THURSDAY, OCTOBER 29, 1885

THE ANTI-CHOLERA INOCULATIONS OF DR. FERRAN

IN the spring and summer of the present year the public in Europe—lay and medical—have been greatly agitated by the exploits of a Spanish medical gentleman, who, during the cholera epidemic then raging in Spain, claimed to have discovered a means of preventing cholera. He was hailed as a great benefactor, and if his deeds had been equal to his professions, he would no doubt fully deserve to rank with Jenner, the greatest benefactor to mankind. But fortunately the medical world, at any rate the scientific medical world outside Spain, is not guided by the allegations of enthusiasts nor by wonder-doctors either. A Don Quixote, who discerns in a windmill giants, in a flock of sheep a squadron of the enemy's soldiers, may present points of interest to the psychologist: to the disciple of physiology and pathology he demonstrates an aberration of the visual nerve centres. I shall show that Dr. Ferran comes very near in rank, not to Jenner, but to his own illustrious countryman, the Knight of La Mancha.

The method of Ferran is practically this:—Ferran says that by a peculiar mysterious method of cultivation—which for a long while he was not going to divulge—he has succeeded in attenuating the action of the comma bacillus of Koch. In these cultivations the comma bacillus after very complex morphological changes, unnecessary to detail here, forms spores. Such cultures introduced in sufficient quantities into the subcutaneous tissue of animals (guinea-pigs) or man produce a disease which is a mild and abortive form of cholera; it manifests itself in local inflammation, and a general constitutional disturbance, febrile rise of the body temperature, headache, nausea, and sickness, and even diarrhœa. After a few days the person inoculated returns to his normal state. Persons once, twice, or thrice inoculated answer, or ought to answer, each inoculation with the said constitutional disturbance. Statistics collected by Ferran and his adherents in the places where these inoculations were practised, notably in Alcira, in and about Valencia, prove, so it is said, that the number of cholera cases and of deaths from cholera decreased in a conspicuous degree after these inoculations had been commenced, and also that those persons that had been inoculated remained almost impervious to cholera, while others not so inoculated fell victims to the plague in large numbers. In these assertions and practices several important questions are involved, each of which demands a direct answer, which ought to be favourable to this theory of Dr. Ferran.

First: Is the so-called cholera-bacillus, or Koch's comma-bacillus, found in the intestinal discharges of cholera patients, the *verà causa* of cholera?

Second: Does this so-called cholera-bacillus form spores, which when introduced into the living tissue germinate into the comma bacilli: in the subcutaneous tissue capable of producing only an abortive and mild form, but in the alimentary canal producing severe and malignant cholera?

Third: Do the cultivations of Dr. Ferran, when inoculated into the subcutaneous tissue, set up a disturbance which can be considered as an abortive form of cholera?

Fourth: Are persons so inoculated really protected or almost protected against an attack of real cholera; and do the statistics collected by Ferran and his adherents prove this?

(1) The first of these questions, it is obvious, forms the basis of the whole theory; for if the comma bacillus of Koch is not the real cause of cholera all the rest of Ferran's assertions, as far as cholera is concerned, fall to the ground. The claims of the comma bacillus of Koch to be accepted as the true cause of cholera, rests on very insufficient evidence: the epidemiological evidence as to the spread of cholera being dependent on soil and season, the anatomical evidence as to the comma bacilli being limited to the cavity of the cholera intestine, they being absent from the tissues and the blood, the misproportion existing between the number of comma bacilli present in the alimentary cavity, and between the severity and acuteness of the disease in many cases, and a number of other facts not necessary to mention here, prove to my mind that the comma bacillus is not the real cause of cholera. Add to this that Emerich of Munich vindicates this claim to be the real cause of cholera, not to the comma bacilli of Koch, but to small straight bacilli, probably identical with those seen and described by the English cholera Commission in India as constantly present in the alimentary canal of cholera patients, and for which bacilli I did not and cannot claim any real infective power; and further, that Emerich's view is backed up by no less an authority than Von Pettenkoffer himself. There is then at present an interesting contest going on between two rival bacilli: one, having Berlin for the head-quarters of its advocates, may be called the northern bacillus; the other, in Munich, may be called the southern bacillus. As to the actual facts, it seems to me the question is not whose claim is stronger, but whose claim is weaker.

(2) All except Ferran, acquainted practically with the comma bacillus in pure cultivations (Koch, Van Ermengem, myself, Mr. Watson Cheyne, Finkler, Emerich, Buchner, Klebs, and many others) are agreed that the comma bacillus in artificial cultivations never forms spores; having multiplied until all the nutritive material in the cultivation is exhausted, a period arrives when the comma bacilli degenerate and die; some undergo this long before the point of exhaustion is reached, others retain their vitality longer, but after weeks and months death has involved all the comma bacilli present in the cultivation. [An impurity accidentally present in the culture would effect this death of the comma bacilli in a much shorter period; in fact, in many instances, they would not have much chance of primarily reaching any considerable number.]

When this period has been reached, the culture becomes incapable of starting a new culture; and *vic versâ*: by this means the point of death of the bacilli present in the culture can be tested and accurately determined. I have a large number of tubes of pure cultivations of the comma bacilli, the nutritive medium being broth, or peptone and broth, or gelatine peptone and broth, or gelatine peptone and meat extract, or Agar-Agar peptone and broth, &c. In each of these media the comma bacilli thrive well and form copious growths. The cultures are pure, contain the comma bacilli only, as all sub-cultures from them yield again the comma bacilli, and comma bacilli only. Now the remarkable fact about such culture tubes is this: that after several months all life in them becomes extinct, as is proved by inoculating from them a series of tubes containing suitable nutritive material, no comma bacillus or any other bacteria making their appearance. I have ascertained this in a great many cases, and it is in perfect agreement with the experience of Koch and many other workers. This clearly proves that there are not present in such tubes spores of the comma bacilli, for, if the comma bacilli, like some other bacilli—*e.g.* bacillus subtilis of hay infusion, or bacillus anthracis, were capable of forming spores, such a total extinction of life could not take place; the spores, although, owing to exhaustion of nutritive material, incapable of germinating into bacilli while in the tube in which they were formed, would undoubtedly germinate when transferred into a fresh and suitable nutritive medium. This total extinction of life does occur not only in tubes in which the nutritive medium is in a fluid condition, but also in all Agar-Agar peptone broth tubes, this material, unlike gelatine, remaining in its solid state, however luxuriant the growth of the comma bacilli may be.

Dr. Ferran claims to have discovered means by which the comma bacilli can be made to produce spores. In his cultures he notices a number of peculiar things which he considers as antecedents to the formation of spores and as fully formed spores. But direct observations that these are really spores, that, like spores, they actually germinate into the bacilli, Dr. Ferran has not deemed it necessary to make. As a matter of fact those to whom Dr. Ferran has shown his specimens, in which these alleged spores were supposed to be present, failed to see them (see the Report of the French Commission headed by Dr. Brouardel; see also Dr. van Ermengem's Report).

The methods of examination and cultivation of bacteria perfected by Koch, which, owing to the thoroughly reliable results they yield, are now universally followed by all who wish to acquire correct ideas and a sound knowledge of the life-history, morphology, and activity of bacteria, have led those practically acquainted with the comma bacilli to the conclusion that they do not form spores. Dr. Ferran is of the contrary opinion; but, judging from the Report of the French Commission, and from that of van Ermengem and others, who have visited Ferran and seen him at work, it is pretty clear that this gentleman is not only unpractised in, but altogether unacquainted with the elements of technique necessary in bacterial investigations; more than this: according to a graphic description by the special correspondent of the *Times*, Dr. Ferran makes his cultivations in broth in a temporary laboratory, the kitchen of an untenanted house, reeking with the effluvia of an untrapped sewer opening into this kitchen. Dr. Ferran's cultivations have been examined microscopically by a Valencia Commission, who found that they contained a motley crowd of various kinds of bacteria; Dr. Chantemesse in a paper read before the Paris Académie de Médecine (see *Brit. Med. Journal*, Sept. 26, 1885) states that as the result of a microscopic examination of Dr. Ferran's cultures he found the fluid variable in its composition; sometimes it is a cultivation of impure comma bacilli, sometimes it contains masses of different micro-organisms, but the comma bacilli are barely present. Add to this that Dr. Ferran, as the special French Commission attested, possesses neither the skill nor uses the ordinary precautions and apparatus indispensable in investigations of this nature, and all Ferran's extravagant assertions as to the behaviour of the comma bacillus in cultivations, as to its peculiar power of forming spores, must be regarded as sheer nonsense.

3. Notwithstanding this deficiency of Ferran in his mode of preparing his so-called "vaccine," it might be said, and it has been said by Dr. Cameron in a powerful and very able article in the *Nineteenth Century* for August 1885, that by subcutaneously inoculating a cultivation of comma bacilli, no matter however impure and contaminated, *e.g.* such as were at Ferran's disposal, the effect is different from the one produced by introducing them into the alimentary canal. In the former case, *i.e.* in the subcutaneous tissue, they are planted in a soil not congenial to them, and their product is only an abortive form of cholera, whereas in the latter, *i.e.* in the cavity of the alimentary canal, they find a more suitable soil, a soil which is their natural breeding ground, and the result is virulent real cholera.

What Ferran by the inoculation of his cultures into the subcutaneous tissue of human beings actually did produce, is, according to a number of witnesses (see the letters of the special correspondent of the *British Medical Journal;* the evidence given in detail by the special correspondent of the *Times*, October 20, and a number of other independent witnesses, English and French), septic infection, the intensity of which, as might be expected, and as Ferran himself admits, depends on the quantity injected. This result, however, is not always produced, the injection being sometimes quite inert, notwithstanding the presence of the comma-bacilli in the "vaccine" fluid. In the very able letter by the special correspondent of the *Times* for October 20 we are informed that Dr. Ferran explained to this gentleman in detail that the culture fluid used for inoculation need not contain any comma bacilli at all, in order to produce the desired result; further, that the comma bacilli can be killed by boiling or otherwise, without impairing the efficacy of the fluid, and that therefore a chemical substance present in the culture fluid, and probably the product of the organisms, must be regarded as the active principle. While this latest assertion of Ferran clearly shows that he is profoundly ignorant of the theory and practice of protective inoculations, such as are employed by Chauveau, Pasteur, Koch, Gaffky, Arloing and Thomas, and many others in a variety of specific diseases (anthrax, some forms of septicæmia, fowl cholera, symptomatic charbon, &c.), and while it is in flagrant opposition to his own assertions of an earlier date, it also proves that the results obtained by Ferran by the inoculations of his "cholera vaccine" into the subcutaneous tissue of human beings harmonise well with the assumption that what he produces is simply septic poisoning, *i.e.* changes such as have been proved to follow the injection of certain chemical substances known as ptomaïnes, and produced by the growth and activity of putrefactive bacteria in media containing proteids. Brieger (" Die Ptomaine," Hirschwald, Berlin, 1885) has published a most important series of observations on the production, nature, and action of ptomaines, and has greatly enlarged our knowledge of this as yet obscure subject. The description of the symptoms observable on persons inoculated by Ferran (as given by a variety of independent witnesses and by Ferran himself), can leave little doubt that the result of these inoculations is septic poisoning, in severe cases dangerous phlegmon and ulceration, and even death. This is also the opinion of a number of medical men (Spanish, English, and French) who have had the opportunity of seeing and examining such persons, as will be seen from the Report by the Special Commissioner of the *British Medical Journal*, the Report by the Special Commissioner of the *Times*, the Report by the Special French Commission, and the Report of the Commission sent by the Spanish Government. Such being the case, the inoculations practised by Ferran and his coadjutors can have no possible prophylactic effect against cholera, even granting, for the sake of argument, that one mild attack of cholera protects against a second severe one, a question which is still *sub judice*, since some competent authorities maintain that such immunity, although holding good in a number of infectious maladies, does not apply to cholera.

4. Now, are persons inoculated by Ferran furnished with immunity against an attack of cholera? The statistics published by Dr. Ferran and his adherents on the marvellous effects of inoculation in Alcira, Valencia and other places, accepted by Dr. Cameron in his article above referred to, show us a picture of brilliant successes, favourably comparing and even surpassing the statistics of the effect of vaccination against smallpox. Those statistics collected by Ferran being endorsed by several medical men and other notabilities of the town of Alcira and elsewhere, Dr. Cameron cannot bring himself to regard as not revealing the truth; he cannot imagine that all these worthy people should conspire to pervert the truth and to prevent the truth from becoming known.

The correspondent of the *Times* in his letter, published October 20, gives a long list of places where the statistics published by the Ferranists are signed and stamped by the Alcalde of the place, the local judge, the priest, the resident doctors, and the notary; all duly signed and stamped. This Englishman, however, probably knowing what value to attach to the competency and veracity of all those worthies, examined the statistics for himself, and the result of his inquiry may be briefly summarised by saying that Dr. Ferran and his partisans have simply "cooked" those statistics. They have done these things: when a person who had been inoculated by Ferran did nevertheless become affected with cholera, and died of it, death was put down as caused not by cholera but by some other disease; false entries were made as to persons who, having been inoculated, nevertheless died of cholera, were not entered as having been inoculated; persons have been registered as having been "vaccinated" by Ferran, but on inquiry were found to have died of cholera several days previous to the alleged "vaccination."

Add to this the fact that in Alcira, for instance, the inoculations and their wonderful effects had not commenced until the population had abandoned the impure water supply; that in some places many of the inoculated persons belonging to the well-to-do classes (a fee being paid for the inoculation) were therefore less exposed to infection, and those statistics become a gross farce and a shameless imposture. And this is practically the opinion of the Special Commission sent by the Spanish Government; this Commission has reported altogether unfavourably on these inoculations, declaring them barren of all scientific value, dangerous inasmuch as persons inoculated and suffering in consequence from a form of septic poisoning become more susceptible to infection from cholera and other diseases, and further condemning them as of no value in giving immunity against cholera.

The fact that Dr. Ferran and his associates took payment for the inoculations—thousands of persons were inoculated and reinoculated in Valencia and elsewhere, for each inoculation a fee of from 5 to 12 francs being charged—gives to the whole business a very ugly look. The *Times* correspondent (*Times*, October 20) does not therefore fully express the real value of Dr. Ferran when he says that he (Dr. Ferran) "is the dupe of illusions, conceived in ignorance."

E. KLEIN

LIFE OF SIR WILLIAM ROWAN HAMILTON

Life of Sir William Rowan Hamilton, Royal Astronomer of Ireland. By Robert Perceval Graves, M.A., Sub-Dean of the Chapel Royal. Vol. II. pp. 719. With Portrait. (Hodges, Figgis, and Co.)

IN a former number of this journal it was our duty to notice the first volume of the life of the illustrious Irish mathematician. We have now to congratulate Mr. Graves on the completion of the second instalment of that great work which has evidently been to him a labour of love. This volume, like its predecessor, bears abundant testimony to the conscientious manner in which the author has sought to delineate a picture of Hamilton, told as far as possible by the letters from Hamilton to his friends and by extracts from his journal. We are again surprised at the extraordinary copiousness of the materials which were available.

The incidents in the life of Hamilton apart from his literary and scientific activity are but few. The last volume conducted us to the year 1832, when Hamilton was in his twenty-seventh year. We had there seen the troubled course of his two earlier love affairs, and at the outset of this volume we are introduced to the third with Miss Bayly, to whom he was married in 1833. His domestic happiness was in the course of years clouded over by the ill-health of his wife, though to the end he remained an attached husband, as she was an attached wife; two sons and one daughter were the issue of this union.

The reader of this work can hardly fail to be struck with the number and the worth of the friends to whom Hamilton was endeared; he possessed to a remarkable degree the power of transforming a casual acquaintanceship into a true and lasting friendship. His intimacy with Wordsworth has been already referred to, and was carried on by occasional letters and visits until the death of the poet. Among his other literary friends we may mention Maria Edgeworth, who writes to him (p. 384):—

"Take your head from the stars or from transcendental mathematics and come and enjoy Folly and Friendship."

There are also copious letters to and from Aubrey De Vere, Lord Dunraven, the Marquis of Northampton, and many others, including not a few to his intimate friend the author of the present work; one of these we would specially mention (p. 357), in which Hamilton sketches the obligations of true friendship. The scientific correspondence of Hamilton with many of the leading philosophers of the last generation occupies, as might have been expected, a large proportion of the volume.

At successive meetings of the British Association Hamilton was a well-known and a conspicuous figure. When the Association visited Dublin in 1835 he was but thirty years old, yet he had already attained a scientific renown which made him perhaps the most eminent man at that meeting. It was on this occasion that the Board of Trinity College entertained the distinguished visitors at a banquet. The guests had assembled in the venerable library of the University. The Earl of Mulgrave, then Lord Lieutenant of Ireland, called Hamilton to the centre of a little circle, and, after conferring upon him the honour of knighthood, said:—

"I but set the Royal, and, therefore, the national mark on a distinction already acquired by your genius and labours" (p. 158).

In speaking at the banquet subsequently, Whewell said, in language which the enthusiasm of the moment might perhaps excuse:—

"It was now one hundred and thirty years since a great man in another Trinity College knelt down before his Sovereign and rose up Sir Isaac Newton" (p. 159).

In the year 1842 Sir R. Murchison, then general secretary of the Association, writes to Hamilton as follows (p. 387):—

"Your letter of the 16th having crossed mine, *I am in despair* at your resolution not to visit Manchester; and in order to shake it if possible, even at the eleventh hour, I enclose you a letter from Herschel, whose resolutions were quite as firm as yours, and who yet has made them fly before Bessel. Think of this philosopher coming on purpose to see such men as Herschel, yourself, and two or three others, and finding Airy and Baily flown to Italy and Sir William Hamilton *lecturing* in Dublin!! Pray put off your class for a week. Make a noble effort and lay it all on Bessel's shoulders, and you will add to your glory."

On this occasion Hamilton had also the gratification of meeting the great mathematician Jacobi, who, after referring to Hamilton as the "Lagrange of your country," said (p. 388):—

"Provided that we give to the dynamical equations that remarkable form under which they have been presented for the first time by the illustrious Astronomer Royal of Dublin, and in which they ought to be presented hereafter, in all the general researches of analytical mechanics."

We also read how Hamilton was received at the Oxford meeting in 1847, in which, to quote his own words from a letter to the author, he says (p. 585):—

"It has several times happened to me to sit between Struve and Le Verrier (both of whom, somewhat to my surprise, and certainly beyond my deserts, assigned to me a high place among British astronomers in their speeches at the concluding meeting). And when I rose to give an account of the application of the calculus of quaternions to the theory of the moon on the Thursday of last week, and saw before me not only those two eminent foreign astronomers, but also Herschel, and Airy, and Adams, and Challis, besides Peacock and Whewell, and others scarcely less distinguished, I could not refrain from acknowledging it to be an alarming and almost an awful thing to speak on any subject of physical astronomy in the presence of such an audience."

Hamilton also records in an unsent letter the following, which refers to the same meeting (p. 585):—

"My friend Struve, of Russia, at Oxford, 1847, said: that, though I held the title of Royal Astronomer of Ireland, my astronomical brethren on the Continent would decidedly prefer my never looking through the telescope to my giving up or less ardently pursuing mathematics. 'You are,' he was pleased to say, 'our teacher.'"

Hamilton was for many years not only the most distinguished member of the Royal Irish Academy but also its president. Many interesting letters will be found in the volume relating to his election to this distinguished post. His rival, if so he can be called in what Hamilton describes as a "contest of generosity," was the late Provost Lloyd. Lloyd retired in favour of his friend, and Hamilton writes many letters, the character of which is fairly represented by one to Lloyd (p. 218), in which he disclaims

"Entertaining *even a thought* which could be construed into *treason* to our long and unclouded friendship, and that the part *you* have taken (while in some respects it adds to my pain) furnishes a new proof of the justice of the high opinion that I have ever entertained of you."

Hamilton discharged in the most exemplary manner the laborious duties of President for several years, until, as he writes (p. 510):—

"The day has at length arrived when I am to accomplish my desire of retiring from the chair of the R.I.A. How joyously, though not without a feeling of solemnity, I received the news of my being elected to the chair; how gladly now I resign it, yet not without a shade of that sadness which belongs to a farewell!"

The chief interest in this volume will be found in the account of the great invention of quaternions, with which the name of Hamilton will be for ever associated. His own appreciation of the importance of this achievement is shown in an extract from a letter to Prof. Lloyd in December, 1851 (p. 445):—

"In general, although in one sense I hope that I am actually growing *modest* about the quaternions, from my seeing so many peeps and vistas into future expansions of their principles, I still must assert that this discovery appears to me to be as important for the middle of the nineteenth century as the discovery of Fluxions was for the close of the seventeenth."

The account of the discovery which, after fifteen years of studious meditation, seems suddenly to have flashed upon Hamilton is told in an interesting letter written from his deathbed many years later to his son Archibald (August 5, 1865), p. 434:—

"On the 16th day of October, 1843, which happened to be Monday, and a council day of the Academy, I was walking in to attend and preside, and your mother was walking with me along the Royal Canal, to which she had perhaps driven; and although she talked with me now and then, yet an *undercurrent* of thought was going on in my mind which gave at last a *result*, whereof it is not too much to say that I felt *at once* the importance. An *electric* circuit seemed to close; and a spark flashed forth, the herald, as I *foresaw immediately*, of many long years to come of definitely directed thought and work, by *myself* if spared, and at all events on the part of *others*, if I should even be allowed to live long enough distinctly to communicate the discovery. Nor could I resist the impulse, unphilosophical as it may have been, to cut with a knife on a stone of Brougham Bridge, as we passed it, the fundamental formula with the symbols i, j, k;—namely, $i^2 = j^2 = k^2 = ijk = -1$, which contains the *solution* of the *problem*, but of course, as an inscription, has long since mouldered away. A more durable notice remains, on the council books of the Academy of that day—October 16th, 1843—which records the fact, that I then asked for and obtained leave to read a paper on *quaternions* at the *first general meeting* of the session, which reading took place accordingly, on Monday, November 13."

Among the most distinguished disciples of Hamilton is Prof. Tait, though *even he* has admitted that he has not read the whole of Hamilton's "tremendous volumes" (lives there indeed the man who has?). Another account of the discovery is found in a letter to Prof. Tait on October 15, 1858 (p. 435):—

"To-morrow will be the fifteenth birthday of the quaternions. They started into life full-grown on the 16th of October, 1843, as I was walking with Lady Hamilton to Dublin, and came up to Brougham Bridge—which my boys have since called Quaternion Bridge. I pulled out a pocket-book, which still exists, and made an entry, on which *at the very moment* I felt that it might be worth my while to expend the labour of at least ten or fifteen years to come. But then it is fair to say that this was because I felt a *problem* to have been at that moment *solved*, an intellectual want relieved which had haunted me for at least *fifteen years before*."

The unmathematical reader may naturally ask the nature of this notable discovery which Hamilton made at "Quaternion" Bridge.

It would seem that at this moment he solved the long-studied problem of the multiplication of directed straight lines, or vectors as he called them. Let a denote a straight line of determined length and direction. Let b denote another straight line at right angles to a, and radiating from the same origin; then the product ab denotes a third straight line from the same origin perpendicular to the plane of a and b; the product ba, however, denotes the perpendicular line on the other side of the plane, so that $ba = -ab$. This formula is eminently characteristic of the method, showing as it does that vector multiplication is non-commutative. It is, however, remarkable that the associative principle obtains in quaternions no less than in ordinary algebra; thus if a, b, c be three vectors, or more, generally quaternions, then $ab \times c = a \times bc$. This theorem, though *true* in quaternions, is still so far from being obvious that it implies the truth of an elaborate geometrical theorem.

If we could single out one point of special significance in the invention of quaternions it would be found in the *dual* interpretation of the symbol of a vector. Thus if the letter i denotes a vector or directed straight line of unit length, then the same symbol may also mean an operation of rotation through a right angle around the vector as an axis. In the formulæ of quaternions the symbols denoting vectors can be interpreted in this dual manner. A quaternion may be regarded as the operating factor which applied to one vector transforms it into another. This operation requires two quantities to specify the plane of the vectors—one to specify the angle between them and one the ratio of their lengths in all four quantities are required, whence the name quaternion.

An interesting letter (p. 536) to the Rev. John W. Stubbs, Fellow of Trinity College, dated October 19, 1846, gives a sketch of the points which Hamilton thought specially novel in his theory:—

"But did the thought of establishing such a system, in which *geometrically opposite factors*—namely, two lines (or areas) which are opposite IN SPACE give ALWAYS a *positive product*—ever come into anybody's head, till I was led to it in October, 1843, by trying to extend my old theory of algebraic couples, and of algebra as the science of pure time? As to my regarding *geometrical addition* of lines as equivalent *to composition of motions* (and as performed by the same rules), that is indeed *essential* in my theory, but *not peculiar* to it; on the contrary I am only one of many who have been led to this view of addition."

A few years later Hamilton commenced the delivery of lectures on quaternions in Trinity College. His own words are (p. 605):—

"It was on Wednesday, June 21, 1848, that I delivered my first lecture on quaternions to a very respectable audience, among the persons composing which were the Rev. George Salmon, Fellow of Trinity College, Dublin, and author of a lately-published treatise on Algebraic Geometry, and Arthur Cayley, Fellow of Trinity College, Cambridge, who first, except myself, has publicly used the quaternions."

These lectures, rewritten and greatly expanded, formed his first and classical volume—"Lectures on Quaternions." (Dublin, 1853.)

The publication of this work drew from Hamilton's many scientific friends cordial letters of congratulation. His old and intimate friend, Sir John Herschel, thus writes on July 21, 1853 (p. 681):—

"Now most heartily let me congratulate you on getting out your book—on having found utterance *ore rotundo* for all that labouring and seething mass of thought which has been from time to time sending out sparkles, and gleams, and smokes, and shaking the soil about you—but now breaks into a good honest eruption with a lava stream and a shower of fertilising ashes. I don't mean to say that there is not a good deal of cloud (albeit full of electric fire)—the good old 'stupendo e orgoglioso pino' of the fiery outbreak surrounding the bright jet, the true product—but the cloud clears as the wind drifts and leaves the hill conspicuous.

"Metaphor and simile apart, there is work for a twelve-month to any man to read such a book, and for half a lifetime to digest it, and I am quite glad to see it brought to a conclusion."

The intercourse, both social and scientific, between Hamilton and Sir John Herschel gives many interesting pages to this volume. Thus, for instance, we find (p. 492) an account of a meeting between these philosophers at the house of their common friend, Dr. Peacock, the Dean of Ely. On Sunday they attended service in the Cathedral in company with Prof. James D. Forbes, and Hamilton recorded the incident in a sonnet which he recited to his friends. The next morning he received an acknowledgment in kind from Herschel. We quote here the two poems: that of Hamilton (p. 493) bears the title "In Ely Cathedral":—

"The sunshine, through the lofty window stealing,
Lit up that vast and venerable fane,
Ely's Cathedral, in dark clouds and rain
Wrapped lately, and shut up from joyous feeling:
In its soft progress all around revealing
Beauty or majesty unmarked before,
It shed its type of heavenly comfort o'er
Three kindred-kingdoms' sons together kneeling.
Oh, may that Church, Episcopal and pure,
One Mother of that kneeling company,
In essènce one, in name and office three,
Mid outward storm and darkness still endure:
Be comforted of Christ in God's good time,
And share the sunshine of a heavenlier clime."

Herschel's sonnet in reply (p. 494) was handed to Hamilton the following morning:—

"On a Scene in Ely Cathedral

"The organ's swell was hushed, but soft and low
An echo, more than music, rang; when he,
The doubly-gifted, poured forth whisperingly,
High-wrought and rich, his heart's exuberant flow
Beneath that vast and vaulted canopy.
Plunging anon into the fathomless sea
Of thought, he dived where rarer treasures grow,
Gems of an unsunned warmth and deeper glow.
Oh! born for either sphere! Whose soul can thrill
With all that Poësy has soft or bright,
Or wield the sceptre of the sage at will
(That mighty mace which bursts its way to light).
Soar as thou wilt! or plunge—thy ardent mind
Darts on—but cannot leave our love behind."

We have introduced these verses not so much on account of the poetical merit they possess, which we confess appears to us to be but slight. They may, however, serve as samples of those poetical effusions with which these volumes teem—indeed they give the impression that there must be some occult sympathy between poetry and astronomy. It is well known that Romney Robinson was a poet, and though it does not appear that Sir George Airy had plunged into verse, yet when he and Hamilton were together at Parsonstown there was an amusing contest between the two Royal Astronomers as to which could repeat most English poetry. The present writer has heard this scene described by the late Earl of Rosse, who said that Sir G. Airy was admitted to have carried off the honours.

As an illustration of one of the less important mathematical labours of Hamilton we may mention his paper on the Hodograph, communicated to the Royal Irish Academy in 1846. This elegant conception is a curve whereof the radius vector to any point from the origin represents both in direction and in amount the velocity of a moving particle. Many interesting applications were made by Hamilton, and are referred to in correspondence with Whewell. A somewhat ludicrous incident in connection with the hodograph is recorded (p. 543). It appears that at the same meeting of the Academy in which the hodograph was discussed, Hamilton also exhibited Prof. Mädler's just published work on "The Central Sun." This precarious speculation was by the reporter injudiciously blended with the hodograph, and an astounding statement went the round of the papers asserting that Hamilton's wonderful calculus had succeeded in discovering the central point of the universe!

It is not, perhaps, generally known that the real discoverer of the hodograph was Bradley (see Rigaud's edition of Bradley's Memoirs, Oxford, 1832, p. 288). Bradley has there given a most elegant geometrical investigation of that circle related to elliptic motion which Hamilton afterwards named the hodograph.

The religious side of Hamilton's character demands a few words of notice. He was a member of the Establishment, and many passages show that he had the sympathies of a sound churchman. He seems to have been an admirer of Pusey, with whom he was also personally acquainted. We also find occasional reference to the midnight vigils with which he awaited the new year, and to the fasting which he sometimes practised for devotional reasons. We should imagine, however, that such exercises were but very occasional to a student so laborious yet so irregular as Hamilton.

He found time to be president of a local branch of the Society for the Propagation of the Gospel. He assumed the duties of a churchwarden, and vanquished Archbishop Whateley in a controversy on the orthodoxy of an inscription on the church window at Castleknock. At Whitsuntide we find him writing a dynamical theory of the ascension of our Lord, in which in mediæval fashion he proceeds to evaluate the *duration* of the phenomenon, which he demonstrates to have been less than the interval between Holy Thursday and Whit Sunday.

It is with evident pain that the biographer has felt himself compelled to record the one great failing of his illustrious friend. The excessive devotion of Hamilton to study and the engrossing nature of those mathematical reveries in which he indulged led to the formation of very irregular habits. He "too often found the dawn surprise him as he looked up to snuff his candles after some night of fascinating labour." The necessary hours for rest and refreshment being disregarded, he was led to the dangerous practice of an undue recourse to alcohol, and occasional intemperance was the consequence. Two or three scenes arising from this cause have been described in this volume. There is one which can hardly have been witnessed except by the biographer himself, but which his conscientiousness has compelled him to record. There is a second on a public occasion which caused the deepest grief to Hamilton's friends, one of whom called upon him with a kind remonstrance which was received by Hamilton in a manner worthy of his high character. There is also a third incident, perhaps the most painful of all, which illustrates the attempt of Hamilton to reform and the circumstances under which he relapsed.

We certainly have no intention of citing these passages in this place, for if torn from their setting in the life of this great man they would probably convey an exaggerated notion of the extent of his infirmity. We would rather record the words of Mr. Graves, where he says (p. 335):—

"It is mournful that what seems to have been an inconsiderate, and at first unconsciously indulged, defect in external regimen of life, for such in the inception was his infirmity, should avail to cast a shade over qualities so solid and so splendid as the moral and intellectual qualities of Hamilton."

We have still to look forward to the third and concluding volume of this important work. In it we are to read how Hamilton continued his stupendous labours which culminated in the appearance of his other great work, the "Elements of Quaternions." We are also promised that extensive correspondence with De Morgan, which will secure the attention of every lover of the "Budget of Paradoxes." At the close of our former notice we insisted on the duty which devolved on the University of Dublin of publishing in a collected form the mathematical writings of their illustrious son. This duty has not yet been discharged; let us hope that it will not be left to some foreign mathematician to undertake the work which it should be the glory of Trinity College to complete.

AN AGRICULTURAL NOTE-BOOK

An Agricultural Note-Book. By W. C. Taylor, Aspatria, Carlisle. (London: Longmans, 1885.)

IT is not often that note-books are published, and it is well. Notes are in their nature fragmentary, and disposed towards brevity, often lapsing into crudity. They are a sort of skeleton of imparted knowledge, or at least rather anatomical than living, moving, and breathing information. The least and the most that may be reasonably expected of them is that they should be correct. The small book which has just been published by Messrs. Longmans does not commend itself to our judgment. It is crude, fragmentary, and almost inarticulate or unintelligible. It purports to contain a body of teaching and of facts, but it really consists of disjointed sentences, the meaning of which it is often very difficult to gather. The grammatical construction of the sentences is also fearful and wonderful. To give an idea of this latest contribution to agricultural science, we select the opening passage, page 1, which reads as follows:—"The science of agriculture. Definitions and terms. Its definitions. Scientific truths taught by the practice of agriculture." "The practice of the farm teaching the science. The laws of agricultural science best learnt when thus taught, and lead to improvements in the application of science to farm practice." If this is a definition, much has been written in vain as to the difficulty of defining. It not only fails in definiteness, but is curiously involved, as well as untrue, for "the practice of the farm teaching the science" is an impossible and impracticable idea.

The word "its" before each paragraph of definitions and terms appears to bear reference to the general heading, "The Science of Agriculture," and cannot be supposed to bear a grammatical relation to "definitions and terms." Taking this view of Mr. Taylor's "notes," we read as follows:—

"Its character in the soil, as temper, will, and disposition. These to be noted: success of farmer depending much on his knowledge of above (sister sciences). *Hungry*, *sick*, *grateful*, *obstinate*, *kindly*, *tender*, &c."

We defy any one to make any sense out of these utterances, whether taken with or without their context.

Next we have an attempt at further amplification. Thus "1 HUNGRY—constantly in want of food." Now, be it remarked that the subject is *soils*, and we are told that a soil is "hungry, constantly in want of food." Also that it is "sick." Here is indeed confusion of metaphor and blind guiding with a vengeance. Only let readers of NATURE endeavour to picture to their minds a hungry and sick soil! No wonder that Mr. Taylor in the richness of his fancy can further enlarge upon its gratitude, tenderness, and kindliness. Page 1 would itself furnish ample matter for review. It is as full of difficulties as the Moabitish stone, although it might so well repay deciphering.

Again we read: "Short supply of organic matter improved by adding clay, where practicable, and vegetable matter." While concurring with the last simply-given advice as remedying the fault in question, we deny that any amount of clay can help towards this end.

Turning p. 1, we come to p. 2, where we begin at the top as follows:—"3. TENDER.—Hard and baked. Improved by rain, drags and harrows at right time." This tender soil is then hard and baked, and it appears also that it is improved by certain natural and artificial agencies which we thought were not only and solely unfit for the amelioration of such tender, albeit hard and baked soils.

On the same page we are thus enlightened as to the primitive rocks:—"The primitive rocks differ from materials yielded by decay, which is accomplished by oxygen (O) and carbonic acid (CO_2), gases invisible and transparent. Both attack rocks and metals, however hard; seen in the mould-board of the plough reducing it (?) to a powder without noise. *Temperature* and *water*, other two *agents* acting on the *Traitor's iron* and *potash*, loosening particles from the hard rock." . . . . These agents are the *friendly helpers* to the farmer. The italics are Mr. Taylor's own. We are irresistibly reminded of Mr. Weg and Mr. Venus, those two "friendly movers" in "Our Mutual Friend."

Passing onwards through the dreary succession of sentences devoid of subject, predicate, or copula, we arrive at p. 12, where instruction is given upon the various component parts of soils. Here we find the following information regarding alumina:—"Alumina. (1) Present in the soil, but not in plant food. (2) Double silicates are (1) silicate of alumina, (2) (*a*) lime, (*b*) potash, (*c*) or of soda, (*d*) or of ammonia. (3) Order of compounds, $H_3N_1K_2CO_3$, Na_2CO_3. The higher favourite puts out a lower and unites with the silicate of alumina. (4) The powers of vegetable life command an influence over each and all the second-rank partners. (5) Performs work of outdoor servant. (6) Reconstructs broken-up partnerships. (7) Amidst the faithless, constant only she. (8) Acts as purveyor of food for the plant."

We leave this extraordinary statement of the eight duties of alumina in the soil to the judgment of any sound scientific man or agriculturist, asking only why young people should be subjected to teaching so completely misleading, erroneous, and unintelligible, on the plea that they are obtaining insight into the principles of agricultural science?

THE PREVENTION OF BLINDNESS

The Causes and the Prevention of Blindness. By Dr. Ernst Fuchs, Professor of Ophthalmology in the University of Liége. Translated by Dr. R. E. Dudgeon. 8vo, pp. 230. (London: Baillière, Tindall, and Cox, 1885.)

UNDER the title of "The Causes and Prevention of Blindness," Dr. Dudgeon has translated an essay, written by Dr. Fuchs, of Liége, under the conditions of a competition announced by the "Society for the Prevention of Blindness in London," and to which the prize of 80*l*. offered by the Society was awarded. The book may be described as containing a succinct exposition of the chief causes of blindness, and an endeavour to render them intelligible to non-medical readers; the object being to obtain the cooperation of the public in the removal of these causes, in so far as that desirable end may be attained by improved hygiene, and by a better knowledge of the most favourable conditions of ocular work.

The causes of blindness which may fairly be said to be thus remediable, even including under blindness high degrees of defective vision, are two in number—namely, the purulent ophthalmia of new-born infants, and the progressive short-sight which is not uncommon in schools. The former is a disease which might frequently be prevented, which is always curable if treated in good time, but which, if neglected, is almost certain to destroy the sight; and to neglect of its early stages among the poor, and in remote country districts, probably four-fifths of the blindness which occurs among children in this country may be ascribed. Several months ago the Ophthalmological Society of the United Kingdom, moved thereto by Dr. McKeown of Belfast, sent a deputation to the Home Secretary to call the attention of the Government to the dangerous character and the easy curability of this affection, and to urge that steps should be taken, through the instrumentality of the Registrars of Births, to diffuse a more general knowledge of the importance of early treatment. Partly through the opposition of the Registrar-General, the deputation met with no encouragement; and the information given by Dr. Fuchs is therefore as opportune as it is valuable, and might with great advantage be communicated to the poor by clergymen, schoolmasters, and others. It may be said, however, that many of his recommendations apply chiefly to countries in which the employment of midwives is more general than in England.

The progressive short-sight of the educational period is a matter which has lately attracted much notice in all civilised countries, and Dr. Fuchs has nothing to say concerning it which is original. He presents, nevertheless, a brief and convenient summary of the facts, and a good description of the methods of school lighting and fitting which are most to be commended. This part of his volume may be studied with great advantage by any teachers and managers to whom the more systematic treatises upon the subject are either unknown or inaccessible. The book contains one serious error, which, in the English version, has been slightly modified by a mistranslation. Dr. Dudgeon writes, with reference to the provision for instruction about eye diseases in the medical schools of Great Britain and Ireland—"There are eye departments in all the large hospitals, but as a rule no regular lectures on ophthalmology are delivered." The word rendered "ophthalmology" is in the original not "*ophthalmologie*," but "*augenheilkunde*," and the correct translation would be "the treatment of diseases of the eyes." On this subject, that is to say, upon so much of ophthalmology as has any direct bearing upon the duties of the medical practitioner, systematic lectures are delivered in every medical school in the United Kingdom; and it is difficult to believe that the translator could have been unacquainted with the fact. "Ophthalmology," of course, takes a much wider range, and embraces branches of optics and of physiology with which the practitioner, unless a specialist in eye disease, has neither time nor reason to concern himself.

OUR BOOK SHELF

Among the Rocks round Glasgow: A Series of Excursion-Sketches and other Papers. By Dugald Bell. Second Edition. (Glasgow: Maclehose, 1885.)

THIS volume furnishes a good example of what a busy man can do in his few intervals of leisure. The volume is mainly based on notes of excursions kept by the author while acting as secretary to the Glasgow Geological Society. It affords a fairly accurate idea of the geological structure of the country round about Glasgow, and of the principal features of interest which the rocks of the district present. The excursions extend as far as Stirling, take in the course of the Clyde and not a few districts on its banks. Many of the papers are pleasant reading; and even geological specialists may find something in the pages to interest and inform.

Three Martyrs of Science of the Nineteenth Century. Studies from the Lives of Livingstone, Gordon, and Patterson. By the Author of "Chronicles of the Schönberg-Cotta Family." (London: S.P.C.K., 1885.)

THE author of this volume tells the story of these three remarkable lives very pleasantly and instructively, more, however, from the religious than the scientific standpoint. A very fair account is given of the work accomplished by Livingstone in Africa, though the author does not seem to be quite aware of the value of the geographical work accomplished by Gordon on the Upper Nile.

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 insure the appearance even of communications containing interesting and novel facts.]

Upper Wind Currents over the Equator

THE importance of an accurate knowledge of the general circulation of the atmosphere over the equator is so obvious and so little known that the following observations, taken on a voyage from Aden to Australia in February, 1885, will be of interest:—

Over the north-east monsoon, north of the line, the surface wind was east-north-east, while the low clouds came from due east. No high cirrus was ever seen.

In 2° N. lat. the surface wind lowest clouds came from N.N.E., the next layer of cirro-stratus from E. rather fast, while the highest cirri drove very slowly from E.S.E.

In about 1° S. lat. the surface came from N.W. (the N.W. monsoon), small flecks of low cloud from N.E., while some high cirri moved from E. at a moderate rate.

In 5° S. lat. the surface wind still blew from N.W., the lowest cumulus moved from N.N.W., the next layer of cirro-stratus from N., while a still higher layer of cirrus came slowly from E. or E.S.E.

In 10° S. lat. the surface wind came still from N.W., and the clouds at moderate altitude from S.E.

In the "Doldrums," which we only reached in 13° S. lat., the surface wind was from S. and the clouds from S.E.

After we entered the S.E. trade, while the wind came from S.E., the clouds drove from S., and when about 25° S. lat. the trade drew into E.; the clouds came from S.E.

The relation of upper to surface winds in the N.E. monsoon is just what might have been expected; but the discovery of an easterly current over the N.W. monsoon and of an upper current over the S.E. trade, more southerly than the surface wind, is not only altogether new, but also quite anomalous.

In Australia, and the Southern Hemisphere generally, the upper current over a N.W. wind is from about W. and over a S.E. wind from about E.

On my way home I ran a section across the Atlantic from Rio to Teneriffe, but the absence of cirrus prevented any satisfactory determination of the upper winds in that region.

The matter is, however, so important that I start again in a few days for the hurricane region of Mauritius, where I hope to observe one of these exceptional cyclones. Then I hope to repeat a section of the Indian Ocean between Mauritius and Bombay, and afterwards, if all goes well, to get some sections in the Pacific to see what the meaning of this curious discovery may be. RALPH ABERCROMBY

21, Chapel Street, S.W., October 26

The Hellgate Explosion and Rackarock

THE statement in NATURE of the 15th inst. (p. 575) that rackarock is "blasting gelatine" or "nitroglycerine with compressed gun-cotton" is incorrect. Rackarock is simply powdered potassium chlorate, impregnated with an inexpensive oily combustible, such as coal-tar oil, and is one of my safety-explosives, which I discovered in 1870, patented in England, April 6 and October 5, 1871, and described more fully in the *Journal* of the Chemical Society for August, 1873, under the title: "On a New Class of Explosives, which are non-Explosive during their Manufacture, Storage, and Transport."

I am not responsible for the quaint name which the Americans have been pleased to give to my child.

As the so-called "rackarock" is not very sensitive or easy to explode, it requires a strong primer or detonator to set it off. This property, which I have fully discussed and particularly accentuated in my paper of 1873, explains why Gen. Newton, the Chief Engineer of the Hellgate mine, took the precaution of placing as a primer such a powerful charge (33 tons) of expensive dynamite on the cheaper charge of the potassium chlorate mixture (107 tons), a precaution carried here perhaps a little too far.

Still it is satisfactory to see that my safety-explosive performed the main part of the labour and rendered good service in the advancement of the works of peace. H. SPRENGEL

Savile Club, 107, Piccadilly

[We are very pleased to insert Dr. Sprengel's correction as to the composition of "rackarock." Up to the time of our notice about the explosion going to press the only information we could obtain was that it was the same substance as blasting gelatine, but with a less portentous name.—ED.]

An Earthquake Invention

IN your number for October 15 (p. 573) your numerous scientific readers will be interested to find a pretty long letter under the above heading from so able a seismologist as Prof. John Milne, of Tokio, Japan. Yet, his invitation notwithstanding, I must decline any discussion with *him*, either about my old letters which he refers to, or his own much changed opinion on their subject, since the occasion for my writing them occurred.

Those points, Mr. D. A. Stevenson, who is also invited, may, or may not, take up. My letters were impersonal, and dealt only with a British Association Report. I desire also to continue to keep them strictly to that, even to the very words of the particular Report as given forth to the world with all the usually unquestioned authority of that mighty Association, in their B.A. volume for 1884, p. 248, Section entitled "Experiments on a Building to resist Earthquake Motion."

C. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, October 16

On the Behaviour of Stretched Indiarubber when Heated

SOME time ago (NATURE, vol. xxv. p. 507) you permitted me to express a doubt as to the invariable success of an often-quoted experiment with cylinders of bismuth and iron, intended to llustrate some relations between specific heat and thermal conductivity.[1] I regret that I have made further progress along the evil road of scepticism. I should like, in fact, to ask whether it is absolutely true to say without qualification, as is done in many text-books, that india-rubber (when stretched) forms an exception to the general law that the volume of a body is increased when the temperature is increased. The usual form of the experiment supposed to prove this is well known: a piece of india-rubber tube or cord is stretched by a weight connected with a long light index-lever, and it is shown that when heat is applied the india-rubber gets decidedly shorter.

I have always had some hesitation in showing and explaining the result of the experiment in the above way, especially as I could not find any proof given that the contraction in length was not compensated, or more than compensated, by an expansion in other directions (like that of a worm in its creeping progress, or of a dry rope when wetted). I had, in fact, lately arranged an apparatus for determining the coefficient of expansion of india-rubber, whether positive or negative, when I found that the subject has been very fully investigated by Dr. J. Russner, of Chemnitz (see Carl's *Repertorium* for 1882, pp. 161 and 196).

His results are briefly these:—

(1) That india-rubber (of which several kinds were examined) has without exception a definite coefficient of expansion which is always positive; experiments made at temperatures varying from 0° to 53°·4 gave, for its value at 10°, 0·000657; at 30°, 0·000670.

(2) That india-rubber in a stretched state expands to the same extent as when it is not stretched. No point of minimum density was observed, such as Puschl supposed to exist.

(3) That the apparently anomalous behaviour of stretched india-rubber when heated is simply a case analogous to those of anisotropic crystals, which expand to different extents in different directions. Iceland spar, for instance, as Mitscherlich showed, actually contracts in a direction at right angles to its principal axis when heated, although its volume is, on the whole, increased.

Although ordinary india-rubber is, of course, isotropic, yet when stretched it becomes anisotropic, as may easily be shown by stretching a piece until it is semi-transparent, and placing it between crossed Nicols; the direction of the strain lying at an angle of 45° with the plane of polarisation. Distinct colours, as with a selenite film, will be seen, varying from red to blue with the amount of strain.

The fact that india-rubber becomes hot when stretched, and especially if stretched and allowed to contract several times in succession, may perhaps be accounted for by molecular friction. It would almost seem, then, that in the account given in many books the truth, as well as the india-rubber, has been slightly "stretched." H. G. MADAN

Eton College, October 23

[1] Perhaps I may mention in passing that if lead is substituted for the bismuth the experiment succeeds perfectly, as theoretically it should do.

The Resting Position of Oysters

IN carrying out a series of experiments on the artificial breeding of oysters in my private aquaria, I noticed that the young oysters born in the tanks rested on the flatter shell when they obtained a flat surface, such as a tile, to adhere to, but when I so arranged that they had irregular surfaces to deal with, such as little bundles of twigs, some adhered one way, and some the other. But where young oysters, nearly two years old, were moved from their original supports, and were compelled to find new ones, they selected the flat shell to rest upon in every instance, except where they were placed on sand, in which case they rested on the convex shell, in order apparently to avoid clogging the mouth of the shell with sand. Is it not possible from these observations that adult oysters vary their position according to the nature of the ground they are on. I have seen adult oysters on muddy ground lying on the convex shell, while wheie adhesion to a flat surface could be obtained, they were all on the flat shell, and pectens are dredged with Balari and other growths on the flat shell in some instances, and on the convex shell in others, principally, however, on the latter.

H. STUART-WORTLEY

South Kensington Museum, October 23

The Value of the Testimony to the Aurora-Sound

I HAVE read with much interest the descriptions of this sound as given by Dr. Sophus Tromholt's correspondents in NATURE of September 24. I was, however, struck by the similarity of these descriptions to the well-known phenomena of *tinnitus* *aurium*, and it occurred to me that since a large number of persons have noises in the head—say one-half the entire adult population—it is probable that, when listening intently, a considerable number of observers heard the sounds of their own ears only. This is especially true of "sizzling," "hissing," and "buzzing" sounds.

If physicians affected with tinnitus are not careful to exclude the noises propagated in their own heads, they may discover many curious physical signs in the chests of their patients in making auscultatory examinations. SAMUEL SEXTON

12, West Thirty-fifth Street, New York, October 12

The Red Spot on Jupiter

ON October 24, at 17h. 32m., this object was estimated exactly central on the planet. As seen with my 10-inch reflector, power 252, the spot was very plain, though the low altitude of Jupiter rendered the telescopic image far from good.

My impression is that this red spot is now decidedly more conspicuous than it was when I last saw it on July 8, and that during the ensuing opposition it will again attract general observation as one of the most prominent features of Jovian detail. This well-known marking has now been watched for more than seven years, and its present aspect leads to the inference that its existence will be indefinitely prolonged. We may therefore justly regard it as a lineament of singular permanency. Though its motion and appearance (*i.e.* tint) have been subject to considerable variation, there has been little, if any change in either the shape or size of the spot. The mystery regarding its origin and real nature may perhaps ultimately be revealed on the basis of renewed and more exact observation in future years. W. F. DENNING

Bristol, October 25

A Remarkable Sunset

WHILE out for a walk this afternoon I was struck by a peculiarity in the sunset which I do not remember to have seen noticed before. The sun set about 4.43 p.m., and there was the usual "after-glow." I began to notice this first about five o'clock; there was then in the west a large bank of cumulus cloud rather low down, above this was a brilliant lemon-yellow, very bright, and this was bounded by a broad arc of a pale pink, the latter fading away into the light blue of the sky. Very soon afterwards I noticed that the pink arc, instead of being continuous, was really made up of a series of beams of bright light, which pointed to the position of the sun. I counted these, and made out five bright rays at unequal distances apart; behind this (as it seemed) there were a few yellow cirrus clouds. A sunset like this I have often noticed before, but what followed is, I think, novel. The bright rays were slowly turning round like the spokes of a huge wheel moving in a direction contrary to the hands of a watch. I noticed also that the breadth between the bright rays altered, two of them seeming to almost coalesce. In about ten minutes' time one ray turned approximately through 90°, and a new ray brighter than the other appeared on the right. The altitude of a ray when vertical was from 30° to 40°, I should say. By 5.15 the rays became very faint and soon vanished, though above the dark bank of cloud I could detect a faint crimson-lake glow.

The day had been fine on the whole, except that there had been a little rain early in the morning, and a very heavy rain shower between 12.30 and 1 o'clock. The air was extremely clear, and the wind was blowing freshly from the west, or perhaps it was a bit north of west. It was blowing slightly from right to left across the line joining me to the sun.

This phenomenon of the pink rays revolving seems to be explained by the dark spaces being due to clouds which were being hurried along by the strong west wind. I should like to know if any one living in a line W.S.W. of Cambridge noticed broken masses of cumulus clouds this afternoon *overhead* between 5.0 and 5.15 p.m. Greenwich time. PAUL A. COBBOLD

Caius College, Cambridge, October 26

A Tertiary Rainbow

THE supposed tertiary rainbow about which I sent a note a month ago must have been a halo formed by ice crystals, as readers of NATURE will perhaps have inferred merely from the recorded distinctness of the colours. It did not occur to me that ice crystals would be found in a horizontal direction from here, over the hot plains of the Punjab on the evening of an August day. But I have since calculated the size of the tertiary rainbow and the order of colours in it, and the calculation leaves no doubt that the phenomenon must have been a solar halo, caused perhaps by a hailstorm over the plains.

Thaudiani, Punjab, Sept. 25 T. C. LEWIS

The Sense of Colour

IN the early English "Lay of Havelok the Dane" the following words occur:—

> "Also he wolde with hem leyke
> That weren for hunger *grene* and bleike."

Mr. Allan Cunningham in his interesting paper (p. 604) does not allude to this old use of the word green. Is it a solitary case? MARGARET HEATON

Belvedere, October 24

Stone Axes, Perak

A CURIOUS Malay superstition has come to my knowledge concerning these implements. They appear to be very rare out here, and those found are treasured by Malays as lucky things to have about the house. I have as yet only been able to procure two specimens. One of these I have described in a paper on the Sakaies read before the Anthropological Society in June last. This nearly resembles Fig. 55 in Dr. Evans' "ancient Stone Implements of Great Britain," and is made of a soft description of slate which can be scratched with the thumb-nail. The other is of a much harder description of slate almost like greenstone; it much resembles Fig. 76 of the same work. It is 7⅞ inches long, 1⅝ inches wide at the widest end, which is sharpened, and 1½ inches wide at the other end, which is not sharpened. The faces are flatter than those figured by Dr. Evans and the sides perfectly squared. It is beautifully polished, but several depressions are left all over it, showing that it had originally been chipped out. The Malays call them Bâtu-lintarh—*i.e.* thunder-stones—and account for their presence by saying that they are the missiles used by angels and demons in their continual warfare.

But the peculiarity of the superstition is this: the Malays aver that the soft implement which I have described has been made by an angel or a demon and buried in the earth to become hard and fit for use, and support their argument by saying that these objects have been found freshly made of clay and quite soft, buried in the earth, where they have lately been deposited by some angel or demon for a future time of battle. The Malays say that the bâtu-lintarh is hard to procure in this state, as it almost invariably drops to pieces. For this reason they do not value it much, and more particularly because it has never inflicted a wound. The hard polished celt which I have just described, however, they value very highly, because they say it has been used in the aërial warfare and has inflicted a wound on one or more of the combatants. They adduce this supposition from the fact of the several depressions left by the chipping out of the implement, and say that these marks were caused by its contact with the body of one of the demon combatants. This last idea is very closely connected with another Malay belief, and most probably took its rise from it. This belief is that if the blade of a kriss or spear is bent or in any way damaged, it has most certainly wounded if not killed a man or some wild animal, and is therefore proportionately of much greater value. A Malay who professes to be a good judge of a kriss will, if asked to appraise the weapon, invariably first glance along the blade to see if it is bent ever so slightly, and if it is he will most certainly add two or three dollars to its value because it has "m'nikam orang" (struck a man). I have very little doubt that if some of the fine limestone caves of this district were thoroughly examined, they would yield a rich harvest of anthropological material. A. HALL

Batu Gaja, Kiuta, Perak, September 6

Photographic Action on Ebonite

AT the back of one of the cases of lecture apparatus facing a north window in this laboratory, there happens to have been standing for six months or more an ebonite plate with a framed glass plate in front of it, the glass having a star-pattern done in little spots of tinfoil all over it. The thickness of the frame, say an eighth of an inch, separated the two plates from each other.

On taking them out of the case the other day I noticed the pattern on the glass clearly and sharply imprinted on the ebonite; every little circle well marked. Dust had been plentifully deposited on all parts not screened by the tinfoil spots, and the striking clearness of the impression was mainly due to this local absence of dust; but even on wiping off some of the dust the pattern could still be detected, owing to some difference of surface between the exposed and the shaded portions.

It evidently is another illustration of Prof. McLeod's observation of the effect of light on ebonite, the modified surface affording an easy lodgment for dust. In case there be anything more in the matter it is proposed to replace the same or similar plates, and observe at intervals.

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THE SLIDE RULE

IT is a perpetual source of amazement to those who are familiar with this instrument that its use is not almost universal. People of every class have to make simple calculations, while those engaged in scientific work, in designing apparatus, or in invention perpetually cover sheets of paper with figures, all of which trouble and the loss of time which it involves might be saved by the intelligent use of a good slide rule, and yet, for reasons difficult to find out, the habitual use of this instrument is limited to a very small proportion of the calculating community.

Most people know that the scales are logarithmically divided—that is, that the distance between the divisions marked 1 and 10 being in imagination divided into 10,000 parts, the division marked 2 is at the 3010th of these parts, the division marked 3 is at the 4771st of these parts, and so on, 3010 being the log. of 2, 4771 the log. of 3, and so on; and further, that the spaces between these whole numbers are similarly divided into fractional parts, thus 1·1 is at the 414th of the imaginary parts and 1·01 at the 43rd of these parts, 414 and 13 being the logs. of 1·1 and 1·01. This is very generally known, but it is more generally believed that to use the rule involves so much thought and anxiety that it is far simpler to work out results in the usual way, or at any rate that the rule can only be of any real assistance when a great number of similar calculations have to be made; and further that, as the results to be obtained are not absolutely correct, that as an extreme error of 1, 1-10th, or 1-100th per cent. is possible, according to the nature of the instrument, it is not really to be trusted. These objections are easily answered. As soon as the slight difficulty of reading the rule has been overcome—a difficulty due to the fact that in ascending the scale the divisions become closer, so that if there is room for ten subdivisions between 10 and 11, there are only five between 20 and 21, and two between 40 and 41—a difficulty which once overcome never recurs—then the simpler calculations, such as multiplication, division, and simple proportion, can at all times without an effort or a thought be instantly performed, while those involving proportions in which some of the terms are squares, cubes, roots, sines, or tangents can, after a moment's reflection, be as easily completed, so that even in the case of single operations time is saved. It is true when many calculations of the same kind present themselves, especially if some of the terms in the series are identical, that the use of the rule is specially advantageous; but in any case mental labour and time are saved.

As to the probable accuracy of results obtained by the use of the rule, they are in general superior to the accuracy with which the figures which require reduction have been determined, or, if this is not the case, they are in general so nearly correct that the error is of no consequence. For instance, if the marks obtained by several examinees are to be reduced to correspond to a total of 100, the commonest rule, which gives an accuracy of 1-300th part, is sufficiently good; for the nearest whole number only, and the right order are all that are needed. It would be absurd to doubt the accuracy of the instrument because it cannot be trusted to give figures correct to one part in a thousand. Or, again, if the weight of a piece of metal has to be determined from its dimensions, a good rule trustworthy to 1 part in 1000 will in almost every case be more than good enough; for, even if the specific gravity of the material be known so truly, it is not often that the piece can be made so near the specified size that the discrepancy which may ultimately be observed will be due more to the error of the rule than to the inaccuracy of construction. In such a case it would be as absurd to discard the rule as untrustworthy as it is to use 7-figure logarithms for the calculations of an ordinary chemical analysis. There are cases, of course, where observations can be made with a degree of accuracy beyond that which is obtainable by any rule—for instance, determinations of mass, length, angles, and time can all be made with extraordinary precision. Where, then, uncertainty is not introduced by observations of another kind, where the entire precision to be obtained in any such observations may be expected in the result, as, for instance, in the determination of the refractive index of the glass of a prism, in such cases the slide rule is unsuitable, and tables of logarithms furnish the most obvious means of making the calculations. Or, again, when pounds, shillings, and pence are involved, a result correct to the nearest farthing is generally desired to make accounts come right, and so, unless the sums dealt with are moderate, the slide rule is again unsuitable. However, the calculation of interest furnishes a good example of proper and improper use of the rule in making calculations. If it is required to find what a certain sum (s) will be worth at the end of a year at so much (r) per cent., the result might be found from the proportion $100 : 100 + r :: s : x$. Here the amount x would be determined with an accuracy of say 1-1000th part, so that if 1000*l.* were involved, an error of 1*l.* might arise. This is an improper use of the rule. A greater degree of accuracy would be obtained by the proportion $100 : r :: s :$ the increase of s. Here the interest is found to the same proportionate accuracy, and so in such a case the greatest possible error could only be one shilling, if the rate is 5 per cent. This example, though obvious, is given because it corresponds exactly with cases that arise in the laboratory, where the rule, if used properly, is of service, but, if improperly, is useless.

Calculations involving only the simple arithmetical rules, when extreme accuracy is required, are best performed by the help of a table of logarithms, or with an arithmometer; in fact with an arithmometer a far greater degree of accuracy can be reached than with ordinary 7-figure logarithms, and though they are also suitable for calculations in which only three or four significant figures are required, their great size and expense compare unfavourably with the portability and cheapness of the rule, and, moreover, trigonometrical and logarithmic functions cannot be found with them. These machines are shown at the Inventions Exhibition by Tate and Edmonson, and are worth examining. There is another calculating machine close to Tate's, by which the interest on any sum at any rate per cent. for any time may be found to the nearest halfpenny in an incredibly short space of time, worthy of the attention of those who have to calculate interest. But, to return to the slide-rule, it is astonishing that an instrument like Gravet's, 10 inches long only, with which all calculations, arithmetical, trigonometrical, and logarithmic, can be worked out so easily and with an accuracy of from 1-500 to 1-1000, according to the nature of the calculation, should be so little used.

This is not the place to give instructions for using the rule, but an outline of the method is necessary to make it possible to compare the different makes, many of which are shown at the Inventions Exhibition.

With two similar scales of equal parts, as inches divided into tenths or centimetres divided into millimetres, it is possible to add numbers, or, conversely, to subtract numbers; thus, if the zero of one scale is placed opposite, say, 6·5 of the other, opposite every number n on the first will be found $n + 6·5$ on the second, and so addition or subtraction could be performed, but there would be no advantage in so adding or subtracting. In the same way the slide of the ordinary slide rule is employed to add distances, but these distances do not correspond to the figures attached, but to the logarithms of those figures, and so the sum which is found by such an addition is not the sum of the figures apparently added, but their product. If the slide is placed at random, all the pairs of figures which are opposite to one another are in the same proportion, and the multipliers which will change either series into the other will be found on each scale opposite the divisions marked 1 on the other. It requires no great amount of memory to bear this in mind: however the slide may be set, those numbers which are opposite to one another are in the same proportion, *i.e.* have a common quotient, which may be found opposite any of the divisions marked 1; and yet this is all that has to be remembered in multiplication, division, and simple proportion. The two top lines of a slide rule are generally identical, and they are used for these simple operations; they are generally distinguished by the letters A and B. In general the bottom line of the slide, that is, the third altogether, is identical with the first two, and is labelled C. This arrangement is convenient, for it is possible to insert the slide upside down, in which case all numbers which are opposite one another on A and C have a common product, which may be found opposite any of the divisions marked 1. This furnishes the most ready mode of finding actual or approximate factors of numbers, and is of great use to those who have to calculate wheelwork; further, by the use of the inverted C line under the A line any harmonical progression can at once be read, and any number of harmonic means can be inserted between two quantities. The fourth line is generally made different from the others in that it is on double the scale, and it is then distinguished by the letter D. If the units of the C and D line are placed opposite one another, a table of squares and roots is formed, or if in any other position the squares of the numbers on D vary in the same proportion as do the numbers that are opposite to them on C. It is in calculations made on the C and D lines that so much time is saved, for proportions in which some of the terms are squares or square roots can be worked out as quickly and as accurately as those in which simple numbers only are employed. If the slide is inverted so as to bring the B line opposite to the D line, then the square of any number on D × the number opposite to it on B is constant. This product may of course be found in B opposite 1 in D. Cube roots, among other things, may be found in this way.

These four lines are all that are generally found in a slide rule; occasionally others are added: thus a line on one third of the scale of the D line (sometimes called an E line) will, with the D line, enable one to directly work proportions in which some of the terms are cubes or cube roots, but this is not often required. With the usual four lines all arithmetical processes, except addition and subtraction, can be performed. There are, however, rules in which on the back of the slide are scales in which the distances are log. sines or log. tangents of the angles marked, then these lines being placed against an ordinary A line so that 90° on the line of sines or 45° on the line of tangents is opposite 1 on the A line, a table of sines or tangents will be formed; and if the slide is placed in any other position, the sines or tangents of the angles denoted by any divisions on either of these special lines will vary in the same proportion as do the numbers which are opposite them on the A line. In those rules in which lines of sines and tangents are given there is generally a scale of equal parts in which the length of the D line is divided into 500 or 1000 parts. If this is placed opposite the D line, with the ends of the two scales opposite one another, a table of logarithms will be seen; thus the logarithm of any number on the D line will be found opposite to it on the scale of equal parts.

Having pointed out the chief uses of a slide rule, it will be possible to describe the differences in construction in the several varieties. The most simple possible form is the original Gunter's scale to be found on any sector. With this and a pair of dividers calculations may be made, for if the dividors are set to the distance between any two numbers, any other pair of numbers which are found by the dividors to be the same distance apart will be in the same proportion, or have a common quotient, just as a common difference would be found if a scale of equal parts were used. This, however, is troublesome; but if the same principle is applied to a scale in the circular form the result is much more convenient. In this case angular distance takes the place of linear distance, and a pair of arms which can be opened to any angle can be moved round, and every pair of numbers covered will bear to one another a constant proportion depending on the extent of the angle. This is the principle of some of Dixon's rules shown at the Inventions Exhibition, near the arithmometers. In the well-known pocket instrument, the calculating circle of Boucher, an instrument like a watch, one hand is fixed and one is movable, and the face is also movable. There is another instrument of the same kind, in which the scale is drawn on a helical line. Here the scale and one hand are movable, and there is one fixed hand. This, which is Prof. Fuller's spiral rule, is made and exhibited by Stanley. Circular instruments are also made, in which scales slide over one another, which are in this respect like the straight rules. There is more advantage in the circular form than appears at first. In the straight rules the A and B lines are each double, the first and second halves are identical; this repetition of the scale is required in order that, however the slide may be placed, the part of each opposite to the other may contain at least a complete scale of numbers. In the circular form, however, the beginning and end of a single logarithmic scale meet, and so the scale itself is its own repetition both above and below. For this reason the openness of the divisors in a circular instrument is the same as in a straight rule, of which the length is six times, instead of three times, the diameter of the circular line.

Of the two types of instrument—one in which one slide works against another, generally straight, sometimes circular, and the other in which there is no slide but only a line divided logarithmically with a pair of hands, which type is always circular—which may be called respectively the slide and the index types, each has certain advantages. The slide form is preferable, in that each setting of the slide furnishes a complete table of pairs of related numbers, as, for instance, of any English and foreign measure, of squares and roots on any scale, such as diameters and areas of circles, or of sines or tangents on any scale, so that, without moving the slide, any number of results may be read off, whereas with instruments of the index type the scale must be moved under the hands, or the hands over the scale, for each result. On the other hand, index instruments are more convenient than the usual slide rules in working out long expressions of the form $\frac{a \times b \times c \times d}{e \times f \times g}$, in which any of the terms may be squares, cubes, sines, or tangents, for the terms are taken alternately from the numerator and denominator and set in order with the fixed and movable hand until all are worked off, when the answer is found under the fixed hand. There is no necessity to observe any result till the process is complete; on the other hand, with slide instruments, each result of the form $\frac{a \times b}{e}$, $\frac{a \times b \times c}{e \times f}$, &c., must be read and set before it can be operated upon by the next pair of factors. In Gravet's rules, however, this disadvantage of the straight form is removed by the addition of a cursor or sliding index, which in other ways is a great comfort.

All instruments of the index type suffer terribly from parallax, owing to the hands being above the face, so that they do not in practice give the accuracy that from the length of scale upon them might be expected.

This is especially the case in small instruments: for instance, Boucher's calculating circle, made in the form of a watch, is probably divided so accurately that on that score an error of one part in a thousand does not exist; yet, owing to parallax, the practical limit is about 1-300. This instrument has, besides the ordinary line, one on a double and one on a treble scale for squares and cubes, a line of sines, and another of equal parts for logarithms.

The possible accuracy of any instrument depends upon the length of the scale included between 1 and 10, called the radius, and also upon the linear accuracy with which a setting or reading can be made; this is at least twice as great in slide as in index instruments. In order to obtain great accuracy various means have been adopted whereby a great length of scale is brought within a small compass. Among slide instruments are Prof. Everett's "Universal Proportion Table," published by Longmans, Green, and Co., and General Hannyngton's slide rule, made and exhibited at the Inventions Exhibition by Aston and Mauder. In these the slide is made in the gridiron form. In Everett's instrument there are twenty bars, the total length of which is about 13 feet; a scale of equal parts is also printed, so that logarithms can be read with it. In both of these instruments only simple proportions can be effected, unless special grids, divided on a double scale or trigonometrically, are provided. Far the most ingenious of all devices for obtaining a great length of radius in a comparatively short space is due to Mr. Beauchamp Tower, whose name is well known in connection with the spherical engine. His instrument is a slide instrument consisting of two tapes running side by side over equal and independent rollers, but the tapes have a half twist in them, so that they have each only one surface and one edge. In this instrument, made privately for his own use, each tape is about 12½ feet long, and as both sides of the tape are used the radius is about 25 feet, and therefore, as far as openness of scale is concerned, it is equivalent to a straight rule 50 feet long, while the instrument itself is only just over 6 feet in length.

Slide rules of the index class can have a great length of scale more readily employed than others. Thus Prof. Fuller's helical instrument has its radius equal to 42½ feet, and is in openness of scale equivalent to a straight rule 85 feet long, while the box which contains it is only $17 \times 3\frac{3}{4} \times 3\frac{3}{4}$ inches inside measure. Dixon exhibits a special rule with the scale extending over 10 concentric circles, but with this form a less degree of accuracy is attainable when using the inner than when using the outer circle. Thus the inner circle is equivalent to a straight rule 30 feet long and the outer to one 60 feet long. There is an outer circle equally and logarithmically divided to find logarithms. In another of Dixon's instruments, similar in size and form, there is the same outer circle for proportions and logarithms, and a series of inner circles divided so as to give sines, cosines, tangents, cotangents, secants, and cosecants. Each of these is on a board 14 inches square. Rules with very extended scales do not in practice give results with an accuracy which is proportional to their length, though the working accuracy is very much increased. They have this advantage, that they can be worked to their limit with ease, while with a well-divided pocket rule the errors of construction are beyond the limits of vision, and so the calculator is apt to strain his eyes to get results as accurate as possible. For instance, results obtained by a good pocket-rule one foot long can be trusted to a thousandth part; at the same rate Prof. Everett's should be accurate to a thirteen-thousandth part, and Prof. Fuller's to an eighty-five thousandth part. In practice a four and a ten-thousandth part are their limits. Again, instruments with very extended scales have only room for one line, so that simple proportions only and logarithms are all that can be directly obtained from them. For general use in the laboratory or elsewhere where calculations of every kind have to be made, the straight form, on the whole, seems most convenient, because of its portability, the quickness with which it can be worked, the diversity of operations that it will directly accomplish, and the extraordinary accuracy in comparison with other forms of the results to be obtained. Far the best instruments of this type that the writer has yet seen are those made by Tavernier-Gravet, of Paris, already alluded to. They are different to those generally used in England in that the line in the slide which works against the D line is itself a D line, so that squared proportions have to be performed by the aid of the cursor. This form has the further disadvantage that the inverted slide cannot be used for finding factors, which is a great loss; on the other hand, the two lower lines may be used for simple proportions, and they will give a double accuracy. On the whole, the original pattern with an A, B, C and D line seems preferable. Of the straight rules shown at the Inventions Exhibition those made by Stanley exceed all the others in workmanship and they are equal in this respect to the Gravet rule. Among them are rules for special purposes, as Hudson's scales and Ganga Ram's rules. Hudson's scales, which are made in card, each having two slides, are a marvel of constructive skill. Dixon shows his "triple radius double slide rule," with which very complex operations may be readily performed. Heath shows a slide rule for converting sidereal to mean solar time, or the reverse, correct to about ·02 of a second, but this is not a slide rule proper, as the scales are not logarithmic.

There is entirely a different class of slide rule shown by Lieut. Thomson. In this there is, as usual, an A, B, and C line, but instead of the D line there is a "P" line, in which the distances, instead of being logarithmic, are logarithms of logarithms. By this instrument fractional powers may be found as readily as simple products or quotients. It has, however, this defect, that the scale converges so rapidly as the numbers ascend that high numbers can only be obtained with a proportionate accuracy far less than is possible with low numbers. It is one feature in the slide rule of ordinary construction that an error of reading of, say, 1-100th of an inch will produce the same proportionate error in any part of the scale. This rule for involution is shown in the straight and circular form. It is right to mention that the same thing exactly was invented by the late Dr. Roget and published by him in the *Phil. Trans.* of 1815.

No attempt has been made to give an account of every special form of rule that is made; those shown at the Exhibition and some other well-known forms, which well illustrate the different kinds of development, have been imperfectly described and the general principles on which all depend sufficiently explained to make evident the advantages of each type of instrument.

C. V. BOYS

HOMING FACULTY OF HYMENOPTERA

IN connection with Sir John Lubbock's paper at the British Association, in which this subject is treated, it is perhaps worth while to describe some experiments which I made last year. The question to be answered is whether bees find their way home merely by their knowledge of landmarks or by means of some mysterious faculty usually termed a sense of direction. The ordinary impression appears to have been that they do so in virtue of some such sense, and are therefore independent of any special knowledge of the district in which they may be suddenly liberated; and, as Sir John Lubbock observes, this impression was corroborated by the experiments of M. Fabre. The conclusions drawn from these experiments, however, appeared to me, as they appeared to Sir John, unwarranted by the facts; and therefore, like him, I repeated them with certain variations. In the result I satisfied myself that the bees depend entirely upon their special knowledge of district or land-marks, and it is because my experiments thus fully corroborate those which were made by Sir John that it now occurs to me to publish them.

The house where I conducted the observations is situated several hundred yards from the coast, with flower gardens on each side and lawns between the house and the sea. Therefore bees starting from the house would find their honey on either side of it, while the lawns in front would be rarely or never visited—being themselves barren of honey and leading only to the sea. Such being the geographical conditions, I placed a hive of bees in one of the front rooms on the basement of the house. When the bees became thoroughly well acquainted with their new quarters by flying in and out of the open window for a fortnight, I began the experiments. The *modus operandi* consisted in closing the window after dark when all the bees were in their hive, and also slipping a glass shutter in front of the hive door, so that all the bees were doubly imprisoned. Next morning I slightly raised the glass shutter, thus enabling any desired number of bees to escape. When the desired number had escaped, the glass shutter was again closed, and all the liberated bees were caught as they buzzed about the inside of the shut window. These bees were then counted into a box, the window of the room opened, and a card well smeared over with birdlime placed upon the threshold of the beehive, or just in front of the closed glass shutter. The object of all these arrangements was to obviate the necessity of marking the bees, and so to enable me not merely to experiment with ease upon any number of individuals that I might desire, but also to feel confident that no one individual could return to the hive unnoticed. For whenever a bee returned it was certain to become entangled in the bird-lime, and whenever I found a bee so entangled, I was certain that it was one which I had taken from the hive, as there were no other hives in the neighbourhood.

Such being the method, I began by taking a score of bees in the box out to sea, where there could be no landmarks to guide the insects home. Had any of these insects returned, I should next have taken another score out to sea (after an interval of several days, so as to be sure that the first lot had become permanently lost), and then, before liberating them, have rotated the box in a sling for a considerable time, in order to see whether this would have confused their sense of direction. But, as none of the bees returned after the first experiment, it was clearly needless to proceed to the second. Accordingly I liberated the next lot of bees on the sea-shore, and, as none of these returned, I liberated another lot on the lawn between the shore and the house. I was somewhat surprised to find that neither did any of these return, although the distance from the lawn to the hive was not above 200 yards. Lastly, I liberated bees in different parts of the flower garden, and these I always found stuck upon the bird-lime within a few minutes of their liberation. Indeed, they often arrived before I had had time to run from the place where I had liberated them to the hive. Now, as the garden was a large one, many of these bees had to fly a greater distance, in order to reach the hive, than was the case with their lost sisters upon the lawn, and therefore I could have no doubt that their uniform success in finding their way home so immediately was due to their special knowledge of the flower garden, and not to any general sense of direction.

I may add that, while in Germany a few weeks ago, I tried on several species of ant the same experiments as Sir John Lubbock describes in his paper as having been tried by him upon English species, and here also I obtained identical results: in all cases the ants were hopelessly lost if liberated more than a moderate distance from their nest. GEORGE J. ROMANES

THE HEIGHTS OF CLOUDS

FROM the Upsala Observatory comes an account of fairly exact measurements of the heights of clouds during the summer of last year, and a very interesting publication it is. It appears that when the circumpolar expeditions were planned the Swedish Meteorological Observatory furnished their station at Spitzbergen with three theodolites, of a somewhat novel though simple construction, for the double purpose of observing the altitude of the aurora and that of clouds. The difficulty that has always been felt in such observations has been that of easy intercommunication between the different observers, so as to fix on the particular part of the cloud of which the height was to be measured. Thanks to modern invention this difficulty was got over by connecting each station with a telephone. The reported good results obtained at the circumpolar station—the publication of which, by the by, has not been done as yet—induced Herr Hildebrandsson, the director of the meteorological observatory at Upsala, to commence a set of similar observations there. On a couple of pillars, about 450 yards apart, and placed on an approximately north and south line, a couple of theodolites were erected, the stations being connected by telephones. The theodolites employed may be described as ordinary theodolites, the object glass of the telescope being replaced by a large open ring, across which were stretched a couple of cross wires, whilst the eye-piece consisted of a simple hole of 3mm. in diameter. When observing near the sun dark glasses would be placed in front of this orifice. As might be expected, there are several unavoidable errors in using these instruments, the principal of which are the uncertainty of an identical point in a cloud being measured at each station, and the want of synchronism of the observation—a very important point when clouds are travelling with any speed. The method of observation was somewhat laborious, and was as follows. The two observers, each at a theodolite, agreed as well as they could on the point in the cloud to be observed, and at a particular time, fixed upon in advance, brought the cross wires on this somewhat indefinite spot, and then read their instruments, noted the time of observation, described the cloud, and if possible sketched it. A second observation of the same point gave the direction and rate of motion of the cloud. Perhaps one of the most easily observed clouds is the cumulus, and we find from a table given that the probable error of observation is very considerable. Thus, in one whose height was calculated to be 1,639 metres, the probable error of one observation was 748 metres, and of the mean of 16 observations 187. Out of 101 observations the mean height of a cumulus was 1,690 metres, and the probable error of the mean 40

"Mesures des Hauteurs et des Mouvements des Nuages." Par N. Ekholm et K. L. Hagström.

metres. The labour to attain even such accuracy is very great. The surprise is that at Upsala they did not adopt a photographic theodolite such as is now, we believe, in daily use at Kew. In the Kew "nephographs," as they are called, the telescope is replaced by a camera, and the observations do not involve half the labour of eye-observations. For instance, when the two nephographs are in a fixed position the manipulations are simplicity itself. One observer telephones to the other the cloud whose height it is desired to ascertain. By means of a very simple pointer both direct their cameras to the cloud, having inserted a dry plate in position. The lenses are closed by shutters, both of which can be opened and then closed with any desired rapidity by an electrical arrangement from one station. The exposures are thus made simultaneously, and the photograph must include every point in the cloud. The position of the cloud is fixed by crossed lines etched on a glass plate which is in contact with the dry plate, and which always occupies the same position, and from these cross lines, which are impressed on the two negatives, any desired point is measured. The readings of the graduated circles of the nephoscope having been taken the height and distance of the cloud is readily calculated. It might be supposed that considerable errors might be made even with this arrangement as the solid angular distance included is somewhere about 55°, and the objects within this are impressed on a plate less than six inches square. As a matter of fact, such is not the case. Measurements of objects a couple of miles off, and at known distances from the observer, have been observed with an error of less than 1 per cent., a base of 250 yards having been used—an accuracy which is far greater than could be obtained by eye-observations when the object to be observed is uncertain in outline, and when there is no definitely fixed point to observe. It must not, however, be supposed that there are no difficulties in photographing clouds of every description. It requires, for instance, a keen judgment to hit off the exposure necessary to differentiate between the white clouds in the higher regions the pale blue sky against which they are projected. All such difficulties are to be overcome with practice. It is to be hoped that before long the Upsala Observatory will adopt such a plan as we have indicated, when the results they obtain will be even more valuable and be less laboriously attained than they are at present.

The following table gives the height of the different characters of clouds at Upsala:—

Stratus		625 metres.
Nimbus (lower)		1,115 "
" higher		2,185 "
Cumulus and cumulo-stratus	top	1,690
	base	1,307
	mean	1,498
Lower alto-cumulus		1,988
Higher " "		4,242
Cirro-cumulus		5,513
Cirrus		6,823

The authors point out that, according to their observations, apparently there are seven levels, each one occupied by a different species of cloud, viz.: 600, 1,100, 1,500, 2,000, 42-4,600, 58-6,600, and 80-8,600 metres; and these levels agree with those deduced by M. Vettin of Berlin, who deduced them from a different mode of observation. There are several remarkable tables, some of which give the diurnal variation in the height of clouds, others the diurnal variation of the frequency of high clouds at Upsala during the summer, others again which discuss the question of the effect of the height of the barometer on the cloud masses. One of the most interesting sections of the memoir is that on the calculation of the velocity of wind at different heights from the movements of clouds.

On the whole, the Observatory at Upsala is to be congratulated on the step it has taken in making systematic observations of cloud heights and velocities. It is a matter of capital importance to meteorology that such should be undertaken in various localities, not only at or near the sea level, but also at as high altitudes as possible. Were the cloud levels, for instance, the same at all places, mountainous districts would be very much more cloud bound than we know is the case. Observations of clouds in the Alps show that the levels at which the different classes are to be found exceed the heights which are shown in the table above; and it remains to ascertain not only the effect of barometic pressure on the levels, but also the disturbing effect caused by the elevations in the land. Such observations might well be added to the observatory at Ben Nevis, and no doubt some enthusiastic meteorologist would be willing to spend a summer in the Alps to make observations at a still higher station. Until work such as this is undertaken the subject can only be partially discussed on scientific grounds.

W. DE W. A.

THE RECENT TOTAL ECLIPSE OF THE SUN

WE have received the following communications:—

THE news that bad weather seriously interfered with the work of the Government Survey parties, sent to observe the eclipse of the 9th inst. from points on the centre line of totality, induces me to send you the accompanying incomplete sketch and hasty account by to-day's mail:—

I observed the eclipse from Tahoraite, the present southern terminus of the Napier-Wellington Railway, a point well within the belt of totality, but some forty miles north of the centre line.

I went, determined to concentrate my whole attention on the corona, and the corona alone—I did not even take my watch. My eclipse observations are therefore necessarily very incomplete.

After a stormy night (alternate showers of rain and hail, with a bitterly cold wind), day-dawn brought a clear sky; but a heavy bank of clouds far away to the south boded no good to observers in that direction. The cold was bitter, and fresh snow lay very low down on the neighbouring hills.

The first contact occurred not long after sunrise, the atmosphere in the east being rather hazy, and the light *pale* (other observers say *ruddy*). At first the temperature of the air seemed to rise steadily, but when the sun's disk was a quarter obscured, it began to fall again, and as totality approached the cold became severe.

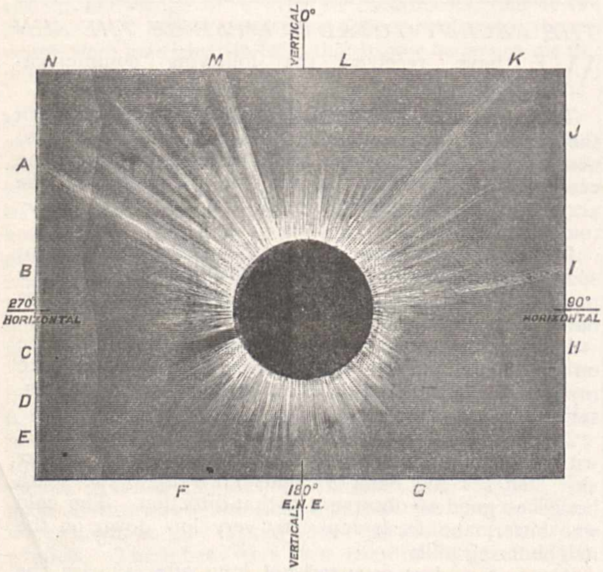
When the occultation of the sun had reached three-quarters, the so-called "livid" character of the light became very marked, and about ten minutes before totality a curious and tremulous play of light on the ground—like dark ripples or moving "marblings," if I may use the word, became apparent.

In order to keep my eyes as sensitive as possible to the faint light of the corona when it should become visible, I only watched the sun (through a telescope) for a few minutes after first contact, I then averted my gaze, and fixed it persistently on the dark-green bush surrounding the Tahoraite clearing. All I noticed during my hasty survey of the disk was two small and one large spot, the latter close to the limb at about 90° (see sketch), and surrounded by faculæ.

The moment "totality" occurred I turned my gaze towards the sun, and having previously, to save time, drawn disks on several pages of my pocket-book, I hurriedly took sketch after sketch of the shape of the corona, the rays of which were much better marked than I had been led to expect. My object in taking several sketches was to record any change i[illegible] osition of the rays. I took five during the short time of totality, and their agreement is so clear as regards the number and relative position and length of the main rays, that it fully confirms the general impression left on my mind as to the fixity of this phenomenon.

I was just engaged in making a last estimate of the extent of the corona between 35° and 90°, when a cry arose from the bystanders, "Look at the red flame shooting out to the left!" I withstood the temptation, and observed the almost sudden disappearance of the corona on the reappearance of the sun. All I can therefore say of the red protuberance which attracted so much notice is that the emergence of the sun is blended in my mind with a vague and fleeting impression of a narrow streak of red light with a broad streak of white light outside it, between 200° and 290°, and almost symmetrically divided by the position of the dark rift in the corona.

An acquaintance who noticed the rift in the corona told me that the red flame shot out close to it. He described the shape of the flame as *ragged;* other intelligent observers compared it to a *sugarloaf;* the most intelligent



Corona as observed during solar eclipse of September 9, 1885, as seen from Tahoraite, North Island, New Zealand, about 40 miles north of centre line of totality:—A, 300°; longest ray, 2-3 diams. B, first contact about 285° (vague). C, Sun apparently reappears about 260° (vague). D, 250°; dark rift in corona. E, approximate position of large red protuberance which shot out just before close of totality. F, corona, ½ diam., rather ragged. G, corona, ½ diam., regular. H, large sun-spot with faculæ close to the limb at time of first contact. I, 80°; 3rd longest ray, 1½ diam. J, between these two long rays corona extends nearly ¾ diam., with some longer rays. K, longest ray, 1¼ diam. L, corona, hardly ½ diam. M, 2nd longest ray, 1½-2 diams. N, between longest rays corona extends ¾ to 1 diam.; 2 long rays 1¼ diam. (N.B.—Corona alone was observed: relative position and lengths of rays reliable; absolute lengths to be taken with caution.)

of those I heard, to a *drop of water* hanging from an object. Other observers, again, stationed two miles off, saw, *not a red* but a brilliant *white* flame shoot out.

I leave these discordant statements and comparisons to be reconciled by other men.

During totality there was a considerable amount of diffused light around the sun. I am unable to state its extent or colour from personal estimate. Some of the bystanders called the colour pearl-grey; others, reddish; others, again, pale blue and white.

The colour of the corona itself seemed to me very pale bluish green.

The sketch I send you should explain itself. I will only mention that the angles are, of course, only estimated, the zero direction being the upper end of the vertical through the centre of disk.

The rift in the corona was very marked, and extended right down to the disk; it was very near to by far the longest of the rays. In conclusion there is nothing that the coronal rays remind me of so much as an auroral display.

N. A. GRAYDON

Hastings, Hawkesbay, N.Z., September 11

Mr. Henry Bedford, of All Hallow's College, Dublin, sends us a copy of the *Marlborough Express*, New Zealand, of Wednesday, September 9, 1885, giving an account of the eclipse as observed at Blenheim and other places in New Zealand.

The eclipse at Blenheim began at 6.30 a.m., and totality occurred at 7.25.

"The totality—if totality it was—could have lasted but a bare moment: for, to the untrained observer, it seemed that a patch of bright sunlight on the upper edge of it was never absent. It must, however, be remembered that Blenheim is on the very outer edge of the belt, and that the apparent duration of totality was so extremely short that, by an optical illusion, it might seem that sunlight was never totally obscured. The corona and sun's flames were plainly visible, and formed a spectacle which no mechanical contrivance can imitate, and no art can reproduce. Several stars in different quarters of the heavens—and particularly one about four sun's diameters below the eclipse—were seen, and the general appearance of the sky and of the shadows on the hill sides and in the water was that of early dawn. The eclipse was certainly a wonderful phenomenon, and almost as interesting to the non-scientific observer as to the man of science who viewed it in his observatory.

"Observations of the eclipse were taken in the cricket ground at Blenheim by Mr. Dobson, C.E., and ten instantaneous photographs were secured by Mr. W. H. Macey, the two gentlemen acting in conjunction. Mr. Dobson's observations were made by the telescope and theodolite, the powerful telescope belonging to Mr. Cullen of Mahikipawa having been erected in the cricket ground for the purpose."

At Wellington, by the time the total phase was reached the sun was sufficiently clear of clouds to give an uninterrupted view. As totality was reached the scene was most impressive, and as the darkness increased the western heavens became illuminated with a deep orange colour, shading off into the most delicate of yellows. A number of stars were plainly seen during the darkness. After about a minute and a half the sun again shone out, and gradually increased. Pigeons and birds began to fly about in a helpless fashion, and sought their roosts.

Dr. Hector reports:—"Heavy southerly squalls, with hail, spoiled the observations. We were at Dryertown, on the centre of the line, but got only partial glimpses. A pink patch surrounded the sun, and extended 15° from it, probably due to the same dust film in a high atmosphere that caused the sun-glows last year."

At Mastertton a heavy south-west gale with rain set in on the 8th, and the morning broke without any signs of clearing. Messrs. M'Kerrow and party, who had camped at the foot of Otahuao, proceeded to the top and fixed their instruments amid driving snow and hail. Just before totality the sky cleared, and all the phenomena were fairly visible. One photograph was taken before totality, three during, and one after. The corona was visible for fully a minute, encircling a ring of light radiating to a distance of about half a diameter of the sun. It was of a pale white colour, like the electric light; of uniform width, except at the sun's equator, where it slightly protruded, and was evidently of greater extent.

We have just received, by the dilatory method of a letter by post, an account of the preparations making and made for the due observation of the Total Solar Eclipse in September, up to within a fortnight of the event coming off; but no more. Our informant, the Venerable the Archdeacon Stock, of Wellington, New Zealand, was momentarily expecting two large auxiliary expeditions, one from Sydney, the other from Melbourne; and had been himself told off for corona work. But though brimming full of fine enthusiasm to do all that man could do in that department, he yet characteristically adds, "but how can we expect to see any of the more refined and minute features through all this Krakatão haze which the sun has still to shine through? In 1882, before that great volcanic eruption, we could see the comet of that year close up to the sun's limb; but now I am certain that nothing of the kind could be visible." C. P. S.

15, Royal Terrace, Edinburgh, October 21

NOTES

PROF. PASTEUR read on Monday evening to the Paris Academy of Sciences a statement, of which the following is the substance as telegraphed to the *Standard*:—M. Pasteur some time ago succeeded in rendering proof against rabies some sixteen out of every twenty dogs experimented upon. But to ascertain that immunity had really been given, he had to wait four months after the inoculation had taken effect. He therefore set himself to obtain virus of different degrees of strength, with the object of obtaining prompter and more certain results. This was effected by the following means:—A rabbit was inoculated with a fragment of tissue taken from the spine of a rabid dog. The incubation of the poison occupied fifteen days. As soon as the rabbit was dead a portion of its spinal marrow was in turn inoculated into a second rabbit, and so on until sixty rabbits had been inoculated. At each successive inoculation the virus became of increased potency, and the last period was not more than seven days. Having ascertained that exposure to dried air diminishes the virus, and consequently reduces its force, M. Pasteur supplied himself with a series of bottles containing dried air. In these bottles were placed portions of the inoculated spinal marrow of successive dates, the oldest being the least virulent, and the latest the most so. For an operation M. Pasteur begins by inoculating his subject with the oldest tissue, and finishes by injecting a piece dating from two days only, whose period of incubation would not exceed one week. The subject is then found to be absolutely proof against the disease. At the beginning of July a young Alsatian, named Joseph Meister, who had been severely bitten in several places by an undoubtedly rabid dog, presented himself at the laboratory. His case, left to itself, being considered hopeless by M. Pasteur, Prof. Vulpian, and other high authorities, the patient was submitted to the same series of inoculations that had been so successful on dogs. As a proof a series of rabbits were simultaneously subjected to the identical processes. In ten days thirteen inoculations were made with pieces of spinal marrow containing virus of constantly-increasing strength, the last being from the spine of a rabbit which had died only the day before. The youth thus operated upon by the successive administrations of weaker virus was made proof against the virus of the intensest strength. It is now 100 days since he underwent the last inoculation, and he is in perfect health. Those rabbits, on the contrary, which were at once inoculated with the strong virus, without first being rendered fit to receive it, became affected within the proper incubation period, and died with the usual symptoms. The first inoculation practised upon Meister was sixty hours after he had been bitten. M. Pasteur has, at the present moment, another human patient under treatment who was bitten a few days ago by a mad dog. M. Pasteur said it would now be necessary to provide an establishment where rabbits might always be kept inoculated with the disease. In this way there would constantly be a supply of spinal tissues, of both old and recent inoculation, ready for use. Before the sitting was adjourned M. Pasteur received an enthusiastic ovation from both the Academy and the public present.

THE annual meeting of the five academies forming the French Institute took place at two o'clock on October 24 in the large hall of the Institut; M. Bouguereau, President of the Academy of Beaux Arts was in the chair. The great prize delivered once every two years was awarded to Dr. Brown-Sequard, the well-known physiologist. M. Paul Bert had written a paper "On Vivisection," which was expected as a sequel to the delivery of the prize to Dr. Brown-Sequard, but it was not read for want of time. The annual banquet took place in the evening for the second time.

IT is rumoured that M. Goblet, the Minister of Public Instruction, proposes to return to the former organisation of the Institut, which was regarded as a universal self-electing body. Each class or special academy had not the privilege of choosing its own members as now, but of proposing a list of candidates to the whole Institut. The increased solemnity given to the annual and quarterly meetings, and the institution of banquets, are considered as preparatory to this important change.

M. BERTRAND, who was nominated member of the French Academy some months ago, will be received on December 10 next, at a solemn sitting, when he will read his inaugural address. It will be answered by M. Pasteur.

A VERY valuable addition has recently been made to the Science Collections now displayed in the Western Galleries at the South Kensington Museum of Science and Art. Mr. Rochfort Connor, of the Inland Revenue Department, has prepared a number of exquisitely finished pen-and-ink drawings of objects viewed with the microscope, often by the aid of very high powers. The collection, which covers two large screens in the rooms devoted to biology and geology, include drawings of insects and other minute forms of animals, and of various anatomical preparations from them, of curiosities of pond-life, and of the skeletons of many organisms, both recent and fossil. Among these last Mr. Connor's highly-finished representation of some of the more complicated forms of the Diatomaceæ, such as *Heliopelta* and *Coscinodiscus*, are especially worthy of admiration, though some of his drawings of Foraminifera, Bryozoa, and Sponge-spicules are scarcely inferior to these in delicacy of execution. These drawings represent, we understand, the leisure hours of a busy life-time, and their author is now engaged in a series of microscopic drawings illustrating the characters of food-products and their adulterants. A few of these are now exhibited as samples, and the series when complete cannot fail to be of great use to public analysts and others.

AT a meeting of the Brookville (U.S.) Society of Natural History, September 22 (according to *Science*), a committee was appointed to confer with the scientific associations, educational institutions, and with individuals throughout the State of Indiana, concerning the advisability of the formation of a State Academy of Science, and if thought advisable, to co-operate with such persons in favour of the formation of such an association. Free expression of opinion is called for by the committee, both as to the need of such an organisation and as to the best plan for its composition. It is now the plan to hold a meeting at Indianapolis between Christmas and New Year's day. It proposed that the organisation shall enable the citizens of Indiana who are engaged in scientific work to meet at certain times "for social intercourse, for the exchange of ideas, and the comparison of results of scientific studies." It would appear from the prospectus that the Academy would be a State society similar to the American Association.

SOME theoretical views on the detonation of meteorites have been recently offered by Signor Bombicci in the Royal Accademia dei Lincei. He supposes the detonation to be that of an explosive gas mixture, formed during the surface-heating of the mass in the atmosphere, and accumulating chiefly in the vacuous space left behind the mass in its very swift flight. The gas mixture is probably of oxygen and hydrogen, and it becomes detonant when the proportions are near those in which the gases form water. The oxygen may be supplied from the air; the hydrogen may come from the meteorite itself, which, having like porous bodies and fused metals, taken it up and condensed it in some region of space, sets it free again as it becomes very hot by friction of the air, and as an enormous difference of pressure arises between the front and the back part. But a portion (and perhaps the larger) of the detonating mixture may come from dissociation of the aqueous vapour in contact with the glowing and fused surface of the meteor. To the idea of an actual explosion of the meteorite by internal energy, Signor Bombicci objects that the ball must be shattered to the finest dust, and that fragments would not be coated with a crust. Sometimes meteorite stones remain quite whole in spite of the detonation. Haidinger's idea of the sound being due to air rushing into the vacuum behind the meteorite is thought improbable because the detonation takes place in very high layers of the atmosphere, where the air is much too rare; moreover the movement of the meteorite until detonation is a quite steady one. The character of the noise, and its repetition at intervals, also the shattering of the mass into fragments forming a cone of dispersion towards the earth all agree, in the author's opinion, with an explosion of gas behind the meteorite. Referring to another point, Signor Bombicci thinks that the earth has by virtue of its magnetism a selective action on cosmic masses; hence the universal presence of iron in meteorites.

MESSRS. A. AND C. BLACK will publish immediately a volume by Dr. Croll, F.R.S., entitled "Discussions on Climate and Cosmology," and also a new edition of "Climate and Time."

ACCORDING to the *Journal of Indian Art* the Government of India has decided to combine the duties of the Archæological Survey and those hitherto performed by the curator of Ancient Monuments. For this purpose India, exclusive of the Madras and Bombay presidencies, has been partitioned into three divisions, one of which has been placed under the control of Major Keith, who superintended the construction of the magnificent Gwalior gate which H.H. Maharajah Scindia has presented to the South Kensington Museum, and which will be a prominent ornament of next year's exhibition.

WE have received from Mr. Saville Kent, Superintendent and Inspector of Fisheries in Tasmania, a very encouraging report of operations for the year ending July 31, 1885. Much of the report is devoted to oyster fisheries, which Mr. Kent is endeavouring to develop on scientific principles. He has established hatcheries at various points, and a laboratory for experiments, and under his care the oyster ought to become an important industrial product in Tasmania. He also advises the encouragement of sponge fisheries. With regard to Salmonidæ, Mr. Kent concludes that no true salmon have yet been established in the lakes and rivers of Tasmania. The fish of large size which abound in the great lakes and other large sheets of water are really essentially the same as the Great Lake Trout or *Salmo ferox* of Great Britain.

IN the Report by the Board of Trade on their proceedings and business under the Weights and Measures Act for the past year, it is stated that the attention of the department has been called by the Corporation of Dublin to the necessity of providing a legal standard measure for testing steam pressure-gauges. In reference thereto regret has been expressed that at present the Standards Department has no power to do this. The question appears to be whether a pressure-gauge is a "measure" within the meaning of the Act. The testing apparatus proposed by the Corporation is a measurer of pressure applicable only for special use, and it belongs to a class of measuring instruments, as barometers, thermometers, &c., not directly provided for by the Act. In the report of last year an opinion was expressed that the time had now arrived when this country might, under proper conditions, join the International Convention on Metric Standards, and in September last Her Majesty's Government made known to the Comité International des Poids et Mesures at Paris that England was willing to join the Convention. This has now been done; and the Comité accepts the reservations of Her Majesty's Government as to the introduction of the metric system into this country, affirming that there is nothing in the articles of the Convention which implies any obligation on the part of a contracting State to attempt to modify the system of weights and measures legalised at the time in that State. The adhesion of England, therefore, is not to be regarded as any expression of opinion that the adoption of the metric system in this country would be desirable. A copy is attached to the Report of a Memorandum on Metric Standards intended for laboratory use; and also a copy of a scale of errors to be permitted on ordinary metric standards used in testing manufacturers' weights. Metric weights from 20 kilograms to 0·001 gram. to be used for the purposes of science and manufacture, or for any lawful purpose not being for the purpose of trade, have been verified for the local authority of Birmingham.

MR. CLEMENT L. WRAGGE, of the Torrens Observatory, near Adelaide, late of Ben Nevis, has been instructed by the Queensland Government to "visit and report as to the best means of establishing meteorological stations in Queensland, including the Cape York Peninsula and Torres Straits." Mr. Wragge, who lately returned to Brisbane from Northern Queensland, will commence his duties early this month, and proceeds shortly to Normanton in the Gulf of Carpentaria.

THE Institution of Mechanical Engineers met at Coventry yesterday, when the following papers were read:—On the construction of modern cycles, by Mr. Robert Edward Phillips, of London; on the distribution of the wheel load in cycles, by Mr. J. Alfred Griffiths, of Coventry; description of a hydraulic buffer-stop for railways, by Mr. Alfred A. Langley, of Derby.

THE aquarium at the Inventions Exhibition has lately received some valuable additions in the form of golden tench, American salmonidæ, and Italian carp, notwithstanding the fact that the Exhibition will shortly close. It is to be hoped that the exhibits from the Buckland Museum collection will be allowed to remain in the aquarium, where they appear to far better advantage than in their previous *locale*.

THE Ichthyological Museum now in course of formation at South Kensington has been lately enriched with further valuable specimens of fish. Amongst them are some prawns unique in size, measuring *twelve* inches long, which were presented by Mr. John S. Charles, of Lower Grosvenor Square.

THE *Scientific American*, in a recent issue, describes the tangent galvanometer constructed at Cornell University, from the designs of Mr. Anthony, the Professor of Physics, to meet the want of a standard instrument for the measurement of heavy currents, and for the direct calibration of the commercial instruments in use for measuring the currents employed in electric lighting, &c. For the measurement of heavy currents there are four circles, two 2 metres in diameter, and two 1·6 metres, mounted, according to Helmholtz's plan, at distances apart equal to their radii. The conductors forming these circles are copper rods, three-fourths of an inch in diameter. The needle is suspended by a silk fibre in a mass of copper, which serves as an effectual damper, and makes it possible to take readings very rapidly. By a peculiar arrangement of mirrors and telescope the deflections are read directly in angular measure on a circle 50 inches in diameter, to within three-tenths of a minute of arc. The copper conductors are mounted on a brass framework accurately turned and adjusted, and the dimensions are all known within one five-thousandth. For the measurement of small currents there are two circles, about 1·5 metres diameter, each having two conductors, and comprising altogether 72 turns of No. 12 copper wire.

THE indications of such an instrument of course depend upon the value of the horizontal intensity of the earth's magnetism, and without some means of determining this quantity in the place where the instrument stands, and at the time when a measurement is being made, no great accuracy is attainable. For making this determination, a coil a metre in diameter, consisting of 100 turns of No. 18 wire, is suspended, so that its centre coincides with the centre of the instrument by means of a single phosphor-bronze wire, which is itself attached to a torsion-head reading to ten seconds of arc. By the aid of this coil, observations may be taken at any moment for the determination of H by the method proposed by Sir William Thomson. The instrument is mounted in a copper building, from the construction of which all iron has been rigidly excluded. Several conducting wires connect the building with the dynamo and other rooms of the physical laboratory, 550 feet distant, and switches in the building serve to send the currents through the several coils of the galvanometer singly, in series, or in multiple arc, direct or reversed. By this means currents from 1 milliampere to 250 amperes can be accurately measured.

THE last number (Heft 33) of the *Mittheilungen der Deutschen Gesellschaft für Natur und Völkerkunde Ostasiens* contains a paper by Herr Hütterott on the Japanese sword, with numerous illustrations of the various forms. It describes the manner in which it is forged, how it reaches the extraordinary degree of excellence for which it is celebrated, in short the *technique* of the making of a Japanese sword in the feudal days. Herr Mayet concludes his account of a visit to Corea, the first part of which we have already noticed. Dr. Naumann, the director of the Geological Survey of Japan, and Japanese representative at the late Geological Congress at Berlin, communicated an extract from a report of his on the geological structure of the Japanese islands.

ONE-TENTH of the "Studentenschaft" at the Zurich University is now female. Twenty-nine young ladies study medicine, fourteen philosophy, and two political economy. Of the forty-five female students, fifteen are Swiss, and ten Russian.

THE after-sunglow has again at times been visible in Stockholm, from the middle of August to the middle of September, being distinct from the ordinary evening aurora.

A FURTHER telegram has just been received by the Russian Minister of War from Col. Prjevalsky, dated Osh, September 30—that is, the 12th inst., new style. Only the concluding passage has as yet been published by the Russian papers:—"August 14 (new style, August 26), Oasis of Tchira.—I have explored the Keria Mountains. We are now proceeding *viâ* Khoten and Aksu, and we shall arrive in Semiretchia towards the end of October. All is well."

THE spheroidal state of liquids has recently been made an object of study by Signor Luvini (*Il Nuovo Cimento*). A curious phenomenon was noticed when air was blown into the drop (to test the view that liquids in that state do not boil because they have lost their dissolved air). There arose bubbles often larger than the mass of liquid, and very persistent; they shared the movements of the drop and sometimes moved independently. Such bubbles were had in pure water, soapy water, alcohol, and ether, and would probably arise in all liquids. Sometimes they appeared only after the tube was withdrawn. Signor Luvini infers that liquids in the spheroidal state do not lose their dissolved air, or lose it very little. The author made arrangements for observing the spheroidal state under different air pressures, and he came to the conclusion that the temperature of each liquid in that state, under a given pressure, is very nearly equal to the least boiling temperature of the liquid under the same pressure.

WE have just received from the secretary, Mr. Charles Bailey, F.L.S., of Manchester, the reports of the Botanical Exchange Club for the years 1883 and 1884. For 1883 Mr. G. Nicholson acted as distributor, and 3735 specimens were received and divided out again amongst the members. In 1884 Mr. Arthur Bennett undertook the labour of distribution, and the number of specimens placed in circulation was 4371. The two reports contain a series of annotations by the distributors upon the more interesting plants which passed through their hands. For a considerable number of species new counties are registered. The most interesting additions to the British flora, of which they make mention, are a *Scutellaria*, intermediate between *minor* and *galericulata*, perhaps a hybrid, found by Mr. Nicholson in a place one would have thought likely to be thoroughly explored long ago—the shores of Virginia Water; *Potamogeton fluitans*, a pond-weed very difficult to recognise, found by Mr. A. Fryer in Huntingdonshire; and *Carex salina*, a boreal species known already in Scandinavia, Iceland, the Faroes, Nova Zembla, and North America, which has lately been discovered by Mr. Grant in Caithness. The Rubi of Britain want carefully comparing with those of the Continent, and Mr Arthur Bennett has done well to send the Club specimens to be verified by Dr. Foche, of Bremen, whose synopsis of the German Rubi has been taken lately by Hyman as a basis for his enumeration of the European forms in his most useful geographical conspectus of the European flora.

A CURIOUS calculation has been recently made by Signor Bartoli regarding the mean density of a body which should contain all the known elements in a solid state, either uncombined, or, if partly combined, each retaining the density belonging to it in the solid state. The author makes three suppositions—(1) the masses of all the substances equal; (2) masses such that the corresponding volumes are equal; (3) masses in ratio of the atomic weights. The corresponding mean densities he arrives at are 2·698, 7·027, and 5·776, and it is pointed out that the last value comes very near that got by Cavendish for the mean density of the earth, viz. 5·67; possibly an accidental agreement, yet interesting.

WE have received from Mr. Francis Day copies of two papers, on a subject on which he also read a paper at the Aberdeen meeting of the British Association. One is entitled "Notes on the Breeding of Salmonidæ," being observations on the fish cultural experiments being carried on at Howietown, and on experiments by the author himself at Cheltenham. The second, from the *Transactions* of the Linnean Society, is on the breeding of salmon from parents which have never visited the sea. This also describes the results of experiments at Howietown.

WE have received the report of the Council of the Leicester Literary and Philosophical Society for the past year. Various important additions have been made to the town museum; the work on the flora of Leicestershire, undertaken and edited by a botanical sub-committee, is now in the press, and will shortly be published; the resolution, adopted at the last general meeting of the society, for the promotion of science classes in the town has, owing to various circumstances, only been partially carried out. Two experimental classes, one for pure mathematics, the other for physiology, have been commenced, and have been attended with fair results. The reports of the various sections show a considerable amount of work done during the year.

Short abstracts of various papers read before the society are given in the *Transactions*.

A MISSION of thirteen youths, belonging to the best families in Cambodia, has arrived in Paris for the purpose of study. They have been placed under the care of M. Pavie, who has constructed a line of telegraphs between Siam and Cambodia. This is the first time since 1864 that Cambodians have come abroad for purposes of education.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii* ♂) from South Africa, presented by Mr. George E. Crisp; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Miss Ethel O'Donoghue; a Kinkajou (*Cercoleptes caudivolvulus*) from Demerara, presented by Mr. John Carder; four Common Squirrels (*Sciurus vulgaris*), six Common Dormice (*Muscardinus avellanarius*), British, presented by Mr. Thomas Weddle; a Tennant's Squirrel (*Sciurus tennanti*) from Ceylon, presented by Miss Maude Bovill; two Vulpine Squirrels (*Sciurus vulpinus*) from North America, presented by Capt. E. E. Vaill; a Coypu (*Myopotamus coypus*) from South America, presented by Mrs. Amelia Appleton; a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Sly Silurus (*Silurus glanis*), European, presented by the Marquis of Bath, F.Z.S.; a Red Lory (*Eos rubra*) from Moluccas, an Alexandrine Parrakeet (*Palæornis alexandri*) from India, deposited.

OUR ASTRONOMICAL COLUMN

PERIODICAL COMETS IN 1886.—Of the now somewhat numerous list of comets of short period, two will be due at perihelion in the ensuing year:—(1) The comet Tempel-Swift, or 1869 III. and 1880 IV., which is likely to return under circumstances that will render observations impracticable, so far at least as a judgment can be formed without actual calculation of the perturbations. (2) Winnecke's comet, last observed in 1875, its track in the heavens near the perihelion passage in December 1880 not allowing of the comet being seen at that return; the perturbabations may be very sensible during the present revolution: neglecting their effect, the mean motion determined by Prof. Oppolzer, for 1880, would bring the comet to perihelion again about August 24·5, under which condition its path would be as follows:—

	R.A. °	Decl. °	Distance from Earth
July 25·5	177·5	+10·2	1·17
Sept. 13·5	241·7	−24·9	0·98
23·5	246·1	−30·2	0·64
Oct. 3·5	264·8	−35·6	0·64
23·5	305·0	−36·0	0·77

The actual orbit of Winnecke's comet approaches very near to that of the planet Jupiter in heliocentric longitude 110°, at which point the comet arrives 720 days or 1·97 years before perihelion passage, the distance between the two orbits is then less than 0·06 of the earth's mean distance from the sun.

It is very possible, however, that the comet which may most interest astronomers in 1886 will be that observed in 1815, and known as Olbers' comet, which, according to the elaborate calculations of Dr. Ginzel, will again arrive at perihelion in December 1886. The most probable date that can be inferred from the observations of 1815, and the computation of planetary perturbations in the interval is December 16, but unfortunately the observations did not suffice to determine the mean motion in 1815 with precision, and consequently Ginzel found for the limits of the period of revolution 72·33 and 75·68 years, hence the comet may reach its perihelion many months earlier or later than the date given by calculation. Extensive sweeping ephemerides have been published, and it may not be too soon to direct attention to a search for the comet at the beginning of the next year, or as soon as the region in which its orbit is projected at the time can be advantageously examined.

A CATALOGUE OF 1000 SOUTHERN STARS.—Vol. iii. of "Publications of the Washburn Observatory" is to contain a catalogue of 1000 stars between 18° and 30° of south declination, formed by Rev. Father Hagen and Prof. Holden from the observations of Prof. Tacchini at Palermo during the years 1867–69, which were printed in the *Bulletino* of that observatory between April, 1867, and July, 1869, and with which Prof. Holden says he became acquainted through M. Houzeau's Vade-Mecum. The stars observed are from the 6th to the 9th magnitudes, and the magnitudes appear to have been very carefully noted, while it is remarked that the positions are excellent. They are reduced to the year 1850, but the mean epoch of observation of each star is appended. The copy before us is a reprint from the above-named volume. Tacchini's observations were made with the Palermo meridian circle fully described in the *Bulletino*.

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, NOVEMBER 1–7

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 1

Sun rises, 6h. 56m.; souths, 11h. 43m. 40·9s.; sets, 16h. 31m.; decl. on meridian, 14° 35′ S.: Sidereal Time at Sunset, 19h. 15m.

Moon (two days after Last Quarter) rises, 0h. 13m.; souths, 7h. 20m.; sets, 14h. 14m.; decl. on meridian, 9° 37′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	7 57	12 22	16 47	18 25 S.
Venus	11 11	14 46	18 21	25 50 S.
Mars	23 54*	7 13	14 32	14 20 N.
Jupiter	2 55	9 9	15 23	2 2 N.
Saturn	19 45*	3 53	12 1	22 18 N.

* Indicates that the rising is that of the preceding day.

Phenomena of Jupiter's Satellites

Nov.	h. m.		Nov.	h. m.	
1	6 48	II. occ. reap.	6	5 18	I. tr. egr.
5	5 1	I. ecl. disap.	7	2 39	I. occ. reap.
6	3 0	I. tr. ing.			

The Phenomena of Jupiter's Satellites are such as are visible at Greenwich.

Saturn, Nov. 1.—Outer major axis of outer ring = 44″·0; outer minor axis of outer ring = 18″·9; southern surface visible.

Nov.	h.	
1	4	Mars in conjunction with and 4° 16′ north of the Moon.
3	7	Mercury at greatest distance from the Sun.
3	9	Jupiter in conjunction with and 0° 52′ north of the Moon.
7	21	Mercury in conjunction with and 6° 16′ south of the Moon.

THE SCOTTISH METEOROLOGICAL SOCIETY

AT the annual meeting of this Society the Report of the Council stated that thirty-eight new members had been added to the Society during the year, and the membership now numbered 698. A new station had been established on the island of Fidra, at the mouth of the Firth of Forth, and that observations had been made for the Society at San Gorge, Central Uruguay. A large number of naturalists and others had availed themselves of the facilities for research offered by the Scottish Marine Station during the summer, there being thirteen working at the laboratories at the present time. Communications were now going on between the Council and several influential gentlemen in Glasgow, which it was hoped would result in the establishment of a permanent station for marine research on the Clyde. Mr. H. N. Dickson, of the Marine Station, communicated the results of experiments and observations which, during the past two months, he had been conducting at Granton, with the view of collecting data from which to determine the corrections to be applied to the readings of thermometers exposed in the ordinary Stevenson screen, in use in many places over the world. Having referred to the errors to which the ordinary screen gives rise, consequent on the varying atmospheric motion and radiation, he proceeded to say that his investigation was carried on chiefly by means of improved screens designed by Mr. John Aitken of Darroch, and that the dew points from the dry and wet bulbs by Glaisher's tables had been compared with those given by a new form of hygrometer designed by Prof. Chrystal of Edinburgh University. As regards Mr. Aitken's screen, in some a fan was introduced in order to secure a proper and uniform circulation of air for the thermometers in all weathers; others were simply sunshades; one consisted of two thermometers, one of which was partially blackened; and another of a thermometer having its bulb inclosed in a tight-fitting silver sheath, highly polished. The construction of Prof. Chrystal's hygrometer was explained and a brief account given of the results either already arrived at or suggested during the investigation, and it was intimated the inquiry was to be resumed at the Ben Nevis Observatory during August and September. At this Observatory, the climate of which offers unique facilities for the prosecution of such inquiries, an instrument of novel construction would be added, which had been designed by Prof. Tait for hygrometric research. Prof. Ewing, of Dundee, then described the arrangements which had been made for commencing the proposed earthquake observations on Ben Nevis this summer. The investigation was to include earthquakes proper; earth movements of so very delicate a kind as to be totally indistinguishable without some form of instrumental assistance, which are conveniently called earth tremors; and there were what might be named changes of the vertical, or those tiltings which the earth's surface seemed to be constantly undergoing. The different seismometers to be employed at the Observatory were then described, and in illustration some of the more striking peculiarities of the earthquakes of Japan were referred to.

PROF. KIESSLING'S INVESTIGATIONS INTO THE ORIGIN OF THE LATE SUNSET GLOWS[1]

THE interesting and important experimental demonstrations lately made by Prof. Kiessling of Hamburg to illustrate the artificial formation of all manner of sunset effects are probably well known to meteorologists in general. The September number of *Das Wetter* contains a valuable series of comparisons tending to show that the conditions under which artificial glows were produced have actually existed whenever the remarkable sunset effects have made themselves prominent. The following abstract may prove of interest to those who do not receive the paper itself.

With regard to the "after-glow," or re-illumination, he suggests two explanations as possible:—(*a*) Simple reflection of the refracted rays essential to the formation of the ordinary sunset-glow (the *first* glow); or (*b*) direct diffraction by a second homogeneous haze at much greater elevation. He considers, however, that the calculated heights of the latter place it out of the question. To the former there are only two important objections, the chief one being the slight polarisation, so far as the very scanty records indicate. The observations are, however, exceedingly deficient. Still, Prof. Kiessling has to allow that they do not tell in favour of the proposed explanation. The other difficulty is the *position* of the glow. It presupposes a mirror-like surface, parallel to the earth, with the intermediate space unusually transparent, conditions at first sight very improbable at the altitudes under consideration. But Prof. Kiessling's own experiments, detailed at the end of his paper on "Die Dämmerungserscheinungen im Jahre 1883," have shown the possibility. In these he obtained results most remarkably similar to those requiring explanation, and by methods reproducing in a striking manner the conditions considered actually to exist in the atmosphere.

A warm, moist stratum of air being produced in contact with a cold stratum the resulting haze along the contact surface formed the site of diffraction phenomena, approaching those actually observed in ordinary brilliant sunsets according to the fineness of the haze particles, and also reflections reproducing the "after-glow."

The almost constant saturation of the cold upper strata in winter is indicated by observations at high-level stations and the persistent upper haze. Let a warm [cyclonic] current come beneath such a layer, then the fine haze at the surface of contact will have beneath it the peculiarly transparent atmosphere common to such conditions and requisite for the transmission of the resulting diffraction (and reflection) phenomena. This should be found to exist in *all* brilliant sunsets, Prof. Kiessling stating the following law:—*An intense purple glow, visible over a considerable area, may occur when, in close proximity beneath a lofty and highly-attenuated haze, there is formed an extensive stratum of air at considerably higher temperature.*

Date of Sunset	Date of Observation (Roman Figures) and Difference of Temperature						
January 30, 1883	XXVIII. − 3·6	XXIX. + 2·7	XXX. + 4·3	XXXI. − 2·6			
February 11	X. − 2·0	XI. + 3·2	XII. + 0·1	XIII. + 6·1	XIV. + 0·5		
April 27 (at Grächen) and 28 ...	XXV. − 4·9	XXVI. − 2·1	XXVII. + 1·2	XXVIII. + 2·9	XXIX. + 0·7		
May 5 (warmer season)	III. − 3·4	IV. − 1·7	V. − 4·5	VI. − 4·1	VII. − 2·5		
But on Rigi (1790 metres)... ...	− 10·4	− 8·8	− 7·7	− 8·5	− 8·6		
Säntis (2467 metres)	− 14·1	− 13·4	− 13·1	− 11·3	− 12·1		
St. Bernard (2400 metres, about)	− 14·7	− 14·4	− 13·6	− 13·6	− 11·4		
September 20	XVIII. − 3·8	XIX. − 0·9	XX. + 5·5	XXI. − 2·0	XXII. − 0·2		
Rigi Culm	− 7·6	− 5·7	+ 1·2	− 6·5	− 7·0		
October 9 (Grächen) and 11	VII. − 1·9	VIII. + 1·4	IX. + 0·3	X. + 1·7	XI. + 6·5	XII. + 2·0	XIII. − 1·5
November 22 and 23	XXI. + 0·6	XXII. + 1·6	XXIII. + 4·4	XXIV. − 2·5			
November 29 and 30	XXVII. + 0·8	XXVIII. − 2·6	XXIX. − 1·6	XXX. + 2·6	I. + 5·2	II. − 2·4	
Rigi } glows generally over }	− 4·5	− 6·9	− 0·8	+ 3·7	+ 3·5	− 6·3	
Säntis } Europe. }	− 7·5	− 9·9	− 4·2	+ 2·3	− 0·7	− 9·7	
Pic du Midi (2859 metres)	− 13·3	− 7·2	+ 0·8	+ 3·8	− 2·8	− 13·3	

Although we cannot ever expect direct observations of temperature at the common surface producing the sunset glows, yet, as Prof. Kiessling shows, if we can prove that the warm under-current always accompanies sunset glows, the proof is practically complete. Such indications may be expected during the colder seasons in the form of abnormal vertical distribution of tempera-

[1] Ueber die Entstehung des zweiten Purpurlichtes und die Abhängigkeit der Dämmerungsfarben von Druck, Temperatur, und Feuchtigkeit der Luft *Das Wetter*, vol. ii. No. 9, p. 161.

ture, an *increase* instead of decrease at higher stations. He brings forward a long array of figures supporting this conclusion, especially for sunrise effects in 1883, as seen from Säntis (2467 metres), in North-East Switzerland, in the bend of the Rhine. Stations to the east—Munich (528 metres) and Hohen Peissenberg (994 metres) are taken for observations on temperature and relative humidity. The last place is about 35 miles south-west of Munich; both may be considered as beneath the sky region producing glows at Säntis. As *difference of temperature* is the most decisive comparison, his tables are here reduced to a series showing the difference of Hohen Peissenberg returns from Munich, in degrees Centigrade. In some cases one or two other returns are also added, reduced in like manner. *Normally*, allowing for difference of height, Hohen Peissenberg should register 2°·5 below Munich.

The final set of observations refer to some of the earlier afterglows. The greater anomaly with greater elevation (increases of 5°·2, 10°·6, 12°·2, and 17°·1 respectively in the figures given) is very suggestive. The reason of the non-agreement in May has already been stated.

Except the last, these observations refer to ordinary sunrise effects, but the only difference between them and the recent glows is considered to be that the latter occur by reflection at a higher level and in a more finely attenuated haze, thus giving the richer effects. The presence of such a haze with the glows was a matter of very common observation.

The question, of course, requires further consideration, especially with respect to observations of the recent glows. Besides this connection with a warm stratum of air, Prof. Kiessling finds another, almost as general, with barometric maxima, as was noticed with the similar phenomena in 1881.

Referring, in his concluding paragraph, to the connection of the glows with the Krakatoa eruption, Prof. Kiessling writes that the thousand or so records of their geographical distribution, now in his hands, "show a perfectly continuous spread of the anomalous glows, and of the diffraction phenomena of Bishop's Ring dating from August 26, 1883, and spreading from the Straits of Sunda as a centre over the tropical and temperate zones."

J. EDMUND CLARK

A CENTURY OF SCIENCE IN BENGAL

IT was a happy idea of the Council of the Asiatic Society of Bengal to commemorate the completion of a century of the Society's existence by publishing a review of the progress made and the services rendered to knowledge by the institution.[1] The idea of a learned society composed of Europeans in India studying the country and communicating to each other at periodical meetings the results of their researches, arose first in the fertile brain of Sir William Jones, who was judge in the Supreme Court at Fort William, and who delivered, on January 15, 1784, to about thirty members of the European community of Calcutta, a "Discourse on the Institution of a Society for Inquiring into the History, Civil and Natural, the Antiquities, Arts, Sciences, and Literature of Asia." As a result of this discourse, the "Asiatick Society," the parent of all such societies, was founded. Its motto, which is taken from Sir William Jones's discourse here referred to, is this: "The bounds of its investigations will be the geographical limits of Asia, and within these limits its inquiries will be extended to whatever is performed by man or produced by nature." After many vicissitudes it has just completed its hundredth year, and the record of its work forms the large volume just mentioned. This is divided into three parts: first, a history of the Society, by Dr. Mitra; its work in archæology, history, and literature, by Dr. Hœrnle; and the work in natural science, by Baboo P. N. Bose. The change which has come over the face of India in the course of a century could hardly be better marked than by the fact that two out of the three parts into which the volume is divided—one of these being on natural science—are written by native gentlemen. In the history of the Society we notice that in 1808 a resolution was proposed by Dr. Hare and seconded by Dr. Leyden (frequently referred to in Lockhart's "Life of Scott"), "that a Committee be appointed for the purpose of physical investigations, the collection of facts, specimens, and correspondence with individuals whose situations in this country may be favourable for such discussions and investigations." It was then agreed to provide two committees—one for science, the other for literature; twenty years later, in 1828, a committee was appointed "to promote geological researches, working under the rules then in force for the Physical Committee," and at the same time the published *Transactions* of the Society were divided into two parts, one devoted to physical, the other to literary subjects. Nearly twenty years later the whole of the work of the Society was delegated to six committees, one having charge of zoology and natural history, another of geology and mineralogy, and a third of meteorology and physics. The establishment of a museum did not occur to the founder, but curiosities were constantly coming in from members, and in 1796 it was proposed to give these a suitable house. In 1814 Dr. Wallich proposed the formation of a museum, and offered duplicates from his own collections, as well as his services in arranging it, and a museum was accordingly started. The story of the growth of the various sections of the Natural History Museum is told by Dr. Mitra. On the whole it is one of great progress, although financial difficulties beset the museum at first. But as soon as the Society became able to pay for scientific curators all went well. In 1865 the Society's zoological, geological, and archæological collections were made over to the Government of India for the public museum in Calcutta. A writer in the *Calcutta Review*, speaking of the Society's exertions for the establishment of the national museum, said: "Had it done nothing else to promote science during the last ten years, it would have entitled itself to the gratitude of posterity for the vigour with which it has prosecuted to success a project fraught with so much public usefulness." The earlier volumes of the Society's *Transactions*, published under the title "Asiatick Researches," created a sensation in the literary and scientific world in Europe. A French translation was speedily published, with notes on the scientific portions by no lesser hands than Cuvier, Lamarck, Delambre, and Olivier. Of the work of the Society in preserving Sanskrit MSS., in translating and publishing various works from the native languages, and other valuable services to literature, Dr. Mitra speaks at length. Amongst the publications, apart from the papers, we notice many of scientific interest, such as catalogues of various sections of the museum, of the mammals and birds of Burmah, of Indian lepidoptera, besides translations of numerous works of Hindoo science. In summing up at the conclusion of his historical sketch the benefits conferred on India and the world by the Society during its hundred years of existence, Dr. Mitra sums up its scientific work (apart from papers, and published volumes above referred to) thus: "It got up an archæological and ethnological museum of considerable extent, a geological museum rich in meteorites and Indian fossils, and a zoological museum all but complete as regards the avifauna of India."

The long review of the work of the Society in natural science is, as already mentioned, written by Baboo Bose. His method is to take the various branches of science in succession, such as mathematical and physical science, geology, zoology, botany, geography, ethnology, and chemistry, and to describe under sub-heads the papers on these subjects contributed to the *Transactions* of the Society, together with a brief biographical sketch of the more celebrated or prolific authors. At the end we get a classified index of all the scientific papers, an alphabetical list according to the author's names being given at the conclusion of the first part. Amongst the latter we notice many whose names are familiar as contributors to NATURE. In the early years of the Society, and down to 1828, the scientific contributions to the Society's *Proceedings* were almost wholly connected with some branch of pure or mixed mathematics, for most of the men who went out to India, especially in the scientific branches of the military service, had been well grounded in this subject. The section on the investigations into the mathematical science of the Hindoos is of great interest. Sir William Jones put before the Society from the outset the object of studying these sciences, and he set the example himself, but the initial difficulty was to find any native capable of assisting him. Baboo Bose records that, although ample stipends were offered by Sir William Jones to any Hindoo astronomer who could name in Sanskrit all the constellations which he would point out, and to any Hindoo physician who could bring him all the plants mentioned in Sanskrit books, he was assured by the Brahmans whom he had commissioned to search for such instructors, that no Pundit in Bengal even pretended to possess the knowledge he required. Geology and mineralogy flourished in the Society from the commencement, while zoology was at first unduly depressed and discouraged owing

[1] "Centenary Review of the Asiatic Society of Bengal, 1784 to 1883." Published by the Society, Calcutta. Thacker, Spink, and Co., 1885.

to the aversion of Sir William Jones to zoological studies, and it was only about 1828 that the papers of Dr. Falconer, Col. Tickell, and others began to occupy an important position on behalf of zoology in the Society's transactions. With Indian botany, geography, and ethnology are connected many names of world-wide fame. With regard to chemistry, it may be said practically there is no chemical research in the Society's publications. Chemistry, as Baboo Bose explains, can only be studied in the laboratory, and until recently India had but few laboratories, and few competent men with leisure to devote to the subject. A curious statement, by the way, creeps into the account of Mr. Piddington, who studied Indian storms, and gave an account of every cyclone in the East between 1839 and 1851. Baboo Bose says his experience was most varied, and then quotes the following from some unnamed source :—"He was one of the few who escaped from the massacre of Amboyna." Now, as the massacre of Englishmen by the Dutch Governor of Amboyna took place in 1622, Mr. Piddington, if he was observing storms in India in 1850, could hardly have been in the Eastern Archipelago two centuries and a quarter previously. Many other portions of this volume, such as the chapters on coins, on ancient Indian alphabets, on the study of the languages and literature of India, and on the study of Indian antiquities, are of deep interest, but we have confined ourselves to the chapters on natural science.

The dominant feeling produced by an examination of this volume is one of satisfaction that so much has been done by this single society towards investigating the past and the present of (or, in the words of Sir William Jones, "man and nature in") our great dependency. For the most part this has been done by private individuals, but on more than one critical occasion the directors of the East India Company, in accordance with their generous traditions, came to the aid of the Society with large contributions; otherwise there appeared no way out of the difficulty except the dissolution of the Society and the abandonment of the works in which they were engaged. If this were the place it would be interesting to compare this method of practically leaving everything to private initiative, with that adopted by the French in Indo-China, of the Government undertaking a series of literary, artistic, and scientific investigations through competent specialists into a new possession. Notwithstanding the great and marked success of the Asiatic Society of Bengal, the French plan has advantages which cannot be overlooked.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Among the more noticeable Natural Science Courses this term are Prof. Dewar's on Dissociation and Thermal Chemistry; Prof. Newton's, on Evolution in the Animal Kingdom; Dr. Gadow's on Human Embryology; Dr. Vines's, on the Physiology of Plants; and Prof. Macalister's, on the Peripheral Nervous System.

Prof. Hughes is lecturing on Methods of Geological Surveying; Dr. R. D. Roberts, on Principles of Geology; Mr. Marr, on Elementary Stratigraphy; Mr. T. Roberts, on Palæontology; and Mr. Hawker, on Elementary Petrology; all at the Woodwardian Museum.

Prof. Roy is lecturing on General Pathology, and also conducting a Practical Course in Morbid Anatomy and Histology.

Prof. Stokes is lecturing on Hydrodynamics; Prof. Cayley, on Higher Algebra; Prof. Darwin, on Orbits and Perturbations; Mr. Glazebrook, on Waves and Sound; Mr. Hobson, on Planetary Theory; Mr. Macaulay, on Theory of Structures; and Mr. Forsyth on Abel's Theorem. Numerous other courses on higher mathematics, open to the University, are being given by college lecturers.

We are glad to notice that Mr. A. Sheridan Lea, M.A., Lecturer on Physiology, and formerly Scholar, of Trinity College, has been elected to a Fellowship at Gonville and Caius College. Mr. Lea's work in connection with Prof. Michael Foster's "Text-Book of Physiology" is well known. Mr. Lea was placed in the First Class in the Natural Science Tripos in 1875, and has since been continuously engaged in the University teaching of Physiology.

Dr. S. Richemann has been appointed assistant to Prof. Dewar, Jacksonian Professor.

Messrs. E. W. Hobson and A. R. Forsyth are appointed Moderators, and Mr. C. H. Prior Examiner, for the next Mathematical Tripos.

King's College offers a Vintner Exhibition of 70*l*. per annum for Natural Science. The examination begins on December 10.

St. John's College offers several scholarships, exhibitions, and sizarships for competition on December 10. Candidates may offer any of the subjects of the Natural Sciences Tripos except Mineralogy, and may be elected on the ground of special proficiency in one only. Particulars will be furnished by the tutors.

A joint examination for Natural Science Scholarships at Emmanuel, Christ's, and Sidney Sussex Colleges will be held on January 5, 1886, and following days. The subjects are Chemistry, Physics, Elementary Biology, Geology, and Mineralogy. Further particulars will be given by the tutors of either college.

Out of the 875 freshmen whose names have appeared in the preliminary lists, about 104 have announced their intention of studying medicine in the University. A few more may be added when the results of the October Previous Examination are known. The Anatomy School is attended by over 130 students, for whom an exceptionally abundant supply of dissecting material is in hand. The Demonstration Lectures have to be repeated from lack of room; indeed, the necessity for increased accommodation in this department is becoming extremely urgent.

LONDON.—We have received a circular stating that "In view of the adjourned extraordinary meeting of Convocation (of London University) to be held on Tuesday, November 3, a number of graduates met on Wednesday last to consider the proposed scheme for the establishment of a Teaching University for London. As the result of their deliberations it was thought desirable that attention should be called to some of the more striking objections to the proposed scheme; and that, having regard to the grave importance of the questions to be submitted to the members of Convocation affecting the very existence of the University as at present constituted, they should be especially requested to attend on Tuesday next, and to give their support to Mr. Bone's amendment, to *receive* the report submitted by Lord Justice Fry, without *adopting it* '*en bloc*.' Should this amendment be carried, the following resolutions, expressing what is believed to be the feeling of the majority of the graduates, will be moved:—(1) 'That Convocation, whilst affirming the general principles of the desirableness of bringing the teachers and the examiners of the University into closer relationship with one another and with the Senate, and of modifying the constitution of the Senate in accordance with the previous recommendations of Convocation, and without giving to the teachers an undue share of representation on the governing body of the University, refers back the scheme to the Special Committee for further consideration.' (2) 'That the number of members on the Special Committee be increased by one-half.'"

SOCIETIES AND ACADEMIES

SYDNEY

Linnean Society of New South Wales, July 29.—The following papers were read:—A monograph of the Australian sponges, part 5, the Auleninæ, by R. von Lendenfeld, Ph.D. Several sponges from various localities in the Australian region have been included by the author in this new sub-family, the members of which are characterised by a very peculiar structure not met with in any other sponges. The new sub-family *Auleninæ* is placed in the family Spongidæ, and consists of the two new genera Aulena and Halme, with three species in all. The anatomy and histology of these is accurately described and illustrated by numerous plates. The Auleninæ form honeycombed or complicated reticulate structures; the cavities form a kind of vestibule and are simple in Halme, where an outer lamella surrounds the whole sponge, or subdivided into numerous small compartments, as in Aulena, where no outer lamella exists. Into the system of Vestibule-Lacunæ both the inhalent and the exhalent canals of the sponge open. The skeleton of Halme is composed of thick main fibres rich in sand, thin, simple and clean connecting fibres, and a hard cortex of sand cemented with spongiolin. The skeleton of Aulena is very peculiar. It consists of a regular network of fine horny threads in the joining points of which large sand grains are found. In the membranes of the Vestibule-Lacunæ of this genus nervous elements, sensitive and ganglia cells have been discovered by the author. These and many other histological details are described in the paper, which dwells also on the morphological significance of these interesting new sponges.—On a sponge destructive to oyster-culture in the Clarence River, by R. von Lendenfeld, Ph.D. In this paper the author describes a new sponge, *Chalinula coxii*, which appeared some years ago on certain oyster beds in the Clarence River, and destroyed some of them completely.—Note on the Glacial period in Australia, by R. von Lendenfeld, Ph.D. The author draws attention to some further evidence of ice action in the Mount Lofty group near Adelaide, where some glacier-polished Siluro-Devonian rocks, with very well preserved striæ, have been discovered and photographed.—Jottings from the biological laboratory of Sydney University, by William A. Haswell, M.A., B.Sc., F.L.S., &c., Lecturer on Zoology and Comparative Anatomy. This paper contains (1) some notes on an Australian species of Bonellia, which seems scarcely to differ from the European species, *Bonellia viridis;* and (2) some observations on aquatic respiration in fresh-water turtles.—On the supposed Glacial epoch in Australia, by Capt. F. W. Hutton, F.G.S., &c. The author discusses the phenomena which have been adduced as evidence for the former existence of a Glacial epoch in Australia, and shows that they are susceptible of a different interpretation. He distinguishes between a Glacier epoch, such as has occurred in New Zealand, in which, owing to various local, but only local, causes, ice-fields prevailed over much larger districts than at present, and a Glacial epoch, such as has been demonstrated in the Northern Hemisphere, which is the result not of variations caused and limited by local circumstances, but of alterations universal or cosmical in character. The Glacier epoch in New Zealand is regarded as anterior to the Glacial epoch of the North.

PARIS

Academy of Sciences, October 19.—M. Bouley, President, in the chair.—Remarks on the 209th volume of the "Connaissance des Temps" for the year 1881, presented to the Academy on behalf of the Bureau des Longitudes, by M. Faye.—Note on the establishment of a laboratory in the Institute for the measurement of the photographic plates obtained during the transit of Venus in 1882, by M. Bouquet de la Grye. Arrangements have been made, by means of which it is hoped that the calculations and measurements relating to 700 plates will be completed in fifteen months.—Note on the Dinoceratidæ discovered by Mr. Marsh in the Eocene formations of Wyoming, United States, by M. Albert Gaudry. These huge pachyderms, which seem most to resemble the European Coryphodon described by M. Hébert, are specially remarkable for the characteristic horny protuberances on the frontal region, whence their name (δεινός, terrible, and κέρας, horn). The skull is also distinguished by its small size, in this respect resembling that of a reptile, as well as of several other mammals of the Lower Tertiary epoch.—On the birational geometrical transformations of the *n* order, by M. de Jonquières.—Note on the fifth part of the topographical map of Algeria, presented to the Academy on behalf of the Minister of War, by Col. Perrier. This part comprises the six divisions of Jebel-Filfila, Bone, Wed-Guergur with Cape Rosa, Menerville, Medeah and Mostaganem to the scale of 1 : 50,000.—Note on the sub-lacustrine ravines of glacial streams, by M. F. A. Forel. During his recent surveys of Lakes Constance and Geneva, M. Hörnlimann has discovered that both the Rhine and the Rhone continue their course under the lacustrine waters through deep ravines excavated beneath the respective submerged deltas. That of the Rhine has been traced for a distance of four kilometres and to a depth of 125 metres below the lake, while that of the Rhone may be followed for over six kilometres from the mouth of the river with a depth varying from 200 to 230 metres.—On the origin and classification of meteorites, by M. Stanislas Meunier. The author discusses the objections urged against his views on the nature and classification of meteoric bodies, by M. Brezina in the "Meteoritensammlung des Mineralogischen Hofkabinets in Wien," Vienna, 1885.—On the latitude of the observatory of Bordeaux, by M. G. Rayet. The mean latitude of this establishment, whose longitude was determined in 1881 at 11m. 26·444s. W., is found to be 44° 50′ 77″·23.—On the integrals of total differentials of the second species, by M. E. Picard.—Questions relating to a bundle of plane cubic figures, by M. P. H. Schoute.—On the torsion of prisms, by M. Marcel Brillouin.—Description of a new apparatus for measuring electric currents (one illustration), by M. F. de Lalande. This apparatus, for which the name of "electric areometer" is proposed, dispenses with a permanent magnet, the source of so much error in other appliances, is highly sensitive and practically unaffected by changes of temperature, while its readings are unmodified by the neighbourhood of metallic bodies or even of powerful magnets.—On the theory of the transmitting electromagnetic telephone, by M. E. Mercadier.—Note on the electrolysis of the salts, by M. Ad. Renard.—Combination of the neutral carbonate of magnesia with the bicarbonate of potassa, by M. R. Engel.—On the adulteration of olive oil intended for consumption by the addition of sesame cotton and other oils extracted from seeds, by M. A. Audoynaud. The bichromate of potassa and nitro-sulphuric acid are proposed as reagents for determining the presence of these substances.—On certain peculiarities in the development of the teeth of the cachalot (spermaceti whale), by M. G. Pouchet.—On the process of development of *Epicauta verticalis*, by M. H. Beauregard.—On the part supposed to be played by the living tissues of wood in the ascension of the sap in large plants, by M. J. Vesque. The author contests the opinion of those physiologists who hold that it is impossible to explain by the aid of purely physical forces the ascension of water in plants over 10 metres high.—On a waterspout observed at Shanghai on August 21, by M. Martial.—Account of the same waterspout, by M. Marc Dechevrens.—Description of M. Buisson's new rifle, by Gen. Favé. For this weapon it is claimed that it can be fired from five to ten times in a minute by troops charging the enemy without stopping an instant to re-load. As many as a hundred rounds may be fired off in this way.

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