THURSDAY, JUNE 23, 1881

LECTURES ON TEACHING

Lectures on Teaching, Delivered in the University of Cambridge during the Lent Term, 1880. By J. G. Fitch, M.A. (Cambridge: University Press, 1881.)

OUR review of this new contribution to the now copious and increasing educational literature of the country has been delayed by causes complimentary to its author. The felicity and charm of the style, the freshness of treatment of even hackneyed topics, and the interest and practicalness of the matter, rendered the reviewer's proverbial dipping into a book impossible in this case, and the work had to be read for its own sake as much as for that of criticism. The author has long been known as one of our most earnest practical and enlightened educationists, and though perhaps not a polemical pioneer in the educational field, an advanced, safe, and healthy thinker on the important problems involved. Of this new utterance of her husband, the *uxor dilectissima,* to whom the book is curiously but most appropriately dedicated, has no reason to be ashamed, even though it is not the newest poem or novel, and only a prosaic, but by no means prosy, volume of "Lectures on Teaching"—a title, by the way, much too modest for the quality of the book, which should in future editions be exchanged for one more worthily distinctive and more expressive of its contents.

The occasion of its production is one of no small interest and importance in the history of educational progress in this country. For some years back, there have been made some laudable attempts to secure for education University recognition and standing, by the appointment of Professors of Education. The first practical effort in this direction was made by the College of Preceptors, which had the merit of appointing, in 1873, the first Professor of Education in Britain, the late enthusiastic and enlightened Joseph Payne. This was followed by the establishment of Education Chairs, in 1874, in the Universities of Edinburgh and St. Andrews. It took some time for our greater conservative Universities to adopt, even in part, such an unwonted innovation, though earnestly and repeatedly pressed to do so in Memorials from the Head-masters of our great Public schools, whom they had trained as scholars but neglected as teachers; leaving them to gain what professional skill they have, as they themselves confess, at the expense of their pupils. At length, in 1879, Cambridge came to the conclusion that it would no longer be derogatory to them to patronise to some extent the new Science of Education; and a "Teachers' Training Syndicate" was appointed, which issued a scheme of examination in the history, theory, and practice of education, with lectureships on these branches of the subject. The first course was given by the well-known genial educationist, Mr. Quick, on its history; the second, by Mr. James Ward, on its science; and the third, by Mr. Fitch, on its practical aspects, which we have now before us. The next step for these Universities to take, which they must—shall we say cordially will?—take before very long, will be to do for education what has been done for other subjects—to give it full University status by the appointment of Professors of Education—a step that will do more than aught yet attempted to give teaching and teachers the standing, influence, and emolument to which the importance of their work to national culture and progress justly entitle them from the lowest to the highest; and a step that will more than repay the Universities themselves, which would thus be entering, as Mr. Fitch well observes, "on an honourable and most promising field of public usefulness," that will help to "make the work of honest learning and of noble teaching simpler, more effective, and more delightful to the coming generations."

Though deprecating any claim to be a systematic treatise, or a "manual of method" on the subject, the book traverses the greater part of the general field of inquiry, and to the practical student and teacher will be found more helpful, suggestive, and scientific than more elaborate and pretentious text-books. The work is singularly readable, attractive, sound, sensible, and practical, and altogether free from hobby-horsiness. Though subordinating theoretical treatment, Mr. Fitch is, as a rule, scientific in spirit and suggestion; and when discussing, as he does, controverted questions that generally rouse polemical combativeness, if not bitterness, he does this with so much of the *suaviter,* and with such genial persuasiveness in favour of his conclusions, which are for the most part sound and abreast of recent opinion, that the book is calculated, in an unusual degree, to carry enlightened conviction on many problems into the conservative ranks of University men, still too impervious to change in their traditional views of education. Canvassing, as Mr. Fitch does, so many subjects of controversy, it cannot be expected that his conclusions will be generally accepted—on not a few we deem him in error, and should be prepared to join issue; but they are always so presented as to command, and to gain, the most careful consideration and to disarm opposition—an invaluable element with the class he specially addresses.

The mere headings of the chapters show the interest and extent of the field traversed. We have "The Teacher and his Assistants," in which an inspiring, noble, but far from Utopian, ideal is held up for imitation: "The School, its Aims and Organisation," in which such important questions, as what a liberal education is, what subjects should be taught, and the like, are discussed: "The School-room and its Appliances," where excellent practical suggestions are given towards making our schools the healthy, comely, and educative centres they ought to be in any wise community: "Discipline," in which this vital element in school life is treated with admirable spirit and wise counsel, traversing the various disciplinary influences that ought to be brought to bear on the child, before the *ultima ratio* of corporal punishment is resorted to, which, though not condemning it altogether, he wisely thinks "is almost wholly unnecessary, does more harm than good, and in just the proportion in which teachers understand their business, they will learn to dispense with": "Learning and Remembering," where some sensible practical hints are given as to the use and abuse of that universal school hack, the memory, the treatment of which he rightly considers a very good test of "the difference between skilled and unskilled teaching," though his psychology of this so-called faculty is questionable, savouring too much of the orthodox Hamiltonian school, and too little of the, at present despised, phrenological, which latter considers memory a mode of action of all the intellectual faculties, and which, on this point at least, contains more truth and practical suggestion than is generally suspected: "Examining," in which the elements of skilful questioning are well put, and not a few fresh suggestions made, as in regard to the use of "Socratic questioning" in school, on which, however, we recommend to the author's consideration, the views of his friend, William Ellis, the liberal and philanthropic founder of the Birkbeck Schools, who was the first to adopt and advocate it in its entirety, making it a true Socratic *dialogue, the pupil also stating difficulties and asking questions of the teacher*, as well as answering questions asked: the "Preparatory Training" of the child, in which much sound practical advice is conveyed as to the early stages of the "three R's" and their congeners, but in which, amidst much appreciation of the Kindergarten, this system is on many points—we are sure unconsciously—greatly misunderstood and misrepresented; as that Fröbel did not take "a large, or very sound, view of the purpose of education as a whole," that the system "does little or nothing to encourage reflection," is "apt to mistake means for ends," "does not train to overcome difficulties," and much else in the same strain, regarding which, we may safely and confidently leave Mr. Fitch to Miss Shireff and other wise and fully-informed *Kindergartener:* "The Study of Languages," in which the vexed question of the place of the ancient classics and modern languages in education is very fairly and estimatively stated, and recent broader views advocated, a chapter that would be studied with advantage by both parties in this wordy strife: "The English Language," where he claims a very high—we should be inclined to claim even a higher—place for the native tongue and literature, in intellectual, cultural, and general educative power, when rightly taught, and gives some excellent practical suggestions to help towards this, his conclusion being admirably expressed thus—"If your scholars do not acquire a positive love for reading; if they do not ask to be allowed to read the whole book or poem of which the extract you take as a lesson forms a part; . . . if they do not feel a heightened admiration for what is noblest and truest in literature, and an increasing distaste for what is poor and flimsy and sensational: then be sure that there must be something incurably wrong in your method of teaching, and that all your apparatus of grammar, paraphrase, and logical and grammatical analysis, will have failed to fulfil its purpose."

Then we have two chapters on "Arithmetic," both as an art and a science, on which, as might be expected from the author of a well-known text-book on the subject, he places high value, for both culture and use, and on which he gives very good hints; on "Geography" and "History" he is equally fresh, suggestive, and practical; and the book concludes with two very good chapters on the teaching of "Natural Science" and on "The Correlation of Studies."

On the place of science and scientific teaching in all true education Mr. Fitch speaks with his accustomed candour, fairness, and perspicuity, and pleads in their favour with a quiet but firm and skilled advocacy, which, with its genial non-polemical incisiveness and force, makes it a real acquisition to the growing literature on the scientific side, which will carry conviction into certain scholastic circles that would be, as they have been, deaf to more formal and strenuous pleading. Here Mr. Fitch reveals himself with unwonted power as a skilled, Socratic, but disguised polemic, in the advancing cause of science in education. His way of making the scientist in education put his case "to those who live in the academic world," is admirable: "You are mistaken in supposing that the domain of physical science is a merely material and practical region, while yours is essentially intellectual. There is here a body of truth of the highest practical utility, no doubt, but also of the greatest value for educational purposes. The laws and principles by which the facts of the material world may be explained and co-ordinated, are quite as uniform, quite as beautiful, and as far-reaching in their applications, as any of the laws of language or the truths of mathematics. Moreover, the processes of thought required in the study of these questions are just as vigorous, just as stimulating, stand in just as close a relation to the intellectual needs of a well-instructed man, as those involved in the older studies. You can make the teaching of physical science as fruitful, as thoroughly disciplinal for all the higher purposes contemplated in a liberal education, as the teaching of Greek or of geometry, if you will only first recognise the possibility of making it so, if you will encourage skilled and accomplished men to take up this branch of instruction, and are ready to give them the same status and encouragement as you now give to accomplished teachers of philology or history. Enlarge your conception of what a liberal education means." Mr. Fitch concludes that "of the legitimacy of these claims there can be no doubt," and wishes "some Huxley or Tyndall had enunciated this message before we ourselves went to school." He shortly discusses the utilities of physical truths, their beauty and intellectual attractiveness; the disciplinal value of the inductive process by which they are discovered, and its function as an invaluable corrective and necessary supplement to the one-sided deductive method of the common scholastic studies. His recommendations on the practical teaching of science in schools, and on technical education, are also good, and to the point. Altogether, this chapter on Science deserves perusal by all interested in its teaching, and we wish our space allowed us to take his arguments up in detail.

The last chapter, on "The Correlation of Studies," is an important one, and in great part sound and sensible, though not a few will be inclined to disagree with the author on some points; for the problem of "the conflict of studies" is as yet more crude and unsettled than any other in the educational world. He wisely controverts the plausible maxim, *non multa, sed multum*. His division of the proportionate times that should be given to the different classes of studies, viz. "nearly half to language and literature and subsidiary exercises, and of the remaining half, rather the larger portion to mathematics, and the smaller to experimental science," will provoke controversy, and is surprising in an advocate of science in schools. He would also have never more than two physical sciences studied at the same time. His remarks on the principle of selection amidst the increasing press of studies should be serviceable to distracted teachers, and help them to a choice; though here, also, he leans overmuch to the linguistic side.

In the curriculum of studies recommended by him, Mr. Fitch has strangely subordinated, if he has not greatly ignored, the studies that prepare the citizen for his duties as a member of the State—the Social and Political Sciences. Surely he has not read, or greatly studied, the admirable pleading in their favour to be found in the works of his friend William Ellis, and also of George Combe, as recently fully presented in his educational contributions, edited by one of his colleagues in the inspectorate. In this respect, there appears to us to exist a serious hiatus in his plan of study. One of the doctrines he enunciates is also enough to raise the old phrenologist from his grave, what he calls "the convertibility of intellectual forces," whereby, he informs us, "every kind of mental power, once worked and applied to a worthy purpose, becomes available for other purposes, and *is capable of being transformed into power of another kind*"—an ancient error in schools, still fruitful of failure and wrong, which we are surprised to find held by a man so generally wise on education. Poor George Combe otherwise fares sadly at the hands of Mr. Fitch, who describes his mission in life as being that of advocating the one doctrine of inherent hereditary aptitudes, and says that "he never could induce his friends seriously to attempt the classification and teaching of a school on his principles, and the experiment yet remains untried"! What of the history of such schools given in the work on Combe's principles just named?

Though not agreeing on many points with the author, as was inevitable in a field so full of controversial matter as the growing science of education, we look upon the book as a valuable contribution to the subject, which, by its unusually attractive style and high tone, will command a wide audience, and, from the auspices under which it is produced, will reach places where sound educational philosophy too seldom penetrates. We cordially recommend it to all interested in education, and specially to teachers; and also to the active Education Society, as, like Prof. Bain's recent work on Education, which they have already taken up, an admirable basis of profitable discussion.

PRACTICAL HISTOLOGY

A Text-Book of Practical Histology, with Outline Plates. By W. Stirling, M.D., Sc.D., F.R.S.E., Regius Professor of the Institutes of Medicine in the University of Aberdeen. (London: Smith, Elder and Co., 1881.)

AT the outset Dr. Stirling informs us that "the purpose of this work is twofold: first, to give plain, definite, and precise directions for the preparation and examination of the animal tissues; and secondly, to ensure that the student executes a drawing of the majority of the microscopic specimens which he makes for preservation. For this purpose a series of Outline Plates is issued with the text."

As regards the first of these objects, there is no doubt that to give "plain, definite, and precise directions" is a desirable and praiseworthy object, which most, if not all "practical" books strive to attain. Those that succeed in this endeavour differ from one another chiefly in the means by which this object is accomplished; in some the author arrives at his object after long-continued patient and diligent work, in other rarer instances he utilises the works of others, and by doing so he may, and sometimes actually does, produce a book which has considerable merits of its own, inasmuch as it gives in plain and simple words valuable and useful extracts of much larger original works full of minute and bewildering details, not easily understood by, and of little practical use to the ordinary student. Dr. Stirling, although his book cannot in any sense claim to be considered other than a book of compilation, has nevertheless succeeded in presenting to the medical student, anxious to acquire the necessary amount of knowledge in practical histology, a work which, conveying in a short and intelligible manner a great deal of information, will, we doubt not, prove of service.

As regards the second object of the book, viz. that the student should for himself make drawings of his microscopic specimens, we fail to see how Dr. Stirling's Outline Plates can advance this object in a satisfactory manner. We always thought that the student drawing the correct outlines of the specimens or of parts of the specimens prepared by himself, has got everything that is essential to guide him in the study of those specimens. To fill in the details in pencil, or, as Dr. Stirling suggests, and what is still more laborious, in colours, in the outline figures drawn for him from somebody else's specimens, appears to us of more than questionable value.

Besides the directions for practical work a considerable part of the book is taken up by the description of the structure of the simple tissues and organs. As far as we can see, these descriptions are in a great measure, to the extent of verbal quotations, borrowed from other books, without even an attempt to mention this fact; by doing so Dr. Stirling has deviated from the accustomed rule, and has proceeded in a rare and unexpected manner. If an author introduces abstracts and verbal quotations from any other work, we believe it will be universally admitted that whatever the aim and nature of the book, the author is bound to mention his source; if he omits to do this, intentionally or unintentionally, he lays himself open to the charge of having committed what in the eyes of every right-thinking man, not to say of every man of science and teacher in a responsible position, must ever be considered a grave offence.

Dr. Stirling has made very extensive use of the "Atlas of Histology" in some chapters, *e.g.* on the salivary glands, the kidneys, the generative organs, and others, making copious extracts therefrom, to the extent of verbal quotations, without in any way indicating that he has done so.

Dr. Stirling's proceeding is greatly to be regretted, since by his numerous and original works in histology he has won the esteem of his *confrères* and has proved himself to be sincerely anxious about the promotion of this science.

The publishers deserve great credit for the handsome style in which the book is brought out.

E. KLEIN

LETTERS TO THE EDITOR

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

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

The Electric Railway in Paris

I HAVE within the last few days received a letter from a friend in Paris, who writes that he had last week travelled on the electric railway in that city. There is still much, he adds, to be done before it can be brought into general use; but nevertheless the train moved satisfactorily. There were fifty-four passengers in the carriage, which was propelled by a large Gramme machine and 160 cells of Faure's battery. The experiments are to be recommenced very shortly with a new motor by M. de Méritens, and a Faure's battery. W. SPOTTISWOODE

41, Grosvenor Place, S.W., June 16

Probably New Variable and Red Star

ON May 22 I found, 2° 51′·7 north of α Cygni, a deep red or crimson star, which is not in the Bonn Catalogue. The nearest to it there is + 47° 3167, which in declination corresponds with a white star that I observed at the same time, but not in R.A.

Dr. Doberck writes to me as follows:—"Markree Observatory, 1881, May 29; observed the *new* star; brick-red, the nicest I ever saw, 8·7 mag."; and "1881, May 31: 1h. a.m., colour same as before; 8·4 m."

Mr. Ward of Belfast, who observed in the early morning of May 31 in strong twilight, describes the star as "deep crimson; beautiful object; two or three comites."

Mr. Gledhill, in Mr. Crossley's observatory, Halifax, found it, on May 30, "strikingly red."

Dr. Ball, the Astronomer-Royal for Ireland, observing at Dunsink, saw it "a superb crimson."

Prof. Krueger, director of the Kiel Observatory, described it as "*auffallend roth*" (remarkably red), on May 30.

On June 2 it appeared to me unchanged in colour, and increased from 9 mag. to 8·4.—June 7 and 8, colour still the same, and 8·3 or 8·4 magnitude.

Dr. H. Kreutz, writing from the Bonn Observatory, states that he finds an observation of the star recorded on June 19, 1857, but not at any other time during the progress of the observations for the Bonn Catalogue, in which it has not been published. There does not appear to be any note of its colour, and I think it will most likely prove a variable of a very remarkable character. Prof. Krueger makes its position for 1855 = α 20h. 36m. 37s.·9; δ 47° 37′ 33″. Herr Kreutz's position is α 20h. 36m. 37s.·0; δ 47° 37′·9. The white star mentioned above is not recorded in any of the Bonn observations; and, on the other hand, I may add that I do not identify + 47° 3167 in the telescope. I estimated the white star at about 9·5 mag. of Argeländer's scale, and therefore within the limits of the *Durchmusterung*.

The small stars seen by Mr. Ward are perhaps too distant to be strictly considered as comites to the red star. They are sufficiently difficult to me, though probably easy to his well-known extraordinary sight. The position of the nearest that I see is about 0°, and I find two others more distant—one at 350°, and one at 110°, with a power of 120° on a 4½ inch O.G.

There seems a peculiar dimness about the star, referable, probably, to the dark shade of its red. An uneducated person with a very excellent eye, and who never heard a description of a red star, compared it, at first view, to "a drop of *black* blood." It may be conveniently and well compared with Nos. 448 and 553 of my "Red Star Catalogue," especially with the former, the colour of which was described by Secchi as "intense"; and in the glowing red of the one object will be remarked a striking contrast with the deep sombre tint of the other.

I make the approximate positions of the red and the white stars for 1855, and corrected from my first observations, as follows:—

	h. m. s.	° ′
The red	20 36 27;	+ 47 37·5
The white...	20 36 18;	+ 47 46·8
Argeländer's position of his + 47° 3167	20 36 28;	+ 47 46·8

Millbrook, Tuam, June 3 JOHN BIRMINGHAM

The Doctrine of the Conservation of Electricity

I WISH to take the earliest opportunity of responding to the courteous letter of M. Lippmann, which appears in the current issue of NATURE, with the acknowledgment that his quotation from the *Comptes rendus* of 1876 establishes in the most conclusive manner his priority of date in the enunciation of the doctrine of the *Conservation of Electricity*. As to my own independent enunciation of this doctrine, it was arrived at without any knowledge of the comparison drawn by M. Lippmann in 1876 between the cyclical flow of heat (of Carnot's theorem) and the cyclical flow of electricity. I approached the matter upon somewhat different and less clearly defined lines, and finally struck upon the fundamental notion of the Conservation of Electricity when endeavouring to think out the relations between electromotive and ponderomotive force in an electric theory of radiation based upon Clerk-Maxwell's Electromagnetic Theory of Light. My speculations on this point were committed to writing some weeks ago, and will shortly be published. I content myself in the meantime with pointing out how near Clerk-Maxwell came to a similar conclusion. In Article 35 of his well-known treatise, he says emphatically: "While admitting electricity, as we have now done, to the rank of a physical quantity, we must not too hastily assume that it is, or is not, a substance, or that it is, or is not, a form of energy, or that it belongs to any known category of physical quantities. All that we have hitherto proved is that *it cannot be created or annihilated*" (the italics are mine). Nevertheless the immediate and logical conclusion that electricity, like matter and like energy, is subject to a law of conservation, appears to have been rejected by Clerk-Maxwell for reasons explained in Article 574 of his treatise, consequent on his inability to discover whether an electric current possessed momentum or could exert a mechanical reaction upon the matter of the conductor through which it flows. The unfortunate dilemma which suggested this experiment could hardly have been raised if it had then been as clearly understood as it now is that there is the same distinction between electrokinetic and ponderokinetic energy as between electromotive and ponderomotive force. But to discuss this matter further would lead me to take up too much space.

SILVANUS P. THOMPSON

University College, Bristol, June 19

Thought-Reading

IT would seem that the "discovery" of reading people's thoughts, lately mentioned in the daily papers, is in no way essentially different from the well-known "game" of "wishing" often played by young ladies. It consists of the following procedure. One person goes out of the room, while others arrange upon what she is to do. She enters blindfolded, and in the particular instance now alluded to, was turned round several times so as to be quite unconscious of the direction in which she was facing. Two persons now place their hands on either side of each shoulder, making their fingers meet at the back of the neck and under the chin; or they may be placed round the waist; but as the forehead appears to be equally sensitive, perhaps it is immaterial where the hands be situated. After standing still a moment or two, the lady moved slowly round in the direction of a sofa under the impression, as she afterwards said, that she was walking in quite another way. Having reached it, she sat down (not even knowing the sofa was close by), and deliberately put out her hand, took up an antimacassar which lay upon the sofa, and raised it, asking, "Is this what I was to do?" This was perfectly correct, the antimacassar having been expressly laid there for the purpose.

It was settled that another lady should walk into the conservatory. To do this she had to pull up a blind, lift an iron bar and open the shutters, then undo the glass door behind them which led into the conservatory. All this she did unhesitatingly, and walked straight into it. I could describe several other instances where ornaments and other things had to be selected out of various groups of objects, &c.: but the above will illustrate the process.

One essential condition of success is that the individual must voluntarily and entirely surrender the will, while those who hold the person blindfolded must *determine* as powerfully as they can that the latter shall do what they wish. Care should be taken not to *push* the individual in the desired direction. This however may be done involuntarily, but it will not account for the person doing all that has been previously determined after arriving at the spot. Some ladies describe a remarkable sensation accompanying the process: a sort of "all-overishness," or even faintness, so much so that the lady first alluded to on one occasion staggered, and could not proceed at all.

It appears to me to be a very peculiar psychological phenomenon well worthy of investigation, if possible, but too well known to be disputed. GEORGE HENSLOW

6, Titchfield Terrace, Regent's Park, N.W.

P.S.—Since writing the above I have heard of a much more remarkable case than the preceding. The operators sat in a circle, silent, but determinately "willing" that a certain lady should do what they had resolved upon. She stood in the centre, and was not blindfolded or touched by any one. In every case she did it correctly. One thing that was agreed upon was for her to take a bottle of wine from one table, carry it to another, and pour out a definite quantity of wine. This she did, not exceeding the amount predetermined. On a second occasion she had to find a key hidden away behind some books. As she approached the place she became very excited and hysterical, but at once extracted the key.

The above cases clearly show that as far as they are concerned "thought reading" is an incorrect expression, as the person operated upon is a passive automaton, while others, as it were, force their wills upon her. "Will-imparting" would seem to be a better term.—G. H.

Notes of the Cuckoo

IN a letter appearing in NATURE, vol. xxii. p. 76, I stated that—"All the cuckoos here intone in a minor key except one, which alone does not flatten the 3rd of the tonic. The key is in all cases precisely D of concert pitch, as proved by a tuning-fork, and the first note is F on the fifth line." This year I find that, while the cuckoos here generally intone in D minor, as above, there is one again that intones in D major, and two others in C major and C minor respectively. Some that I casually heard in other places in the neighbourhood intoned in D minor.

Millbrook, Tuam, June 1 JOHN BIRMINGHAM

Notes on the Indian Glow-Fly

HAVING failed to find any critical description of these interesting insects, it is possible that the notes I am now able to send you may cause others to enter the field of inquiry. Situated some 2900 feet above the sea, and in Central Southern India amidst hills, valleys, and streams, I have had peculiar opportunities for observing them.

They are not to be seen during the daytime, but so soon as darkness steals upon twilight, so surely do these small natural lanterns become visible, and their numbers rapidly increase, much indeed as the visibility of the stars increases as the evening passes into midnight.

The fire-fly, when examined individually, is by no means a pretty-looking insect, and comparing it to other insects and flies, it is certainly both large and ungainly. An ordinary house fly is five-sixteenths of an inch in total length and weighs ·25 grains, but the subject of my notice has a total length of nine-sixteenths of an inch and weighs ·66 of a grain; we thus at once learn something as to his size and weight. The glow-fly—or beetle as I should term it—has a black head and antennæ; the thorax and abdomen are of a yellow-red colour. This latter part of the insect's body is divided into six rings, and, counting from the thorax, it is the fourth ring that emits the light. There is a rectangular opening in this ring which is merely covered by a very thin skin; it is in fact a *window* from which the light emerges. The insect has only one pair of wings: these are small, most delicate and thin, and are sheathed. It is worthy of careful notice that these insects fly both rapidly and slowly, but make no noise or buzz in the air. To test this further I have frequently liberated several of these glow-beetles in my bedroom, and in the dark they have only appeared as *fairy stars*, as no humming could be detected.

As regards the character and quantity of the light, I have to observe that one insect enables me to see the time by a white-faced watch when four inches distant; twelve of them placed in a glass jar enable me to read a book with ease, and are equal to a small Geissler's tube. The light is of an exceedingly beautiful colour—a sombre yellow tinged with green, but at intervals it is brilliant. A preliminary examination of the light in the spectroscope (a large one made for me by Browning) shows a distinct clear continuous spectrum, no lines or bands of any kind being visible.

The insect made to crawl on a card placed over the poles of a powerful compound permanent magnet showed no signs of uneasiness or change of light. Similarly placed over an electro-magnet (ten Grove cells) and rapidly alternating the current caused no change. Placed within a coil of covered wire, no change. Blowing very gently, my breath on the insect caused no change; this was also tried with a blowpipe. Cold air at 50° caused a distinct diminution of the light; on the other hand, air at 100° caused an increase of light.

I now placed several of the insects in a bell-jar, and gave them a good supply of clean oxygen gas; the luminosity at once increased fully 25 per cent. On a dead insect (which still sheds light) oxygen gave similar results, and on extracting the luminous part and blowing oxygen upon it the light was much increased.

It will prove interesting to mention that, so soon as darkness has fairly set in, millions of these insects invade the trees, and as my bungalow is near to a stream and level with the tops of the trees, I am able to notice them with much care. The curious pulsation or flashing of their light is remarkable: the insects resting on the tree all act in perfect concert, *i.e.* five seconds of no light, then seven rapid flashes; five seconds, no light, seven flashes; and so the game continues throughout the dark hours.

At first I had reason to believe that the insect when flying only emitted light; this however is not the case; for when observing the Pole Star for variation with my theodolite, it occurred to me during a passing cloud to turn a telescope on to the glowing tree. At once I had the field of view filled with tiny stars, but both fixed and wandering.

It is also worthy of special notice that all the glow-insects on a dozen or more trees will continue to keep up the most perfect time as to the flashing of their light and the interval of pause, and this for many consecutive hours; but this singular agreement as to the time relates to close clusters of trees only. Thus distinct groups of trees separated by one or more hundred yards may not agree, and do not do so as a rule.

I have been informed on safe authority that the Indian bottle-bird protects his nest at night by sticking several of these glow-beetles around the entrance by means of clay; and only a few days back an intimate friend of my own was watching three rats on a roof rafter of his bungalow when a glow-fly lodged very close to them; the rats immediately scampered off.

In conclusion, these insects see by day as well as by night, and I incline to the idea that the beautiful light they carry serves as a means of intimidation or protection, and certainly as a means whereby to recognise friends.

As I gaze from my verandah down the Nadgani Valley into the dark night I see the pulsations of light here, there, and everywhere! and as my optical powers increase so do these gaseous, nebulous patches become resolved into real living stars! H. A. SEVERN

Wynaad, India, May 5

Birds Suffering from Cold

THE unusually severe weather (5° – 12° Réaumur) of these last twelve days struck heavily on the swallows of our country. They have been found dead by hundreds. The distress of the poor animals must have been extreme. Suffering from hunger and cold, they pressed against the windows, and being brought in suffered to be petted and fed, but died from exhaustion. In Kopidlno about 300 took shelter under a balcony, and the cold growing more and more intense towards night, they clustered on each other like bees until morning, when thirty were found dead. I have been walking this afternoon in the suburbs of Prague, where a fortnight ago I have seen swallows skipping on the river and hunting in gardens, but although the weather was now clear and warm, I could not see a single one. Tidings of suffering swallows come from the country, where people have been kind to them, feeding them on ants' eggs and flies, but they would not eat, and died. In some nests the young ones were found starved alone, in others their mothers were with them. J. V. SLÁDEK

Prague, June 16

An Optical Illusion

THE illusion of the inverted pin was shown me about the year 1846-47, and I well remember, when I was at Cambridge, working out the explanation inductively. In the autumn of 1847 I was spending an evening with Dr. P. M. Roget, at his house in Woburn Square, when among other subjects we conversed upon was that of optical illusions. The inverted pin was one of his illustrations, and I think he mentioned having explained it in some scientific serial.

Some years ago the late Mr. Becker, formerly scientific foreman to Messrs. Elliot Brothers, constructed for me a binocular apparatus for showing the union of two shadows, one on each retina. To my surprise I found the resulting phantom did not differ in position from the single shadow. C. M. INGLEBY

Athenæum Club

How to Prevent Drowning

I ONLY write in the interests of humanity. Let those who will go in for swimming, and I wish sincerely that every one could swim. Treading water however conducts at once to swimming. Every one can tread water who likes. It is just as easy, if we only knew it, to tread water as to tread the earth, and proximately just as safe. Men and women might walk into the deep sea and out again when they pleased. Nature has not been so niggard with us as some persons imagine. Why are we not as safe in water as is the dog? It is simply because he treads water, and we do not. As often as I chose to chuck my stick into the Causey surge my dog brought it out. I could have done the same; any one could do the same who chose. But assuredly I should have paddled water as the dog did. In treading water the body is erect, or nearly so; in swimming we sprawl, and are comparatively helpless. The admirals, both of them, have given valuable testimony as regards the efficacy of treading water. Before the present pier at the Cape was built, vessels in bad weather could not communicate with the shore, even by boats. Men, then, treading water amid the mountain seas, carried communications to and fro in oilskin caps. I have heard it was the same at Madras. Young Gordon, apprentice to the sea, fell into mid-ocean while fisting a sail. The poor fellow's heart sank when he saw the ship sailing away. But, as he afterwards told me, he trod water, and kept up till the boat reached him. I have trodden water again and again with a big boy on my back. Any one might do the same. Not one woman in ten thousand, not one man in a thousand, I suppose, can swim. They do not know they can tread water when they fall in, and of course drown, as two fine young women who had got a little out of their depth in this place did last year. But ignorance and prejudice cannot always rule, and the day will surely come when human beings, better instructed, shall enjoy the same immunity in the water that other animals not human beings, now enjoy. HENRY MACCORMAC

Bournemouth, June

Buoyancy of Bodies in Water

A propos of the question of drowning, as the same is now raised in NATURE, and especially so as to the alleged "fact that men are very different in buoyancy," allow me to say that when stationed many years ago at Pembroke Dock, South Wales, two soldiers were drowned there within a few days of each other. One of these casualties occurred off an island named the Stack Rock, in Milford Haven, that was garrisoned by invalided artillery, while the other took place in the creek that separates the town and dockyard from the huts. In the former instance the body of the (drowned) man remained floating upright in the water, "bobbing up and down with every wave"—as an eye-witness assured me—for a considerable time, or until it was lost to sight or recovered (I forget which just now). In the latter the body—that of a healthy, muscular man—was picked up a day or so afterwards by a passing boat as it was floating out with the tide to sea; and I have since seen several fresh bodies floating in the Ganges. Indeed the survivors always attach weights to the remains of even the poorest of their kindred ere they deposit them in that sacred stream; but this may be for the purpose of counteracting the current; and it is, I think, generally assumed in books and courts of law that all bodies, human and bestial, sink as a rule in water as soon as life is extinct; in other words, it is stated that they remain submerged till decomposition sets in, or sets up such an amount of gas within them as enables them to overcome all resistance from above, and float. If such be the case we must either suppose that the corpses referred to within possessed some special attributes of their own, or that "men are very different in buoyancy" after death than they were during life. Assuredly these men could not have been lost in this way had their bodies been able to float in the one state as well as they were in the other; and I heartily agree with Mr. Hill when he says that "no amount of coolness or presence of mind will either supersede the art of swimming or alter the laws of gravity."

Ashton-under-Lyne W. CURRAN

Resonance of the Mouth-Cavity

THE observation of Mr. John Naylor, forwarded to you by Mr. Sedley Taylor (p. 100), admits of being made with more striking (because louder) results than by the method described, and so far from being a "discovery," is well known to most schoolboys. Tap with the thumb-nail upon the front teeth, and at each tap alter the shape of the mouth-cavity so as to produce the note desired; any tune may then be played loud enough to be heard at the other end of a large room. It is remarkable that without previous practice one instinctively shapes the mouth-cavity so as to produce, in almost every case, the exact note required. GEORGE J. ROMANES

Thunder Storm at the Cape

A YOUNG man of my acquaintance, who some time ago joined the Cape Mounted Rifles, has just forwarded to me an account of a severe storm which occurred on the evening of Thursday last, April 14. C. TOMLINSON

Highgate, N., June 13

"The storm set in about 6 p.m., whilst the men were at stables, and was accompanied by loud thunder and vivid flashes of lightning. At 6.15 there was a fearful roll of thunder, accompanied by a most vivid flash, which lit up the square for at least thirty seconds. It struck the barracks at the upper end, ran past a room to the stables, which have iron roofs; it ran along the course of the roofs into the stables, striking down two men in the doorway. It then ran along the iron of the manger, flooring all the horses, nineteen in all, and so went to ground. One man was struck in the left shoulder bone, the fluid passing from there under the left arm to his watch in the left-hand trousers pocket, and burnt a hole clean through the silver case. From the watch it struck again six inches below, and travelled round the leg under the knee, and from thence probably to the spurs, as no burn was found below the knee. The extremities of both tracks were marked by large burns, and each track by a burn two inches over. The surgeon says it was the most miraculous escape he ever saw, the watch having saved the man's life. The second man was merely stunned, and lost the use of his legs for some hours; he was standing in the stable behind the first, and although only slightly burnt, is still unable to walk. The other is doing well, but is rather dazed. Ten other men were floored, but soon regained their legs. As to the horses, one was struck dead in the forehead: two others, blind in both eyes, were shot yesterday; and four more blind in one eye are condemned. A horse in town was struck, and his fore-leg broken in four places.

"Within a hundred yards of the barracks is a powder magazine full of powder, fitted with conductors which were struck several times. This occasioned great alarm to the inhabitants, as it contains many tons of powder.

"JOHN P. CUNNINGHAM

"King William's Town, South Africa, April 18"

A Six-Fingered Family

IT may interest some of your readers to hear that there is at present living in Brown's Town, Jamaica, a family in whom the possession of six fingers has been hereditary for at least four generations. Unfortunately they consider the sixth finger a deformity, and always amputate it, so that there is very little opportunity of observing it. There is a little girl there however upon whom this operation has not been performed, and I much regret that, as her parents had taken her up into the hills to work in their provision grounds, I could not see her. As I am informed, the sixth finger springs from the little finger knuckle at right angles to the little finger, and when it is free of it, it turns up parallel to the rest, being a little shorter than the little finger, but quite perfect, with nail and two joints. It is bent and extended with the rest on opening or closing the fist.

Another fact, which I daresay however is usual in such cases, came under my notice at Brown's Town, viz. two perfectly black parents having a family all pure albinos.

Kingston, Jamaica, May 26 THOMAS CAPPER

Singular Behaviour of a Squirrel

A NEIGHBOUR of mine, whose cottage is thickly surrounded with trees, observed a squirrel, during the severe weather of winter, occasionally stealing food from the troughs set out for the poultry. At first it caused great commotion among the birds, but latterly they were less uneasy in its presence. Taking an interest in the wild creature he began to lay out refuse food for it, including bits of ham, which it greedily appropriated. Getting more courageous, it ventured within doors. After a time it got caught in a trap set for rats underneath the bed. Being freed from its irksome position it was thought that the squirrel would venture no more within doors. Neither the incident of the trap nor confinement for some time within a cage availed to restore to it its original shyness. With the coming of summer its visits have been less regular, but occasionally it looks in still. May not a habit like this, affecting only one out of many, be looked upon as corresponding to a "sport" in the vegetable world, and shed some light on the subject of the domestication of animals? The squirrel seems to have been quite a wild one to start with, for there is no one in the district who had been in the habit of keeping one as a pet. J. SHAW

Dumfriesshire

Hot Ice

IN reply to a very interesting letter on this subject recently published in NATURE (vol. xxiii. p. 504) by Dr. Oliver J. Lodge, I wish to express my views of the theoretical and practical possibility of the experiment of Dr. Carnelley. I wish to start from some well-known principles accepted by everybody acquainted with the mechanical theory of heat and its applications. According to these principles the volume "v" (and also the total amount of internal energy) of water can be expressed as a function of its pressure "p" and temperature "t"; $v = f(p, t)$. The form of this function, which we need not discuss here, will change with the state of aggregation, so that we shall have three different equations expressing the volumes of water in the solid, liquid, and gaseous form.

$$\left.\begin{array}{l} v = f_{\text{i}}\,(p, t) \;\ldots\;\ldots\; \text{ice} \\ v = f_{\text{ii}}\,(p, t) \;\ldots\;\ldots\; \text{water} \\ v = f_{\text{iii}}\,(p, t) \;\ldots\;\ldots\; \text{vapour} \end{array}\right\} \begin{array}{l} p \text{ and } t \text{ being considered inde-} \\ \text{pendent variables.} \end{array}$$

Geometrically the volumes of ice, water, and vapour will belong to three different surfaces extending between certain limits. Thus the surface $v = f_{\text{i}}(p, t)$, which represents the volumes of ordinary ice, is situated between the limits $q\,p$, $l\,m$, $m\,d$; the surface representing liquid water lies between $m\,n$ and $m\,d$, though it may be extended a little on either side of these limits, if it applies to water heated or cooled over its regular boiling or freezing temperatures, which are situated along the lines $m\,d$ and $m\,n$.[1] The values of p and t, which belong to $m\,d$ and $m\,n$, will satisfy two equations—$\phi(p, t) = 0$ and $\psi(p, t) = 0$. At these points the water will change its form of aggregation and pass over in the state of saturated vapour along the line $m\,n$ [equation $\psi(p, t) = 0$], or into ice along $m\,d$ [equation $\phi(p, t) = 0$] in a continuous and *reversible* way. At any other point, which is not situated on $m\,n$ or $m\,d$, water may also be liable to change of aggregation, but this process will not be *reversible*. The line $m\,n$, where the surface $v = f_{\text{ii}}(p, t)$ breaks up and liquid water changes into vapour, is the *curve of tension of saturated vapour* contained in the renowned table of Regnault. The boiling-points of water under varying pressure are situated on $m\,n$, and may be found by solving the equation $\psi(p, t) = 0$. At the point m ($p = 4{\cdot}6$ mm., $t = -0^\circ{\cdot}0078$ C.) the line $m\,n$ terminates, but is continued by $l\,m$ [equation $\chi(p, t) = 0$], along which the vaporisation of ice takes place in a reversible way. According to the table of Regnault there is no sudden rupture at the point m, the pressure of saturated vapour at 0° C. being identically the same, if the vapour is in contact with water or with ice. The differential coefficients $\frac{dp}{dt}$ of the functions $\phi(p, t)$, $\psi(p, t)$, and $\chi(p, t)$, or the tangents to the lines $m\,d$, $m\,n$, and $m\,l$ are found by application of Carnot's Theorem to be of the general form $\frac{dp}{dt} = \frac{A\,r}{[s - s_1][273 + t]}$ [r = latent heat; s and s_1 = the specific volumes of water in two different forms of aggregation].

The point m, where $m\,n$, $m\,d$, and $m\,l$ unite, is of particular interest. J. Thomson called it "the triple point," and Guldberg the "Fällespunkt" of water. Lately (in *Berichte*, 1880) I ventured to call it the "absolute point of sublimation," not because I wished to introduce a new term for a well-known scientific object, but only to point out some important consequences of the phenomenon just then announced by Carnelley, of which Prof. Lothar Meyer of Tübingen had published an interpretation different from mine. This point m, situated $-0^\circ{\cdot}0078$ C. below the ordinary freezing-point of water, is really the upper limit of sublimation, because at any higher temperature ice first changes into water before it evaporates. At $-0^\circ{\cdot}0078$ C., where the boiling- and melting-point of water coincide, a real sublimation of ice begins, provided that the barometric pressure does not exceed 4·6 mm. (= "the critical pressure" of Carnelley).

Now according to the discovery of Dr. Carnelley, ice at pressures lower than 4·6 mm. would exist by temperatures up to $+178^\circ$ C. Thus the surface $v = f_{\text{i}}(p, t)$, which we have hitherto supposed to be inclosed between the limits $q\,p$, $q\,l$, $l\,m$, $m\,d$ would extend far beyond $l\,m$ nearly up to k, but always at pressures smaller than 4·6 mm. Geometrically this new and unforeseen

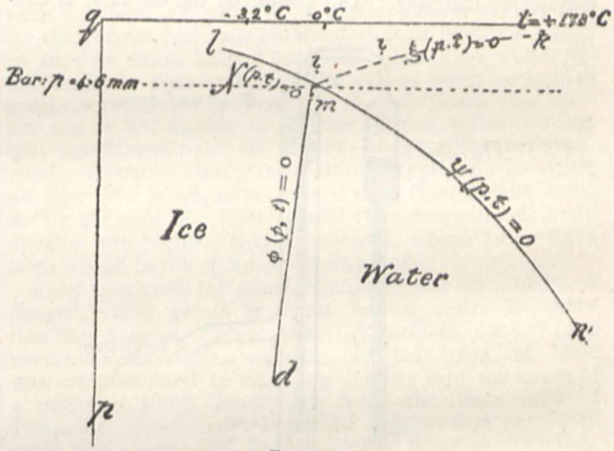

FIG. 1.

extension of the surface of ice is represented by the area $l\,m\,k$. Here the process of Carnelley, whereby ice of low pressure is heated to astoundingly high temperatures, would go on. The area $l\,m\,k$ would of course be entirely a *terra incognita* to the science of the present day, but there is nevertheless no theoretical objection why the surface of ice $v = f_{\text{i}}(p, t)$ should not extend farther than to the limiting line $l\,m$ pointed out by Regnault. Confiding in the experimental proofs already furnished by Dr. Carnelley, I concluded (*Berichte*, 1880): if the surface of ice really extends upwards to about $+178^\circ$ C. there must be a limiting line $m\,k$ to the area $l\,m\,k$, since this area cannot extend so far as to the dotted line in the figure indicating the critical pressure = 4·6 mm. At this new limit, $m\,k$, corresponding to an equation $\xi(p, t) = 0$, the vaporisation of the "hot ice" may go on in a reversible way, just as liquid water gives up saturated vapour at those pressures and temperatures which belong to the line $m\,n$ (equation $\psi(t, p) = 0$). The line $m\,k$ would in many respects be the continuation of $m\,d$ (just as $m\,l$ forms the continuation of $m\,n$), but naturally the symbols entering the equation of its differential coefficient $\frac{dp}{dt} = \frac{A\,r}{(s - s_1)\,(273 + t)}$ must change their signification on the other side of the point m, so that r here would represent the latent heat of *vaporisation* of the hot ice, s its specific volume, &c. I did not expressly mention this in my paper in the *Berichte*, because I thought it unnecessary. This omission on my side may probably have misled Dr. O. Lodge as to the real meaning of my words, since he declares my opinion that an equation $\xi(p, t) = 0$ having a differential

[1] The surface corresponding to the volumes of aqueous vapour $v = f_{\text{iii}}(p, t)$ is not sketched in the figure, which gives only *the projection* of the surfaces on the plane of co-ordinates p and t, not the real situation of these surfaces in space. The reader will also observe that the limiting lines $m\,n$, $n\,d$, $l\,m$, $m\,k$ are the *intersections* of vertical cylindrical surfaces ("Uebergangsflächen") with the plane p, t.

coefficient $\frac{dp}{dt}$ of the general form above mentioned will still exist for pressures below 4·6 mm. and temperatures higher than −0·0078° C., where it is now supposed to have a *point d'arrêt*, to be "naturally erroneous," an assertion which I hope the learned English inquirer may feel inclined to withdraw after the preceding explanation. Of course the necessity of my conjecture may be disproved by facts—if there really should exist no "hot ice"—but nevertheless it deserves to be discussed as well as the opinion of Dr. Lodge, who considers the existence of hot ice to depend entirely on an irreversible process of vaporisation from the ice resembling the evaporation of water in an atmosphere which is not saturated with damp. This observation only regards the experiment, not the theory. I fully admit that Dr. Carnelley's experiment is carried on in an irreversible way, but that is the case with every distillation or sublimation which is *practically* performed. Nevertheless there exists a line *m n* where the liquid water changes gradually and reversibly into saturated vapour, and that may be the case also with the hot ice at the limit *m k*. Any irregularity in the operation will not exclude the possibility of the existence of an equilibrium established by nature. The difference of temperature between the hot ice in the experiment of Carnelley and the cooled vacuum bottle is no objection to this, because we *might* carry on the operation in quite another way, dispense with the vacuum bottle and the cooling mixture, and keep up the necessary minimum of pressure, which is the only *sine quâ non*, by means of a powerful air-pump. In a similar experiment (with $HgCl_2$) Dr. Carnelley operates in that way. Dr. Lodge, on the contrary, is con-

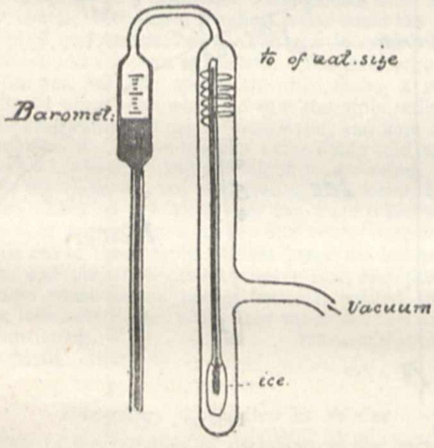

Fig. 2.

vinced that ice, which has once passed the triple point *m* can sustain whatever augmentation of pressure and temperature may be applied to it. We may destroy the vacuum, so scrupulously kept up by all experimenters, and allow the air to enter through hot pipes, nevertheless the ice will not melt. I interpret this in the following way:—Dr. Lodge admits that the surface $v = f_i(p, t)$ extends over the limit *l m*, and even surpasses the critical pressure 4·6 mm., the vaporisation of the hot ice going on irreversibly the whole time.[1] This is indeed an interesting hypothesis, which well deserves to be tested by experiments, but yet lacks any foundation from facts. I therefore think that the proper method of resolving the entire problem would be:—

1. To try (by experiment) if ordinary ice under low pressure by sufficient supply of heat can be made to pass over the limit *l m* and assume higher temperatures than those corresponding to the equation $\chi(p, t) = 0$ (or Regnault's table).

2. If this should be the case it remains to ascertain if the vaporisation of the "hot ice" tends towards any limit (*m k*), where this process becomes reversible, saturated vapour being formed [my conjecture], or if there is no such limit [theory of Dr. Lodge].

The apparatus employed (see Fig. 2) differs from those recently used by Messrs. Boutlerow, McLeod, L. Meyer, &c., only by its combination with a barometer, by means of which the variation of the pressure of the vapour given up by the ice during the whole process could be exactly measured. The only drawback to this was that the barometer of the apparatus did not *instantly* indicate the variation of the pressure, because the *upper* part of the barometer was made of a very wide glass tube to avoid the influence of capillarity. The effect of the vacuum, which consisted of a 4-litre glass bottle, was very powerful, since the full heat of two strong gas-lamps, each furnished with three pipes, must be employed on the outside of the glass tube in order to raise the temperature of the ice covering the bulb of the thermometer from − 15° C. or − 11° C., up to 0° C. The result of the experiment was (the ice being heated *only by radiation* from the glass tube):—By intense heating the temperature of the ice slowly (in about six minutes) rose from − 11° C. to 0° C., when it became *constant* for half a minute. Then the ice melted, and the first drop of water falling upon the bottom of the heated glass tube was sufficient to crush the apparatus. During the process of heating the niveau of the mercury in the barometer-tube constantly fell, the internal pressure augmenting as the temperature of the ice rose. It was quite impossible to raise the temperature of the ice without simultaneously augmenting the pressure.

	Experiment I.	*Experiment II.*
The ordinary barometer showed	= 756·8 mm.	= 771·1 mm.
The barometer of the apparatus showed	= 755·0 mm.	= 769·5 mm.
The initial pressure in the apparatus	= 1·8 mm.	= 1·6 mm.
The initial temperature of the ice	= − 11°·0 C.	= − 15° C.

Experiment I.	Experiment II.
By heating the temp. rose to = −8°; the press. = 2·5	$t = -9°$ p. = 1·8 mm.
,, ,, ,, = −6°; ,, = 2·90	$t = -6°$ p. = 2·6 mm.
,, ,, ,, = −4°; ,, = 3·5	The mercury in the stem of the thermometer separated by the heat.
,, ,, ,, = −0°; ,, = 4·0	
The ice melted.	

TABLE OF REGNAULT.

Tension of saturated vapour at—

$t = -20°$ C.; $p = 0·927$
$t = -15°$ C.; $p = 1·700$
$t = -10°$ C.; $p = 2·093$
$t = -5°$ C.; $p = 3·113$
$t = -0°$ C.; $p = 4·600$

I also repeated the experiment of Mr. Hannay by substituting a little sealed tube containing frozen water under atmospheric pressure, instead of the bulb of the thermometer. I found, in accordance with Mr. Hannay, that the enveloping ice melted before the ice in the tube.

After the experiments published by Messrs. McLeod, Boutlerow, L. Meyer, v. Hasselt, de la Rivière, and Hannay, I think it may be considered as a matter of fact that ordinary ice under low pressure cannot be heated over 0° C. In the experiments I. and II. I vainly tried to raise the temperature of the ice without simultaneously augmenting the tension of the vapour in the apparatus. It seems probable therefore that the area corresponding to $v = f_i(p, t)$ does not extend farther than to the limit *l m* [equation $\chi(p, t) = 0$], since the temperature of the ice and the tension of its vapour vary almost exactly in the ratio given by Regnault's table, which in Fig. 1 is represented by the line *l m*. We may conduct the heating of the ice so as to follow almost continuously the line *m l* [Experiment I.] without ever being able to pass over it or to reach temperatures situated beyond *l m* (*i.e.* in the area *l m k*). Still I think these experiments to be strictly convincing only in the case of *ordinary ice*. Nobody has yet repeated Dr. Carnelley's experiment exactly in the same way as Dr. Carnelley himself. In his experiment the ice on the bulb of the thermometer is formed not by the freezing of a quantity of water, but by the sublimation and condensation of icy vapour to thin layers. It *may* be possible that ice, by sublimation under low pressure, changes into another allotropic modification, just as the red modification of HgI_2 is changed into yellow iodide by sublimation. In this case we may foresee the existence of a new surface, $v = f_{iv}(p, t)$ on the other side of *l m*. For, according to the principles of the theory of mechanical heat, there ought to be a new function, $v = f_{iv}(p, t)$ for every new allotropic modification of a body which, geometrically, is represented by a surface (?? in the figure). We are scarcely authorised to deny the possibility of the existence of hot ice, since Dr. Carnelley has obtained several pieces of ice, which

[1] *Or:* Dr. Lodge supposes that the volume of ice which has once passed the limits, beyond which liquid water cannot exist, is totally *independent* of the temperature and pressure. In this case no theory can be applied to account for the existence of hot ice, because every theory must start from the assumption that there exists a certain relation between the variables v, p, and t, and that the volume of ice, as long as it *is* ice, is not *arbitrary*, but regulated by an equation $v = f_i(p, t)$. Therefore I do not think that this explanation can be in accord with the views of Dr. Lodge.

did *raise* the temperature of the calorimeter. I have tried to repeat this experiment, but I never could obtain the whole bulb of the thermometer entirely covered with sheets of sublimated ice, and without this the experiment will be illusory. Many times I obtained lozenges of sublimated ice, which did adhere very strongly to the bulb of the thermometer, until it showed + 35° or + 40° C. Then the lozenges generally fell off. I do not consider this to be any deciding proof, as it may depend on a phenomenon similar to that of Leidenfrost, nor do I think it very probable that ice really can exist at those temperatures, but if that should be the case the simplest manner to account theoretically for the existence of hot ice would be to assume a new allotropic modification, since it may be regarded as a matter of fact that *ordinary ice* cannot be heated over the limits pointed out by Regnault. If this should be the case I think that the importance of the discovery of Dr. Carnelley could hardly be overrated.

Upsala, May 28 OTTO PETTERSSON

TEMPERATURE OF RAIN-WATER.—"A Subscriber" asks where he can find records of the temperatures of rain-water when falling, and of the earth a few inches below the surface, during any or all the months of the year.—As regards the British Islands, the most extensive and long-continued observations on the temperature of the soil are those published by the Scottish Meteorological Society since 1857. A *résumé* of the first five years' observations was published in the Society's *Quarterly Report* for October-December, 1862. In the Society's *Journal* (vol. i. p. 320) is a discussion of valuable series of observations made on the temperature of drained and undrained land at various depths; also in *Journal* (vol. ii. p. 273, and vol. iii. p. 211) discussions of hourly observations on the temperature of the soil and of the air at different stations in Scotland. With respect to the temperature of falling rain, little, if anything at all, quite satisfactory, has been accomplished, the practical difficulties in the way being either not apprehended by the observer or not satisfactorily disposed of. Our correspondent may also consult with advantage the publications of the various Continental organisations for the prosecution of forest meteorology.—ED.

NOTES ON ALGÆ[1]

THE publication of the second part of Bornet and Thuret's volume on Algæ seems a fitting opportunity to notice it in some little detail. While the First Part, published in 1876, treated chiefly of the red Algæ, by far the larger portion of this Part treats of the Nostocs; while the First Part contained a good deal of the notes of Thuret, the present is practically the work of Bornet, and the drawings are in almost every instance from this author's pencil.

Under the modest title of Notes, we find in this handsome quarto volume, of over a hundred pages and twenty-four plates, in addition to notes on the higher Algæ, a most exhaustive treatise of a very interesting group of simple Phycochromaceous plants.

The illustrious Thuret had laid the foundation of a knowledge of the Nostocs; his friend Bornet has built thereon a very solid structure. He has not attempted to write a complete monograph of the group, including therein all the "book" species, but having had access to most of the published collections of dried Algæ, to the collections of the Paris and Dublin Herbaria, and to the original types of de Brebisson, Lenormand, Montague, Harvey, Grunow, and Le Jolis, he has performed wonders in the way of clearing up a most tangled synonymy.

It might shock the nerves of some botanists to recommend that all defective descriptions of Algæ—of which no original type specimens exist—ought to be overlooked. We believe, however, that it would be for the advantage of science that such a step should be taken. We may here mention that the collection of Dr. Hassall, from which most of the drawings of that author's "History of British Freshwater Algæ" were made, has been long since dispersed; so far as the Nostocs are concerned, this does not much matter, as all the species were described from authentic specimens still attainable.

The Nostocs (this name is traceable to Paracelsus—"Sic etiam quicquid aër gignit et ex aëre est vivitque vel oritur ut Tereniapin, Nostoch," &c.—and yet no one seems to know its meaning) are well-known plants. One common species makes its appearance on lawn or garden walks in summer or autumn in the form of olive green (rarely bluish), irregular, and more or less shining masses. It is strange to hear the guesses that are made as to the nature of these. We have had them sent to us as the "peculiar spawn of earthworms," and again as the eggs of some foolish frog that had mistaken dry land for water. Some species delight in moist banks over which water continually trickles; some live a wholly aquatic life on stones in streams. The species have an enormously extended, but not yet accurately defined geographical area. As to size, they vary much, some being scarcely visible to the unassisted vision, some forming masses as large as the upper joint of one's thumb.

The details of such a volume as the one before us are too special to be of general interest, so we rest satisfied with indicating the chief contents. The genus Nostoc is treated of very fully; the reproduction of the species by hormogones and by spores is well illustrated. Instead of the term trichome, we would have preferred that of filament, for the former has now obtained such a common usage in another sense among botanists. Despite a wonderful uniformity in their structure, the spores seem to furnish good diagnostic differences. It is unfortunate that they are not as yet known in all the species, while in some they are difficult to hit off. Twenty-nine species are formulated. Carefully-conducted culture experiments, carried on over four years, have proved that Nostoc cells found within the cells of aquatic plants (Potamogeton, &c.) will develop into regular Nostoc colonies, which latter have been traced to the spore-producing stage.

Four species of the genus Nodularia are described and figured. This genus is better known under its more familiar title of Spermosira. *Nodularia litorea* is a somewhat remarkable species. In July, 1874, M. Crié was commissioned to make an inquiry into the cause of a noisome smell proceeding from the little lake of Deauville (Calvados). It would appear that for some years this district had been a regular focus of maladies, and those living near it had remarked that the fœtid odour perceived at times came from a reddish matter which periodically covered the surface of the water. M. Crié soon found that this consisted of ruddy masses of this Nodularian, spreading over the surface of Ruppia, and that its periodic decomposition—at the moment of greatest heat and lowest water—was the cause of the stench. The perfect remedy was found in guiding a stream through the little lake or pond, and thus preventing the too rapid growth of the Alga.

Of the other genera treated of we must mention Lyngbya, Plectonema (for *Conferva mirabilis* of Dillwyn), Scytonema (twenty-one species, of which a provisional analysis is given. Some twenty-one species (?) are included under *Scytonema thermale* (Kutz.), and a very important Appendix gives a list of plants determined NOT to belong to the genus, though referred to it); Calothrix (several of our native species are figured and described); and lastly, Glœotrichia (of which six species are admitted).

Enough has been written to prove how valuable an addition to our works on the lower algal forms this volume is. To the worker on this group—ever increasing in interest—this contribution to our knowledge of it will be very welcome. Such will call to mind, too, that there are still lower and more confusing forms of these Algæ, and will be glad to hear that it is probable that the same patient and clever hand hopes shortly to have reduced even them to something like order. E. P. W.

[1] Notes Algologiques: Recueil d'Observations sur les Algues, par M Ed. Bornet et G. Thuret. Deuxième fascicule. Paris, 1880. G. Masson.

PENNSYLVANIA OIL REGIONS

THREE years ago the Second Geological Survey of Pennsylvania, under the able leadership of Mr. J. P. Lesley, published a report on the oil-well records of the State—a laborious compilation by Mr. J. F. Carll. During the interval the value of this report has been duly tested and acknowledged. It is a treasury of facts classified and indexed for the guidance of the compiler of statistics, the well-sinker, the mining engineer and the geologist, while the general reader may learn much of interest from its pages. Another report by the same author is just about to appear. It forms a volume of about 500 pages with two indexes, twenty-three plates, and an atlas of twenty-two sheets of maps, well-sections, and working drawings of machinery and tools. We have been favoured with advance-sheets of the Letter of Transmission prefixed to the report by Mr. Lesley, from which we make the following extracts:—

"The main feature of the report is the settlement of the true character of the Venango oil-sand group as a distinct and separate deposit, with characteristic marks distinguishing it from the Palæozoic formations of a preceding and a succeeding age; the differentiation of the group into three principal and other subordinate layers of gravelly sand, holding more or less oil or gas; the local variability of these sands, their singular persistency beneath long and narrow belts of country, their change into barren shales elsewhere, and their independence of other oil-bearing sands and shales of an earlier and of a later date."

Some characteristically caustic remarks are made as to the consequences of the contempt entertained by "practical" men for what they consider the "theoretical" opinions of geologists, and a flagrant example is given of the results of trusting to mere empirical guidance. These passages ought to be well studied by oil-men in Pennsylvania and Canada. Mr. Lesley goes on to relate an incident in his own experience. "In 1841," he says, "I was ordered by the chief of the First Geological Survey to report on the counties lying along the New York State line, and down the eastern bank of the Allegheny River, as far as the Kiskiminitas. Other assistants on that survey had already discovered and reported the geological structure of the Allegheny River and Beaver River water basins, and the rate of descent of the rocks southward and south-westward in relation to tide level had been calculated. My business was to follow and locate upon the map the anticlinal and synclinal rolls which locally change and modify this general dip, and to identify the principal coal beds over a large area.

"After the discovery of petroleum (which of course did not in the least set aside or essentially change the structure of Western Pennsylvania as established by the First Survey), I happened to be employed by the Brady's Bend Company to examine their property, and to give them, among other items, an opinion upon the probable existence and depth of oil beneath it. To do this, I merely did what any geologist who had thoroughly studied that country would have done; I calculated the vertical distance from the oil sand on Oil Creek up to coal A, then I calculated the dip of the measures between Oil Creek and Brady's Bend, and then I identified coal A at Brady's Bend. I reported that the Venango oil sand, *if it extended under ground as far as Brady's Bend*, ought to lie at 1100 feet beneath water-level. Any geologist who knew the country could have done this. It required no genius, no uncommon knowledge, nothing but a plain, simple, systematic, or scientific, in other words, true theoretical method of applying known facts for discovering the unknown. Any oil-man could have done the same if he had noticed the rocky layers as he went up and down the river, and put this and that carefully together.

"Yet, when after a few months, oil was actually struck at Brady's Bend within a few feet of the depth which I had assigned to it, the astonishment of all classes of oil-men was ludicrously extravagant; a score or two of copies were made from the manuscript report, and these copies passed from hand to hand as precious things, and their author was looked upon as a prodigy of mental penetration, and was offered large sums of money to locate wells in different districts, none of which offers, of course, were accepted, because he was as ignorant of the *actual existence of an oil-bearing sand in any given locality* as everybody else.

"The story has its moral. Let 'practical' men believe in and respect the slowly, carefully reached conclusions of 'theoretical' men enough to take them into consideration, so far as to comprehend them, and to govern themselves by them in their own collection and collation of facts relating to their own pecuniary interests."

Notwithstanding the amount of detailed information now collected regarding the occurrence of the liquid hydrocarbons in these ancient American formations, it must be frankly confessed that we seem to be as far as ever from a clue to their source and history. "The origin of petroleum," says Mr. Lesley, "is still an unsolved problem, and Chapter 26 of this Report merely suggests queries respecting it. That it is in some way connected with Palæozoic sea-weeds, the marks of which are so infinitely abundant in the rocks, and with the infinitude of coralloid sea-animals, the skeletons of which make up a large part of the limestone formations which lie several thousand feet beneath the Venango oil-sand group, scarcely admits of dispute; but the exact process of its manufacture, of its transfer, and of its storage in the gravel beds, is utterly unknown. That it ascended rather than descended into them seems indicated by the fact that the lowest sand holds oil when those above do not, and that upper sands hold oil where they extend beyond or overhang the lower. The chemical theory, so-called, which looks upon petroleum as condensed from gas, the gas having been previously distilled from the great black shale formations (Marcellus and Genessee), must face the objection that such a process, if chemically possible, which is doubtful, ought to have distributed the oil everywhere, and permanently blackened and turned into bituminous shales the entire thickness of this part of the earth crust, several thousand feet thick. It fails to explain the petroleum obtainable from the Cannel coals, and from the roof shales of bituminous coal beds. And it fails also to explain the entire absence of petroleum from immense areas of not only shales, but sand and gravel rocks equally underlaid by the Marcellus and Genessee formations."

One of the most generally interesting questions in the report is one discussed in great detail by Mr. Carll—an episode in the history of the glacial period in North America. Certain oil-bearing river-gravels are connected with a very thick "deposit of Canadian rock fragments not only upon the surface, but to the depth of several hundred feet beneath it in Northern Pennsylvania, a deposit which forms a great belt, more than a thousand miles long, across the continent from Cape Cod in Massachusetts to Iowa and Minnesota beyond the Mississippi River. It was brought from the north by a vast sheet of moving ice which filled the great lakes and rode over the highest mountains to the south of them—burying all New England and New York, Northern New Jersey, Northern Pennsylvania, the Western Reserve in Ohio, and large portions of the States lying further west—projecting long tongues or slowly moving torrents of solid ice southward as far as and even beyond the Ohio River in Kentucky. It drove slowly before it the reindeer, musk ox, caribou, moose, and other Arctic animals whose bones are found in the diluvial clays of the Kentucky caves; while the walrus inhabited the shores of the Atlantic as far south as the Ashley River in South Carolina. The Esquimaux race no doubt accompanied these animals into the Gulf States, just as it did in France as far south as the Pyrenees. By the deposit of this vast pile of moraine matter, sand, clay, scratched rocks and huge boulders, the valleys by which our rivers had previously flowed into Lake Erie were filled up so that the waters were turned southward into the Ohio."

"THOUGHT-READING"

THE public mind has of late been somewhat agitated by the doings of a Mr. Bishop, who has come before the world of London society in a capacity no less startling than that of a professed reader of thought. Armed with a favourable letter of introduction from Dr. W. B. Carpenter, he has not only taken by storm the general public and daily press, but also succeeded in convening an assembly of scientific men to witness his performance, which in point of numbers and importance resembled in miniature a *soirée* of the Royal Society, while still more recently he has had the honour of exhibiting his powers before the Heir Apparent to the Crown. There is no doubt that Mr. Bishop owes this wide and sudden celebrity to the patronage which was extended to him by the great opponent of all humbug; and although Dr. Carpenter doubtless intended his letter to exert a salutary influence by recommending Mr. Bishop to the attention of the credulous, it is to be regretted that it served to recommend him also to the attention of the scientific. This is to be regretted, because the result was to endow the powers which were afterwards exhibited with a fictitious degree of importance in the eyes of the public, and also to bring a large number of distinguished men into the somewhat undignified position of acting the stalking-horse to Mr. Bishop's notoriety. But however this may be, it seemed to Prof. Croom Robertson worth while to make a more careful trial of Mr. Bishop's powers than was possible in the first crowded assembly, and he therefore invited Mr. Francis Galton, Prof. E. R. Lankester, and myself, who were all present on the first occasion, to join him in an investigation. When we had assented to the proposal, Mr. Bishop was invited to meet us at Prof. Croom Robertson's house. He immediately accepted the invitation, and it is but just to state that throughout the investigation which followed he placed himself entirely in our hands, and with the utmost good nature submitted to all our requirements. He professes that he is himself ignorant of his *modus operandi*, and merely desires that this should be adequately investigated and satisfactorily explained.

Two meetings were arranged. At the first, which was held on May 28, Prof. Lankester was not able to attend, and his place was taken by Mr. Leslie Stephen. Mr. Alfred Sidgwick was also present. At the second meeting, held on June 11, there were present as before, Prof. Croom Robertson, Mr. F. Galton, and myself, but Mr. Leslie Stephen and Mr. Alfred Sidgwick were absent, while Prof. Lankester was present. The room in which both meetings were held was a double drawing-room of the ordinary shape of those which usually have folding-doors; here however the folding-doors were absent. The extreme length of the room was 36 feet, the width of its front part was 19 feet, and of its back part 12 feet.

First, Mr. Bishop was taken out of the room by me to the hall down stairs, where I blindfolded him with a handkerchief; and, in order to do so securely, I thrust pieces of cotton-wool beneath the handkerchief below the eyes. In all the subsequent experiments Mr. Bishop was blindfolded, and in the same manner. While I was doing this, Mr. Sidgwick was hiding a small object beneath one of the several rugs in the drawing-room; it having been previously arranged that he was to choose any object he liked for this purpose, and to conceal it in any part of the drawing-room which his fancy might select. When he had done this the drawing-room door was opened and the word "Ready" called. I then led Mr. Bishop up stairs, and handed him over to Mr. Sidgwick, who at that moment was standing in the middle line between the two drawing-rooms, with his back to the rug in question, and at a distance from it of about 15 feet. Mr. Bishop then took the left hand of Mr. Sidgwick, placed it on his (Mr. Bishop's) forehead, and requested him to think continuously of the place where the object was concealed. After standing motionless for about ten seconds Mr. Bishop suddenly faced round, walked briskly with Mr. Sidgwick in a direct line to the rug, stooped down, raised the corner of the rug, and picked up the object. In doing all this there was not the slightest hesitation, so that to all appearance it seemed as if Mr. Bishop knew as well as Mr. Sidgwick the precise spot where the object was lying.

This is Mr. Bishop's favourite experiment; so I may give some of our other observations relating to it before passing on to the variations which we introduced. It was soon found that he succeeded much better with some of us than with others; so at the second meeting, in order to make a numerical comparison, he was requested to try two experiments with each of the four persons who were present. With Mr. Galton, Prof. Robertson, and Prof. Lankester he failed utterly, while with myself he succeeded once perfectly and the second time approximately. For on the first occasion I concealed a pocket-matchbox upon the top of a book behind the leather lap of a book-shelf. After feeling along the rows of books for some time he drew out the one on which the matchbox was lying. In the second experiment I placed a visiting-card on the key-board of a grand piano and closed the cover. After going about the room in various directions for a considerable time he eventually localised the piano, and brought his finger to rest upon its upper surface about six inches from the place where the card was lying. It will thus be seen that his success with me, although so much better than with any of the other three persons present that evening, was not so immediate and precise as it had been with Mr. Sidgwick the evening before. It has also to be mentioned that in one of the experiments which he tried with Prof. Robertson the evening before, he was, after a good deal of feeling about, successful in localising a particular spot on an ordinary chair which Prof. Robertson had selected as the spot to be found. From this it will be seen that it made no difference whether a particular article or a particular spot was thought of; for if the subject thought of was a certain square inch of surface upon any table, chair, or other object in the room, Mr. Bishop, in his successful experiments, would place his finger upon that spot. Neither did it make any difference whether the article or place thought of was at a high or a low elevation. Thus, for instance, in one of the experiments I placed a small pencil-case high up in the chandelier of one of the drawing-rooms. There was first a great deal of walking about in various directions, examining tables, book-shelves, &c., so that it was thought that the experiment was about to prove a failure. (It may here be mentioned parenthetically that in all the experiments tracings were taken of the routes which Mr. Bishop traversed, but it seems needless to occupy space with recording the analysis o these results.) Then, while feeling over the surface of a table in the other drawing-room, and not far from the corresponding chandelier, Mr. Bishop suddenly pointed at arm's length vertically to the ceiling. He remained motionless in this position for a few seconds, and then set off at a brisk pace in a straight line to the other drawing-room, until he came beneath the other chandelier. As his finger was all this time pointing to the ceiling, it touched this chandelier on his coming beneath it. He then stopped and pointed as high as he could, but not being a tall man, was not able to touch the pencil-case, which had been purposely placed above his reach. After satisfying ourselves that his determination to reach up at that particular spot could not be attributed to accident, but rather that his finger appeared to be smelling the object of his search, the experiment was concluded. As a rule, unless success is achieved within the first two or three minutes, it is never achieved at all; but in some cases, as in the one just quoted, after several minutes of feeling about in various places and directions, a new point of departure seems suddenly to be taken, and Mr. Bishop starts off straight to the right spot. As an instance of this I may quote another experiment, in which I placed a shilling beneath a sheet of paper lying on a table which was crowded with other articles. After going about the room in various directions for a considerable time, this table was reached, apparently by accident, and just at the time when I was thinking that the experiment would certainly prove a failure, Mr. Bishop suddenly became more animated in his movements, and exclaiming "Now I am within two feet of it," began to hover the point of his finger over the table, and eventually brought it down upon the sheet of paper just where the shilling was lying beneath.

Mr. Bishop can also very frequently localise any spot on his subject's person of which the subject may choose to think. As in all other cases he presses the hand of the subject upon his forehead with one hand, and uses the other as a feeler. Here again he succeeds much better with some persons than with others, and the persons with whom he succeeds best are the same as those with whom he does so in his other experiments. Thus he altogether failed with Mr. Galton, although the latter, in order to fasten his attention the more exclusively on one particular spot, pricked this spot with a needle. With Prof. Lankester success was partial; for while he thought of the point of his nose, Mr. Bishop was only able to say that the point thought of seemed to occupy the median line of the body on the front aspect. But on a previous occasion at Bedford Square Mr. Bishop localised correctly a pain (slight toothache) from which Prof. Lankester was suffering. With Prof. Croom Robertson success was better, though not quite perfect, for while the place thought of was the ball of the right thumb, Mr. Bishop localised it in the right wrist. In the only two experiments tried in this connection with myself the results were somewhat peculiar. In the first experiment I thought of a spot situated under the left scapula, and Mr. Bishop localised it as situated under the right; in the second experiment I thought of my right great toe-nail, and for a long time Mr. Bishop prodded round and on the left great toe-nail, though he eventually changed to the right one, and so localised the spot correctly. In both these experiments, therefore, it seemed that with me Mr. Bishop experienced a strong tendency to confuse symmetrically homologous parts.

From this brief summary of the results gained by following Mr. Bishop's own methods, it will be seen that on the whole his power of localising objects or places thought of by a person whose hand he clasps is unquestionably very striking. Of course the hypothesis which immediately suggests itself to explain the *modus operandi* is that Mr. Bishop is guided by the indications unconsciously given through the muscles of his subject—differential pressure playing the part of the words "hot" and "cold" in the childish game which these words signify. Mr. Bishop is not himself averse to this hypothesis, but insists that if it is the true one he does not act upon it consciously. He describes his own feelings as those of a dreamy abstraction or "reverie," and his finding a concealed object, &c., as due to an "impression borne in" upon him. But however this may be (and of course we had no means of testing the statement) all our experiments have gone to show that the hypothesis in question is the true one, and that Mr. Bishop owes his success entirely to a process of interpreting, whether consciously or unconsciously, the indications involuntarily and unwittingly supplied to him by the muscles of his subjects. Thus when his subject is blindfold and loses his bearings, failure results. Failure also results if the connection between Mr. Bishop and his subject is not of a rigid nature—a loose strap, for instance, being apparently of no such use to him for the establishment of connection as a walking-stick. Similarly, although he was very successful when he grasped my left hand when I did not know where the object was concealed, but when my left wrist was held by Mr. Sidgwick, who had concealed the object; he failed when, under otherwise similar circumstances, Mr. Sidgwick held my right hand—so establishing a limp instead of a firm connection through my person.

Lastly, a number of other experiments were tried, in deference to some statements which Mr. Bishop made concerning his occasional success in reading thoughts of a kind which could not be indicated by muscular contraction. From these experiments, it is needless to say, we did not anticipate any results; but (with the exception of Prof. Lankester) we thought it was worth while to make them, not only because Mr. Bishop seemed to desire it, but also to satisfy the general public that we had given the hypothesis of "thought-reading," as well as that of "muscle-reading," a fair trial. The experiments consisted in the subject looking at some letter of the alphabet which Mr. Bishop could not see, and the latter endeavouring to read in the thoughts of the former what the letter was. Although this experiment succeeded the first time it was tried, it afterwards failed so frequently that we entertain no doubt as to the one success having been due to accident, and therefore conclude that if Mr. Bishop has any powers of "thought-reading" properly so-called, he has failed to show us evidence of the fact.

Deeming it a remarkable thing that such precise information as to a mental picture of locality should be communicated so instantaneously by unconscious muscular movement, we thought it desirable to ascertain whether Mr. Bishop, who is able so well to interpret these indications, is endowed with any unusual degree of tactile sensibility or power of distinguishing between small variations of resistance and pressure. We therefore tried the sensitiveness of his finger-tips with the ordinary test of compass-points, but found that he did not display more than a usual delicacy of tactile perception, while his power of distinguishing between slight differences in weights placed successively on a letter-balance concealed from his eyes was conspicuously less than that displayed by Prof. Croom Robertson. As Mr. Bishop is not opposed to the hypothesis by which we conclude that his results are obtained, there is no reason to suppose that he tried to depreciate his powers of tactile sensibility and of distinguishing between small differences of weight. In their main features Mr. Bishop's experiments are frequently performed as an ordinary drawing-room amusement, and we are therefore inclined to think that he does not enjoy any peculiar advantages over other persons in regard to sensitiveness of touch or power of appreciating pressure, but that his superior success in performing the experiments is to be ascribed merely to his having paid greater attention to the subject.

In conclusion, we desire to express our thanks to Mr. Bishop for the trouble which he has taken in submitting to the numerous experiments, the general results of which have now been stated.

This report has been read in proof by Prof. Croom Robertson, Mr. Francis Galton, and Prof. E. R. Lankester, and meets with their full approval.

GEORGE J. ROMANES

THE WEATHER AND HEALTH OF LONDON[1]

II.

An examination of the curve for the whole mortality (Fig. 4, NATURE, vol. xxii. p. 144) shows that the great preponderance of deaths in London takes place during the coldest months of the year. Of the diseases to which this excessive mortality is due, the first place must be assigned to diseases of the respiratory organs, the more marked of which are given in Figs. 12 to 15. About one in eight of all deaths that occur is caused by bronchitis, and one in sixteen by pneumonia; so that nearly one-fifth of the deaths is occasioned by these two diseases of the respiratory organs. Our researches appear to warrant the conclusion that the greatest fatality from these diseases occurs when the temperature is between 32° and 40°. In New York, when the winter temperature is 10°·0 lower than in London, the mortality from bronchitis and pneumonia is greatly less; on the other hand, in Melbourne, where the winter temperature is about 10°·0 higher than that of London, the mortality from diseases of the respiratory organs forms but a small fraction of the whole deaths.

These four curves of the mortality from diseases of the respiratory organs are substantially the same, each having its maximum in the cold months and its minimum in the warm months. Asthma shows, in the amplitude of its annual range, the greatest sensitiveness to weather, and pneumonia the least. They all show, though in different degrees, a double-ridged maximum: the one ridge being in the middle of January, when the temperature falls to the annual minimum, and the other in March, when the combined qualities of cold and dryness are at the annual maximum. Asthma and bronchitis are decidedly at the maximum when the weather is coldest, whereas laryngitis has its maximum in March, when the weather is coldest and driest, the last disease thus forming the link connecting the more strictly throat diseases with diseases of the nervous system.

But an element of weather other than mere temperature plays an important part in bringing about the high death-rate from these diseases. That deleterious atmospheric influence is fog; and in cases where the fog is dense and persistent the mortality from diseases of the respiratory organs becomes truly appalling, as happened in London early in 1880, when the mortality was nearly doubled. An examination of the fogs of London shows that they do not commence till the autumnal equinox; and it is at this epoch that asthma (Fig. 12), by far the most sensitive of all diseases to fog, starts from its annual minimum; and in the end of November and begin-

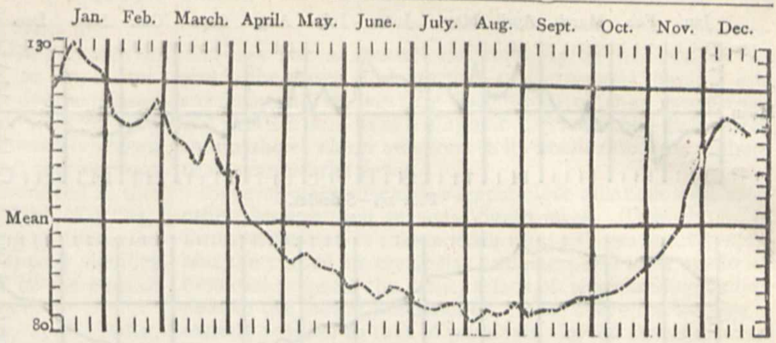

FIG. 12.—Asthma.

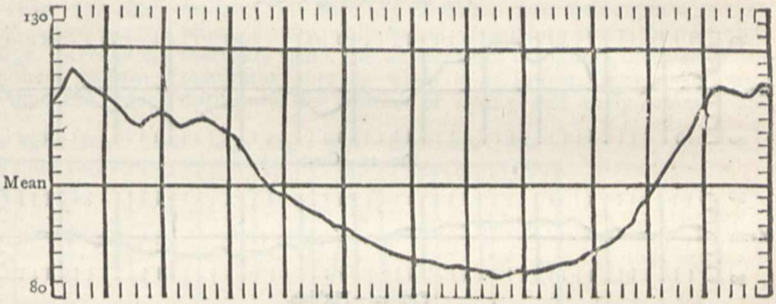

FIG. 13.—Bronchitis.

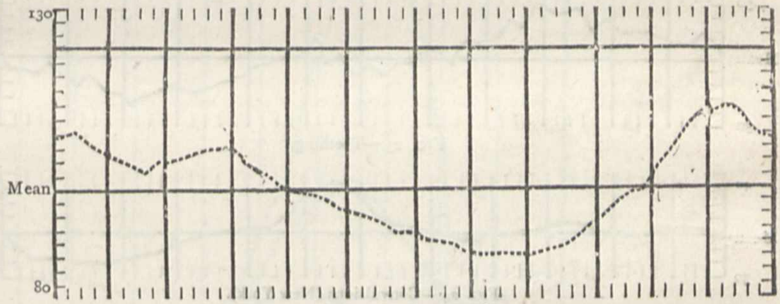

FIG. 14.—Pneumonia.

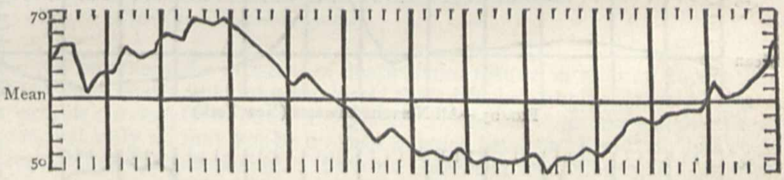

FIG. 15.—Laryngitis.

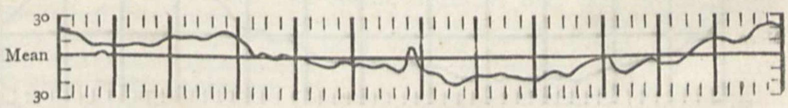

FIG. 16.—Apoplexy.

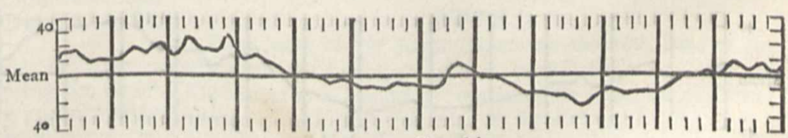

FIG. 17.—Convulsions.

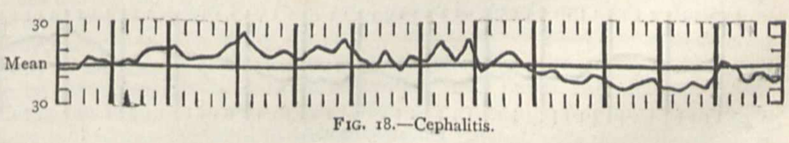

FIG. 18.—Cephalitis.

[1] Substance of a Lecture delivered at the Royal Institution, March 25. Continued from p. 146.

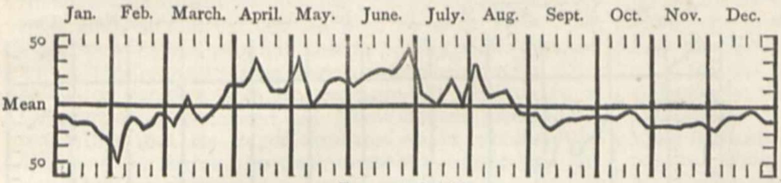

Fig. 19.—Suicides.

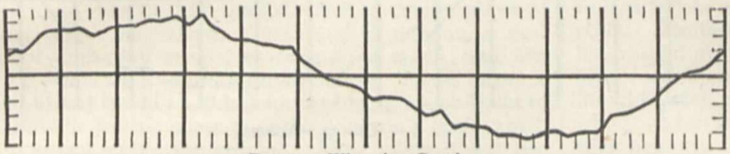
Fig. 20.—Whooping Cough.

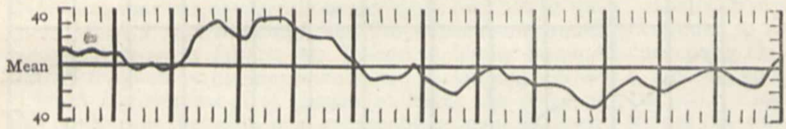

Fig. 21.—Gout.

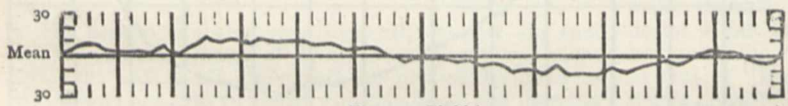

Fig. 22.—Phthisis.

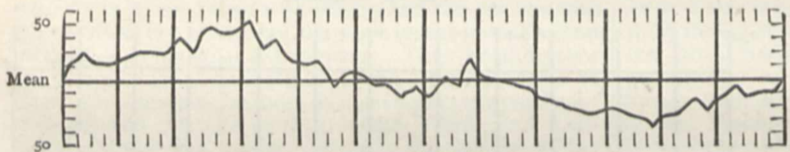

Fig. 23.—Teething.

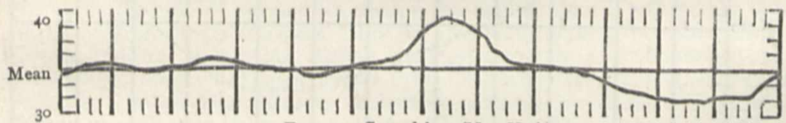

Fig. 24.—Convulsions (New York).

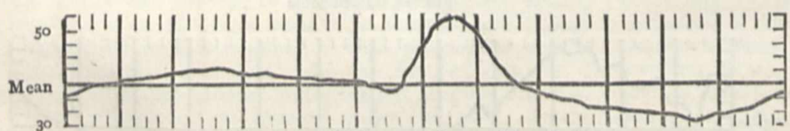

Fig. 25.—All Nervous Diseases (New York).

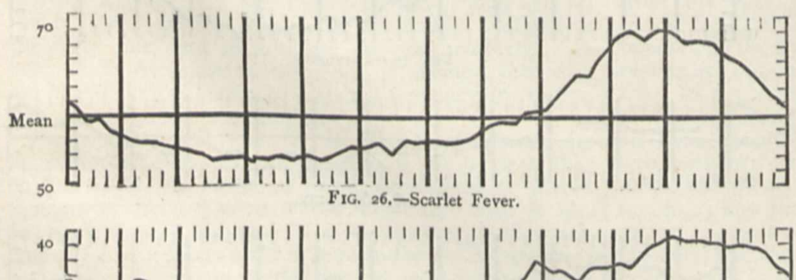

Fig. 26.—Scarlet Fever.

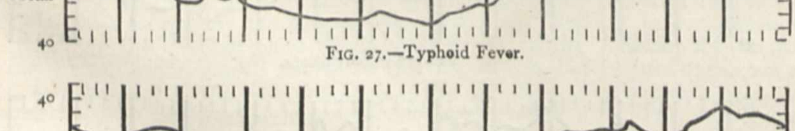

Fig. 27.—Typhoid Fever.

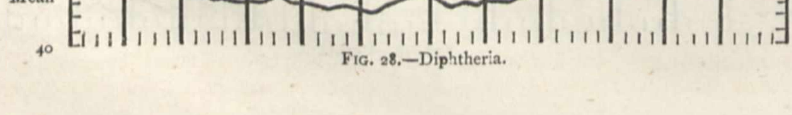

Fig. 28.—Diphtheria.

ning of December, when fogs become most frequent, the curves for asthma and bronchitis shoot up with startling suddenness.

Figs. 16, 17, and 18 represent the curves for three of the nervous diseases, viz. apoplexy, convulsions, and cephalitis. Apoplexy will be observed to show a double-ridged maximum quite analogous to that of the diseases of the respiratory organs; whereas, in the case of convulsions, the maximum may be regarded as quite single, and occurring in spring, this being the season when nervous diseases generally are most fatal. On the other hand, the curve for cephalitis stands alone among nervous diseases as having its annual maximum somewhat later, and keeping above the mean till at least the end of July, thus covering that portion of the year when the climate is driest and hottest, as well as driest and coldest. The intimate relations observed between the curve for suicides (Fig. 19) and that for cephalitis is very striking.

The maximum mortality for whooping-cough, Fig. 20, gout, Fig. 21, and phthisis, Fig. 22, occur in the same season as that for the nervous diseases. The maximum mortality from whooping-cough occurs in the spring months, and the curve suggests that this is more a disease of the nervous system than of the respiratory organs, a view which, singularly enough, was maintained by the elder Dr. Begbie, one of the most distinguished of our Edinburgh physicians, upwards of thirty years ago. The relations of gout to diseases of the nervous system are too obvious to call for remark. Phthisis is one of the two most fatal scourges of our British climate, one out of every eight deaths which occur being caused by consumption. Its mortality-curve, Fig. 22, shows unmistakably its intimate relations to nervous diseases, thus affixing greater significance to its known complications with hereditary insanity, scrofula, and some other mental diseases.

Reference has been made to the influence of the heat of summer on certain of the nervous diseases. That influence acts fatally, both indirectly through the bowels in the case of the young, and directly on the nervous centres. The curve for convulsions, Fig. 17, is identical with that for teething, Fig. 23, and it may be added that the curve for hydrocephalus is simply a reproduction of the same curves. Now these curves show a small, but distinct, and, as revealed by each year's figures, a constantly recurring secondary maximum in summer, which in the case of London is almost wholly due to the bowel complications of these diseases. The curve (Fig. 24) for convulsions for New York, where the summer temperature is 10°·0 hotter than in London, shows this feature of the curve enormously magnified, so much so, indeed, that instead of being, as in London, an insignificant secondary maximum, it stands out as the prominent feature of the curve. Whilst this result is doubtless largely due to complications with bowel complaints, it is, as an examination of the statistics shows, in no small degree caused by the direct influence of the great summer heat of New York on the nervous centres. This is impressively shown by the mortality curve for the whole of the nervous diseases (Fig. 25), which is even more pronounced in this particular than the curve for convulsions alone (Fig. 24). Keeping this fact in view, the peaks showing an increased fatality in London from cephalitis (Fig. 18) and suicides (Fig. 19) during July and August acquire, in the eyes of the physician, a more impressive significance.

The curve for the whole mortality (Fig. 4, NATURE, vol. xxiv. p. 144) shows September and October to be two of the healthiest months of the year. The three curves, scarlet fever (Fig. 26), typhoid (Fig. 27), and diphtheria (Fig. 28), are the most striking exceptions to this, these curves all indicating either a large increase in the death-rate or a high mortality during these months. While closely related to each other, each of these three diseases has a distinct individuality of its own as regards the times of occurrence of the annual maxima and minima, and the varying amplitudes of their range from the mean line. It is a singular circumstance that diphtheria shows closer relations in its death-rate with typhoid than with scarlet fever.

Several other diseases suggest close alliances with each other through their seasonal death rates. Thus the curve for mortification is substantially that of nervous diseases, and the curves for erysipelas and puerperal fever are in all essential respects the same, a fact of singular suggestiveness to the family practitioner. The curve for old age is exactly parallel to that of paralysis, the old man's disease. The curves for skin diseases, rheumatism, dropsy, pericarditis, Bright's disease, and kidney disease exhibit most striking, and in many cases the closest alliances with each other. Lastly, while bowel complaints attain their greatest mortality when the temperature is highest, diseases of the respiratory organs when it is lowest, nervous diseases during the dry weather of spring and early summer, and

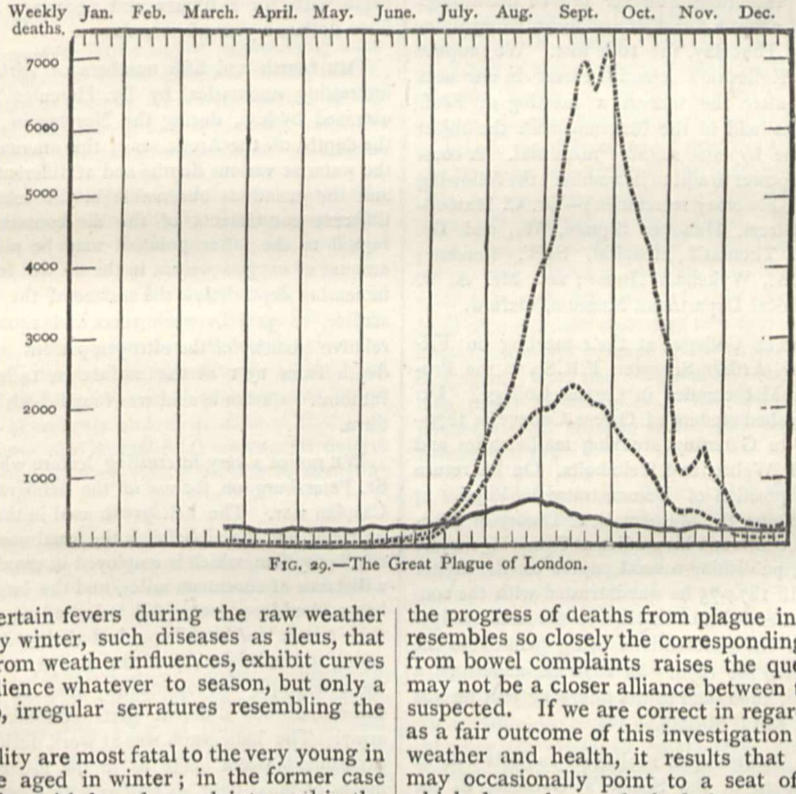

FIG. 29.—The Great Plague of London.

skin diseases and certain fevers during the raw weather of autumn and early winter, such diseases as ileus, that are quite removed from weather influences, exhibit curves which show no obedience whatever to season, but only a succession of sharp, irregular serratures resembling the teeth of a saw.

Atrophy and debility are most fatal to the very young in summer, but to the aged in winter; in the former case the complication being with bowel complaints, and in the latter with diseases of the respiratory organs. The annals of influenza show that a special character is given to this epidemic according to the season of the year in which it occurs. Thus when it occurs in spring the head and nervous system are most affected, but the bowels when the epidemic prevails in summer and autumn.

Fig. 29 shows by the doubly-dotted line, or highest curve, the weekly mortality of London during the Great Plague of 1665, the lower dotted curve the mean weekly mortality of the last six plagues, and the solid curve the mean weekly mortality from all other diseases during the continuance of the last six plagues. The manner in which the plague, as a death-producer, obeyed the weather is striking, and full of interest. It did so exactly in the way in which we have seen bowel complaints to be influenced by weather. The curve of mortality for the plague bears no resemblance whatever to that for typhus, or indeed any other disease except bowel complaints. The fact that the progress of deaths from plague in relation to weather resembles so closely the corresponding progress of deaths from bowel complaints raises the question whether there may not be a closer alliance between them than has been suspected. If we are correct in regarding such a question as a fair outcome of this investigation of the relations of weather and health, it results that such investigations may occasionally point to a seat of morbid processes which have been cloaked by prominent phenomena, apparently of a primary, but in reality of a secondary character.

ALEXANDER BUCHAN

NOTES

THE death of Sir Josiah Mason on the 16th inst., at the advanced age of eighty-six, closes a remarkable career. Born at Kidderminster in humble circumstances, he began life as a street hawker of cakes, and after trial of shoemaking, baking, and a variety of other things in his native place, he went to Birmingham and found employment in the gilt toy trade. In 1824 he set up on his own account as a manufacturer of split-rings by machinery, and he afterwards added the manufacture of stee pens, of which he became really the largest producer, though less known than Gillott and Mitchell, owing to his pens being supplied by Messrs. Perry of London. He shares the credit of perfecting the modern steel pen, the history of which practically dates from the discovery of the art of splitting by machinery. Sir Josiah Mason also carried on for many years the business of electro-plating, copper-smelting, and india-rubber making, along with the late George Richard Elkington. While he was very much a self-taught man, his very liberal benefactions indicated his sense of the value of good education. Conspicuous among these is the Erdington Orphanage, established at a total outlay of 300,000*l.*; and the munificent gift to Birmingham of a Science College (the building of which cost 60,000*l.*, while the total value of the endowment is probably little short of a quarter of a million) is fresh in public memory.

OUR readers will learn with profound sorrow the loss which biological science has sustained in the death, at the comparatively early age of fifty-one, of one of its most brilliant and gifted cultivators, Prof. Rolleston of Oxford. He had spent the greater part of the winter in Southern Europe, his medical advisers having hoped that a warmer climate and rest from his incessant labours might have averted the malady with which he was threatened. All precautions however proved unavailing. He returned to England about a fortnight ago in a sinking state, and died at his home on Thursday, the 16th inst. We propose to give a sketch of Dr. Rolleston's scientific career in our next number. Immediately after the funeral a meeting of Prof. Rolleston's old pupils was held in the Museum, with the object of perpetuating his name by some suitable memorial. A committee was formed, with power to add to its number; the following gentlemen being elected honorary secretaries:—Dr. C. Mansell-Moullin, 17, George Street, Hanover Square, W., and Dr. Theodore Acland, St. Thomas's Hospital, S.E., London; Mr. E. B. Poulton, M.A., Wykeham House; and Mr. A. P. Thomas, M.A., Anatomical Department Museum, Oxford.

THE Council of Owens College, at their meeting on Friday, June 17, elected Dr. Arthur Schuster, F.R.S., to the Professorship of Applied Mathematics in Owens College. Dr. Schuster was a distinguished student of Owens College in 1870–71; he then proceeded to Germany, studying mathematics and physics under Kirchhoff, Weber, and Helmholtz. On his return he first occupied the position of Demonstrator in Physics at Owens College, lecturing on the Mathematical Theory of Electricity. Afterwards he continued his studies at Cambridge under Maxwell and Rayleigh, publishing several papers on the higher branches of physics. In 1874-75 he was intrusted with the conduct of the Government expedition to observe the total eclipse in Siam, the results of his observations being printed in the *Philosophical Transactions* for 1878. In 1878 he undertook a similar expedition to Colorado, and in the following year he was elected a Fellow of the Royal Society.

THE Davis series of lectures upon zoological subjects will be given in the lecture-room in the Society's Gardens, in the Regent's Park, on Thursdays at 5 p.m., commencing June 16, as follows:—June 16—Whales, by Prof. Flower, LL.D., F.R.S.; June 23—Dolphins, by Prof. Flower, LL.D., F.R.S.; June 30—Extinct British Quadrupeds, by J. E. Harting; July 7—The Limbs of Birds, Prof. W. K. Parker, F.R.S.; July 14—Birds, Ancient and Modern, by W. A. Forbes; July 21—Zoological Gardens, by P. L. Sclater, F.R.S.; July 28—Chameleons, by Prof. Mivart, F.R.S. These lectures will be free to Fellows of the Society and their friends, and to other visitors to the Gardens.

AMONG other features of the forthcoming meeting at York, the noble Guildhall is placed at the Association's use as reception room. The theatre of the Museum of the Yorkshire Philosophical Society has been granted for the Geological Section. The beautiful grounds, containing the ruins of St. Mary's Abbey, &c., will be open to members and associates. The Yorkshire Fine Arts Institution will also be open, and the great hall will be used for some of the evening meetings. The Minster will be thrown open for inspection. Excursions are being organised to several places of interest, including Scarborough (where the Spa Company give free admissions), Whitby, Castle Howard, and the works of Messrs. Bolckow, Vaughan, and Co., at Middlesborough.

THERE has been recently some talk of establishing at Athens or Smyrna an American Institute for the training of Archæologists, and as a permanent committee for archæological research and correspondence. Two institutes with like aims are at present in existence, viz. the German Institut für archäologische Correspondenz, having its seat in Rome, with a branch in Athens, and the École Française d'Athènes, which has a branch in Rome. Mr. Thomas Davidson describes the work of these in a recent issue of the *Nation*, and advocates Smyrna as the place for the American Institute, as offering a more promising field of research than Athens, while there would be a better prospect of getting any antiquities discovered for museums. The cost of such an institution is estimated at 5000 dollars to begin with, for a library and necessary apparatus, and about 6000 dollars a year afterwards.

THE fourth and fifth numbers of *Naturen* for 1881 contain interesting summaries, by Dr. Hercules Tornö, of the results obtained by him, during the Norwegian Arctic Expedition, of the depths of the Arctic seas; the amount of salt contained in the water at various depths and at different distances from land; and the variations observable in the relative quantities of the different constituents of the air contained in sea-water. In regard to the latter point, it may be observed that the mean amount of oxygen present in the air was found to diminish with increasing depth below the surface of the water from 35·3 at the surface, to 32·8 between 1000 and 1400 fathoms; while the relative quantity of the nitrogen present rose with the increased depth from 13·1 at the surface to 14 between 600 and 1000 fathoms. Carbonic acid was found both in a gaseous and basic form.

WE notice a very interesting lecture which has been given at St. Petersburg on the use of the heliograph during the Trans-Caspian war. The heliograph used in the Russian army is that of Mans, and the alphabet is the usual one, that of Morse. The smaller system, which is employed in cavalry, transmits signals to a distance of seventeen miles, and the larger, employed in forts, has a double power. All independent parts of the army, on their march to Akhal-Tepe, had their "heliograph-drafts," and owing to the bright sky of the steppe, and to the level country, the heliograph was continuously used for establishing communication between different parts and small detachments of the army. The heliograph was at work during all the battles, and experiments were made as to the use of it during night, by means of lunar light, as well as with special lamps. The latter, however, being fed with turpentine, which evaporates very soon during the hot days of the summer, did not render great service. It was observed also that the sight of those who receive the heliogram gets very soon fatigued, which occasions error. But altogether the heliograph has rendered very great services during this campaign.

THE French Minister of Postal Telegraphy recently sent to the several telegraphic offices forms for recording all the observations connected with thunderstorms. The forms have been drawn up by M. Mascart, the head of the Meteorological Office, and printed at its expense.

A REPORT has been presented to the Paris Municipal Council on the state of telephonic exchanges in Paris, and the propriety of putting a tax on them for the use of sewers in which the wires are located. The number of telephonic halls will be increased, and six of them will be established shortly, which will bring their number to ten. At present the number of persons renting wires is a little more than 1000, and the average number of messages a little more than five a day for each.

M. MAREY has asked the Paris Municipal Council for the grant of a space of 1200 metres in the Park of the Champ de Mars for establishing a zoological station; but such a large space could not be afforded without inconvenience to foot-passengers, so he has accepted the grant of a space which was tendered to him at Passy, in the Park of Princes.

THE Congrès archéologique of France holds its forty-eighth session at Vannes (Morbihan) on June 28. Among other subjects to be discussed are the megalithic monuments of the Gulf of Morbihan, the chronology of sepultures, influence of soil on the distribution of megalithic monuments, bronze objects and other remains found in tombs of Brittany, Gallic and Roman coins, ante-Roman remains in Brittany.

TWENTY shocks of earthquake were reported from Szt. Ivan Zelina and Blazedotoc (Hungary) between May 20 and June 7. Some rather severe shocks also occurred on June 11 and 12, direction north-east to south-west.

THE Caucasian Museum at Tiflis is fast approaching completion under the active and energetic direction of Dr. Radde. The visitors from Western Europe, who are expected at the Archæological Congress, will already find a tolerably numerous collection of natural history objects and archæological specimens.

THE discoveries of remains of palæolithic man in Russia continue to be most interesting. Recently M. Shaposhnikoff discovered a great quantity of stone implements in the district of Valdaï, where a forest has been cut down and the wind has denudated the sand of the subsoil. The implements belong to four categories: (1) knives and saws similar to those of Moustier, St. Acheul, and Solutre, more perfect than any found previously; (2) the same in miniature, most accurate, and made of the finest kind of flint; they might have been used as ornaments, or for tattooing; (3) figures of animals and men made in flint, and relief pictures of the same, also in flint; (4) ornamental designs on stone. The collection is very rich, especially in miniature implements.

A LITTLE book just published by the Kössling'sche Buchhandlung (Gustav Wolf) of Leipzig is named "Naturwissenschaftlich-mathematisches Vademecum," being an alphabetical and systematic compilation of all modern publications in the domains of natural sciences and mathematics.

AT the meeting of the South-Eastern Railway Company the other day Sir Edward Watkin announced the complete success of the preliminary borings of the Channel Tunnel, and the resolution of his own Company on this side and the French Company on the other to make a further important step. A gallery seven feet in diameter has already been driven from the shaft near Abbots' Cliff for half a mile towards France, and an agreement has been made to push forward a similar headway under the sea for a mile on each side of the Channel. At the present rate of progress this will probably be done within the next six months, and then it is expected that the further nine miles on each side will be undertaken at once. All the conditions seem favourable to the project. The soil is found to be exactly similar at both ends. It is, as was expected, grey chalk impervious to water; and there is every reason to anticipate that it will be found to stretch in an unbroken bed across the Channel. Last week the machinery excavated sixty-seven yards of lineal distance, equal to about two miles a year. At this rate the two headways might meet under the middle of the Channel in about five years; and probably a nearly equal period might be occupied in enlarging this mere seven-feet burrow to a capacious railway tunnel.

IT is proposed in Edinburgh to make a three months' trial of lighting Princes Street and the North Bridge as far as the Tron Church with the electric light, on the Brush system.

BIRMINGHAM has resolved to invite the British Association to hold their annual meeting for 1883 in that town.

THE last number of the *Journal de Physique* describes a set of registering electrometers and magnetometers which are being tried at the Collège de France. The magnetic bars and the apparatus generally are very small. The instruments have been invented by M. Mascart, who believes they will give trustworthy results, and will compare favourably with the large magnetometers used in Kew and other places.

THE past winter cold in Norway, between October, 1880, and March, 1881, has exceeded the normal mean by 7° C. The greatest cold yet registered at any of the Norwegian meteorological stations occurred between January 13 and 15. At Karasjok, the lowest temperature was observed on February 4, when the thermometer fell to $-50^{\circ}\cdot 6$ C., the lowest ever noted in Norway with trustworthy instruments.

A WORK on the Butterflies of Europe, illustrated and described by Dr. Henry Charles Lang, F.L.S., will shortly be published in about twenty monthly parts. It will give accurate coloured figures of all the species of Rhopalocera found in Europe, showing both the upper and under side where necessary, as well as the differences of sex, if requiring a separate figure; and the most important of the named varieties will, when possible, be also represented. Typical illustrations of larvæ and pupæ will from time to time appear. The figures will be drawn from specimens in the author's collection. The description of a species will, whenever possible, include a notice of its transformation, habitat, and times of appearance, along with the principal synonyms and necessary references. The arrangement and nomenclature will be mainly those of Dr. Standinger's well-known catalogue. Each part will contain four coloured plates and sixteen pages of letterpress. The cost is very moderate. The publishers are Messrs. Reeve and Co., of Henrietta Street, Covent Garden.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*) from South Africa, presented by Mr. Thornburgh-Cropper; four Harvest Mice (*Mus minutus*), British, presented by Mr. Henry Laver; a Banded Grass Finch (*Poëphila cincta*) from Queensland; two Yellow-bellied Liothrix (*Liothrix luteus*) from India, presented by Mrs. Hylton Joliffe; a Red-legged Partridge (*Caccabis rufa*), European, presented by Mr. Arthur Morrell, School Ship *Cornwall*; a Horrid Rattlesnake (*Crotalus horridus*) from Bahia, presented by Dr. A. Stradling, C.M.Z.S.; a Patas Monkey (*Cercopithecus ruber*) from West Africa, a Blue Jay (*Cyanocitta cristata*) from North America, purchased; a Rhesus Monkey (*Macacus erythræus*), a Cape Buffalo (*Bubalus caffer*), born in the Gardens; seven Australian Wild Ducks (*Anas superciliosa*), five Chiloe Widgeons (*Mareca chiloensis*), a Mandarin Duck (*Aix galericulata*), two Geoffroy's Doves (*Peristera geoffroii*), two Turquoisine Parrakeets (*Euphema pulchella*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

THE SOLAR PARALLAX.—At the sitting of the Paris Academy of Sciences on the 6th inst. M. Tisserand communicated a note received from Mr. Todd of the office of the *American Ephemeris* at Washington, giving the value of the solar parallax deduced from the photographic operations of the American expeditions, as detailed in the "General Discussion of Results," a volume which has just been issued. The number of photographs is 213, distributed over various stations thus:—

Northern Hemisphere.		Southern Hemisphere.	
Wladiwostok	13	Kerguelen	8
Nagasaki	45	Hobart Town	37
Pekin	26	Campbelltown	32
		Queenstown	45
		Chatham Island	7

Putting dA for the correction of the difference of R.A. between the Sun and Venus, dD for the correction of the difference of declination, and $d\pi$ for that of the assumed value of the parallax ($8''\cdot848$) the solution of the equations of condition corresponding to the distances gives—

$$dA = +1''\cdot181 \ldots dD = +2''\cdot225 \ldots d\pi = +0''\cdot0397 \pm 0''\cdot0418.$$

Similarly from the equations corresponding to angles of position there result—

$$dA = +1''\cdot109 \ldots dD = +0''\cdot637 \ldots d\pi = +0''\cdot0252 \pm 0''\cdot0595.$$

Combining these values, the final results become—

$$dA = +0^s\cdot076 \ldots dD = +2''\cdot083 \ldots d\pi = +0''\cdot035 \pm 0''\cdot034,$$

and the corrected value of the solar parallax is thus $8''\cdot883$. It will be seen that the magnitudes of the probable errors are very large in proportion to the corrections obtained.

BIELA'S COMET IN 1805.—Those who have acquainted themselves with the history of this remarkable comet will remember that at its appearance towards the end of 1805 it was last observed in Europe by Thulis at Marseilles on the evening of December 9, at which time it was at its least distance from the earth, and was moving rapidly southwards. The comet was then visible to the naked eye, and it was known that it would probably continue so for some days, and might attract attention in the other hemisphere. But there was no southern observatory in existence at that time. Prof. Hubbard, in his masterly investigation on the motion of Biela's comet, remarked how greatly observations taken in the southern hemisphere at this appearance might have contributed to the progress of the theory of the comet. Gauss was then applying his methods for the determination of elliptic orbits, or rather of orbits without the assumption of a particular conic section, and it could hardly have happened that with good southern observations the nature of this comet's orbit would not have been detected from the observations of 1805, and the comet might thus have been re-observed in 1812, if not in 1819.

Up to a quite recent date it was not known that the comet had been observed in the other hemisphere, but Prof. Winnecke has discovered (where he does not say) some observations made at the Mauritius by MM. Malavois and Dupeloux on December 14 and 15. They were brought to light while he was inquiring into the periodicity of the comet detected at Strassburg by Dr. Hartwig on September 29, 1880, and he gives an account of them in the last number of the *Vierteljahrsschrift der Astronomischen Gesellschaft*. They are entitled "Observations sur la Comète qui a paru à l'Ile de France dans le cours de frimaire au 14e par M. Malavois et M. Dupeloux." M. Malavois states that he was apprised of the appearance of the comet on the morning of December 14. Four days previously (and therefore the day after it was last seen in Europe) it had been detected by MM. Laprie and Dabadie, the one a censor, the other a professor of the colonial Lycée: they had remarked that it had passed in a very short interval "over the space between the constellations Grus and Pavo, moving almost from north to south." On the evening of December 14 Malavois "avec un excellent sextant à lunettes d'un pied de rayon," measured the distance of the comet from Achernar and Canopus, and the distance between the stars, and Prof. Winnecke has been at the trouble to reduce these observations accurately, and to compare them with Hubbard's elements with the aid of an ephemeris calculated therefrom by Herr Kaufmann. Referring to the above-mentioned periodical for particulars, we may say that Hubbard's orbit is found to place the comet too much to the west by 13′ in right ascension, and in declination one minute to the north. But while giving these differences between calculation and observation, Prof. Winnecke only aims at proving that Biela's comet was recognised in the southern hemisphere, and well observed, considering the means available.

The description of the comet's appearance given by Malavois is worthy of particular remark:—"Cette comète est photosphèrique, c. à d. entourée d'une sphère lumineuse dont le diamètre m'a semblé être en totalité d'environ 45 minutes: mais la partie la plus lumineuse, ou l'auréole vue au commencement du crépuscule du soir n'étoit guère, que de 20 à 25 minutes; on la distinguoit alors très bien, tandis qu'à peine on pouvoit apercevoir les étoiles de 7me grandeur. La comète vue avec une lunette qui grossit seize fois le diamètre des objets m'a paru divisée en deux par une petite band obscure; j'ai jugé son diamètre apparent d'environ une minute, mais les bords m'ont semblé confusément terminés et se foudre avec la lumière nébuleuse; une étoile de 4e à 5e grandeur que je distinguais très bien dans l'auréole à 4 ou 5 minutes de la comète, s'est trouvée dans une position et une distance différente par rapport à cet astre dont le mouvement propre étoit en effet considérable, comme on la voir." Malavois' observations were limited to the evening of December 14, absence preventing his observing it on the following night, and clouds interfering on December 16 and 17; and he adds: "les jours suivans j'ai cessé de l'apercevoir."

Prof. Winnecke draws attention to the remarkable appearance of the comet under a power of 16, in the evidently small telescope, and asks: "Can we recognise in the 'petite bande obscure,' which divided the head into two parts, the commencement of the action which led to the highly important result, that in 1846 and 1852, instead of a single comet, two were observed, and later the comet has been no longer visible as such." In connection with the Mauritius observation it must be remembered that the aspect of the comet was particularly noted on December 8, when it was nearer to the earth than on December 14, by Olbers, Bessel, and Gauss, who agree in their description: on the same evening it was examined and measured by Schroeter with powerful reflecting telescopes at Lilienthal. Neither observer has any reference to the appearance of a division in the head of the comet on that date. Olbers says "it had a small but very distinctly defined nucleus, surrounded by an extensive nebulosity, without any appearance of a tail." The comet was visible to the naked eye with the brightness of a star of the third or fourth magnitude, and could be well seen after the moon had risen. Schroeter noted that without the telescope it appeared nearly as large as the moon: in his 13- and 15-feet reflectors it was apparently much diminished, his measures giving a diameter of only 5½ minutes: the diameter of the brightest part of the nucleus he found to be $4''\cdot05$, and that of the whole nucleus $6''\cdot42$; if a division had existed at the date of these measures it is hardly probable that it would have been overlooked by Schroeter. At the reappearance of the comet in 1826, and again in 1832, nothing of the kind was remarked. Hubbard, we know, considered that the division of the comet, from whatever cause it might be produced, took place at the end of the year 1844.

GEOGRAPHICAL NOTES

THE hydrographical expedition for the exploration of the mouth of the Obi River has started from St. Petersburg. It consists of four officers of the navy, one astronomer, M. Fuss, and two students of the St. Petersburg University, one of whom is a surgeon and the other a zoologist. Two small steamboats were sent to Perm, on the Kama River, and they will be transported to the Obi.

HERR SIEGFRIED LANGER of Vienna is about to undertake an exploring tour in Arabia under the auspices of the Vienna Geographical Society. His researches will be mainly of a linguistic nature, but scientific research is not excluded from his programme; he has prepared himself for the tour during the last few years at the Vienna University.

THE geographical department of the British Museum during the past year was deprived of the services of its able curator, Mr. R. H. Major, through ill-health, and on his retirement the opportunity was taken to reduce it to a sub-department. If we may judge by the newly published report of the British Museum, this change has not tended to increase activity in geographical matters, and among the most interesting acquisitions of the past year all that can be mentioned seem to be some old plans of towns and the like.

IN the last volume of Consular Reports S. de Zuccato at Venice furnishes an interesting map showing the lines proposed and in course of construction for the completion of the network of railways in Venetia. M. Consul Bernal also contributes a plan of the Havre docks.

IT is announced that the Portuguese travellers, Capello and Joens, are about to publish an account of their expedition under the title of "De Benguella as terras d'Iacca." The work will be issued in parts, and when complete, will form two volumes, illustrated with engravings.

HERR SCHÖLER, who was sent out by the German African Society, has returned to Zanzibar after founding a station at Kikoma.

THE Italian traveller, Piaggia, returned to Khartum on April 30. It is believed there that he will be appointed governor of the Fashoda district, and that the Austrian, Maruo, will become governor of the province of the Blue Nile.

THE Scientific Commission, recently despatched from Paris, has arrived at Zanzibar on its way to examine M. Paiva's vast concession in the Zambesi region, which it is proposed to develop by means of a company. The Commission is to investigate the resources of the territory, chiefly with regard to the mineral wealth supposed to exist there.

PHYSICAL NOTES

A FEW months ago the phenomenon of the "passive state" of iron was examined by M. L. Varenne, who attributed it to the presence of a film of nitrous acid gas upon the surface of the metal. The question has been recently reinvestigated by M. E. Bibart, who finds reason to doubt M. Varenne's conclusions. M. Bibart states that any oxidising agent aids, and any deoxidising agent hinders, the production of passivity.

IN a long memoir presented to the Académie des Sciences by Edmond and Henri Becquerel some very valuable data are given respecting the fluctuations of underground temperatures during 1880 beneath different surfaces. Their observations extended to a depth of 36 metres. The fluctuations were of less extent beneath herbage than below a bare soil, the maxima and minima being more retarded and of less amplitude in the former case. Another interesting point is the protecting effect of a bed of snow. Though the temperature of the air fell to $-15°$, and continued below 0° for long periods, that of the surface of the soil was rarely below $-1°$, never below $-1°\cdot5$.

ACCORDING to Nies and Winkelmann, who have lately studied the expansion exhibited by bismuth, cast-iron, and other metals during their solidification, the specific gravity of bismuth is between 1·031 and 1·0497 times as great in the liquid as in the solid state; a sample whose (solid) density was 10·2 assumed a density of 10·77 when melted. The ratio of the density in liquid state to that in solid state was greater than unity also for the metals tin and zinc, the ratio for tin being 1·0070, and for zinc 1·002. Our readers will doubtless recall the recent experiments of Mr. Wrightson and Prof. Chandler Roberts in the same direction.

HERR STUCKER concludes from experimental inquiry (*Wied. Ann.* No. 5), that the gases chlorine, bromine, and iodine, in regard to thermal behaviour, form a group by themselves among biatomic gases. The ratio of the kinetic energy of the progressive motion of the molecules to the total energy is different for them from that for the others. In their molecules the atoms seem to have a different reciprocal action. From the behaviour of biatomic gases it is inferred that neither Boltzmann's nor Maxwell's supposition as to the nature of the mobility of atoms in the gaseous molecule has a general validity.

WITH regard to the subject of hot ice, Herr Wüllner describes fresh experiments (*Wied. Ann.* No. 5), and he finds that so long as the thermometer-bulb is wholly surrounded with dry ice its temperature does not reach 0°. If the thermometer rises higher, either the bulb is no longer quite covered with ice, or it is surrounded with water, along with a thicker ice-layer. The author's method was to have the thermometer-bulb first coated with ice in a separate vessel; then introduced into the heating-tube and fixed in a caoutchouc stopper; this tube is connected through a tube and spherical vessel with the air-pump, and with the sphere is surrounded with a cold mixture while the vacuum is produced.

THE subject of double refraction of light in moving frictional liquids has been taken up anew by Herr Kundt (*Wied. Ann.* No. 5), using a method which Maxwell did not succeed with, viz., rotation of a cylinder within another cylinder, and sending a beam of polarised light in axial direction through liquid in the annular space. Herr Kundt got positive results in this way with various liquids. 1. The amount of internal friction of liquids is not a certain measurement of the occurrence of double refraction in motion; liquids with small friction giving considerable refraction, and *vice versâ*. 2. The liquids which, with small internal friction, prove doubly refractive, belong to the so-called colloids (gelatine, gum, collodion) or the oils. Solutions of crystalloids did not give the phenomenon by the method described. 3. The double refraction did not markedly affect the rotation of the plane of polarisation in the circularly polarising liquids (but the strongest refraction, it is to be noted, produced a difference of only about half a wave in penetrating a pretty long column of liquid). 4. In collodion-solutions the axes of the double refraction do not lie in the azimuths required by theory. The anomaly was not accounted for. Herr Kundt further offers some general remarks on the relations between the elastic properties of liquids, their coefficients of friction, and the double refraction developed in them.

IT has been hitherto supposed that light directly reflected from a diffraction-grating has the same state of polarisation as light passing through the same plate unruled, or reflected from its smooth surface. Herr Fröhlich now finds, with a very finely-ruled grating, that it is not so. The proof and numerical amount of the difference are indicated in *Wied. Ann.* No. 5.

IN the cold of last winter M. Damien (*Journ. de Phys.*, May investigated the indices of refraction of water under 0° (*i.e.* in surfusion) down to $-8°$. He measured the indices corresponding to the three hydrogen lines by the prism method. Starting with a temperature of $+20°$, he first confirmed M. Jamin's observation that the passage through the maximum of density does not at all disturb the course of the indices, and he further found that the indices continue to increase below zero, though the density diminishes. The variations of the indices are very small. M. Damien hopes, next winter, to apply the interferential method. (The use of freezing mixtures does not present such favourable conditions as the very slow cooling of the atmosphere.)

RECENT researches by Herren Sohncke and Wangerin on Newton's rings (*Wied. Ann.* Nos. 3 and 4) appear to require a considerable change of ideas as to this phenomenon, and more especially as to the place where interference occurs. The starting-point was an experiment in which the rings produced by a beam of parallel sodium light falling at an angle on a horizontal plate above a plane convex lens were examined with a microscope inclined at the same angle, and capable of being moved horizontally as well as in the direction of its axis. The microscope was first so placed that one part of a dark ring was as sharply defined as possible; the instrument being then moved along to another ring, or another part of the same ring, it was found necessary to move it axially, higher or lower, to get the maximum definition for that part; indicating that the rings do not lie in a horizontal plane, but in some other position. The amounts of axial displacement for different parts of the ring-system were carefully noted. For details of the results we must refer to the original, merely noting, *inter alia*, that the places of interference in the plane of incidence going through the centre of the rings seem to lie in a straight line rising towards the side whence the light comes. In a central plane at right angles to that of incidence, all the places are at the same depth. Herr Sohncke undertook the experimental part in this investigation, while Herr Wangerin has worked out the theory of the phenomena.

ACCORDING to experiments by Herr Kundt (*Wied. Ann.* No. 4), the common surface-tension between liquid and gas decreases considerably with increasing pressure of gas in the case of alcohol, ether, alcoholic solution of calcium-chloride, sulphide of carbon, chloroform, and water. The decrease is greater at low pressures than at high. For a given liquid it varies with the nature of the gas compressed. With alcohol, ether, and alcoholic chloride of calcium solution, air causes a greater decrease of the capillary constant than hydrogen. The decrease is so great with some liquids (*e.g.* ether in air) that probably, with pressures reached without much difficulty, the surface-tension is *nil*, the liquid passing at ordinary temperature into the Cagniard de la Tour state. (The author's experiments were concluded before he knew of Cailletet's experiment, in which a mixture of five vols. of CO_2 and one vol. of air is compressed at a low temperature till the meniscus of CO_2 disappears, and the Cagniard de la Tour state is reached.)

THE ratio of intensity of the two sodium lines has been estimated by Herr Dietrich (*Wied. Ann.* No. 4) using apparatus of great dispersion with a Vierordt double slit giving one spectrum above another, and allowing of displacement, so that one of the two lines in one spectrum is brought directly over the other in the other spectrum. The photometric parts included a Nicol capable of rotation, a right and left rotating double quartz, and a fixed Nicol. The mean value of the ratio sought, from measurements on three days, was 1·60 ± 0·01; the probable error of an observation being ± 0·032.

THE amount of electric expansion in caoutchouc has been investigated by Herren Korteweg and Julius (*Wied. Ann.* No. 4). They used tubes of white vulcanised caoutchouc made insulating in water by being kept twenty-four hours in oil; the tube was filled with water and placed in a water-bath (to form a condenser), while hydrostatic pressure was varied, and the changes of volume were measured by means of a connected tube-system. The change of volume is shown to be proportional to the square of the striking distance, and inversely proportional to the square of the thickness of tube-wall. It is the same whether the inner liquid be charged negatively or positively. The maximum was reached when the Holtz machine, left to itself, gradually slackened speed till the last spark passed. A table, giving also Quincke's data for glass, shows that both for this and for caoutchouc the volume-changes are, roughly speaking, in inverse ratio of the coefficients of elasticity, so that they must be ascribed to the same cause.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—During the Long Vacation there will be a course of lectures in the University Laboratory by Mr. Fenton, one of the Demonstrators, on Organic Chemistry. The Laboratory will be open for practical work.

During the Michaelmas Term there will be twelve courses of lectures on Chemistry and various branches of Physics, including one by Mr. Shaw on the Conservation of Energy and Theory of Unitation, and a course by Prof. Lewis on the more Common Mineral Rock-Constituents, and another on Descriptive Crystallography by the same.

Prof. Dewar's subject will be Physical Chemistry; and among other advanced lectures will be a course on the General Principles of Chemistry by Mr. Pattison-Muir.

Prof. Hughes and Mr. Tawney will divide between them the work in Geology, Mr. Tawney taking Palæontology in the Michaelmas Term.

Lectures on Botany will be given by Messrs. J. W. Hicks, Vines, Saunders, and Hillhouse. Dr. Vines' lectures in the Michaelmas Term will be on the Physiology of Plants, with practical work, at Christ's College.

Prof. Newton and the Demonstrator of Comparative Anatomy, Dr. Michael Foster, and his corps of lecturers, Prof. Humphry, Mr. Creighton, and Mr. Balfour, will give their usual series of lectures and demonstrations during the Michaelmas Term. Mr. Lea will give advanced lectures on Digestion and Chemical Physiology, and Mr. Langley will lecture on the Histology and Physiology of Muscle, Nerve and the Nervous System.

Dr. Bradbury will lecture on Pathological Anatomy, Prof. Latham on General Therapeutics, Prof. Paget on Clinical Medicine, and Mr. Carver on Clinical Surgery.

The Natural Sciences Tripos, Part I., under the Old Regulations, has just been completed, and the pass-list contains the names of thirty-three men, and three are excused the General Examination. The second part of this examination takes place in December.

The first part of the examination under the New Regulations, by which men can enter for the examination in their second year if they prefer, has just resulted in the publication of a list with five names, in alphabetical order, in the first division, viz. Messrs. Daniels (Trinity), Duncan (Caius), Earl (Christ's), Sherrington (Caius), and Wilberforce (Trinity).

The special examinations in Natural Science for the ordinary B.A. degree have yielded seven men in the first class in Chemistry, and eighteen in the second class. In Geology there was but one man in the first and one in the second class; in Botany, one in the first class; in Zoology, one in the second class.

At Trinity College the prizemen in the June examination in Natural Science were: Third year, Hillier and Ritchie; second year, Daniels and Wilberforce; first year, Davis, Head, Ransom, Thompson. The prizemen at Christ's College are Shipley (first year), Earl (second year), and Parkyn (third year).

The Chancellor of the University (the Duke of Devonshire) has (with the concurrence of Earl Cairns, Chancellor of the University of Dublin) declared that the statutes of Cambridge do not preclude the University from using the Previous and the Tripos Examinations for the purpose of testing the proficiency of women, as sanctioned by the Senate on February 24, 1881.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 19.—"Molecular Magnetism," by Prof. D. E. Hughes, F.R.S.

1. *Influence of an Elastic Torsion upon a Magnetic or an Electric Conducting Wire.*—In my paper of March 7th on "Molecular Electro-Magnetic Induction," I showed that induced currents of electricity would be induced in an iron wire placed on the axis of a coil through which intermittent currents were passing, and that these currents were produced only when the wire was under the influence of a torsion not passing its limit of elasticity. It became evident that if the intermittent magnetism induced by the coil produced under torsion intermittent currents of electricity, an intermittent torsion under the influence of a constant current of electricity or a constant magnetic field would produce similar currents. This was found to be the case, and as some new phenomena presented themselves indicating clearly the molecular nature of the actions, I will describe a few of them directly relating to the subject of this paper.

The apparatus used was similar to that described in my paper of March 7th. An iron wire of 20 centims. was placed in the centre or axis of a coil of silk-covered copper wire, the exterior diameter of the coil being 5½ centims., having an interior vacant circular space of 3½ centims. The iron wire is fastened to a support at one end, the other passing through a guide, to keep it parallel but free, so that any required torsion may be given to the wire by means of a connecting arm or index. A sensitive telephone is in direct communication with the coil, or a galvanometer may be used as the currents obtained by a slow elastic torsion are slow and strong enough to be seen on a very ordinary galvanometer. I prefer, however, the telephone, because it has the inestimable advantage in these experiments of giving the exact time of the commencement or finish of an electric current. It has, however, the disadvantage of not indicating the force or direction of the current; but by means of the sonometer the true value and direction of any current is at once given. The current from a battery of two bichromate cells is sent constantly through the wire if we wish to observe the influence of the torsion of the wire upon the electric current, or a constant field of magnetic energy is given to the wire by either a separate coil or a permanent magnet. The currents obtained in the coil are induced from the change in the molecular magnetism of the wire, but we may equally obtain these currents on the wire itself without any coil by joining the telephone and rheotome direct to the wire; in the latter case it is preferable to join the wire to the primary of a small induction coil, and the telephone and rheotome upon the secondary, as then the rheotome does not interrupt the constant electric current passing through the wire. As the results are identical, I prefer to place the telephone on the coil first named, as the tones are louder and entirely free from errors of experimentation.

If we place a copper wire in the axis of the coil we produce no effect by torsion, either when under the influence of a constant magnetic field or a current passing through it, nor do we perceive any effects if we place an iron wire (2 millims. in diameter), entirely free from magnetism, and through which an electric current has never passed. I mention this negative experiment in order to prove that all the effects I shall mention are obtained only through the magnetism of the wire. If now I pass an electric current for an instant through this same wire, its molecules are instantly polarised, and I have never yet been able to restore the wire to its original condition, and the magnetisation induced by the passage of a current is far more powerful and more persistent in soft iron than tempered steel. This may be due, however, to the fact that in tempered or softened steel we find traces only of a current during the rotation by torsion of its molecules some two to three degrees of sonometer, whilst iron gives constantly a current of 70 sonometric degrees.[1]

In order to obtain these currents, we must give a slight torsion of 5° or 10° to and fro between its zero point. We then have a current during the motion of the index to the right, and a contrary current in moving the index to the left. If we use a

[1] 0·8 of a Daniell battery.

galvanometer, we must time these movements with the oscillations of the needle; but with the telephone it gives out continuous sounds for either movement, the interruptions being only those caused by the rheotome. The direction of the current has no influence on the result; either positive to the free arm or index or negative gives equal sounds, but at the moment of reversal of the current a peculiar loud click is heard, due to the rapid change or rotation of the polarisation of its molecules, and this peculiarly loud momentary click is heard equally as well in steel as in iron, proving that it is equally polarised by the current, but that its molecular rigidity prevents rotation by torsion.

If a new soft iron wire of two millims. (giving no traces of a current by torsion) has passed through it a momentary current of electricity, and then the wire observed free from the current itself, it will be found to be almost as strongly polarised as when the current was constantly on, giving by torsion a constant of fifty sonometric degrees. If, instead of passing a current through this new wire, I magnetise it strongly by a permanent magnet or coil, the longitudinal magnetism gives also 70° of current for the first torsion, but weakens rapidly, so that in a few contrary torsions only traces of a current remain, and we find also its longitudinal magnetism almost entirely dissipated. Thus there is this remarkable difference, and it is that whilst it is almost impossible to free the wire from the influence produced by a current, the longitudinal magnetism yields at once to a few torsions. We may, however, transform the ring or transversal magnetism into longitudinal magnetism by strongly magnetising the wire after a current has passed through it; this has had the effect of rotating the whole of the molecules, and they are all now symmetrical with longitudinal magnetism, then by a few torsions the wire is almost as free as a new wire; and I have found this method more efficacious than heating the wire red hot, or any other method yet tried. If I desire a constant current from longitudinal magnetism, I place at one of the extremities of the wire a large permanent magnet, whose sustaining power is five kilometres, and this keeps the wire constantly charged, resembling in some respects the effects of a constant current. The molecular magnetism or the current obtained by torsion is not so powerful from this, my strongest magnet, as that produced by the simple passage of a current, being only 50 sonometric degrees in place of 70° for that due to the passage of a current. The mere twisting of a longitudinal magnet, without regard to the rotation of its molecule having no effect, is proved by giving torsion to a steel wire strongly magnetised, when only traces of a current will be seen, perhaps one or two degrees, and a constant source of magnetism or electricity then giving no measurable effect.

If we magnetise the wire whilst the current is passing, and keep the wire constantly charged with both magnetism and electricity, the currents are at once diminished from 70° to 30°. We have here two distinct magnetic polarisations at right angles to each other, and no matter what pole of the magnet or of the current the effect is greatly diminished; the rotation of the two polarities would now require a far greater arc than previously. The importance of this experiment cannot as yet be appreciated until we learn of the great molecular change which has really occurred, and which we observe here by simply diminished effects.

If we heat the wire with a spirit flame, we find the sounds increase rapidly from 70 to 90, being the maximum slightly below red heat. I have already remarked in my previous paper this increased molecular activity due to heat, and its effects will be more clearly demonstrated when we deal with the sounds produced by intermittent currents.

2. *Influence upon the Molecular Structure of an Iron or Steel Wire by Electricity or Magnetism.*—Being desirous to modify the apparatus already described, so that it would only give indications of a current if they were of a spiral nature, the wire was kept rigidly at its zero of strain or torsion, and the coil was made so that it could revolve on an axis perpendicular to the wire; by this means, if the wire was free from strain, the centre or axis of the coil would coincide with that of the wire. Thus, with a straight copper wire, we should have a complete zero, but if this wire formed a right or left-handed helix, the coil would require moving through a given degree (on an arbitrary scale) corresponding to the diameter and closeness of the spirals in the helix; the degrees through which the coil moved were calibrated in reference to known copper helices. 50° equalled a copper wire 1 millim. diameter, formed into a helix of 1 centim. diameter, whose spiral turns were separated 1 centim. apart.

In order to obtain a perfect zero, and wide readings, with small angular movement of the coil, it is necessary that the return wire should be of copper, 2 millims. diameter, offering comparatively little resistance, and that it should be perfectly parallel with the steel or iron wire.

The rheotome is joined to a battery of two bichromate cells, and by means of a reversing switch, an intermittent current of either direction can be sent through the wire. The telephone is joined direct and alone to the coil, thus no currents react upon the coil when perpendicular to the iron, and its return wire, if not of a spiral nature.

Placing an iron wire 0·5 diameter, and passing a current through it, I found a change had taken place similar to those indicated in my paper of March 17th; but it was so difficult to keep the wire free from magnetism and slight molecular strains, that I preferred and used only in the following experiments tempered steel wire (knitting-needles I found most useful). All the effects are greatly augmented by the use of iron wire, but its molecular elasticity is so great that we cannot preserve the same zero of reading for a few seconds together, whilst with steel, 0·5 millim. diameter, the effects remained a constant until we removed the cause.

I have not as yet been able to obtain a steel wire entirely free from magnetism, and as magnetism in steel has a remarkable power over the direction of the spiral currents, I will first consider those in which I found only traces. On passing the intermittent current through these, the sounds were excessively feeble for either polarity of current, but at each reversal a single loud click could be heard, showing the instant reversal of the molecular polarity. The degree of coil indicating the twist or spirality of the current was 5° on each side of its true zero. The wire was now carefully magnetised, giving 10° on each side for different currents. The positive entering at north pole indicating 10° right-handed spiral, negative entering the same pole, a left-handed spiral, we here see in another form, a fact well known and demonstrated by De la Rive by a different method, that an electric current travels in spirals around a longitudinal magnet, and that the direction of this spiral is entirely due to which pole of an electric current enters the north or south pole. I propose soon, however, to show that under certain conditions these effects are entirely reversed.

If through this magnetised wire I pass a constant current of two bichromate cells, and at the same time an intermittent one, the spiral is increased to 15°, but the direction of the intermittent current entirely depends on that of the constant current; thus, if the positive of the constant current enters the north pole, the intermittent positive slightly increases the spiral to 17°, and the negative to 13°, both being right-handed; the two zeros of the constant battery are, however, as we might expect from the preceding experiment, on equal opposite sides of the true zero; but if we magnetise the wire whilst a constant current is passing through it, a very great molecular disturbance takes place; loud sounds are heard in the telephone, and it requires for each current a movement of the coil of 40°, or a total for the two currents of 80°. This, however, is not the only change that has taken place, as we now find that both constant currents have a right-handed spiral; the positive, under which it was magnetised, a right-handed spiral of 95°; the negative, a right-handed spiral of 15°, and the true central or zero point of the true currents indicates a permanent spiral of 55°.

This wire was magnetised in the usual way, by drawing the north pole of my magnet from the centre to one extremity, the south from the centre to the other, and this repeated until its maximum effects were obtained; in this state I found, sliding the coil at different portions, that the spiral currents were equal, and in the same direction throughout.

It now occurred to me to try the effect of using a single pole of the magnet; this was done whilst a constant current was passing through the wire, commencing at the extremity, where the positive joined, drawing the north pole through the length of the wire, from positive towards the negative; the effect was most remarkable, as the steel wire now gave out as loud tones as a piece of iron, and the degree on the coil showed 200°. The constant and intermittent currents now showed for either polarity a remarkably strong right-handed twist; the positive 200 right, and the negative 150 right-handed spirals; the molecular strain on its wire from the reaction of the electric current upon the molecular magnetism was so great, that no perfect zero would be obtained at any point, a fact already observed when a wire was under an intense strain, producing tertiary currents that superposed themselves upon the secondary. In order to compare these spiral currents with those obtained from a known helix, I found that taking a copper wire of similar diameter (0·5 millim.), and winding it closely upon the steel wire ten turns to each centimetre, having a total of 200 turns, with an exterior diameter of 1·5 millims., withdrawing the steel wire, leaving this closely wound helix free, that it gave some 190°, instead of the 200 of the steel wire alone; thus the spiral currents fully equalled a closely wound copper wire helix of 200 turns in a similar length.

If it were possible to twist a magnetised wire several turns to the right, and that its line of magnetism would coincide with that of the twist, then on passing a positive or negative current, there would be an apparent augmented or diminished spirality of the current, but both would have a right-handed twist. The result would be identical with the phenomenon described, although the cause is different.

The explanation of this phenomenon can be probably found in the fact that the constant spirality now observed is that of the electric current under which it was magnetism, for whilst magnetising it we had a powerful source of magnetism constantly reacting upon the electric current, and the constant spirality now observed is the result or remains of a violent molecular reaction at the instant of magnetisation, and the remaining evident path or spiral is that of the electric current. On testing this wire as to its longitudinal magnetic force, I found that it was less than a wire simply magnetised in the usual way; thus the effects are internal, affecting the passage of the electric current, giving, however, no external indications (except apparent weakness) of the enormous disturbance which has taken place.

3. *Molecular Sounds.*—The passage of an intermittent current through iron or other wire gives rise to sounds of a very peculiar and characteristic nature. Page in 1837 first noticed these sounds on the magnetisation of wires in a coil. De la Rive published a chapter in his "Treatise on Electricity" (1853) on this subject, and he proved that not only were sounds produced by the magnetisation of an iron wire in an inducing coil, but that sounds were equally obtained by the passage direct of the current through the wire. Gassiot, 1844, and Du Moncel, 1878–81, all have maintained the molecular character of these sounds. Reis made use of them in his, the first electric telephone invented, and these sounds have been, since the apparition of Bell's telephone, often brought forward as embodying a new form of telephone. These sounds, however, for a feeble source of electricity, are far too weak for any applied purposes, but they are most useful and interesting where we wish to observe the molecular action which takes place in a conducting wire. I have thus made use of these sounds as an independent method of research.

The apparatus was the same as in the last chapter, except no telephone was used. The intermittent electric current was connected by means of switch key, either with the coil inducing longitudinal magnetism in the wire, or could be thrown instantly through the wire itself, thus rapid observations could be made of any difference of tone or force by these two methods; a reversing key also allowed when desired a constant current of either polarity to pass through the wire under observation.

Iron of all metals that I have yet tried gave by far the loudest tones, though by means of the microphone I have been able to hear them in all metals; but iron requires no microphone to make its sounds audible, for I demonstrated at the reading of my paper, March 31st, that these sounds with two bichromate cells were clearly audible at a distance. A fine soft iron wire (No. 28) is best for loud sounds to be obtained by the direct passage of the current, but large wires (1 millim.) are required for equally loud tones from the inducing coil. By choosing any suitable wire between these sizes, we can obtain equal sounds from the longitudinal magnetism or direct current. The wire requires to be well annealed; in fact, as in all preceding experiments, the sounds are fully doubled by heating the wire to nearly red heat. There are many interesting questions that these molecular sounds can aid in resolving, but as I wish to confine the experiments to the subject of the two preceding chapters, I will relate only a few which I believe bear on the subject.

On sending an intermittent electric current through a fine soft iron wire we hear a peculiar musical ring, the cadence of which is due to that of the rheotome, but whose musical note or pitch is independent both of the diameter of the wire and the note which would be given by a mechanical vibration of the wire itself. I have not yet found what relation the note bears to the diameter of the wire; in fact, I believe it has none, as the greatest variation in different sizes and different conditions has never exceeded one octave, all these tones being in our ordinary treble clef, or near 870 single vibrations per second, whilst the mechanical vibrations due to its length, diameter, and strain vary many octaves.

I believe the pitch of the tone depends entirely upon molecular strain, and I found a remarkable difference between the molecular strain caused by longitudinal magnetism and the transversal or ring magnetism produced by the passage of a current, for if we pass the current through the coil, inducing magnetism in the wire, and then gradually increase the longitudinal mechanical strain by tightening the wire, the pitch of the note is raised some three or four tones (the note of the mechanical transversal vibrations being raised perhaps several octaves); but if we tighten the wire during the passage of an electric current through it, its pitch falls some two or three notes, and its highest notes are those obtained when the wire is quite loose. A similar but reverse action takes place as regards torsion; for if the wire is magnetised by the coil we obtain an almost complete zero of sound by simply moving the torsion index 45° on either side, and as this was the degree which gave silence in the previous experiments for the same wire, it was no doubt due to the same rotation of its polarised molecules. If we now pass a constant current through the wire whilst the intermittent one is upon the coil, we hear augmented sounds, not in pitch but loudness, and if we give torsion of 45° to one side we have silence, or nearly so, whilst the other side it gives increased tones which become silence by reversing the battery. If whilst the wire by torsion has been brought to zero, we decrease or increase the mechanical longitudinal strain, then at once the polarised molecules are rotated, giving loud sounds; and we further remark that when the wire is loosened, and we again tighten it, we gradually approach a zero, and on increasing the strain the sounds return; thus we can rotate the molecules by a compound strain of torsion and longitudinal strain.

If we wish to notice the influence of a constant current passing through the wire under the influence of the intermittent current in the coil, we find that if the wire is free from torsion that on passing the current the tones are diminished or increased according to the direction of the current; the tones then have an entirely distinctive character, for whilst preserving the same musical pitch as before, the tones are peculiar, metallic, and clear, similar to when a glass is struck, whilst the tones due to longitudinal magnetism are dull and wanting in metallic timbre. If we now turn the index of torsion upon one side, we have a zero of sound with or without the current; but the opposite direction gives increased tones whilst current is passing through the wire, but zero when not. Here again a peculiarity of timbre can be noticed, as although we have loud tones due only to the action of the current through the wire, the timbre is no longer metallic, but similar to that previously given out by the influence of the coil; evidently then the metallic ring could only be due to the angular polarisation of the molecules, and when these were rotated by torsion the tones were equally changed in its action upon the wire.

I have already shown that a permanent magnet brought near the wire could rotate its polarisation, and it equally can produce sound or silence in these molecular sounds (during that the wire is at its zero of torsion, and a constant current sent through the wire as in the last experiment). We find that either pole of the natural magnet has equal effect in slightly diminishing the sound by an equal but opposite rotation from the line of its maximum effects; but if the wire is brought nearly to zero by torsion, then on approaching one pole of the natural magnet we produce a complete silence, but the opposite pole at once rotates the molecule to its maximum loudness, and on taking away the magnet we have comparative silence as before.

Heating the wire to nearly red heat by a spirit lamp increases the tones of longitudinal magnetism induced by the coil some 25 per cent., but it has a much more marked increase on the tones produced by the direct passage of the current where they have more than 100 per cent. increase; and if we pass the intermittent current through the coil and constant through the wire, we find no direct rotation of the molecules by heat, although an apparent rotation takes place if we by the required torsion first place the wire at its zero. Then on the application of heat faint sounds are heard, which become again almost silent on cooling; this is simply due to the diminution by heat of the effect of the elastic torsion.

Tempered steel gave exceedingly faint tones, requiring the use of the microphone; but on magnetising with a constant current, inducing spiral magnetism, the sounds became audible, some 15° sonometer against 175° for iron; thus the molecular rigidity of steel as observed by previous methods was fully verified.

I have, I believe, demonstrated by actual experiments which are easy to repeat, that—

1. An electric current polarises its conductor, and that its molecular magnetism can be reconverted into an electric current by simple torsion of its wire.

2. That it is by the rotation of its molecular polarity alone that an electric current is generated by torsion.

3. That the path of an electric current through an iron or steel wire is that of a spiral.

4. That the direction of this spiral depends on the polarity of the current, or that of its magnetism.

5. That a natural magnet can be produced, having its molecular arrangement of a spiral form, and consequently reversed electric currents would both have a similar spiral in passing through it.

6. That we can rotate the polarised molecules by torsion or a compound strain of longitudinal and transversal.

7. That the rotation or movements of the molecules give out clear audible sounds.

8. That these sounds can be increased or decreased to zero by means that alone have produced rotation.

9. That by three independent methods the same effects are produced, and that they are not due to a simple change or weakening of polarity, as when rotation has been incomplete a mere mechanical vibration has at once restored the maximum effect.

10. That heat, magnetism, constant electric currents, mechanical strains and vibrations have all some effect on the result.

Linnean Society, June 2.—Sir John Lubbock, Bart., F.R.S., in the chair.—Mr. R. Romanis of Rangoon was elected a Fellow of the Society.—Dr. G. Hoggan exhibited and made remarks on preparations of the lymphatics of vascular walls.—Mr. Elwes exhibited samples of quinine made by a new process by Mr. Gammie of Sikkim.—Mr. Thos. Christy drew attention to living rubber plants from West Africa, viz., *Urostigma Vogelli* and *Tabernæmontanum crassa*, and he showed products of *Pistacia terebinthus*, viz., the nuts, the resin, and the so-called butter, separated from the resin, and used for sweetmeats in the East, also Chian turpentine from the same tree.—Sir J. Lubbock afterwards read a paper on the habits of ants, for abstract of which see p. 142.—Mr. S. O. Ridley read a paper on the genus *Plocamia* of Schmidt and some other Echinonematous sponges, with reference to the genus mentioned, for which he accepted Prof. Duncan's name of *Dirrhopalum*. He enumerated three species already described, but assigned to other genera, which must be added to it; the distribution is thus extended from the tropical Atlantic to the British, Portuguese and Ceylon Seas. He described a New Zealand species which proves to be new to science and appears to decide a point which has been disputed, viz., the existence of ceratinous material in the skeleton. Geological facts were brought forward showing the existence of the genus in the Eocene, Upper Chalk, and Greensand formations. A new genus of the same order was described, based on a species of Lamarck and two other species; it is closely allied to *Dictyocylindrus*; its distribution extends from Arabia to Australia.—Prof. Duncan also read a communication on two new species of sponges of the genus *Dirrhopalum* from the Atlantic sea-bed.—The ninth part of the Rev. Boog Watson's mollusca of the *Challenger* Expedition, family Pleurotomidæ, was read by the Secretary.

June 16.—Sir John Lubbock, Bart., president, in the chair.—Mr. Alex. Somerville, Capt. J. T. Wright, and Mr. John Forrest, the Australian explorer, were elected Fellows of the Society.—Mr. W. Hood Fitch exhibited a set of folio drawings of new orchids, species of *Odontoglossum*.—The Rev. W. Higgins showed a Holothurian (*Psolus squamatus*) got between the Falklands and Patagonia, originally figured by Otho Fred. Müller in his "Zoologia Danica," and now recorded of wide distribution.—A letter was read from Mr. W. Ferguson of Colombo, mentioning his having found *Wolffia arrhiza*, Wimm., in abundance, and discovery of *Adiantum Æthiopicum*, L., in the Kandyan country, both plants being new to Ceylon.—Mr. J. G. Baker exhibited a specimen of the inflorescence of *Aloe Paryii*, which had flowered for the first time in this country, although the drug obtained from the plant had been known for 2000 years.—Surgeon-Major Aitchison then read a communication on the flora of the Kuram Valley, Afghanistan (Part II.); he showed by a map the peculiarities of the vegetation of the district, and in illustration of his paper referred to a series of dried specimens of the plants and the products in use by the natives, and otherwise characteristically interesting.—The next paper read was on Central African plants collected by Major Serpa Pinto, by Prof. Count Ficalho and W. P. Hiern. The specimens herein discussed were collected by Major Serpa Pinto in the month of August, 1878, along the upper course of the River Ninda, an affluent of the Zambesi, on the west side of the high plateau. As regards the climate of this locality the temperature is described as variable, the weather as very dry during seven or eight months of the year, and very wet during two or three months. The nature of the soil is metamorphic argillaceous schist; the latitude is 14° 46′ S., the longitude 20° 56′ E., and the elevation 1143 metres above the ocean. The rest of the botanical collections made by Major Serpa Pinto at different points of the journey, which were much more considerable, were lost. The present little collection consists of seventy-two numbers, comprising sixty-five species in thirty-nine genera; twelve of these species are new or not previously described and published, and at least one new genus appears amongst them. Some of the specimens are too imperfect for final determination, and several of the grasses and sedges can only be generally referred to their approximate position, and not specifically ascertained. In the case of the previously-known species, the affinities of many of the species are not only with the flora of Huilla in South Angola, but also in several instances with that of extra-tropical South Africa; only a few of the species are widely distributed in the tropics of this and other continents.—There followed a paper by Mr. Edward J. Miers, viz., revision of the Idoteidæ, a family of Sessile-eyed Crustacea; and another by Prof. Ewart on the nostrils of the Cormorant. Certain structural peculiarities in the latter were described, these apparently accounting for or being related to a certain extent with the habit of the bird of flying with its mouth open.

Physical Society, June 11.—Lord Rayleigh in the chair.—New Members: Mr. J. E. H. Gordon and Mr. J. E. Stead, E.I.C.—Prof. Fleming exhibited a new form of B.A. unit-resistance coil devised by him for experiments in the Cavendish Laboratory, with a view to obviate the leakage in the older form due to condensed moisture on the paraffin insulating the electrodes, and also to facilitate the equalisation of the temperature of the coil with the medium it is placed in. For this purpose the wire is wound bare, each layer being insulated from the rest by ebonite fenders nitched to receive the turns, and the coil is inclosed in a brass box screwed together. This box is water-tight, and may be soldered or provided with a leaden washer between the two flanged halves, which screw together. Dr. Stone said he usually insulated the B.A. coil, when plunging it in water, by putting it in a beaker of paraffin oil, which was immersed in turn in the water. Prof. Ayrton pointed out the advisability of makers aiming rather at turning out exact resistances of any definite value, rather than wasting time and increasing the cost of the coils by adjusting them to a given figure, such as 1 or 10 ohms.—Prof. W. Chandler Roberts read a paper on the hardening and tempering of steel. He pointed out that few questions connected with the metallurgy of iron or steel are attracting more attention now than the relation between a metal and the gases it comes into contact with during manufacture. The carburisation of iron has long been of great interest, as shown by the work of Clouet at the end of last century, and that of Margueritte in 1865, who showed that though the conversion of iron into steel could be effected by contact with carbon even in the diamond form, it is nevertheless true that in the ordinary process carbonic acid plays a considerable part which had been overlooked. Graham's 1867 paper on the occlusion of gases by metals gave point to Margueritte's work by showing that carbonic oxide can penetrate to the centre of a mass of iron. It is introduced, in fact, at a comparatively low temperature, while a high temperature is necessary to enable the metal to appropriate the carbon and become steel. Metallurgists are now carefully investigating the effect of occluded gases in iron and steel. Prof. Roberts considered the point recently raised as to whether the hardening and tempering of steel might not be influenced by the occlusion or expulsion of gas. He described experiments by which he proves that, as steel hardens when rapidly cooled *in vacuo*, gases could not play any part in the tempering. He also dwelt on the precautions necessary to keep the metal in the experiment free from occluded gas. He then showed that

Bergman (to whom we owe our knowledge that the difference between wrought iron and steel depends on the carbon in the latter) showed in 1781 that fixed air could give up its carbon to iron; and he concluded by showing that Réaumur, so long ago as 1722, actually employed the Torricellian vacuum in experiments on the tempering of metal, the metal being placed red-hot in a highly rarefied atmosphere. Réaumur also had a clear view of the effect a gas might have on the physical properties of a metal—a point of great interest to physicists in general. Prof. Hughes expressed the opinion that temper was not due to absorption of hydrogen, but to the combination of carbon with the iron. Mr. Stroh had found that an electrically-fused steel contact was glass hard. Prof. Guthrie exhibited a steel chain which he had beautifully blackened by dipping in fused nitre. The skin might be useful in the arts, and was perhaps analogous to that produced by Barff's process. Lord Rayleigh, M. Walenn, Mr. Lecky, Dr. Coffin, Prof. Ayrton, and others continued the discussion.—Mr. Grant then read a paper on curves of electro-magnetic induction, which he had traced out by means of primary and secondary coils, sliding on frames so as to take different positions with respect to one another. The paper was illustrated by experiments and diagrams.—Prof. Reinhold then read portions of a paper by Prof. S. P. Thompson on the opacity of tourmaline crystals. The optical and electric properties of these crystals are related; and Prof. Thomson propounds an explanation of this connection based on the late Clerk-Maxwell's electro-magnetic theory of light. The full paper will be published in the *Journal* of the Society.

Meteorological Society, June 15.—Mr. G. J. Symons, F.R.S., president, in the chair.—Eleven gentlemen were elected Fellows of the Society, viz. F. Crowley, A. M. Davis, Rev. R. Drake, F. H. D. Eyre, W. M. Gibson, E. W. Mathew, J.P., D.L., J. Parnell, M.A., F.R.A.S., J. Rigby, T. G. Rylands, F.L.S., F.G.S., H. Smith, and A. H. Wood.—The following papers were read:—The use of synchronous meteorological charts for determining mean values over the ocean, by Charles Harding, F.M.S.—The climate of Fiji, by R. L. Holmes, F.M.S. This paper gives the results of meteorological observations taken at Delanasau, Bua, Vanua Levu, during the ten years 1871–80.—Note on the formation of hail, by J. A. B. Oliver.—Note on a comparison of maximum and minimum temperature and rainfall observed on Table Mountain and at the Royal Observatory, Cape Town, during January and February, 1881, by John G. Gamble, M.A., M.Inst.C.E., F.M.S.—Mr. E. J. Spitta exhibited and described a new mercurial maximum and minimum registering thermometer.

PARIS

Academy of Sciences, June 13.—M. Wurtz in the chair.—The first volume of Annals of the Rio Janeiro Observatory was presented.—The following papers were read:—On a simple law of natural circular or magnetic double refraction, by M. Cornu. The decomposition of a wave polarised rectilinearly into two waves polarised circularly in opposite directions is such that the mean of the velocities of propagation of the resultant waves is equal to the velocity of the single wave which exists where the causes of decomposition do not act.—On dialdanic alcohol, by M. Wurtz.—On reproduction, by aqueous method, of orthose felspar, by MM. Friedel and Sarasin. The felspar was formed at a high temperature in the heart of a mother-water rich in alkaline silicate. The crystals were very small.—Summary account of experiments made at Pouilly-le-Fort, near Melun, on *charbon* vaccination, by M. Pasteur, with MM. Chamberland and Roux. We now possess vaccine matter of *charbon*, capable of preserving from the fatal disease, but not itself fatal, capable of cultivation at will, transportable anywhere without alteration, and prepared by a method which may probably be generalised. M. Milne-Edwards noted the analogy of some of M. Pasteur's facts to the phenomenon of alternating generations, asking whether, by changing the biological conditions, one or other term of such alternation might not be had at will.—Observations on M. Pasteur's paper, by M. Bouley. He calls attention to the successful vaccination, by MM. Arloing, Cornevin, and Thomas, against *symptomatic charbon* (which is distinct from *bacteridian charbon*). They do not use attenuated virus (like M. Pasteur), but natural virus, attenuating the effects by bringing it directly into the blood.—Reply to observations by M. de Lesseps at the last *séance*, by M. Cosson. This relates to the Tunisian Chotts.—On a system of differential equations, by M. Briordier.—On the means of saving water in double locks and acceleration of the service, by M. de Caligny.—M. Milne-Edwards presented a *brochure* on some macruran Crustaceans from great depths in the Caribbean Sea, calling attention, *inter alia*, to a large Crustacean, *Phoberus cæcus*, quite blind, which is a transitional form between groups hitherto thought very different.—M. Fouqué was elected Member in Mineralogy in room of the late M. Delesse.—On linear differential equations with periodical coefficients, by M. Floquet.—On the treatment of vines with sulphide of carbon, by M. Boiteau.—On the functions of two variables arising from the inversion of integrals of two given functions, by M. Fuchs.—On certain systems of differential equations, by M. Halphen.—On the influence of temperature on radiophonic selenium receivers, by M. Mercadier. These tend more or less quickly to a stable state relatively to effects of temperature. At ordinary temperatures and even up to 100° the resistance varies inversely as the temperature. Between 5° or 6° and 35° these variations may be approximately considered proportional one to the other.—On some means and formulæ of measurement of electric elements and coefficients of utilisation with the arrangement having two galvanometers, by M. Cabanellos.—Hemeralopia and retinian torpor, two opposite forms of Daltonism, by MM. Macé and Nicati. Hemeralopia is in general Daltonism for blue. Persons having retinian torpor are as if in a poor light; other rays than the blue are badly perceived, especially the red.—Water-raising machines, by M. de Romilly. He describes a machine, consisting, in its general form, of a shallow cylinder with vertical axis, wide circular opening above, and near the circumference the orifice of a pipe which curves upwards. The cylinder (turbine) is rotated, and the water accumulates by centrifugal force at the circumference, where it leaves tangentially through the pipe. Water can thus be raised much higher than hitherto by centrifugal force (*e.g.* 150 m. with a turbine driven by hand). Two modifications are described. The same principle is applied in oiling the machine.—Cyanides of strontium, calcium, and zinc, by M. Joannis.—Industrial preparation of crystallisable formic acid, by M. Lorin.—Researches on tertiary monamines: I. action of triethylamine on monobromised propylenes, by M. Reboul.—Nervous system of Ophiurans, by M. Apostolides.—On the squamous temporal bone in the vertebrate series, by M. Lavocat.—On *Phytolacca dioica*, by M. Balland.—On the Carboniferous fauna of Rémy (Loire), and its relations with that of Ardoisière (Allier), by M. Julien.—On the dissolution of false membranes of *angina pellicularis* by local applications of papaïne, by M. Bouchut.—On an apparatus for suppressing the dangers of movable stoves, by M. Godefroy. The air for combustion is drawn from the chimney itself by a second tube; the chimney and stove may thus be hermetically closed.

CONTENTS