by the production of beats between the sounds emitted by the two tubes.—On a property of meromorphic functions, by M. Emile Borel.—On certain systems of equations to the derived partials, by M. J. Beudon.—On the electrostatic capacity of resistance bobbins, and its influence in the measurement of coefficients of induction by the Wheatstone bridge, by M. J. Cauro. Bobbins with double coils should only be used for small resistances; if the resistance increases, a capacity-error greater than that due to self-induction may be caused. To minimise capacity-errors the Chaperon winding should be used. These capacity effects may be neglected in measurements made with ordinary Wheatstone bridges; but they come into play with bobbins having small coefficients of self-induction with great resistances.—On the measurement of luminous flux, by M. A. Blondel.—On the passage of light across a thin plate in the case of total reflection, by M. Ch. Fabry. The formulæ of thin plates are shown to apply without modification to the case of a transparent spot at and surrounding the point of contact of a convex and plane-glass surfaces when the included thin plate is viewed at the angle of total reflection.—On the lowering of the freezing-point of dilute solutions of sodium chloride, by M. A. Ponsot. The author obtains results by his own method differing from those of previous observers. He finds that in the limit the lowering of the freezing-point is proportional to the number of grams of salt existing in 100 grams of solution.—On sulphide of gold, by M. A. Ditte. The behaviour of gold sulphide in presence of alkaline sulphides, with and without excess of dissolved sulphur, is described. The compounds: $Au_2S.2Na_2S\ 20H_2O$ and $Au_2S.Na_2S\ 10H_2O$ and $Au_2S\ 4K_2S_2\ 12H_2O$ are noted.—On a method of causing the crystallisation of precipitates; manganese and zinc sulphides and cupric hydrate, by M. A. Villiers.—On cinchonigine, a case of dimorphism of a compound having a specific molecular rotatory power, by MM. E. Jungfleisch and E. Léger. Cinchonigine is dimorphous, its two forms readily passing from one to the other. The monoclinic form is stable at the ordinary temperature, the rhombic form at about 35°.—On the plurality of chlorophylls; the second chlorophyll isolated from lucerne, by M. A. Étard.—A comparison between the coloured and colourless derivatives of hexamethyl triamidotriphenylmethane, by M. A. Rosenstiehl.—On a new type of ethereal salt, methylene lactate, by M. Louis Henry. The preparation and properties are described of the substance represented by the formula

$$\begin{matrix} CH_3 . CH & . & CO \\ | & & | \\ O.CH_2 & & O \end{matrix}$$

.—On the relation between the general form and the composition of the body in protozoa, by M. Félix Le Dantec.—A study of the agricultural value of the phosphate of aluminium from Grand-Connétable, by M. A. Andouard.—On the contact phenomena of the lherzolite of the Pyrenees, by M. A. Lacroix.—New relations between the barometric movements in the northern hemisphere and the movements of the sun and moon in declination, by M. P. Garrigou-Lagrange. (1) In the northern hemisphere the atmosphere oscillates about the 30th parallel, corresponding to the movement of the moon in declination, so that the pressures are lower with boreal than with austral moon below 30°, and inversely above. (2) The gradients take corresponding modifications. The barometric slopes from 30° south and north, are alternately increased and lowered, more strongly with boreal moon below 30°, more feebly above, and inversely with austral moon. (3) These differences in pressures and gradients augment towards the pole, at least up to the 70th parallel. (4) These movements are superposed on more general movements.

AMSTERDAM.

Royal Academy of Sciences, December 29, 1894.—Prof. Van de Sande Bakhuyzen in the chair.—A treatise on a simple method of roughly distinguishing between Scandinavian and southern diluvial sand, and between alluvial and diluvial sand, by separating the heavy minerals from the sand through immersion of the sand in a fluid of a specific gravity of 2·88, invented and described by Dr. J. L. C. Schroeder van der Kolk, formed the subject of a report drawn up by Mr. Van Diesen and Dr. Behrens—Mr. Schols and Mr. Martin reported upon an essay by Mr. J. J. A. Muller, in which the author calculated the dislocation undergone by some parts of the mountain system of Sumatra, in consequence of the earthquake of May 17, 1892. These calculations, which are based on data supplied by the measurements executed in the said island, on behalf of the secondary triangulation, prove with great certainty a horizontal shifting of the following three points to the extent, and in the direction indicated :—

Sorik Merapi	1·23 M.	344° 57′
Tor Si Hite	0·64 M.	149° 2′
Goenveng Malintang ...	1·24 M.	304° 28′

the directions being counted from the north point, going round through the east.—Dr. Jan de Vries discussed some methods of deducing from given configurations more complicated configurations. In particular a series of configurations, first described by Andreef, was deduced from the tetrahedron by means of a system of polar coordinates.—Dr. Bakhuis Roozeboom, in considering the experiments of Prof. Spring, on the conversion of black into red HgS, showed that this author was mistaken in the nature of the phenomenon, which belongs to the category of the conversion of unstable modifications into stable ones. The pressure required for a conversion of this description does not admit of being expressed by a simple law.—Dr. P. van Romburgh has examined, in the laboratory of the "culture" garden at Tjikeumeuh, a number of coca-leaves grown in Java, in order to ascertain their volatile constituents. Those of *Erythroxylon Coca Lam.* var., *Spruceanum* (Burck), when distilled with water, produced methyl-salicylate (about 20 c.c. was obtained from 140 kgrs. of fresh leaves). In the water was also found a little acetone and methylic alcohol, and perhaps also traces of salicylic aldehyde. The quantity of methyl salicylate decreases in proportion as the leaves grow older; in fresh unexpanded top leaves Dr. van Romburgh found 0·13 per cent., in young leaves from 0·06 to 0·07 per cent., in old leaves even less than 0·02 per cent. The leaves of *Erythroxylon Bolivianum* (Burck) were also proved to contain methyl salicylate, but only 0·004 per cent., as well as those of *E. ecarinatum* (Burck), but not those of *E. Burmanium* (Grift) and *E. longepetulatum* (Burck), while in the case of *E. spec. insul. Comor* the results were doubtful.—Prof. Kamerlingh Onnes read a paper on the Kryogene Laboratory at Leyden, and on the production of extremely low temperatures. The object of the author in starting his investigations, upwards of ten years ago, viz. the combination of Wroblewski's and Olszewski's labours with those of Pictet, has been quite satisfactorily attained with the least possible means. Liquid oxygen is stored in a glass vessel adapted for experimenting and observing purposes; the oxygen vapours are continuously compressed, liquefied, and again poured into the said vessel, so that the evaporation from the surface takes place at a level pretty nearly constant. With the aid of a small quantity of circulating oxygen, a bath of liquefied gas of quarter to a half litre can be maintained under normal or reduced pressure, *ad libitum*. With this method no use is made of Dewar's vacuum vessels. The vessel is protected from convective transference of heat by the oxygen vapour, which cools a special chamber with plate-glass windows. These windows remain quite free from hoar-frost, and do not interfere with the formation of images. The condensation of oxygen is obtained in a spiral tube immersed in liquid ethylene boiling in a copper flask connected with a conjugate vacuum pump and compressor. The circulating ethylene is liquefied in a condenser cooled down to − 80° by a circulation of methyl chloride, or in some cases by carbonic acid. The apparatus is so arranged, and the flask especially is so devised, that only a minimum of condensed gases is required. Only *one and a half* kilogrammes of ethylene is used in the author's ethylene circulation to obtain the abovementioned permanent liquid oxygen bath, in contradistinction to the great quantities mentioned in the accounts of Dewar's experiments.—The purifying of gases by means of fractionating at low temperatures was also treated, and a modified form of Cailletet's mercury plunger compressor, used specially for this purpose, was described. The author concluded with a few observations on certain investigations and apparatus in course of execution, and intended to be preparatory to the manipulation of liquid hydrogen in the Kryogene Laboratory of the future.

GÖTTINGEN.

Royal Academy of Sciences.—In the *Nachrichten*, part 4 (1894), the following papers of mathematical and physical interest appear:—Fv. Schilling, the fundamental polygon of Schwarz's *s*-function for the case of complex exponents; O. Bolza, a *pu* function for the general hyperelliptic case; R. Dedekind, on ideals (in the theory of numbers); E. Riecke, the equilibrium between a deformed homogeneous solid and liquid in contact with it; E. Riecke, Clausius' condition equation; A. Hurwitz, on the theory of ideals; R. Haussner, the numerical coefficients of Weierstrass's σ-series; G. Pick, invariant processes for higher binary forms; A. Schönflies, the hexagonoid of Eberhard; P. Bachmetjew, results as to electrical earth-currents in Bulgaria; E. Ritter, extension of the Riemann Roch theorem to sets of forms; A. Sommerfeld, mathematical theory of the inflexion of light and electricity; W. Voigt, on piezo-electricity in crystals without a centre of symmetry.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Cod-Liver Oil and Chemistry: Dr. F. A. Möller (Peter Möller).—Mechanics, an Elementary Text-Book, Theoretical and Practical. Dynamics: R. T. Glazebrook (Cambridge University Press).—Colour Vision: Captain W. de W. Abney (Low).—The Student's English Dictionary: Dr. J. Ogilvie, new edition (Blackie).—The Story of the Stars: G. F. Chambers (Newnes).—Universal Electrical Directory, 1895 (Alabaster).—On the Geographical Distribution of Tropical Diseases in Africa: Dr. R. W. Felkin (Edinburgh, Clay).—Philosophy of Mind: Prof. G. T. Ladd (Longmans).

PAMPHLETS.—Le Service Chronométrique a l'Observatoire de Genève, &c.: Prof. R. Gautier (Genève, Aubert Schuchardt).—Origen Poliédrico de las Especies: A. Soria y Mata (Madrid, Establicimiento Tipográfico).

SERIALS.—Engineering Magazine, February (Tucker).—Journal of the Franklin Institute, February (Philadelphia).—Himmel und Erde, February (Berlin, Paetel).—Internationales Archiv für Ethnographie, Band viii. Heft 1 (K. Paul).—Journal of the Anthropological Institute, February (K. Paul).—Journal of the Asiatic Society of Bengal, Vol. lxiii. Part 2. No. 3 (Calcutta).—Astrophysical Journal, February (Wesley).—Journal of the Chemical Society, February (Gurney).

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