THURSDAY, NOVEMBER 24, 1887.

CHARLES DARWIN.

The Life and Letters of Charles Darwin, including an Autobiographical Chapter. Edited by his Son, Francis Darwin. In Three Volumes. (London : John Murray, 1887.)

TO write a biography is a task which is almost a proverb for difficulty. It is no easier for a relative than for a stranger, because, if a more intimate knowledge of the details lightens the labour, affection is apt to warp the judgment, and checks perfect freedom of expression. In the biography, however, of Charles Darwin, there was no temptation to reticence, no need for firmness. His was a life, simple, noble, blameless. Still, this very simplicity and unostentatious rectitude presented their own difficulties. After the long and interesting voyage in early manhood, it was a life singularly uneventful, a life of patient labour, one long struggle against sickness. Thus its record when written might readily have been unexceptionable, but dull.

This cannot be said of the Life of Charles Darwin. It will take its place, I venture to predict, with Boswell's Life of Johnson, Lockhart's Life of Scott, Stanley's Life of Arnold, and the comparatively small number of biographies which have attained to first-class rank in literature. Mr. Francis Darwin has made excellent use of the materials at his disposal. These were considerable. They consisted of a short sketch written in his later years by Charles Darwin himself, for the information of his family, and of a large number of letters. Mr. Francis Darwin had, in addition, the special advantage of having shared in the labours of his father during the last eight years of his life.

The chapters written by the editor are of the highest interest and value, but as far as possible the story is told by Charles Darwin himself; the letters being merely strung together by occasional explanatory paragraphs, which form a connecting thread. Selection must have been no easy task, but it has been well done, and numerous as are the letters and large as is the book one almost wishes they had been more and it had been larger. Yet Darwin was hardly what most people would call a good letter-writer. His letters were often written hurriedly, and bear the marks of hasty composition; but there is not seldom a terseness of phrase, and always a vigour of expression, which makes them peculiarly attractive. These letters, too, are thoroughly characteristic of the man. They breathe the quenchless energy, the "dogged" endurance, the hidden tenderness, the sweet reasonableness, the imperturbable equanimity, of his nature; and they show, on rare occasions, that capacity for indignation without which a character so amiable might have degenerated into weakness.

The autobiographical sketch tells us the particulars of Charles Darwin's early life. Born at Shrewsbury in 1809, the son of a physician in large practice, and grandson of the well-known Dr. Erasmus Darwin, author of "Zoonomia," Charles Darwin was educated at the grammar-school of that town, under Dr. Butler, one of its most noted head masters. Neither in childhood nor in boyhood does he appear to have given promise of exceptional powers, though the taste for collecting manifested itself at a very early age, and he was obviously more thoughtful and determined to understand things thoroughly than the average boy. But the school system of education which, as usual then, was wholly classical, did nothing to bring out the special powers of his mind. Indeed, he was once even rebuked by the head master for wasting his time on such a useless subject as chemistry. He passed, in short, through Shrewsbury school as a well-conducted boy of ordinary ability, perhaps a little below the average. "A good lad, but not quick or particularly studious," would probably have been the verdict of his masters.

After leaving Shrewsbury, Darwin studied for a couple of years at the University of Edinburgh, attending medical lectures, with the idea of adopting that profession. But for the surgical side of it he already felt a disinclination; and unfortunately for him, as he relates, the dreariness of the lectures on anatomy indirectly deterred him from the practice of dissection, which would have been a useful training for his later life. He made, however, friends, who aided in developing his love for natural history, though he tells us that the dullness of the geological lectures produced in him "the determination never as long as I lived to read a book in geology, or in any way to study the science." A resolve, happily, before long rescinded.

From Edinburgh, Charles Darwin went to Cambridge. Here he entered at Christ's College, where his elder brother, Erasmus, was already a student. There was then an idea that, as he clearly had no strong taste for medicine, he should be ordained. As he says, "Considering how fiercely I have been attacked by the orthodox, it seems ludicrous that I once intended to be a clergyman." Darwin had at no time of his life any tendency to superstition, otherwise one might observe that extremes in religious opinion are not so wide apart as the remark just quoted seems to imply. At the same time he tells us that a German phrenologist had declared that he "had the bump of reverence developed enough for ten priests."

He brought little classical knowledge from Shrewsbury, and left much of that behind at Edinburgh; he had no taste for mathematics, and natural science was not then recognized in the curriculum at Cambridge. So he read little, and took an ordinary degree. Thus he doubtless appeared to be wasting his time, and accuses himself of so doing. But one can see that the groundwork was being laid for the future. He acquired friends, some of like tastes; his interest in natural history increased, and was developed by the opportunities which the district afforded, especially for collecting beetles, then the ruling passion. His health, too, at this time appears to have been good; he was an active pedestrian and a keen sportsman, enjoying society, and not without a love of music.

But the friendship of Henslow was probably the greatest boon which he owed to Cambridge. Acquaintance soon ripened into steadfast friendship, and the wider knowledge and formed habits of the older man produced, as Darwin gratefully admits, the best possible influence on the younger. Through Henslow also, shortly after Darwin had taken his degree, the offer was made to join the *Beagle* as naturalist, which may fairly be called the turning-point of his life. It is interesting to see how evenly balanced the reasons for and against acceptance then seemed to be, and how nearly the offer was refused. He wished to go, but Dr. Darwin, his father, for various reasons—among them the fear that so long a voyage would unsettle his son for life—was opposed to the plan. Chiefly through the influence of Darwin's maternal uncle, Mr. Josiah Wedgwood, the father's objections were overcome. Capt. Fitzroy, however, who was a disciple of Lavater, was nearly refusing his services because of the shape of his nose, which was not sufficiently indicative of determination and energy ! Considerable delay arose from one cause or another, but the vessel finally sailed from Plymouth on December 27, 1831.

Though Henslow was Professor of Botany, it was impossible to know him without being infected with geology ; so by this time the resolution against that science had been rescinded, and Darwin had even accompanied Sedgwick on one of his journeys in North Wales. The tale of the work during the voyage of the *Beagle* has been told in the well-known volumes ; but we have here a series of letters which record many incidents of the journey, and indicate the development of the writer's mental powers and the thoughts which were already beginning to quicken into life. It is amusing to read that now the new love is sometimes stronger than the old. "But geology carries the day ; it is like the pleasure of gambling. Speculating on first arriving what the rocks may be, I often mentally cry out, 'Three to one Tertiary against Primitive,' but the latter have hitherto won all the bets." Later there is a conflict even in his geological preferences. "I am quite charmed with geology, but, like the wise animal between two bundles of hay, I do not know which to like the best, the old crystalline group of rocks or the softer and fossiliferous beds." But notwithstanding these mental "lovers' quarrels," notwithstanding the serious drawback of incessant suffering from sea-sickness, and one grave illness of an unknown nature, a large number of specimens and a wonderful store of observations were accumulated in almost every department of natural history, which served as the foundation for the great superstructure to which his life was devoted.

On Darwin's return to England in 1836, he oscillated for a time between Cambridge and London, working at the results of his voyage as hard as his health, now seriously impaired, permitted. In 1839 he married, and after residing in Gower Street till the autumn of 1842, moved, in the hope of benefit from country air, to a house which he purchased at Down, in Kent, and in which the remainder of his life was spent.

Here, after the immediate fruits of the *Beagle* voyage were given to the world, he began to develop the great idea of which the germ had been sown and quickened during his wanderings. One chapter describes how "the foundations of the Origin of Species were laid" between the years 1837 and 1844 ; another narrates its growth. In 1856, partly in consequence of Lyell's advice, the book was begun for which such long preparation had been made, and by the month of June 1858 about one-half was written. It was, however, on a much greater scale than that which has now become classic in England,—"three or four times as extensive." Then suddenly all was changed by the receipt of a manuscript from Mr. Wallace, from his distant sphere of work in the Malay Archipelago, which coincided so exactly with his own views that, as Darwin writes to Lyell, "if Wallace had my manuscript sketch written out in 1842, he could not have made a better short abstract." The story of this interesting episode, so honourable in every respect to all concerned, is told, chiefly by means of letters, in one chapter of the book. In these days, when too often the stream of scientific life is ruffled by miserable personal squabbles about priority in some trifling discovery, it is well to learn how men acted whose hearts were as large as their intellects were great.

The result of the simultaneous announcement of the hypothesis thus independently framed was that the plan of the larger work was abandoned, and the "Origin of Species by means of Natural Selection," or "An Abstract of an Essay on the Origin of Species through Natural Selection," as the author would have preferred to call it, was published in 1859. It is needless to epitomize the story of its reception by the public—of the opposition which it encountered—of the storm which it aroused—of its ultimate triumph : all this is admirably told by Prof. Huxley in a chapter which he has contributed to the work.

Of all the accusations brought against Darwin, perhaps the most unreasonable was the frequent one that he had "abandoned the true Baconian method." I do not profess to be very familiar with the philosophy of Bacon ; but if accumulating a mass of facts, co-ordinating them, and then drawing inductions, is not the true method of science, I do not know of any other ; and this method inspires every chapter of the "Origin of Species."

In the correspondence which was written during the remainder of the author's life, occupying nearly half the work, we read of how the "Origin of Species" won its way, edition following edition, and of the series of later works and occasional papers which continued till within a few months of the end. This came rather suddenly, though in the fullness of years. For the last ten years of his life his health had appeared somewhat better than formerly, but in the early months of 1882 it gave repeated cause for alarm, and at last, on April 19, after a brief period of suffering, he ceased from that labour which only sickness had ever made a burden.

The quantity of work which he had accomplished is astonishing when its quality—always the best that could be done by the man—is considered. True, Darwin, though only to be called wealthy towards the end of his life, was always free from the necessity of bread-winning. But then—and what a terrible set-off this implies—"for nearly forty years he never knew one day of the health of ordinary men, and thus his life was one long struggle against the weariness and strain of sickness."

In one respect Darwin was *felix opportunitate vitæ*. He lived before scientific literature had attained its present overwhelming proportions. It is charming to read such a passage as this : "Geology is a capital science to begin, as it requires nothing but a little reading, thinking, and hammering." We might add, "with the mind of a Darwin,"—at least to get such wonderful results as in the "Geological Observations." If anyone of the present day is getting proud of what he may have done in petrology, I would prescribe Part I. of that work as a corrective. But if now we learn much from others, and gain much from the perfection of our means of research, we are apt to lose in independence and vigour of mind, to say nothing of the time which is wasted in the weary wading through piles of periodicals, often with but little fruit as the result. Mathematicians know that solving problems gives a strength to the mind which cannot be obtained from the most careful study of book-work, and I have often ventured to think that to write the section on the "literature of the subject" as the last stage of a research is not so much to "put the cart before the horse" as it seems. Something, too, may be lost through the very perfection of the means of research in natural history: the mind may be tempted to dwell too much on details; and the over-careful study of these may lead men to miss the greater principles. Darwin was an observer, precise and minute as any, but it is interesting to note that he was always guided by a selective principle.

The greatest charm of the "Life" is that it draws so vivid a picture of the man himself—partly from the unconscious self-portraiture of his letters, partly from the tender touch of his son's hand, aided by the loving memories of other members of his family. Before us rises that tall, slightly stooping form, either walking with swinging though often feeble step, cloaked and staff in hand, along the "sand walk," or seated or reclining in that study which bore silent testimony to the orderly habits learnt in the tiny cabin of the *Beagle*; we see that massive forehead, those keen yet kindly eyes, shadowed by those overhanging brows, the sparse gray hair, the long gray beard, that winning smile which lit up those rugged features; we hear once more the kindly voice; but better still, there rises, fresh and ever instructive, the memory not only of one of the grandest intellects, but also of one of the noblest and truest natures, among the sons of men. Unruffled by carping criticism and virulent abuse, in silent dignity Charles Darwin laboured on, in the quiet consciousness of strength and the conviction that truth would at last prevail. No one can read the life of Darwin without feeling as if some healthful air from a better world had braced his moral fibre and nerved him for more earnest and more unselfish work.

Truly the last scene of all was a "Great Lesson." His family would have laid him in the quiet churchyard near his own home, but his fellow-workers in science desired and obtained that his grave should be made in Westminster Abbey. Some quarter of a century before that day many thoughtful men hesitated in accepting, or even opposed, the views which he had maintained; while the camp-followers and swash-bucklers of the religious world had discharged at him their volleys of vituperation. The one had been for the most part persuaded; the other had slunk away to growl in obscurity. Now, around that grave in the Valhalla of Britain, were gathered the leaders in literature and science, men of every rank in life, of every form of creed—from the most sincere Christian to the no less sincere Agnostic. Time had shown that there was no necessary opposition between the inductions of science and those deeper aspirations and beliefs upon which we must not here touch, and men who on such points felt deeply but differently from Charles Darwin came no less willingly than others to pay the last honours to one who was not only a great philosopher but also emphatically a good man. T. G. BONNEY.

OUR BOOK SHELF.

A Treatise on the Integral Calculus. Part I. Containing an Elementary Account of Elliptic Integrals and Applications to Plane Curves, with numerous Examples. By Ralph A. Roberts, M.A. (Dublin: Hodges, 1887.)

MOST students, on taking up this book, will be disposed to ask, "Is there any room or necessity for another work on the Calculus just now? Is not Williamson up to date?" Mr. Roberts gives no sign, and so we are led to search out for ourselves a reason for the existence of the work, and a justification of the same. In his two previous books our author makes great use of elliptic functions, and a chapter is devoted to the discussion of them in the book before us, and, further, this fact is prominently noted on the title-page; hence we conclude that Mr. Roberts has had in view mainly the treatment of these integrals, and to make his treatise self-sufficient he has surrounded this special subject with such preliminaries and accessories as he deems suitable for the elucidation of his theme. The author has produced a capital book, for he writes with extreme care, and full knowledge and command of his subject. There appears to us to be in many parts a novel treatment—*i.e.* considering the matter in the light of English treatises on the Calculus—and there is copious illustration. There is large opportunity for practice afforded by the numerous examples inserted in the body of the work, and also at the end. Many of these are not intended, or at any rate are not suitable, for babes; they are strong meat for adults. There is an index and the usual table of contents.

Solutions to Problems contained in a Treatise on Plane Co-ordinate Geometry. By I. Todhunter, F.R.S. Edited by C. W. Bourne, M.A. (London: Macmillan and Co., 1887.)

THIS is not a work brought out with a rush, for the greater portion of the solutions were drawn up by the author fifteen years ago. To students using the text-book this will be a valuable companion, for Mr. Bourne has executed his task with care and ability. Geometrical as well as analytical solutions are given, and impart a pleasant feature to the book. For Mr. Bourne's sake we regret that the foundation is giving way, as few students now read the "Conics," for that fate is befalling it which the author himself says is "the fate of all academical text-books," viz. obscurity ("W. Whewell," vol. i. p. 24). Todhunter's own views respecting "Printed Solutions" are given in his "Essays" (p. 81). The exercises, however, will retain their utility as tests for ascertaining a pupil's grasp of the subject, in spite of the decay of the setting, and the "Solutions" we can recommend to students "after a vigorous effort has been made to obtain the solution without the book."

Lectures on Bacteria. By A. De Bary. Second Improved Edition. Authorized Translation by Henry E. F. Garnsey. Revised by I. B. Balfour, F.R.S. (Oxford: Clarendon Press, 1887.)

THIS work is in the main an abridgment of a number of lectures, some of which were delivered in a connected series as a University course, others as occasional and separate addresses. The author's aim is to set forth the present state of our knowledge respecting the objects included under the name of Bacteria. Having dealt with cell-forms, cell-unions, and cell-groupings, he describes the course of development of Bacteria, and then proceeds to discuss questions as to the position of Bacteria in the organic world, and as to their origin and distribution. A chapter on vegetative processes is followed by one on the relation of Bacteria to, and their effect upon, their substratum; and this leads to an account of the forms which excite fermentation, and of parasitic Bacteria. The remaining chapters are on the harmless parasites of warm-blooded animals, on anthrax and fowl-cholera, on the causal connection between parasitic Bacteria and infectious diseases, especially in warm-blooded animals, and on diseases caused by Bacteria in the lower animals and in plants. The work will be very useful to all who may wish to obtain "a general view" of this important subject. It has been well translated, and we may note that a valuable list of publications relating to Bacteria is given at the end of the volume.

Mattie's Secret. By Émile Desmaux. (London: George Routledge and Sons, 1887.)

THIS book is evidently a French work very well translated into English. It is practically a book of delightful gossip, touching on many important points of science; while theoretically it is a pleasing story of a sister who devotes her time to her little brother driven from school and books by approaching blindness. The scientific part opens with the explosion of fire-damp, and goes on to the history of coal, how it is found, in what shapes; and then to the coal-mine itself, how the work is done, and the precautions which have to be taken. Next follows the history of diamonds, what they are, how they are shaped into different forms; and then comes graphite manufactured into pencils. The history of beer here follows, how it is prepared, and its use. Then the author explains torpedoes and torpedo-boats, how they are worked, and the method of launching the torpedo. Glycerine, dynamite, and gunpowder, their dangerous properties, and how they are prepared, are next referred to, and this is followed by an introduction to the phenomena of sound.

The book contains a hundred good illustrations showing the different scientific processes, and it is thoroughly interesting throughout.

The question arises whether fairy tales of science are not as interesting to children as fairy tales of the ordinary description. The author is evidently of this opinion, and we are inclined to agree with him. A. L.

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.*]

Politics and the Presidency of the Royal Society.

YOUR leader of last week reminds me not a little of one of those days that begin somewhat brightly but end with a thunderstorm. As a Fellow of the Royal Society, I fail to see what our President has done to incur the reprobation of the writer of this article. I will take in order the two charges brought against him. Of these, the first is that he became President of the Victoria Institute, the second being that he has allowed himself to be returned as member of Parliament for his own University.

I do not fancy the writer means to assert that the predecessors of Prof. Stokes, more than one of whom held strong views connected with theology, virtually laid these aside during their tenure of office. At any rate, they gave ample expression to them both before and after this tenure. I gather that the objection rather is that Prof. Stokes, during his tenure of office, became President of another Society—the Victoria Institute. Now, Sir, I can see at once an objection to the President of the Royal Society being at the same time President of any of the other scientific Societies, such as the Astronomical, the Physical, the Chemical, or the Zoological. But I confess I fail to see any objection to his taking office in a Philosophical Society, which treats of subjects not really connected with science.

It may perhaps be thought that the Victoria Institute was deficient in breadth of view, and I think that until lately it was open to this objection. But I have reason to think that Prof. Stokes has infused into it a better spirit, and his admirable opening address to this Society has, if I mistake not, appeared in your columns.

In this address he acts entirely the part of a peacemaker, endeavouring to show that the conclusions of science cannot be held to come into collision with what may be regarded as the essential truths of the Christian religion. It is probable that a minority of Fellows of the Royal Society may believe that certain scientific doctrines have disposed of the claims of Christianity. Must, therefore, the President be precluded from going to church during his tenure of office? Unquestionably the going to church implies taking part in a public action about which the opinions of the Fellows could be divided.

It is in truth exceedingly difficult, if not impossible, to formulate a principle which shall extinguish the peculiarities of one individual while it leaves untouched the field around him representing the rights and privileges of others. The thing was tried once before in the time of Darius the Mede, but the results of the experiment were not of an encouraging nature.

I come now to the second and most important charge against our President. And here I confess I cannot help being a little amused at the writer's dread that the President will be hunted out of his scientific chair at all inconvenient hours, and driven to his seat at St. Stephen's by one of the Whips of the House. And I confess that I am equally amused at the thought of the Royal Society suffering the fearful political degradation depicted by the writer, entertaining as I do the most complete confidence in the integrity of this Society. I grant freely that under ordinary circumstances it is undesirable that the President of the Royal Society should enter the House of Commons. But these are no ordinary times, and we are now engaged in a struggle that means more than mere party warfare.

I do not wish to introduce politics into these pages, but I may state that in my opinion, and I think I may say in that of many Fellows of the Royal Society, the question just now is one between law and anarchy. But in a state of anarchy, what will suffer more than knowledge? In such a state will there be leisure to investigate—leisure even to dispute? And if this be so, should not Culture, which is more threatened than anything else, raise her voice in the Legislature and do what in her lies to prevent this deplorable consummation?

Surely it is this grave question, rather than any wish to represent the every-day interests of science, that has induced our President to enter the House. He has chosen to be an Englishman first, and a man of science afterwards. Who will blame him for this? BALFOUR STEWART.

THE able article which appeared in your last number (p. 49) under the title of "Politics and the Presidency of the Royal Society," raises a question of such magnitude, not only in its relation to science, but also to many other branches of human activity, that I trust to your courtesy and impartiality to give me an opportunity of briefly indicating some considerations calculated to lead to a conclusion different from that at which the writer of the article in question has arrived.

The Royal Society is composed of members who differ from one another in their views on political and many other subjects; nay, more, of men who differ from one another in their views on many scientific subjects. Their devotion to the advancement of natural knowledge is the common ground on which they meet.

The political opinions of our President are entirely unknown to us officially, and it may confidently be asserted that he is as highly esteemed and valued as President by those among us who may happen privately to differ from him widely in politics as by those who entertain similar political opinions to his own. His action in political matters concerns us as little as his opinions. No doubt we should be concerned if he were to undertake any duties of so engrossing a kind as to prevent him from fully discharging the duties of President, but we should be so equally if the additional work were not political.

It is conceivable, though we will hope not very likely, that at some future time the Society might have to return a member to the House of Commons; the Society would then be in a similar position to that in which several of our Universities are placed; the arguments used by the writer of the article might then be applied.

Our President cannot, however, be supposed to have entered the House as the political representative of the Royal Society, for two reasons: first, because the Society has, in its corporate capacity, notoriously no political opinions to be represented; and, secondly, because we have not sent him to the House.

ALEX. W. WILLIAMSON.

High Pitfold, Haslemere, November 19.

"The Conspiracy of Silence."

THOUGH I am sorry to have misunderstood the meaning of the Duke of Argyll in his "Great Lesson," when I supposed him to accuse scientific men of virtually conspiring to suppress any unwelcome truth, I think I am not without excuse. Certainly I was not alone in the illusion, and I believe that many would even now say that the Duke of Argyll—in writing some of the passages which I quoted, and in using such phrases as "reluctant to admit such an error in the great idol," "slow and sulky acquiescence," "reluctantly, almost sulkily," "a grudging silence," not to quote any others—has certainly not expressed with felicity the lesson which he intended to inculcate. Further, in regard to the special instance brought forward by the Duke (that of Mr. Murray's paper) it does not appear to me that he has even now established his charge. The Duke states that he has seen a letter, written by the late Sir Wyville Thomson, most strongly urging Mr. Murray to withdraw the paper which he had sent to the Royal Society of Edinburgh. The Duke further tells us candidly that no reason is alleged in the letter. Hence, Sir Wyville Thomson's motive is a matter of inference only. I hope I shall not give offence to my friend Mr. Murray if I suggest that it may have been different from that which the Duke supposes. In 1877, so far as I can ascertain, Mr. Murray had not had much practice in writing papers. There is an art in this, which we have to learn by practice and the kindly criticism of our manuscripts by friends. As the best meat may be spoiled by an inexperienced cook, so the best material may be damaged by an inexperienced author. Sir Wyville Thomson would naturally feel very sensitive about any communications bearing the names of members of the *Challenger* Expedition, for if among its first-fruits had been a paper unsatisfactory either as to style or arrangement, yet controverting the deliberate conclusions of those hardly less well qualified to judge, a spirit of criticism and of distrust as to the thoroughness of the work of the Expedition would have been aroused. Of course this is an hypothesis only, which I trust Mr. Murray will forgive me for making, but I can assure him that I am conscious of my own youthful imperfections (not to mention the mistakes of maturer years), and I submit that it is at least as good as the Duke's, and more charitable to the memory of Sir Wyville Thomson.

In regard to the new case which the Duke of Argyll brings forward, and with which he connects my name, he is not quite accurate in his facts and is wrong in his inference. Mr. Guppy's paper was not "refused" by the Geological Society of London. The President has the power in certain cases, and under certain conditions, to refuse to put down for reading a paper written by a Fellow. I did not exercise that power. The Council, after a paper has been read, can refuse to print it. As Mr. Guppy's paper was never read, obviously this did not happen. Probably the circumstances were as follows,—I say probably, for I have no distinct recollection of them. Mr. Guppy's paper may have been sent, as is often done, for an informal expression of opinion as to whether the paper seemed suitable for the Society's consideration. In such case it would be shown either to one of the secretaries or to the President, and the opinion, favourable or otherwise, communicated to the author, who would then be free to act as he thought best. Now, if Mr. Guppy's paper was identical with that printed in the Proceedings of the Royal Society of Edinburgh (vol. xiii. p. 857) I have no doubt that my answer was to this effect: that it contained so much matter which belonged rather to natural history than to geology that I thought it was likely to suffer much excision before it was printed in our Journal, especially at that time, and was more suited for a Society of a wider scope than our own. I have again referred to the paper, and, without entering upon its merits, of which I am fully sensible, am still of opinion that, while it is in its place in the Proceedings of a Royal Society which includes all branches of science, it would have to be considerably abridged to fit it for those of a Geological Society. Of course that is only my opinion, but after full ten years' experience, eight of them as an officer, on the Council of the Geological Society of London, I may claim some knowledge of the principles on which that body acts. Moreover, at that time the Society was suffering from a falling off in revenue, with no corresponding decline in the number of papers which it was invited to publish. This I knew had compelled the Council to exceptional strictness. The difficulties of the Society were indeed so considerable that I commented on them in my address on quitting office in 1886, expressing at the same time my own view as to how they should be met. But though, as I have said, I have no clear recollection of the circumstances, I can speak positively of one thing, that if in any way I discouraged Mr. Guppy from communicating his paper it was not because I "smelt a heresy." It is something quite new for me to stand accused of being a prompt suppresser of heresies. My orthodoxy has not always been considered unimpeachable among the clergy, and surely my scientific papers are not generally on the side of "established views."

To conclude, the Duke still—and this is our special complaint—treats the matter rather according to ecclesiastical than to scientific methods. He is fully persuaded of the excellence of Mr. Murray's hypothesis, and considers it to be "one of those discoveries in science which are self-luminous," and "must carry conviction to all." Very well, but there are some people, not very few in number, who do not share his opinion. He cannot understand that our doubts can be due to anything else but "prepossession," which has prevented our minds from being "alive to the breadth and sweep of the questions at issue." I humbly reply that this is not the case; that we claim to exercise the right of private judgment, and decline to submit to any pope, from whatever part of the United Kingdom he may issue his Bull.

T. G. BONNEY.

Instability of Freshly-Magnetized Needles.

YOUR reviewer objects to a statement in my "Theory of Magnetic Measurements," to the effect that freshly-magnetized needles give untrustworthy readings for several minutes after magnetization (see NATURE, vol. xxxvi. p. 316). In reply to his statement that this is contrary to experience, I wish to say that it is not contrary to my experience. In working with two 8-inch needles I continually observed this phenomenon for years, and it was so marked that I could not feel satisfied to omit the precaution which the critic condemns. I know of one other observer who has had a similar experience with another needle. My needles were not very hard, and perhaps this may have had to do with the phenomenon.

It is not desirable to make any reply to criticisms, even though they seem not quite fairly taken, but it ought to be suggested that those who are unable to apply general formulæ to a special form of instrument after they have been shown how to apply them to a similar instrument might perhaps meet with more success in some other line of business.

FRANCIS E. NIPHER.

IN the passage to which Prof. Nipher refers I contrasted what seemed to me the excessive precautions prescribed in the directions for obtaining the dip with a rather rough-and-ready method of manipulation elsewhere suggested by him. That the magnetic axis of a piece of steel may shift is possible. My criticism was directed to the question as to whether, as a matter of experience, such a shift is a cause of error of practical importance in the determination of the dip. It would, therefore, be interesting if Prof. Nipher would publish the details of the observations on which his conclusion is based, so that the extent to which a measurement of the inclination may be rendered untrustworthy by not waiting for some minutes after magnetizing the needle may be in evidence. Meanwhile it may be well that I should define my own views on the matter.

On looking through the observations made in the magnetic survey of Missouri, which Prof. Nipher is conducting, I find that the dips obtained with different needles vary widely. Thus, taking the last Report to which I have access, in which the work of the year 1881 is described (Trans. Acad. Sci. St. Louis, vol. iv. No. 3, p. 480), the dip was determined with two needles at fifteen stations. At seven of these the difference between the results obtained by the two needles was equal to, or greater than, 4′. At one station it was 24′·8, and at others 17′·2, 11′·7, 9′·4, and 8′·9 respectively. If these are examples of trustworthy readings (and from their publication we must suppose that they are so), and if the differences obtained when the observations are untrustworthy on account of the shift of the magnetic axis are greater than these, Prof. Nipher's experience is totally at variance with my own.

During the last four years, Dr. Thorpe and I, assisted at some Scotch stations by Mr. A. P. Laurie, have made about three hundred independent determinations of the dip. The observations have been made in the course of a magnetic survey of the United Kingdom at various stations, in all weathers, and without any delay after magnetization. We have used two sets of 3½-inch needles, and have made determinations with two needles at nearly all the stations. In no single case does the difference between the results obtained with two needles amount to 4'. In three or four cases only does it exceed 3', while differences of 2' are relatively rare. Thus in forty-six Scotch stations (for which alone the results are fully tabulated) the difference exceeded 2' in nine cases only. Mr. Welsh, in the survey of Scotland, recorded in the Report of the British Association for 1859, and the Rev. S. Perry obtained results in which the discordance between the two needles was of the same order of magnitude as in our own work.

If, therefore, Prof. Nipher refers to differences comparable with those exhibited by his published observations, they are contrary to the experience of observers working with better constructed instruments. If he refers to errors smaller but observed with an instrument with which a discordance between the two needles of from 10' to 24' can be tolerated, I should doubt if his apparatus is suitable for the elucidation of the point. If he is in possession of good evidence that, in the case of needles for which the maximum difference between observations made without delay after magnetization is not greater than 4', the accord between them is improved by delay, the matter is no doubt of interest for observatory work. My own experience has been chiefly gained in the field, and I can only say that I have never noticed anything which led me to suspect such a cause of error.

It is, however, capable of proof that the improvement can be but small, as results obtained in a laboratory and without the precaution Prof. Nipher insists on agree nearly to the limit to which the instrument can be read.

This can be illustrated from the observations made by Dr. Thorpe and myself at Kew for the purpose of testing our survey instruments. At first we employed only a circle by Dover, No. 74. The following observations were made with it in the magnetic house by Mr. Baker, the Chief Assistant, and ourselves:—

Date, 1884.	Observer.	Needle.	Dip.	Mean.
June 24	Baker	1	67° 36'·8	67° 36'·6
		2	36·5	
July 18	Thorpe	1	67 36·1	67 36·0
		2	36 0	
„ 19	Rücker	1	67 36·5	67 36·3
		2	36·1	

These results were about 2' lower than those obtained by Mr. Baker with the Kew instrument about the same time, but whatever the cause of this may have been they certainly do not convey the idea of instability.

Lately we have again compared No. 74 with the Kew instrument and with Dover No. 83, which belongs to the Science and Art Department. Thus six needles (two belonging to each instrument) were used. I quote the results, not as in any way extraordinary, but as types of the accuracy usually obtained by competent observers with good instruments:—

Date, 1887	Observer.	Instrument.	Needle.	Dip.	Mean.
Sept. 30	Rücker	Dover 74	1	67° 35'·4	67° 35'·1
			2	34·8	
Oct. 11	Thorpe	Dover 83	1	67 33·9	67 34·4
			2	34·9	
„ 13	„	„	1	67 36·0	67 36·0
			2	36·1	
„ 18	„	„	1	67 34·2	67 34·2
			2	34·2	
„ 19	Rücker	Dover 74	1	67 35·0	67 34·2
			2	33·4	

Mr. Baker's observations with the Kew instrument are again (as is shown below) a little higher than those obtained with Dover's circles.

Date, 1887.	Needle.	Dip.	Mean.
Sept. 22	1	67° 35'·3	67° 35'·6
	2	36·0	
Sept. 24	1	67 37·6	67 37·5
	2	37·4	
Oct. 25	1	67 38·8	67 37·9
	2	37·0	

Judging then from these results and from our own field observations, I do not believe that, apart from small instrumental errors, the error of the determination of the dip with a single needle, and without any delay after magnetization, will in general exceed ± 1'. Under unfavourable circumstances it may reach ± 1'·5. These estimates embrace not only the assumed instability of the magnetic axis, but that and all other causes of error combined. That some effect of the kind referred to by Prof. Nipher, which only affects the result below these limits, may exist even in good needles is perhaps possible. As the verniers of the circles only read to minutes it could not be detected except by making a number of observations for the purpose.

In conclusion I may add that for good dip observations good instruments are essential. In a preliminary survey in the neighbourhood of Mull, made in 1883, we employed an older instrument which had been a good deal used in a laboratory. The measurements made with it were less satisfactory than those above described, but the largest difference between the two needles did not exceed 6'. For survey purposes small needles and circles seem on all accounts better than the large ones used by Prof. Nipher. ARTHUR W. RÜCKER.

South Kensington, November 2.

Greek Geometry.

IN the notice of the last part of "Greek Geometry from Thales to Euclid" (NATURE, vol. xxxiv. p. 548) I was uncertain whether Dr. Allman intended it to be Part vii. or not; I observe from the extract before me (*Hermathena*, No. xiii., 1887, vol. vi. pp. 269–78) that the present part is so entitled. The author's plan led him to the temporary omission of Theætetus of Athens, a pupil of Theodorus of Cyrene, and also a disciple of Socrates, who greatly advanced the science of geometry. How his gifts and genius impressed both Socrates and Plato is well known from the dialogue which bears his name. From an analysis which our author makes of part of this dialogue it appears that Theætetus, in addition to Eudoxus and the Pythagoreans, was one of the original thinkers to whom Euclid was most indebted in the composition of the "Elements." Dr. Allman thus recapitulates:—"In the former parts of this paper we have seen that we owe to the Pythagoreans the substance of the first, second, and fourth books, also the doctrine of proportion and of the similarity of figures, together with the discoveries respecting the *application*, *excess*, and *defect* of areas, the subject-matter of the sixth book. The theorems arrived at, however, were proved for commensurable magnitudes only, and assumed to hold good for all. We have seen, further, that the doctrine of proportion, treated in a general manner, so as to include incommensurables (Book v.), and consequently the recasting of Book vi. and also the method of exhaustions (Book xii.) were the work of Eudoxus. If we are asked now: In what portion of the 'Elements' does the work of Theætetus survive? we answer: Since Books vii., viii., and ix. treat of numbers, and our question concerns geometry; and since the substance of Book xi., containing, as it does, the basis of the geometry of volumes, is probably of ancient date, we are led to seek for the work of Theætetus in Books x. and xii.; and it is precisely with the subjects of these books that the extracts (*d*), (*e*), and (*f*) are concerned."

The extract (*d*) states that Euclid, x. 9, is attributed to Theætetus by an anonymous scholiast, probably Proclus; extract (*e*) translates, discusses, and illustrates fully the passage (147 D–148 B) of the Dialogue; and extract (*f*) mentions the statement by Suidas, that our geometer taught at Heraclea, and that he first wrote on "the five solids," as they are called. Attention is drawn to the difference in expression employed by Proclus, viz. that Euclid *arranged* many works of Eudoxus, and *completed* many of those of Theætetus, from which Dr. Allman infers that, "whereas the bulk of the fifth and twelfth books is due to Eudoxus; on the other hand, Theætetus laid the foundation only of the doctrine of incommensurables as treated in the tenth book. In like manner from (*f*) we infer that the thirteenth book, treating of the regular solids, is based on the theorems discovered by Theætetus; but it contains, probably, a recapitulation, at least partial, of the work of Aristæus" [cf. NATURE, *ubi supra*].

The author, in conclusion, draws the inference that the principal part of the original work of Euclid himself is to be found in the tenth book. "De Morgan suspected that in this book some definite object was sought, and suggested that the classification of incommensurable quantities contained in it was undertaken in the hope of determining thereby the ratio of the circumference of the circle to its diameter, and thus solving the vexed question of its quadrature. It is more probable, however, that the object proposed concerned rather the subject of Book xiii., and had reference to the determination of the ratios between the edges of the regular solids and the radius of the circumscribed sphere, ratios which in all cases are irrational. In this way is seen, on the one hand, the connection which exists between the two parts of the work of Theætetus, and, on the other, light is thrown on the tradition handed down by Proclus, that Euclid proposed to himself the construction of the so-called Platonic bodies as the final axiom of his systematization of the 'Elements.'" Here for the present I take leave of the author. I have read his several parts as they have appeared with very great interest, and have endeavoured, without going far into technical details, to indicate the results arrived at, and I hope that some will have been induced to go to the fountain-head for undiluted draughts from this refreshing stream. I need only repeat the expression of the wish, more than once previously uttered, that the several papers may be collected into a handy volume, in which case they will fitly go side by side with the works of Bretschneider, Cantor, Tannery, and other distinguished labourers in the same field. R. T.

The Chromosphere.

HAVING lately devised a spectroscope with two small sextant telescopes and two small prisms, one of "extra dense" glass by Hilger, I attached it to a 2½-inch telescope, and tried its powers on the sun on the 6th inst., with the result that not only were the rays C and D^3 easily visible as bright lines, but I also found that by opening the slit and keeping the brighter part of the spectrum out of view I could see the actual ragged surface of the "storm-tossed sea of hydrogen."

I found the depth of the chromosphere to be about 10″, by estimating the length of the bright line when exactly tangential to the limb.

This result shows what is possible with small instrumental means, though probably much was due to an exceptionally clear sky. JOHN EVERSHED, JUN.

Perception of Colour.

IN answer to Mr. T. W. Backhouse, I would suggest that he should use the spectroscope in the following manner. Hold it between the luminous object (moon or street lamp) and the eye at a distance of about 12 or 15 inches from the latter, so that only part of the spectrum is seen. Then remove the spectroscope sideways, and pass it quickly through its old position. A flash of coloured light will be seen, and no matter what may be the direction of the spectrum with reference to the line of motion the flash will always be seen to travel from the red end towards the blue end. Each part of the spectrum can be examined separately.

Whether this phenomenon is due to a later perception or longer retention of the blue light as compared with the red I cannot at present say, but I think it is independent of the intensities. C. E. STROMEYER.

Swifts.

ON June 19, and again on June 21 last, in the evening, I watched a vast concourse of swifts flying over this town. They slowly soared upwards, shrieking and striking at each other, and at last went so far up in the sky as to look like a cloud of black gnats. I watched them till dusk, when their faint cries were still audible, and when these had died away in the distance I waited long for the birds to descend, but they did not, probably because they were old birds which had been sitting all day, and were glad of an opportunity to stretch their long wings in a few hours' flight. No great height would necessarily be attained by the birds during the short midsummer nights. I noticed on several subsequent evenings that at least some of the swifts of the town did not stay up till dusk; but I am not the less positive that on June 19 and 21 they spent the night in the sky.

Stroud. C. B. WITCHELL.

Note on a Madras Micrococcus.

THE sole charge of a Presidential Museum and the study of that high-road to pathological eminence, bacteriology, are unfortunately not compatible, but I have not been able to resist the rough investigation of a phenomenon which stands prominently out before my eyes as I write. It consists of a thin, homogeneous, pale pink pellicle, covering the *chunam* (shell-lime) walls of my house on the side exposed to the heavy monsoon rain, which is at present varying the monotony of our "fine sunny days," which so impress our energetic cold-weather visitors, who learn all about India from Calcutta to Cape Comorin in a three weeks' tour. So evenly is the pink-coloured material distributed in my library, that its walls look as if they had been painted on one side, and whitewashed on the other three sides. This coloration, which is well known in Madras, is, I believe, commonly attributed to some occult chemical action on the lime, but a cover-glass specimen stained with methylene blue, and examined with a ⅛-inch objective, decides at a glance that it is caused by a Micrococcus, which, in its microscopical appearance, presents nothing remarkable.

The mode of growth of this organism on or in artificial nutrient media I have not attempted to investigate, but I notice that white lead does not agree with it, as its growth ceases abruptly at the painted framework of the doors and windows.

As I cannot find any description, in the reference-books at my disposal, of a Micrococcus which corresponds to the one here described, I christen it provisionally *Micrococcus madraspatanus*, Madraspatan being the old name of Madras, which is, according to Lassen, a corruption of Manda-rājya, meaning "realm of the stupid." EDGAR THURSTON.

Government Central Museum, Madras, October 26.

Catharinea undulata.

IN October 1886 I found, in Hertfordshire, two specimens of *Catharinea undulata*, Web. et Mohr., bearing fruit in the axils of the leaves; those specimens I unfortunately lost.

When this summer in Norway, I had the good fortune and pleasure of meeting Prof. S. O. Lindberg, of Helsingfors, and I mentioned the fact of the discovery to him. He then told me that similar specimens had been found in Norway some little time before, and described under the name of *C. anomola*, Lindberg and Bryhn. In consequence of my conversation with Prof. Lindberg I looked again this autumn for specimens similar to those I had found last year, and after some little search I found some half-dozen or so near the same spot where I had found them last year.

The specimens I now have in my possession bear fruit at the apex of the stem, and also one, or sometimes two, setæ in axils of leaves below the apex. These pleurocarpous setæ differ from the acrocarpous by being twisted in a spiral manner, not being straight as the acrocarpous fruits are; they are inserted in a vaginula in the axil of the leaf, without any perichætial leaves.

I should be glad if bryologists generally would look out for specimens of this form. I should also consider it a great favour if any collectors who may find specimens would let me know, and provide me with an accurate description, or send me the specimens for inspection. Specimens should be preserved in strong methylated spirit, otherwise it may be difficult to verify some important details.

There is a brief reference to the Norwegian specimens in the *Botanische Centralblatt*, Band xxix. p. 2, 1887; the full description is, I believe, to be found in the *Botaniska Notiser*, 1886, p. 157; the latter I have not yet been able to obtain access to, though I hope to do so soon. J. REYNOLDS VAIZEY.

Botanical Laboratory, Cambridge, November 18.

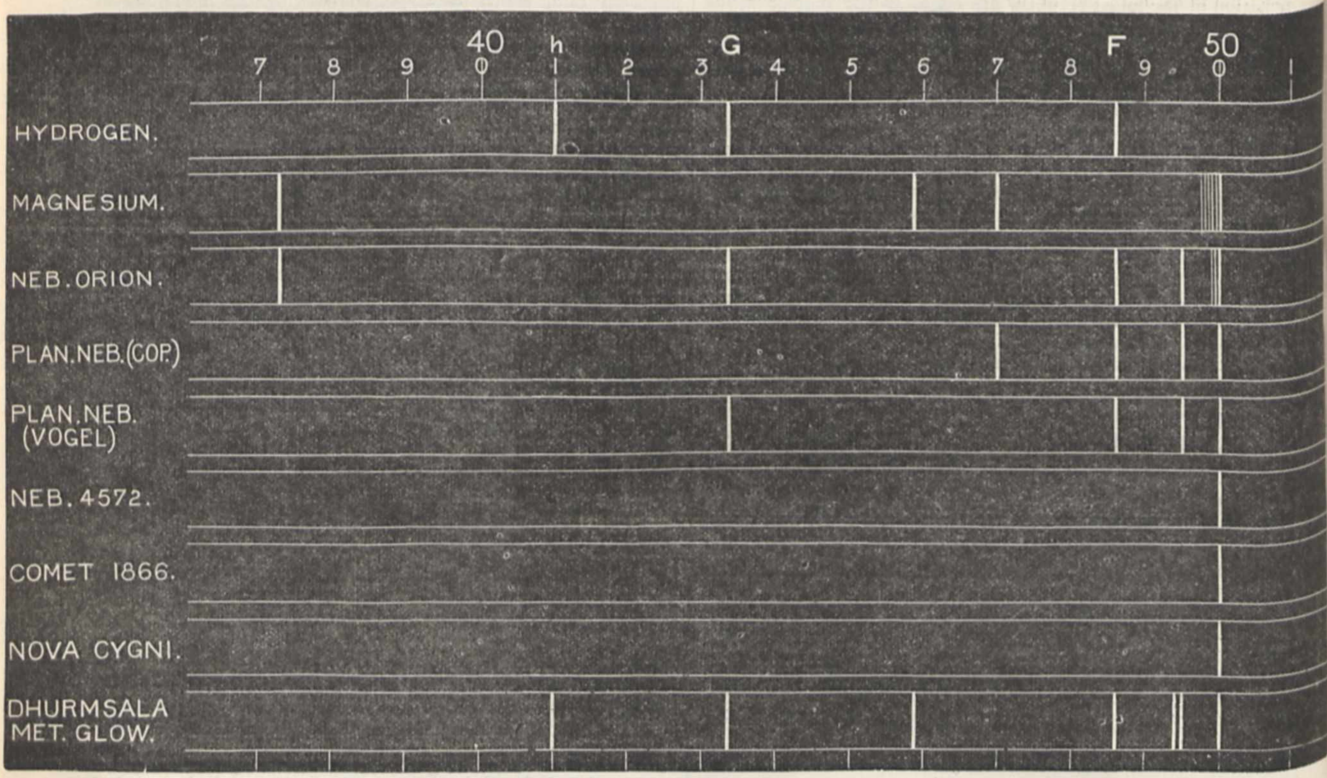
RESEARCHES ON METEORITES.[1]

II.

The Cases of Nova Orionis and R Geminorum.

THE stars with bright carbon flutings, the same as those seen in comets, are not limited to first-magnitude stars, such as α Orionis, but include at least one new star, Nova Orionis. Because the latter star lasted but a short time we might expect the phenomena presented to be different from those found in the first-magnitude star, which is a variable, like others with similar composite spectra. Practically there is little difference, for in α Orionis, α Herculis, and others of that type, we find well-marked dark absorption flutings of manganese, as well as line-absorption of sodium and magnesium.[2] The absorptions are not so well developed in the Nova, for the reason, perhaps, that condensation due to gravity had not taken place to such a great extent, so that the heat of the stones themselves was not so great, and further because the local absorption around each meteorite would be cloaked by the bright radiation of the interspaces, which gives, as in comets, the maximum intensity to the bright fluting, wave-length 517. In R Geminorum the demonstration of the same meteoric constitution, but without the strong absorption, is given by the fact that in that star so much of the light proceeds from the vapour produced by the meteorites, and from the carbon in the interspaces, that the carbon flutings and the bright lines of barium and strontium, and other substances present in meteorites, are visible at the same time, exactly as they are seen in the glow over a meteorite in an experimental tube, in which, as the pressure is reduced, the edges alone of the carbon flutings are visible, and put on the appearance of bright lines, almost exactly resembling the bright lines of the heated meteorites.

I give on a map the spectra of these two stars side by side with the bright flutings of carbon and the dark flutings of manganese with a view of showing that, both in the Nova and the first magnitude one in the same constellation, many of the phenomena are the same and are therefore probably produced by the same cause. Some time after Dr. Copeland's original observations of this star were published, it was pointed out, by



MAP 4.—Spectra of nebulæ compared with the spectra of hydrogen, cool magnesium, and meteorite glow.

Dunér, Vogel, and others, that some of the bright parts of the spectrum observed by him were really coincident with the bright parts of the spectrum of α Orionis; this, of course, is beyond question. But in addition to these bright spaces Dr. Copeland gives some bright regions which, I think, have not been touched by the arguments of Vogel and Dunér above referred to. It will be observed that in the case of R Geminorum, given on the same map as Nova and α Orionis, the bright lines correspond almost exactly with the bright spaces shown in the above-named stars and certain lines seen in meteorites—that is to say, a meteorite glow, when the carbon spectrum is bright, gives us all the lines recorded in the spectrum of the star, showing that some of the lines correspond with the brightest flutings of carbon.

There can be no question, I think, that in R Geminorum we have another stage, doubtless a prior stage, of the life-history not only of the Nova, but of α Orionis itself.

[1] Continued from p. 61.

[2] The manganese absorptions agree with some of the manganese flutings seen in the Bessemer flame by Marshall Watts (*Phil. Mag.* February 1873).

III. The spectra of meteorites glowing in tubes with the bright lines observed in celestial bodies—

(α) Comparison with the lines seen in nebulæ when C and F (bright) are either present or absent.

(β) Comparison with bright lines (not associated with flutings) seen in stars.

α. "Nebulæ."

Only seven lines in all have been recorded up to the present in the spectra of nebulæ, three of which coincide with lines in the spectrum of hydrogen and three correspond to lines in magnesium. The magnesium lines represented are the ultra-violet low-temperature line at 373, the line at 470, and the remnant of the magnesium fluting at 500, the brightest part of the spectrum at the temperature of the bunsen burner. The hydrogen lines are *h*, F, and Hγ (434). Sometimes the 500 line is seen alone, but it is generally associated with F and a line at 495. The remaining lines do not all appear in one nebula, but are associated one by one with the other three lines. The lines at 500 and 495 and F have been seen in the glow of the Dhurmsala meteorite when heated, but the origin of 495 has not yet been determined.

The result of this comparison then is that the nebula spectrum is as closely associated with a meteorite glowing very gently in a very tenuous atmosphere given off by itself as is the spectrum of a comet near the sun by a meteorite glowing in a denser one also given off by itself when more highly heated.

Further, it has been seen that the nebula spectrum was exactly reproduced in the comets of 1866 and 1867, when away from the sun. As the collision of meteorites is accepted for the explanation of the phenomena in one case, it must, *faute de mieux*, be accepted for the other. The well-known constituents of meteorites, especially olivine, fully explain all the spectroscopic phenomena presented by luminous meteors, comets, and nebulæ.

I published many years ago an experiment in which I had found that the gases evolved from meteorites under some conditions gave us the spectrum of hydrogen and under others the spectrum of carbon; but in the globes I then used I was not enabled to study the spectrum of the glow itself.

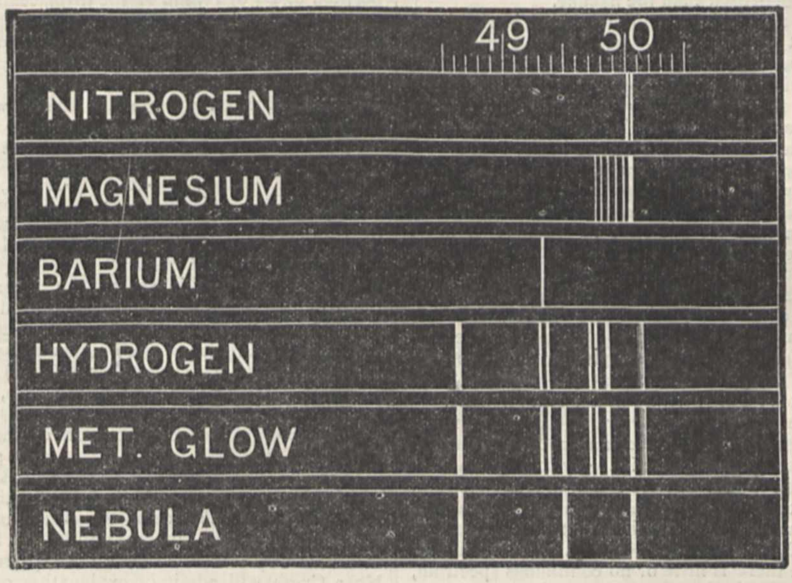
I should add that the line at 495 makes its appearance much more rarely than the one at 500, in meteorite glows.

β. "Stars" with bright lines.

On reference to the maps which I exhibit to the Society, though they and the discussion of them are yet incomplete, it will be seen that the principal lines which are seen bright in star spectra are, if we make due allowance for the discrepancies likely to occur in observations attended with great difficulties, lines which either have been observed in the vapours and gases given off by meteorites in vacuum-tubes or which we might expect to see in a combined series of observations on meteorites having different chemical constituents. Among these lines are H_α, H_β, H_γ, H_δ, 464, 540, 570, 580, 587; in one case (1st Cygnus) there are lines at 5065 and 5268, the latter due to iron. The difficulties attending this part of the inquiry are referred to subsequently, and it must be understood that in the absence of a detailed discussion especially of the spectra of the "Novas," which I have not yet completed, the opinion I express in the next part of this preliminary notice with regard to bright-line stars must be regarded rather as suggestions than as final conclusions.

Discussion of the Maps showing the Bright Lines visible in Stars and Nebulæ.

It results from the discussion of the bright lines seen, whether associated with the bright lines C and F of hydrogen or not,



MAP 5.—Shows the positions of three of the nebula lines as compared with well-known lines.

that, while on the one hand we have a class of bodies—the nebulæ—which give us the lines visible at the lowest temperature of chemical elements known to exist in meteorites, we have in the other class—the "stars" with bright lines—those lines visible at somewhat higher temperatures in meteorites. In the stars with bright lines the two most important lines, which have been separately mapped by Vogel,[1] occur at 540 and 582. The mean readings of all the observations gives the positions of these lines as 540 and 580. In an experiment on the glow of a meteorite rich in manganese, the line of Mn at 5395, easily seen at the temperature of the bunsen, is distinctly seen in addition to the structure-spectrum of hydrogen. There is reasonable ground therefore for supposing that this line, the only one of the iron group of metals visible at the temperature of the bunsen, may be the origin of one of the two lines seen alone in the spectrum of these "stars." It will be seen that in the map it has been easy to arrange all the bright lines hitherto seen in stars into one order, in which we begin with this line of manganese, and a line of iron seen at the temperature of the oxy-coal-gas flame, the wave-length of which is 579. As other lines indicating other substances are added to these fundamental ones, we pass from those stars in which C and F are not visible to those in which they make their appearance. Here, however, it is necessary to move with caution, because it may be that we are in presence of some of the lines visible in the structure-spectrum of hydrogen. The chief lines of hydrogen, as seen in the end-on tube when the conditions are such that C and F are not visible, have been already stated. Some of the lines observed in these stars, even the one at 540, have been found to be very nearly coincident with bright lines seen in the structure-spectrum, as well as with lines seen in the spectra of meteorites.

The suggestion, therefore, that some of the lines seen in bright-line stars are lines of cool hydrogen must be noted, although there are strong grounds for rejecting it, as will shortly appear. One objection is that strong lines of the H structure at 607-610 and 574 have not been recorded in star spectra with those at 540 and 580.

In the nebulæ (see Map 5) we deal chiefly with lines seen in the spectrum of magnesium at the lowest temperature; and these, as far as observations go, have not yet been associated with the lines at 540 and 580 to which reference has just been made, although they may or may not be associated with the bright lines C and F of hydrogen. In the nebulæ, however, no lines coincident with the lines of cool hydrogen have been observed. It will be seen, there-

[1] *Potsdam Observations*, vol. iv. No. 14.

fore, that we have here again strong ground for rejecting the view that the lines seen in "stars" at 540 and 580 are due to cool H, for since hydrogen is common to both nebulæ and stars, there is no reason why structure lines should occur in "stars" any more than in nebulæ.

Another ground for rejecting cool hydrogen as the origin of any of the lines in "stars" is that the structure-spectrum of hydrogen is only seen in confined glows, which is just the condition which cannot occur in space.

At the same time, the apparent coincidences of many meteorite lines with structure lines of hydrogen greatly increases the difficulties of laboratory work; in fact, the structure-spectrum of hydrogen is to observations of meteorite glows in the laboratory what continuous spectrum is to observations of bright lines in stars.

If it be agreed that we are not dealing with cool hydrogen, then it will follow that the only difference between celestial bodies with bright lines in their spectra comes from no difference of origin or chemical constitution, but from a difference of temperature.

At one point in these researches I was under the impression that the differences in the systems of bright lines seen in the nebulæ and the bright-line stars might arise from a preponderance of irons or stones in the swarms. But I was led to abandon this idea, not only by the observation of the meteoritic glows, but by the consideration that even telescopically the "stars" in question are more condensed than the nebulæ.

The spectrum of the nebulæ, except in some cases, is associated with a certain amount of continuous spectrum, and meteorites glowing at a low temperature would be competent to give the continuous spectrum with its highest intensity in the yellow part of the spectrum; so that in this way we should understand that lines due to any gas or vapour in that part would be very much more likely to escape record than those in the part of the spectrum which the continuous spectrum hardly reaches. The general absence, however, of bright lines of metallic vapours, except 495 and 500, and of the bright lines of hydrogen, evidently justifies the conclusion that we are here in presence of those bodies in celestial space, connected with which the temperature and the electrical excitation are at the minimum, and it is very remarkable how the lines seen in a Geissler tube under the conditions stated, when either magnesium, or olivine, or other meteoric constituents are made to glow, should appear, one may almost say, indiscriminately among the orders of bodies in the heavens which up to the present time have been regarded as so utterly different in plan and structure as stars and nebulæ.

The records of purely continuous spectra in the case of many nebulæ, as, for example, the Great Nebula in Andromeda, is in all probability an indication of our inability to observe them properly. For a nebula to give a perfectly continuous spectrum, it is evident that the component meteorites must be incandescent, but still at a lower temperature than that required to give bright lines. Now, the Mg line 500 is seen in some of the faintest nebulæ, where there is little or no continuous spectrum, and it therefore seems likely that these are at a lower temperature than the nebulæ said to give perfectly continuous spectra. This being so, it is difficult to believe that other lines, which require a somewhat higher temperature for their existence than the line at 500, do not become visible at this increased temperature.

There can be little doubt that when our instrumental appliances and observing conditions become more perfect it will be found that the so-called continuous spectra are really discontinuous. There is, indeed, an element of doubt as regards some of the existing observations; thus, the spectrum of the companion to the Great Nebula in Andromeda appears to end abruptly in the orange, and throughout its length is not uniform, but is evidently crossed by lines of absorption or by bright lines (Huggins, Phil. Trans. vol. cliv. p. 441).

Again, the Great Nebula in Andromeda is generally regarded as having a continuous spectrum pure and simple, but an observer at Yale College (name not stated) has observed three bright lines in its spectrum (*Observatory*, vol. viii. p. 385). The lines are—the F line of hydrogen, and two other lines at wavelengths 5312·5 and 5594·0. The latter two lines are mentioned by the same observer as bright lines in γ Cassiopeiæ and β Lyræ, and are recorded by Sherman (*Astr. Nach.*, No. 2591) as bright lines in these stars and in Nova Andromedæ. No other observations with which I am acquainted give these two lines in γ Cassiopeiæ or β Lyræ, but Maunder (*Monthly Notices*, vol. xlvi. p. 20) gives them as two of the lines seen in Nova Andromedæ. It is possible, therefore, that the two lines in question, in the Yale College observation, had their origin in Nova Andromedæ; at all events there is no evidence to show that they are visible in the Great Nebula of Andromeda under normal conditions.

It is not impossible that the lines at 540 and 580 may be eventually traced in some of the brightest nebulæ, since these are apparently the lines next in order, as regards temperature, to the Mg line 500.

It is right that I should here point out that some observers of bright lines in these so-called stars have recorded a line in the yellow which they affirm to be in the position of D_3; while on the other hand, in my experiments on meteorites, whether in the glow or in the air, I have seen no line occupying this position.

I trust that some observer with greater optical means will think it worth his time to make a special inquiry on this point. The arguments against this line indicating the spectrum of the so-called helium are absolutely overwhelming. The helium line so far has only been seen in the very hottest part of the sun which we can get at. It is there associated with *b* and with lines of iron which require the largest coil and the largest jar to bring them out, whereas it is stated to have been observed in stars where the absence of iron lines and of *b* shows that the temperature is very low. Further no trace of it was seen in Nova Cygni, and it has even been recorded in a spectrum in which C was absent.

It is even possible that the line in question merely occupies the position of D_3 by reason of the displacement of D by motion of the "stars" in the line of sight. On this point no information is at hand regarding any reference spectrum employed. If, however, it should eventually be established that the line is really D_3, which probably represents a fine form of hydrogen, it can only be suggested that the degree of fineness which is brought about by temperature in the case of the sun is brought about in the spaces between meteorites by extreme tenuity.

The Case of Nova Cygni.

The case of Nova Cygni is being discussed, and it appears likely that this "star" passed through all the stages of temperature represented by "stars" with bright lines, comets, and nebulæ. In the initial stage, the principal lines recorded were those of hydrogen, cool magnesium, and sodium. At a later date, in addition to these, lines apparently indicating hotter magnesium and carbon were observed. On the date of its highest temperature (December 8, 1876) the lines observed by Vogel indicate H, Na, Mg, C, Fe, Mn, and Ba, the "star" having then, it would appear from the discussion so far as it has yet gone, approached the condition of the great comet of 1882 at perihelion. The Fe, Ba, C, and Na gradually disappeared, then the hydrogen followed, and the last stage of all was that in which Mg (500) appeared alone, as in the comets of 1866–67 and in nebulæ. The complete discussion, however, must be reserved for a future communication. It is sufficient to say here that it is very probable that all the spectroscopic phenomena of Nova Cygni will admit of explanation on the supposition that it was produced by the collision of two swarms of meteorites. The outliers were first engaged, and at the maximum the denser parts of the swarm.

Difficulties connected with the Discussion.

An inspection of the maps, on which are shown all the observations already made upon bright lines recorded in the spectra of celestial bodies, will indicate at first sight an apparent variation of the positions of the lines greater than might have been expected. This, however, I think will vanish on the consideration of the whole question; and for my part certainly all the examinations which I have been able to make have led me to the conclusion that the various observations have been far better than it was almost possible to hope for when the great difficulties of the observations themselves are considered.

When it is remembered that, in order to get a determination of the position of a bright line, comparison-spectra and prisms are needed, and that, from mechanical considerations alone, the application of these aids to research is very frequently attended with difficulties and uncertainties; and further, when we consider that many of the observations have been necessarily made without these aids; the striking coincidences on the maps become of very much greater importance than the slight variations seen between the positions of the same line recorded by different observers in the same star.

It will be observed, too, that the information in some cases is fuller in the blue part of the spectrum. Here again a reference to what the maps are really intended to show is necessary. The maps do not show the complete spectrum observed, but only the bright lines recorded in it. The actual observations have really consisted in picking out these bright lines from the background of continuous spectrum, whether in stars, nebulæ, or comets; and, as the continuous spectrum will be generally brightest in the yellow and green, so in this part of the spectrum we must expect, first of all, to get the least information, and then, when the information is obtained, to get the greatest uncertainty, on account of the difficulty brought about by the greater luminosity of the background on which the line appears.

The discussion by Hasselberg and others of the various observations of comets which have been made from time to time indicates that under certain circumstances, where men of the highest skill and with the greatest care have determined the wave-lengths of the carbon bands, discrepancies exist too great to admit of their being attributed to errors inherent in this branch of observation.

If for a moment we consider alone the two bright flutings visible in the spectrum of carbon, one with its bright edge just more refrangible than b_4—this is the high-temperature spectrum—and the other—the low-temperature spectrum—with a fluting just less refrangible than b_1, it is at once suggested that sudden changes in comets may very likely be accompanied by a transition from one condition of carbon vapour to the other, so that on this account apparent discrepancies in the measurements of the same comet at different times may represent real facts. Then again we have the motion of the swarm along its orbit, which in some cases we know is comparable to the velocity of light, so that variations of wave-length are produced as indicated in comet 1882. We also have the possibility that the velocity of the vapours in the jets, and that due to the electric repulsion—which, according to Zöllner's view, is the origin of comets' tails—may also produce changes of refrangibility.

Although as a rule the bright fluting seen in comets appears to be that due to high temperature, this is apparently not always the case. In the experiments on the glow of magnesium wire, the flutings of carbon have always been seen, and when the vacuum is approached the flutings have been those of the low-temperature spectrum. When the glow of the metal is seen under certain conditions, mixed with carbon vapour, b_1 and b_2 are seen as bright dots or short lines inside the carbon fluting, exactly as they were observed, probably, by Huggins in Brorsen's comet (Proc. R. S., vol. xvi. p. 386).

Authorities used in the Maps.

The map showing the bright lines in *Stars* is based upon the following authorities:—

3rd Cygnus, B.D. + 36° No. 3956, R.A. 20h. 10m. 6s., Decl. + 36° 18′.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 19.

2nd Cygnus, B.D. + 35°, No. 4013, R.A. 20h. 7m. 26s., Decl. + 35° 50′·8.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 19.
Wolf and Rayet.—*Comptes rendus*, vol. lxv. (1867), p. 292. The wave lengths were obtained from a curve based on the measurements given.

Argelander-Oeltzen 17681, R.A. 18h. 1m. 21s., Decl. − 21° 16′·2.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 15.
Pickering.—*Astronomische Nachrichten*, No. 2376.
Pickering.—*Observatory*, vol. iv. p. 82.

γ Argus, R.A. 8h. 5m. 56s., Decl. − 46° 59′·5.
Copeland.—*Copernicus*, vol. iii. p. 205.
Ellery.—*Observatory*, vol. ii. p. 418.

Stone 9168 (star in Scorpio), R.A. 16h. 46m. 15s., Decl. − 41° 37′·6.
Copeland.—*Copernicus*, vol. iii. p. 205.

1st Argus, R.A. 8h. 51m. 1s., Decl. − 47° 8′.
Copeland.—*Copernicus*, vol. iii. p. 206.

2nd Argus, R.A. 10h. 36m. 54s., Decl. − 58° 8′.
Copeland.—*Copernicus*, vol. iii. p. 206.

Gould 15305 (Argo), R.A. 11h. 5m. 19s., Decl. − 60° 21′.
Copeland.—*Copernicus*, vol. iii. p. 205.

Star in Centauri, R.A. 13h. 10m. 37s., Decl. − 57° 31′.
Copeland.—*Copernicus*, vol. iii. p. 206.

Star in Cygnus, B.D. + 37° No. 3821, R.A. 20h. 7m. 48s., Decl. + 38° 0′·1.
Copeland.—*Monthly Notices of the Royal Astronomical Society*, London, vol. xlv. p. 90.

Lalande 13412, R.A. 6h. 49m. 15s., Decl. − 23° 46′·3.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 17.
Pickering.—*Astronomische Nachrichten*, No. 2376.

1st Cygnus, B.D. + 35° No. 4001, R.A. 20h. 5m. 48s., Decl. + 35° 49′·7.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 17.

γ Cassiopeiæ, R.A. 0h. 50m. 4s., Decl. + 60° 7′·2.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 15.
Vogel.—*Beobachtungen zu Bothkamp*, Heft ii. p. 29.
Gothard.—*Astronomische Nachrichten*, No. 2581.
Konkoly.—Quoted by Gothard in *Astronomische Nachrichten*, No. 2581.
Observatory, vol. vi. p. 332.

β Lyræ, R.A. 18h. 45m. 55s., Decl. + 33° 13′·9.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 15.
Vogel.—*Beobachtungen zu Bothkamp*, Heft i. p. 33.
Gothard.—*Astronomische Nachrichten*, No. 2581.

The map showing the bright lines in *Nebulæ* is based upon the following authorities:—

Nebula in Orion.
Huggins—Proceedings R.S. vol. xiv. p. 39.

Planetary Nebula, R.A. 21h. 22m., Decl. + 47° 22′.
Copeland.—*Copernicus*, vol. i. p. 2.

Planetary Nebula.
Vogel.—*Monatsberichte der Akademie der Wissenschaften zu Berlin*, April 1878, p. 303.

No. 4572, 2075h., 16 H. iv. R.A. 20h. 16m. 7·9s., N.P.D. 70° 20′ 19″·3.
Huggins.—Philosophical Transactions, vol. clvi. p. 385.

Comet 1866.
Huggins.—Proceedings R.S. vol. xv. p. 5.

Nova Cygni.
Lord Lindsay and Dr. Copeland.—*Copernicus*, vol. ii. p. 109.

The map showing the coincidences of flutings of carbon, manganese, and zinc, with bright lines and flutings in stars and comets, and in a meteorite glow, is based upon the following authorities:—

Hydrocarbon ... ... } Work at Kensington.
Low-temperature carbon ... }
High-temperature carbon ... }

Comet *b* 1881.
Copeland.—*Copernicus*, vol. ii. p. 225.

Manganese flame.
Lecoq de Boisbaudran.—"Spectres Lumineux."
Work at Kensington.

Nova Orionis.
Copeland.—*Monthly Notices* of the Royal Astronomical Society, vol. xlvi. p. 109.

α Orionis.
Vogel.—*Beobachtungen zu Bothkamp*, Heft. i. p. 20.

R Geminorum.
Vogel.—*Astronomische Nachrichten*, No. 2000.

Meteorite Glow.
Work at Kensington.

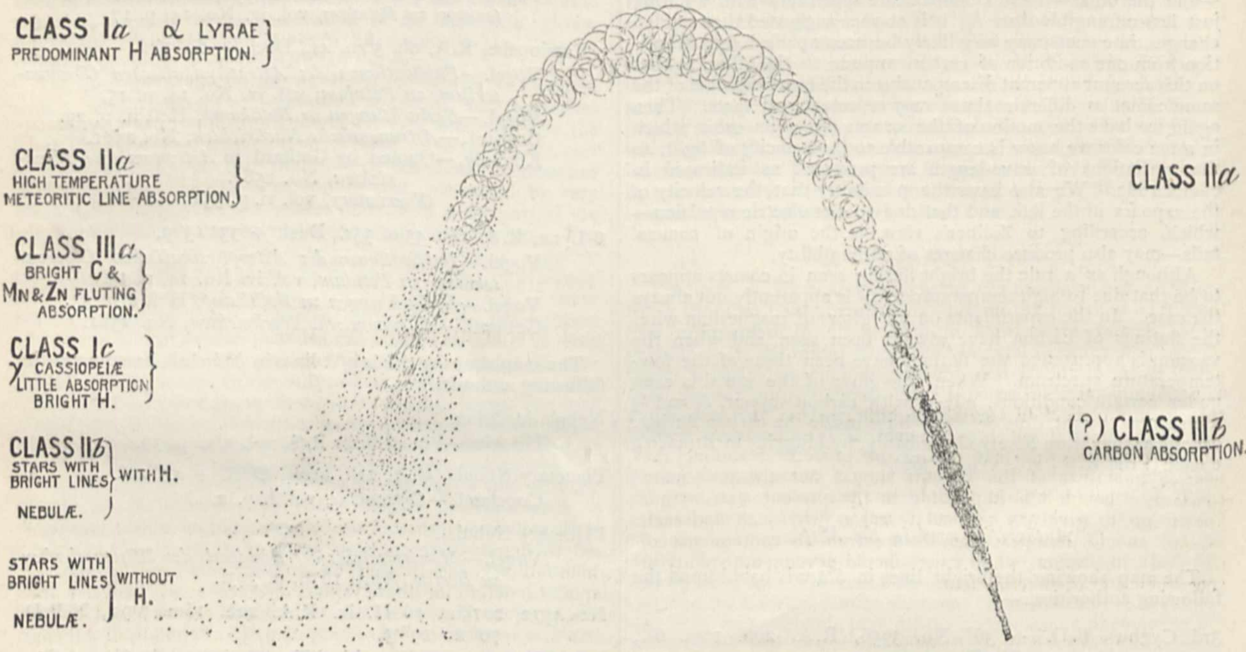
Schjellerup 152.
Vogel.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, vol. iv. No. 14, p. 30.

On the Absorption Phenomena of Stars with Bright Lines.

In addition to the map showing the bright lines visible in those stars the spectra of which contain them, I have prepared another map showing the absorptions which also occur. The two maps present a remarkable agreement—that is to say, there is the same progression in the absorption phenomena as there is in the bright-line phenomena. In those stars in which bright lines are seen without the lines of hydrogen (in which stars the meteorite swarm is probably at a slightly higher temperature than that observed in the nebula when only the line at 500 is visible) we have no marked absorption-lines, but rather bands. When the hydrogen lines are added, as in γ Cassiopeiæ, then we get the absorption of sodium and *b* of magnesium, as we should expect. The individual meteorites therefore are much cooler in these stars than in the Novas, seeing that the absorption is so little developed. Speaking generally, therefore, we may say that there are two causes of minimum absorption phenomena in stars. In the first place, as in the bright-line stars, only little vapour surrounds each meteorite, and that vapour consists of the substances visible at the lowest temperature; while, on the other hand, in stars like Sirius, in consequence of the absolute state of vapour, we only get practically the absorption of hydrogen, or at all events the absorption of hydrogen in great excess, due, I have very little doubt, in part, to the fact that most other substances have been dissociated by the intense heat resulting from the condensation of the meteorites.

NOTES ON THE PROVISIONAL TEMPERATURE CURVE.

In order to bring the various results referred to in this communication in a definite form before my own mind, I have prepared a diagram which I have called a temperature curve, so



Provisional Temperature Curve.

that on one side of it we may consider those stages in the various heavenly bodies in which in each case the temperature is increasing, while on the other arm of the curve we have that other condition in which we get first vaporous combination, and then ultimately the formation of a crust due to the gradual cooling of the mass. At the top of such a curve we shall of course have that condition in which the highest temperature must be assumed to exist. In a letter to M. Dumas in the year 1872, I suggested that possibly the simplification of the spectrum of a star might be associated with the highest temperature of the vapour, and that idea seems to have been accepted by other observers since that time. We shall have then stars of the first class at the top of the temperature curve. On the one arm of the curve representing increasing temperature we shall have at various heights those aggregations which give us indications of a gradually increasing temperature brought about by collisions, beginning with meteorites as widely separated as they can be to *keep up* any luminosity at all, and finally vaporous condensations due to gravity.

On the arm of the curve descending from stars of the first class to dark bodies like, say, the companion to Sirius, we must place those bodies where absorption of compound molecules is indicated. This we find in stars of Class III.*b* of Vogel. But here a very interesting question arises. Between stars of the first class and that of III.*b* we are bound to insert stars of Class II., already located naturally on the ascending arm.

The Case of Equal Temperatures on Either Side of the Curve.

Speaking roughly, it may be said that the construction of such a curve as this suggests that similar or nearly similar temperatures will be found on either side. This in the main, of course, is true; but it must be pointed out that, on the rising curve, the temperature will be that, as a rule, of individual meteorites and the vapours given out by them, while on the descending arm it will be the temperature of the consolidated mass, whether vaporous or becoming solid. But it is obvious that if we take two points near the top of the curve we shall have very nearly the same temperature of the atmosphere, by which I mean the temperature of the layers in either case which are most effective in producing the phenomena of absorption. To take a concrete case, stars of the second class are obviously, by the consent of all, of a lower temperature than stars of the first class: on which side, therefore, of the curve must they be placed? Or, to take a more concrete case still, our sun is a star of the second class: on which arm of the curve must we place the sun? Here we find ourselves in a position of some difficulty, but it would appear that future work may enable us really to divide stars of the second class into two series, and if we can do so there is very little doubt that one series will represent the phenomenon of decreasing temperature of the absorbing layers, while the other series will represent the phenomenon of increasing temperature.

What considerations are likely to help us in such an inquiry as this? The atmosphere of a star built up by meteorites should resemble in its constitution the totality of the chemical constitution of meteorites, and therefore it might be inferred that the spectroscopic phenomena presented by such an atmosphere would not be widely different from the spectroscopic phenomena presented by the vapours of many meteorites volatilized together.

To investigate this question I have obtained composite photographs of the spectra of several meteorites, with a solar spectrum for purposes of comparison. I find that, while, on the one hand, the composite photograph giving us the spectrum of the meteorites greatly resembles that of the sun, as it should do, there are some variations which suggest the line of separation to which I have before alluded. From Dr. Huggins's magnificent photographs of the stars we have learned that, as I had predicted years before the photographs were taken, the thickness of H and K varies very greatly in different stellar spectra. In those stars, presumably the hottest ones, in which we get the series of hydrogen lines almost alone as great absorbers, K is almost absent; it finally comes in, however, and after a certain stage has been reached it is the most important line in the spectrum. But there are stars in which the lines *h* and G of hydrogen are not very much more developed than they are in the case of our own sun, in which K is much thinner than in the solar spectrum; and associated with this condition of K there is the absorption of a hydrogen line more refrangible than K at wave-length 3800, which is not represented in the solar spectrum with anything like the intensity. The question arises, therefore, whether the enormous thickening of K observed in the sun and some other stars may not be limited to those stars which, like our sun, are reducing their temperature; for we certainly are justified in assuming that the temperature of the sun now is not so high as it was in an earlier stage of the development of the system. Such a difference as that, if it is subsequently established, can only come from the atmosphere, as an effect of cooling, becoming richer in those substances the lines of which get broader as the star cools down. We can easily imagine that during the process of cooling the relative quantities of the vapours should not always remain constant, although it is impossible in the present state of our knowledge to give any particular reason why such and such vapours should disappear from the spectrum in consequence of chemical combination, while others should develop apparently in consequence of their retirement.

Hydrogen plus Carbon indicates Mixed Swarms.

If we assume a brightening of the meteor-swarm due to collision as the cause of the so-called new stars, we have good grounds for supposing that in these bodies the phenomena should be mixed, for the reason that we should have in one part of the swarm a number of collisions probably of close meteorites, while among the out-liers the collisions would be few. We shall in fact have in one part the conditions represented in Class III.*a*, and in the other such a condition as we get in γ Cassiopeiæ. I have in another part of this paper discussed the flutings observed in Nova Orionis, and have shown that so far as they were concerned we have the radiation of carbon and the absorption of manganese; but there is evidence to show that with these fluted appearances bright lines were observed—D_3 and F, although no mention is made of C.[1]

We have here, there is little doubt, the *vera causa* of stellar long-period variability. 12 per cent. of stars of Class III.*a* are variable, and 9 per cent. of Class III.*b*. In the one case, meteor-swarms produce the increased brightness by colliding with those of the condensing one. In the other, they do so by their periastron passage round the dim condensed one. There is no variability, in the usual sense of the word, in stars like the sun and α Lyræ, and the reason is now obvious.

[1] Konkoly, *Astr. Nach.* 2712, D_3 and F; Riccò indicates D_3 in *Astr. Nach.* 2707.

THE CONDITIONS OF COLLISIONS OF METEORITES.

The Chemical Elements most frequently determined in Meteorites.

I think it well to give here as a reminder a short table showing the chief substances met with in meteorites. It will indicate the cause of the continued reference to the spectra of Mg, Fe, and Mn in what follows.

SIDERITES.

Nickel-iron, copper, manganese.
Troilite = FeS.
Graphite.
Schreibersite = iron and nickel phosphide.
Daubréeite = iron and chromium sulphide.

SIDEROLITES.

CHONDRITIC—

(α) Non-carbonaceous =	Olivine = chrysolite = peridot = $(MgFe)_2O_4Si$ = SiO_2 41·3, MgO 50·9, FeO 7·7.
	Enstatite MgO_3Si = SiO_2 60, MgO 40.
	Bronzite = enstatite in which some Mg is replaced by Fe.
	Nickel-iron, manganese.
	Troilite.
	Chromite = iron protoxide 32, chromium sesquioxide 68, + Al and Mg.
	Augite = pyroxene, SiO_2 55, CaO 23, MgO 16, MnO 0·5, FeO 4.
	Silicate of calcium, sodium, and aluminium.
(β) Carbonaceous	Carbon in combination with H and O.
	Sulphates of Mg, Ca, Na, and K.
NON-CHONDRITIC =	Anorthite.
	Enstatite.
	Bronzite.
	Olivine.
	Augite.
	Troilite.

The Numbers of Meteorites in Space.

It is well known that observations of falling-stars have been used to determine roughly the average number of meteorites which fall on the earth each twenty-four hours; and having this datum to determine the average distance apart between the meteorites in those parts of space which are traversed by the earth as a member of the solar system, Dr. Schmidt, of Athens, from observations made during seventeen years found that the mean hourly number of luminous meteors visible on a clear moonless night by one observer was fourteen, taking the time of observation from midnight to 1 a.m.

It has been further experimentally shown that a large group of observers who might include the whole hemisphere in their observations would see about six times as many as are visible to one eye. Prof. H. A. Newton and others have calculated that making all proper corrections the number which might be visible over the whole earth would be a little greater than 10,000 times as many as could be seen at one place. From this we gather that not less than twenty millions of luminous meteors fall upon our planet daily, each of which in a dark clear night would present us with the well-known phenomenon of a shooting-star.

This number, however, by no means represents the total number of minute meteorites that enter our atmosphere, because many entirely invisible to the naked eye are often seen in telescopes. It has been suggested that the number of meteorites if these were included would be increased at least twenty-fold: this would give us 400 millions of meteorites falling on the earth's surface daily. If we consider, however, only those visible to the naked eye, and if we assume that the absolute velocity of the meteors in space is equal to that of comets moving in parabolic orbits, Prof. H. A. Newton has shown that the average number of meteorites in the space that the earth traverses is in each volume equal to the earth about 30,000. This gives us a result in round numbers that the meteorites are distributed each 250 miles away from its neighbours.[1]

If, then, these observations may be accepted to be good for any part of space, we may, and indeed must, expect celestial phenomenon which can be traced to meteorites in all parts of space.

Further, we have the experience of our own system that these meteors are apt to collect in groups.

A comet, it is now generally accepted, is a swarm of meteors in company. Such a swarm finally makes a continuous orbit by virtue of arrested velocities; impacts will break up large stones and will produce new vapours in some cases, which will condense into small meteoroids.

A meteorite in space under any of the conditions indicated by the comets, new stars, and such first-magnitude stars as α Orionis, will evidently be subject to collisions, but only to a greater number of collisions than those which must ordinarily occur if space is as full of meteorites as Prof. Newton's calculations, from observations made on the earth, would naturally seem to indicate.

The Velocity of Luminous Meteors.

In spite of the difficulties which attend the observations necessary to determine the velocity of meteors entering our atmosphere, many observations have been made from which it may be gathered that the velocity is rarely under 10 miles a second or over 40 or 50. It is known that the velocities of some meteor-swarms are very different from those of others. Prof. Newton, our highest authority on this subject, is prepared to consider that the average velocity may be taken to be 30 miles a second.

Result of Collisions.

If we take these velocities as representing what happens in other regions of space, and assume the specific heat of the meteorites to be ·10, the increase in their temperature when their motions are arrested by impacts will be roughly as follows:—

Velocity	1 mile per second	3,000° C.
,,	10 ,, ,, ...	300,000°
,,	20 ,, ,, ...	1,200,000°
,,	30 ,, ,, ...	2,700,000°
,,	60 ,, ,, ...	10,800,000°

It is clear, however, that we should under the conditions stated be more frequently dealing with grazes than collisions.

Comets due to Collisions of Meteorites.

The fact that comets are due to swarms of meteorites was first established by Schiaparelli in 1866, when he demonstrated that the orbit of the August meteors was identical with that of the bright comet of 1862.[2]

Nebulæ due to Collisions of Meteorites.

So far as I know the first suggestion that nebulæ were really in some manner associated with meteorites and not with masses of gas was made by Prof. Tait in 1871.[3] I have used the suggestion in my lectures ever since, and it is now some years ago since I put it to an experimental test by showing that both the spectra of comets and nebulæ, so far as carbon and hydrogen were concerned, could be produced from a vessel containing the vapours produced by meteorites. More recently, M. Faye has stated in his works on the nebular hypothesis that the solar nebula may as probably have consisted of a cloud of stones as of a mass of gas. This view, however, has not been favoured by Dr. Huggins, who in his observations both on nebulæ and comets has inferred from the near coincidence of the line of 500 with the strong air line that we are probably in presence of nitrogen, or of a form of matter more elementary than nitrogen; the line at 373 being attributed by him also to some unknown form of hydrogen on account of its coincidence with one of the series of hydrogen lines in the ultra-violet observed in the spectra of stars of the first class.

"New Stars" due to Collisions of Meteorites.

The idea that the *Novas* which appear from time to time are due to collisions of meteorites was, I think, first advanced by myself in 1877, when I wrote in connection with Nova Cygni:—

"The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that, if the body which thus put on so suddenly the chromospheric spectrum were single, it might only weigh a few tons, or even hundredweights, and, being so small, might be very near us. Mr. Hind's telescope was dismounted, and I have not yet got any information as to change of position; and as I am now writing in the Highlands, away from all books, I have no opportunity of comparing the position now given by Lord Lindsay in R.A. 21h. 36m. 52s., Decl. + 42° 16′ 53″, with those given on its first appearance by Winnecke and others.

"We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceeding slow.

"Let us consider the case, then, on the supposition of small masses of matter. Where are we to find them? The answer is easy: in those small meteoric masses which, an ever-increasing mass of evidence tends to show, occupy all the realms of space."[1]

The Effects of Collisions.

The question of what must happen to the meteorites themselves in consequence of this system of collisions is worth going into thoroughly. A very cursory examination seems to indicate that much light is thrown on the condition of meteorites as we know them, and their division into iron and stony.

As 30 miles per second is a very frequent value obtained for the velocity of meteorites when they enter our atmosphere, it is possible to compare temperatures brought about by collisions with those produced by passage through our atmosphere. Two masses of meteoric iron meeting each other in space would probably, if moving with a certain velocity, be formed into a pasty conjoined mass, and this process might go on until an iron of large dimensions was formed, and the various meteorites thus welded together would present in time a very fragmentary appearance. While irons were thus increasing in size, collisions with smaller meteorites would be attended with very local increases of temperature, perhaps sufficient to volatilize the surface or allow it to be indented, and in this manner the well-known "thumb-marks" receive explanation.

The masses of iron, when in a state of fusion, whatever their size, would be able to include stony meteorites in their vicinity. In the case of stones it is easy to see that the result would be very different. Their collisions would have, most probably, the effect of reducing large pre-existing masses to smaller ones, and the collision of a large stone with a large iron would probably effect the driving of the stone into fragments, while the iron would be liquefied so as to inclose some of the fragments in its mass.

These operations of Nature might go on either in free space, or in the head of a comet, or in meteor-swarms. They probably cause the appearance of the so-called new stars, and in these various circumstances the rate of subsequent cooling would of course be very different, so that the results would be very different indeed.

Large masses on collision probably destroy each other, produce fragments and vapour, which again condense. The heterogeneous structure is thus to a certain extent explained. On collision the part of the substance of the meteorite given up will depend on the temperature, and thus a mass of metallic iron mixed with silicates at low temperature will get rid of the iron at once, which must then perforce condense in a separate swarm; therefore under low temperature conditions, say at aphelion, irons alone will be formed and the stones will become spongy. The stones will absorb the C and H vapours.

[1] Article on "Meteorites," Prof. Newton, "Encyclopædia Britannica," vol. xvi.

[2] Letters to Father Secchi, printed in the *Bollettino* of the Collegio Romano, and reproduced in *Les Mondes*, t. xiii.

[3] "It seems to me that we have a series of indications of what (for want of a better phrase) may be called the *period of life* of a star or group, beginning with the glowing gases developed by impacts of agglomerating cold masses (planetary nebulæ and others irresolvable, such as those of Orion, Lyra, &c., where the spectrum consists of a very few bright lines only)" (Prof. Tait, Proc. R.S. Edin., 1871).

[1] NATURE, vol. xvi. p. 413.

I have finally to express my great obligations to Messrs. Fowler, Taylor, and Richards, who have helped me in various ways in the researches embodied in this paper. Mr. Fowler, the assistant to the Solar Physics Committee, has made most of the observations on meteorites, and low-temperature spectra generally, which have been recorded on the maps, and he has carried out this work with a care, skill, and patience beyond all praise. The observations have in nearly every case been checked also by myself. Mr. Taylor, the Demonstrator of Astronomy, has been chiefly responsible for looking up the literature and mapping the results, in which he has been aided by Mr. Richards.

J. NORMAN LOCKYER.

SIR JULIUS VON HAAST, F.R.S.

SCIENCE in Australasia, and especially in New Zealand, has recently sustained a great loss by the death, on August 16 last, of Sir Julius von Haast. He was born on May 1, 1824, at Bonn, where his father was a wealthy merchant. After passing through the grammar-schools of Bonn and Cologne, he entered the University of Bonn, and devoted a considerable portion of his time to geological and mineralogical studies. He then spent some years in France, and made journeys for the purpose of scientific exploration in Russia, Austria, and Italy. Being invited by an English firm of ship-owners to visit New Zealand on their behalf in order to report upon its fitness as a field for German emigration, he went to London, and accepted their offer after some negotiation; and on December 21, 1858, he arrived at Auckland. The next day, by a lucky chance, the Austrian ship *Novara*—then on its voyage of scientific research—put into Auckland; and when Dr. von Hochstetter was left behind, at the request of the New Zealand Government, he took Mr. Haast as his lieutenant and companion in all his journeys in these islands. After the departure of Hochstetter, Mr. Haast was engaged by the Provincial Government of Nelson to explore the west coast of the province, and in the journey undertaken in the pursuit of these duties he commenced his examination of the physical geography and geology of the Southern Alps. The results of the exploration were published in a report printed by the Nelson Government and dated January 1, 1861.

Immediately after the conclusion of the Nelson journey—namely, in December 1860—he undertook to report to the Government of the Province of Canterbury as to the possibility of constructing a tunnel through the hills which separate Christchurch from its port of Lyttelton; and in the following year he was appointed to the command of the Geological Survey of Canterbury, being thus the first Government geologist in New Zealand. It was in this capacity that he accomplished the most valuable part of his scientific work. The most striking of his achievements were the examination of the Mount Cook district; the sketching and mapping out of the great glaciers of the Southern Alps, named by him the Tasman, Franz Joseph, Hochstetter, Hooker, and Müller glaciers, and many others; and the forecast and subsequent examination made of the auriferous districts of Westland. All this, with the geographical, zoological, botanical, and meteorological researches carried on side by side with the more exclusively geological work, was in continuation of what had been done in the Nelson or northern portion of the same mountain system. The results of his investigations were set forth in the chief book published by him—namely, "The Geology of Canterbury and Westland." He was also the author of many papers in scientific periodicals.

Last year he acted as New Zealand Commissioner at the Indian and Colonial Exhibition. Afterwards he visited Paris, Brussels, Berlin, Dresden, Vienna, Halle, Venice, Florence, and other centres, obtaining a vast number of things for the Canterbury Museum, the flourishing condition of which is mainly due to his energy and zeal. His labour in connection with the Exhibition, and the subsequent wear and tear of travelling while in weak health, appear to have overtaxed his strength, and he died of heart-disease a month after his return to New Zealand.

NOTES.

THE fourth session of the International Geological Congress will be held next year in London. The Congress was founded at a meeting of the American Association for the Advancement of Science at Buffalo in 1876, the first session being held at Paris in 1878, the second at Bologna in 1881, the third at Berlin in 1885. The following is a list of the Organizing Committee appointed to carry out the arrangements:—H. Bauerman, W. T. Blanford, F.R.S., Rev. Prof. T. G. Bonney, F.R.S., Prof. W. Boyd Dawkins, F.R.S., John Evans, F.R.S., Prof. W. H. Flower, F.R.S., Arch. Geikie, F.R.S., Prof. James Geikie, F.R.S., Sir Douglas Galton, F.R.S., Prof. A. H. Green, F.R.S., Rev. Prof. S. Haughton, F.R.S., Prof. T. H. Huxley, F.R.S., W. H. Hudleston, F.R.S., Prof. T. McK. Hughes, J. W. Hulke, F.R.S., Prof. E. Hull, F.R.S., Prof. J. W. Judd, F.R.S., Prof. J. Prestwich, F.R.S., F. W. Rudler, H. C. Sorby, F.R.S., Sir W. W. Smyth, F.R.S., W. Topley, Rev. Prof. Wiltshire, Henry Woodward, F.R.S. The duty of this Committee will be to nominate the officers, to appoint Executive Committees, and to fix the exact date of meeting. The Congress at Berlin requested that the meeting should be held in London between August 15 and September 15.

DR. DAWSON, Assistant-Director of the Canadian Geological Survey, who headed the party sent by the Dominion Government to explore the country adjacent to the Alaska boundary, has returned to Victoria. Two of his party, Messrs. Ogilvie and McConnell, will winter in the district, preparing the way for the establishment of the international boundary. The Expedition so far has secured a great deal of geological, geographical, and general information about the country, which is far from being the Arctic region it is sometimes represented to be. The point from which Dr. Dawson turned back was at the junction of the Lewis and Pelly Rivers. It is 1000 miles north of Victoria. There the flora was found to differ but little from that on the banks of the Fraser. A great deal of open grassy country exists along the stream's tributary to the Yukon. No areas of tundra or frozen swamps, such as are to be met with in the interior of Alaska, were discovered by the Expedition. Dr. Dawson's conclusion is that the whole country, from Cassiar to the vicinity of Forty-mile Creek on the Yukon River (which must be near the eastern boundary of Alaska), yields more or less gold in placer deposits. This would constitute a gold-bearing region fully 500 miles in length, and of indefinite width.

AT a meeting of the Council of University College, Bristol, held on Wednesday, November 16, it was decided, at the suggestion of the staff of the College, to suspend for a year the office of Principal. Prof. Lloyd Morgan was in the meantime appointed academical head of the College, and Chairman of the Educational Board, with the title of Dean.

AT the Royal Institution, Sir Robert Stawell Ball, the Astronomer-Royal of Ireland, will give a course of six lectures (adapted to a juvenile auditory) on Astronomy: the Sun, Moon, Planets, Comets, and Stars. The course will begin on December 27. Courses of lectures will also probably be given by Lord Rayleigh (Professor of Natural Philosophy at the Royal Institution), Dr. G. J. Romanes, Mr. Hubert Herkomer, Prof. C. Hubert H. Parry, the Rev. W. H. Dallinger, and Mr. William Archer.

WE are requested to state that the lectures to be given on behalf of the Anthropological Institute by Mr. Francis Galton—which were postponed in consequence of that gentleman's indisposition—will be delivered in the Lecture Theatre of the South Kensington Museum on Saturday, the 26th inst., and the two following Saturdays, December 3 and 10.

AN International Exhibition will shortly be held by the Ornithological Society at Berlin.

A MAGNETIC Observatory is about to be erected near the Solar Observatory, on the Brauhausberg, near Potsdam.

A CORRESPONDENT writes to us from Venice that on the 9th inst. an earthquake occurred there at 1.32 a.m. There were five gentle undulations, which lasted ten seconds. On the same morning—at 1.30 a.m.—a shock at Ferrara is said to have lasted seven seconds.

THE other day Mr. Ruskin sent to the *Times* the following extract from a letter, dated November 14, which he had received from a friend at Florence :—"We had an earthquake this morning, which frightened everybody, and my door shook so that I thought somebody was trying to break in, and then there was a terrible noise, but I believe no harm done. The bells rang of themselves at the Carmine, and some say that one or two chimneys fell, but nobody seems to know." From a report issued by Signor Passerini, Director of the Meteorological Observatory connected with the Agricultural College of Scandicci, we learn that three shocks were felt there on the 14th, the first taking place about 5.20 a.m. It was accompanied by a rather loud rumbling, and was powerful enough to shake all the furniture in Signor Passerini's room. A second shock, weaker, and not accompanied by rumbling, was felt about twenty minutes later. At 6.49 a.m. the third shock, the strongest of all, and accompanied by loud rumbling, was felt. During the continuance of this shock people in the open country saw trees much shaken, and quantities of leaves were observed to fall. The direction of the shocks was from north-north-west to south-south-east.

AT the suggestion of Prof. Mushketoff, who has just returned from his official visit to Semiretchia, a special Commission has been appointed to watch the course of any earthquakes that may happen, and to report on them, in those parts of the Russian Empire which are most frequently visited, such as the Caucasus, Turkestan, and the Transbaikal region.

ADVICES from Baku state that a naphtha spring has burst forth near the town of Balachany, the oil being thrown to a height of over 100 feet and carried away long distances by the wind. Sometimes the oil falls like rain over the adjacent districts, and forms small streams, whilst heavy naphtha gases fill the air.

ANOTHER contribution to the subject of photography in colours is published by Mr. Carey Lea in the November number of the *American Journal of Science*. Although the interpretations placed upon his former experiments have not received universal acceptance in this country, still the experiments themselves have been generally received with considerable interest and surprise, and indeed are at the present time being repeated and considerably extended in more than one English laboratory. The appearance of another communication from Mr. Carey Lea is therefore most opportune, and will doubtless form the subject for considerable discussion. It will be remembered that the so-called photo-salts of silver, a description of which appeared in these columns a few months ago, were said to consist of combinations of ordinary chloride of silver with small quantities of subchloride. Mr. Carey Lea now finds that silver chloride combines with small quantities of many other chlorides, besides its own subchloride, to form coloured salts, comparatively stable and remarkably less sensitive to light. Thus if silver nitrate be added to a solution of ferric chloride in presence of free hydrochloric acid, the precipitate obtained is buff-coloured, and the ferric chloride carried down by the silver chloride cannot be washed out even by hydrochloric acid. The most remarkable property of this silver-ferric chloride is that it is almost unacted upon by light. Chlorides of cobalt, nickel, manganese, and mercury give analogous combinations, each having a characteristic colour. As those chlorides, such as ferric and mercuric, which readily part with one equivalent of chlorine, act most energetically in reducing the sensitiveness, it appears probable that the traces of chlorine thus capable of being given up, simply hold in check the commencement of the movement towards reduction.

WE have received from Mr. Stewart Culin the reprint of a paper read by him before the Anthropological Section of the American Association at its meeting at New York during the past autumn. It is entitled "China in America: a Study in the Social Life of the Chinese in the Eastern Cities of the United States," and describes the special districts in Southern China from which the immigrants mostly come, the guilds and associations they form, their mode of life, their pleasures, which are somewhat few and simple, and much else in respect to them that is of a very interesting character. His own contact with the Chinese in the United States leads him to form a favourable estimate of their character and attainments, which have been the subjects of much misconception. They are not "the dregs of the people, given up to gambling and opium-smoking, and distinguished only by their vices," as the anti-Chinese orators aver; nor are their mental and moral qualities quite so high as others allege. But we fear very much that Mr. Culin is over sanguine in the anticipation that the returning emigrants will some day carry enlightenment to their own country. Their work is not of a kind that enables them to acquire very great knowledge of the resources of the West; they come with a special object, viz. the acquisition of a competence, they toil unremittingly until that is attained, when they speed home again, usually with no very pleasant memories of the land of their sojourn. To China herself we must look for the elements of her regeneration, and time, which is the great solvent, will have its slow effect on that huge mass of humanity.

THE German publisher, Herr Trewendt, of Breslau, has just issued the twentieth part of a Dictionary of Zoology, Anthropology, and Ethnology; the twenty-fourth and twenty-fifth parts of a Dictionary of Chemistry; and the twentieth part of a hand-book of Botany. These works belong to the elaborate "Encyklopædie der Wissenschaften," edited by Dr. W. Förster, Dr. A. Kenngott, Dr. A. Ladenburg, and other scientific writers.

PROF. FOREL is at present studying the penetration of light into the Lake of Geneva, by means of the photographic effect on chloride of silver paper. Six photographic apparatus are attached one above another to a rope at 10-metre intervals. They are let down into the lake after sunset, left there one day or more, and taken up again at night. The depth-limit of absolute darkness has been found this year, in the beginning of March, 100 m.; of May, 75 m.; and of July, 45 m. Prof. Forel hopes to carry on these experiments for a whole year, every two months, and so obtain the curve for penetration of light into the lake.

THE loss of electricity by a conductor in moist air has been lately studied by Signor Guglielmo (Turin Academy). He finds that with potentials less than 600 volts, moist air insulates as well as dry air, but with higher potentials, there is more loss in moist air, and more the moister the air, and the higher the potential. The potential at which the difference becomes perceptible is the same for a ball as for a fine point. It occurs with extremely smooth surfaces, and so cannot be attributed to discharges in consequence of roughness of surface. With equal potential the loss of electricity has the same magnitude, whatever the dimensions of the balls used as conductors. In air saturated with vapours of insulating substances, the loss of electricity of a conductor is nearly the same as in dry air.

THE large Russian ironclad *Tchesme*, now being finished at Sebastopol, and having a displacement of over 10,000 tons, is to have boilers heated with petroleum. If the results correspond to what the Sebastopol engineers expect, the example is likely to be followed elsewhere. In this connection we may note an account in *La Nature* of November 5, of a gas-boat, as it may be called, the *Volapuk*, recently constructed by M. Forest, in which a gas-engine of six horse-power is driven, not by coal-gas, but by air charged with carbureted hydrogen, by passage through petroleum-oil. There are two pistons, and the explosive mixture is ignited by means of a spark from a magneto-electric arrangement. The engine consumes six litres of petroleum-oil per hour, giving a speed of sixteen kilometres per hour.

AMONG the various uses of celluloid, it would appear (according to the *Annales Industrielles*) to be a suitable sheathing for ships, in place of copper. A French Company now undertakes to supply the substance for this at 9 francs per surface-metre and per millimetre of thickness. In experiments by M. Butaine, plates of celluloid applied to various vessels in January last were removed five or six months after, and found quite intact and free from marine vegetation, which was abundant on parts uncovered. The colour of the substance is indestructible; the thickness may be reduced to 0·0003 metre; and the qualities of elasticity, solidity, impermeability, resistance to chemical action, &c., are all in favour of this use of celluloid.

THE following interesting observations with regard to the mobility of loess have been made by M. Potanin during his last journey through the region south of the Ordos. As wind steadily moves the shifting sands, so also water steadily moves the loess, transporting it from higher to lower levels. The underground water which filtrates through the loess, begins by making in it a kind of cavern; then a circular crevice appears on the surface over the cavern, and a cylindrical vertical hollow, which soon becomes a deep well, is formed through the thickness of the upper layers of the loess. The whole surface of the loess deposits is dotted with such wells, very dangerous to cattle. By and by the formerly cylindrical well begins to extend in the direction in which the underground water flows, and a narrow ravine grows until it joins the main valley. Then masses of loess continually fall down into the ravine, increasing its width. The fall of these masses is favoured by the numerous crevices in the loess, and it is so frequent that natives warn foreigners not to approach the borders of a ravine. Of course the fallen masses are further dislocated by water, and the loess is thus steadily transported at a remarkable speed to lower levels.

HITHERTO it has been generally supposed that the glaciers of the Caucasus are far from having the same development as those of the Alps. It appears, however, from the last researches of Abich, that, although no glaciers of the Caucasus are as long as the Aletsch and Unteraar glaciers, or the Mer de Glace, there are a great many of them. From tables compiled by M. Smirnoff in a recent issue of the *Bulletin* of the Moscow Naturalists' Society, it appears that the average lowest levels of the Caucasus glaciers are: 2504 metres in the Elburz Chain; 2176 metres in the chain to the west of the Adai-kokh; 2266 metres in the high valley of the Ingur; 2898 metres on the eastern slope, and 2238 on the northern slope, of the Kazbek; from 2428 to 2658 metres in Daghestan; 2776 metres on the Great Ararat; and as much as from 3162 to 3194 metres on the Shah-dagh. Comparing these heights with those reached by the lower extremities of glaciers in other highlands, M. Smirnoff concludes that in the main Caucasus ridge the altitudes of the snow-line and the glaciers are intermediate between the corresponding altitudes in the Alps and those in the chains of Central Asia (Thian Shan and Hindu Kush); and that in the western parts of the Caucasus the altitudes of the perennial snow-line are nearer to those of the Austrian Alps. There is some analogy between West Caucasus and the Himalayas, inasmuch as the lowest limits of perennial snow in both chains are higher on the northern slope than on the southern.

A "PANORAMA-BIJOU" (or toy panorama), has been recently brought before the French Société d'Encouragement, by M. Benoist. It is meant to give a succession of connected views of photographed scenery, &c. Externally the instrument appears as a cylindrical case with a handle projecting from its curved surface. The observer looks through a lens, in the axis, towards a mirror inclined 45°, which reflects a panoramic view fixed round the interior of an inner cylinder which is rotated by clockwork. The back of the case is of ground glass, admitting diffuse light. The instrument may be found a suitable companion to the stereoscope on the drawing-room table.

FROZEN fish are now imported into France, and a Society formed in Marseilles for the purpose of developing the trade (the Société du Trident) has a steamer and a sailing-vessel engaged in it. The steamer *Rokelle* lately came into Marseilles with some 30,000 kilogrammes of frozen fish in its hold, the temperature of which is kept at 17° C. below zero by means of a Pictet machine (evaporating sulphurous acid). The fish are caught with the net in various parts of the Mediterranean and Atlantic. After arrival they are despatched by night in a cold chamber. Experiment has shown that fish can be kept seven or eight months at low temperature without the least alteration. These fish are wrapped in straw or marine Algæ, and have been sent on to Paris, and even to Switzerland.

AT the establishment of the National Fish-Culture Association, Delaford Park, the American char, *S. fontinalis*, spawned as early as October 15. The thriving capacity of these beautiful fish is becoming yearly more and more marked. Their rate of growth at Delaford has been extraordinarily rapid.

DR. R. BALTZER, Professor of Mathematics at Giessen University, died at Giessen on November 7. He was born January 27, 1818.

ON October 22 a monument to Prof. Oswald Heer was unveiled in the Zürich Botanical Gardens. The bust of the great Swiss naturalist has been executed in a masterly manner by Prof. Hoerbst.

THE additions to the Zoological Society's Gardens during the past week include fifty-nine Pleurodele Newts (*Molge walti*), seven Marbled Newts (*Molge marmorata*) from Spain, presented by the Lord Lilford, F.Z.S.; two Moufflons (*Ovis musimon* ♂ ♀) from Sardinia, two Barbary Wild Sheep (*Ovis tragelaphus* ♂ ♀) from North Africa, two South American Flamingoes (*Phœnicopterus ignipalliatus*) from South America, deposited; ten Silky Bower Birds (*Ptilonorhynchus violaceus*) from New South Wales, eight received in exchange, and two deposited; an African Wild Ass (*Equus tæniopus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

AMERICAN OBSERVATORIES.—It is reported that the Dearborn Observatory of the Chicago Astronomical Society is to be removed to Evanston, Ill., the North-Western University at Evanston having made an advantageous offer with respect to it. A large astronomical Observatory is proposed to be erected in connection with the Lake Forest University, Governor Ross, President of the Board of Trustees of the University, having guaranteed the cost. A new Observatory has been established at Smith College, Northampton, Mass., and Miss Mary E. Byrd, formerly assistant at Carleton College Observatory, has been appointed Director. The equipment of the Observatory at Carleton College, Northfield, Minn., is proceeding rapidly, the new meridian-circle by Repsold is already erected, and one of the two large steel domes is in place. The telescope it is to cover, an 8¼-inch refractor by Alvan Clark, will, it is expected, be ready for use within a few days. Mr. Grinnell, the founder of Grinnell, Iowa, has furnished funds for the erection of an Observatory to be attached to the Iowa College, and the building is being rapidly brought to completion. The new Observatory is to have an 8-inch equatorial by the Clarks. Prof. Asaph Hall is to act as the Consulting Director of the Washburn Observatory, whilst Prof. George Comstock will have the more immediate superintendence of the institution as Associate Director.

U OPHIUCHI.—Mr. S. C. Chandler gives, in No. 162 of *Gould's Astronomical Journal*, an investigation of the light-curve of this well-known Algol-type variable, the result of which seems to indicate a curious but well-marked retardation in the increase of brilliancy some half-hour or so after minimum is passed. A similar irregularity has been noticed in the light-curve of S Cancri, and occasionally in that of Algol. It is clearly of great importance to ascertain whether this is merely subjective, due to some habit of observation, or a real peculiarity of the star itself. If the latter, it would throw considerable doubt on the satellite theory, which at present seems on the whole the most plausible explanation of variability of the Algol type.

THE NEW ALGOL VARIABLES.—Mr. Chandler also gives an ephemeris for the minima of the two new Algol-type variables, viz. R Canis Majoris, R.A. 7h. 14·3m., Decl. 16° 11′ S., and Y Cygni, R.A. 20h. 46·6m., Decl. 34° 10′ N., as follows:—Y Cygni, Nov. 26, 22h. 42·5m.; Nov. 29, 22h. 36·1m.; Dec. 2, 22h. 29·7m. R Canis Majoris, Nov. 29, 18h. 48·3m.; Nov. 30, 22h. 4·2m.; Dec. 2, 1h. 20·1m. Greenwich civil time, reckoning from midnight to midnight.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *NOVEMBER* 27—*DECEMBER* 3.

(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 27

Sun rises, 7h. 40m.; souths, 11h. 47m. 44·8s.; sets, 15h. 56m.: right asc. on meridian, 16h. 12·0m.; decl. 21° 8′ S. Sidereal Time at Sunset, 20h. 21m.

Moon (Full on November 30, 15h.) rises, 15h. 5m.; souths, 21h. 52m.; sets, 4h. 50m.*: right asc. on meridian, 2h. 17·7m.; decl. 8° 30′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 52 ...	10 39 ...	15 26 ...	15 3·4 ...	14 39 S.
Venus.....	3 10 ...	8 46 ...	14 22 ...	13 9·8 ...	5 23 S.
Mars	0 59 ...	7 21 ...	13 43 ...	11 45·1 ...	3 33 N.
Jupiter....	6 15 ...	10 49 ...	15 23 ...	15 12·8 ...	17 1 S.
Saturn....	20 26*...	4 13 ...	12 0 ...	8 35·8 ...	19 2 N.
Uranus ...	3 1 ...	8 36 ...	14 11 ...	12 59·5 ...	5 39 S.
Neptune..	15 39 ...	23 20 ...	7 1*...	3 46·6 ...	18 7 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Dec.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
1 ...	119 Tauri...	... 5½ ...	16 28 ...	17 18 ...	65 248
1 ...	120 Tauri...	... 6 ...	17 0 ...	17 46 ...	38 272

Dec.	h.	
...	9 ...	Venus at greatest elongation from the Sun, 47° west.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·3 ...	81 16 N. ...	Nov. 27,	1 27 *m*
			Dec. 2,	1 6 *m*
Algol	3 0·8 ...	40 31 N. ...	,, 3,	5 57 *m*
λ Tauri...	3 54·4 ...	12 10 N. ...	Nov. 29,	4 32 *m*
			Dec. 3,	3 25 *m*
U Monocerotis ...	7 25·4 ...	9 33 S. ...	,, 1,	*m*
S Cancri	8 37·5 ...	19 26 N. ...	,, 3,	0 41 *m*
S Boötis	14 19·3 ...	54 20 N. ...	Nov. 29,	*M*
β Lyræ...	18 45·9 ...	33 14 N. ...	Dec. 1,	22 0 *M*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 1,	*M*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 2,	2 0 *M*
S Sagittæ	19 50·9 ...	16 20 N. ...	,, 1,	5 0 *m*
δ Cephei	22 25·0 ...	57 50 N. ...	Nov. 30,	0 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near λ Persei	60° ...	50° N. ...	Very swift.
α Can. Ven. ...	194 ...	42 N. ...	Very swift; streaks.

GEOGRAPHICAL NOTES.

AT the International Exhibition to be held at Brussels next year, a special Section will be devoted to topography, cosmography, geography, and the related sciences. The following are the classes of objects desired for contribution to the Section:—(1) Maps and atlases, topographical, geographical, geological, hydrographical, astronomical, &c.; (2) physical maps of all kinds, plans in relief, terrestrial and celestial globes and spheres; (3) statistical works and diagrams, tables and ephemerides for the use of astronomers and navigators; (4) general, historical, and classical works; (5) instruments, aide-memoires, and articles of equipment for explorers. Among the "desiderata" are the following:—(1) The best map of the Congo, showing the most recent discoveries; (2) the best national map of any country; (3) utilization of the sheets of a topographical map for the preparation of special maps on the same or a different scale; (4) the execution of relief-maps; (5) transference of relief to a plane surface; (6) construction of an apparatus suitable to demonstrate by experiments the various geographical features which may be presented by a river, such as torrents, lakes, cataracts, and rapids, erosions and alluvial accumulations, subterranean streams, islands, and backwaters, freezing and breaking up of ice, floods, deltas, bars, &c.; (7) construction of a tellurium; (8) portable equipment for an explorer; (9) statistical atlases and globes. The Secretary of the Section is Prof. Du Fief, 22 Rue des Palais, Brussels.

IN the *Verhandlungen* of the Berlin Geographical Society, No. 8, Dr. Mense describes in some detail a journey up the Kwango, the great southern tributary of the Congo, which he made last December in company with the Rev. G. Grenfell. It contains a good deal of local information.

THE November number of the *Alpine Journal* contains Mr. D. Freshfield's diary during his recent visit to the Caucasus, when he ascended some of the highest peaks, and visited some of the principal glaciers. The diary itself and the many excellent illustrations of the peaks and glaciers visited will be found to afford useful geographical information.

AT the last meeting of the Paris Geographical Society, Dr. Verneau described the results of his recent missions to the Canary Islands. His special aim was to work out the ethnology of the islands, and for that purpose he has collected many skulls and bones from caves and graves, and made many observations on the present inhabitants. The Guanches he professes to recognize as the direct descendants of a people the type of which is exhibited in the famous prehistoric Cro-Magnon skull—the troglodytes of the Vézère. He maintains that about the end of the Quaternary there must have been a great migration of what he calls the "Cro-Magnon" race from the north to the south, and a section of the migrants found their way to the Canaries. After a lapse of time these were invaded by Numidians and Semites from the north of Africa, people of a superior type and more advanced culture to the Guanches, who were troglodytes. Dr. Verneau has made many collections of anthropological interest from the Canaries, and these are likely to be of much more service to science than his theories.

METEOROLOGICAL NOTES.

MR. H. ALLEN has contributed an article to the *American Meteorological Journal* for October, on the behaviour of pressure and temperature in low and high pressure systems. Recent investigations by M. Dechevrens (and others) tend to show that, while a high temperature accompanies a low pressure at sea-level, the fluctuations are reversed at some height above sea-level. Mr. Allen maintains that this conclusion is not supported by his examination of observations made on Mount Washington, where the minimum pressure does not coincide with the passage of the storm centre over the station, but lags about eleven hours behind it, and he considers that this fact explains the peculiar results obtained by M. Dechevrens. The same number also contains an article by Prof. F. Waldo, "On the Absolute Reduction of Wind Observations at Sea." He recommends the use of some instrument to assist the judgment of different observers, at the actual time of observation.

THE results of meteorological observations made at the Radcliffe Observatory, Oxford, in the year 1884, contain daily means of eye observations and of the self-recording instruments, comparisons of the mean monthly temperatures at 5 and 105 feet above the ground, and rainfall observations on the ground at 22 and 112 feet. Interesting tables are given showing the relations of pressure, temperature, &c., under different winds. The total sunshine during 1884 was 1260·9 hours, being 173·7 hours less than the mean of five years. The observations are reckoned for astronomical and for Greenwich mean time.

ON September 19, 1887, the Russian Government gave notice that storm signals (consisting of day and night signals) would be made at their principal ports in the Black Sea. The signals are shown for forty-eight hours, unless instructions are received to lower them before that time has elapsed: also, the cause assigned for hoisting each signal will be posted up at the respective signal stations. The day signals consist of a cone, hoisted either alone, or with a drum, both painted black, and each about 3 feet in diameter. The night signals consist of three *red* lights, hoisted at the angles of an equilateral triangle, of the same size as the cone used by day. These signals correspond to those in this country—except that the drum is not now used, and night signals are only exhibited at very few stations.

IN *La Nature* of November 12 M. Jules Girard contributes an article entitled "The Probable Temperature of the Pole," based upon the results of the circumpolar expeditions of 1882–83, and upon the observations of some earlier expeditions, in which he has tabulated the mean temperatures for each month. From these data the author traces two principal centres of intense cold, one in the north of Siberia near the mouths of the Lena, and the other to the north of Hudson's Bay, near Boothia. The lowest mean temperature quoted for July is 30° at Jeannette Island, to the north of the islands of New Siberia, and the lowest mean for January is − 49° at Fort Yukon, Alaska.

THE BRITISH ASSOCIATION AND LOCAL SOCIETIES.

THE third Annual Conference of Delegates of Corresponding Societies was held at Manchester, thirty-two of these affiliated Societies having nominated Delegates to attend the meeting. The following Report of the Conference, signed by Mr. Francis Galton as Chairman, and by Prof. R. Meldola as Secretary, has just been issued:—

At the first meeting of the Conference the chair was taken by Prof. W. Boyd Dawkins, F.R.S., the Corresponding Societies Committee being represented by Dr. J. G. Garson and Prof. R. Meldola, F.R.S., Secretary.

The Chairman, in opening the proceedings, stated that the British Association was anxious to be brought into as close a relationship as possible with the local Societies of this country. The work carried on by many of these Societies was of the greatest value to science, and it was felt that their efforts might be promoted by simplifying and unifying their labours. The present meeting was called for this purpose, and for that of bringing together the representatives of the various Corresponding Societies.

The Secretary read the Report of the Corresponding Societies Committee which had been presented to the General Committee of the Association at the meeting on Wednesday, August 31.

The names of the Delegates who desired to be attached to the Sectional Committees as "Delegate Members" were collected by the Secretary in accordance with the resolution passed at the Conference last year at Birmingham (see last Report, NATURE, vol. xxxv. p. 78).

The Chairman called upon the Delegates to make any statements respecting the action that had been taken by their Societies with reference to the suggestions put forward last year, and which had been embodied in the Report just read.

Prehistoric Remains Committee.—Mr. J. W. Davis stated that the Prehistoric Remains Committee had been carrying on their work during the past year, and they proposed to apply for reappointment. Two reports had already been obtained relating to the bronze implements of the East and West Ridings of Yorkshire, and several others had been promised for next year.

Preservation of Stonehenge.—With reference to the preservation of Stonehenge, Dr. Garson stated that the resolution which had been submitted last year to the Delegates at the Birmingham Conference had been considered by the Committee of Section H, and, having been adopted by them, had been brought before the General Committee, and also accepted. He believed that in consequence of this action negotiations were now going on between the Council of the British Association and the proprietor of these remains.[1]

Prof. Boyd Dawkins remarked that the state of neglect into which Stonehenge had been allowed to fall had by no means been overstated in the resolution referred to. A person had recently been seen on a ladder chipping off pieces from the horizontal stone of one of the trilithons.

Ancient Monuments Act.—The Chairman and Dr. Garson made some remarks in explanation of the working of the Ancient Monuments Act. It was pointed out by the latter that the local Societies could do good service by inducing the proprietors of prehistoric remains to communicate with General Pitt-Rivers, the Inspector of Ancient Monuments, with the object of placing these remains under Government protection. The Chairman urged those Delegates who represented the Northern, and especially the Scotch Societies to use their influence in inducing the owners of ancient remains to assist in carrying out the objects of the Act. In reply to a question by Mr. F. T. Mott, as to whether camps and earthworks were to be taken into consideration, the Chairman did not think that any Government could be expected to become a landowner to the extent of all the earthworks in the country.

Provincial Museums Committee.—With reference to the work of this Committee, Mr. F. T. Mott stated that they had been engaged during the past year in collecting particulars respecting museums other than those in London. Considerable assistance had been given by the Secretaries of many of the local Societies. If the Committee was reappointed, as he hoped it would be, he thought there were one or two matters on which the local Societies might possibly render still more valuable aid. The Report of the Committee was not yet passed, but it would, no doubt, be read in the course of the present meeting of the Association, and would then be accessible.

Prof. Boyd Dawkins stated that the schedule issued by this Committee was a very difficult one to fill up, and he expressed a hope that something shorter and simpler would be sent out.

The Rev. H. Winwood expressed similar views.

Mr. Robert Pullar and Mr. J. W. Davis mentioned two museums which the Committee had not heard of—viz. that of the Perthshire Society of Natural Science at Perth, and Mr. Davis's museum at Chevinedge, Halifax.

Earth Tremors.—Prof. Lebour stated that the subject of earth tremors, which he had brought forward at the Conference of Delegates last year at Birmingham, had since taken a more

[1] The following extract relating to this matter is from the Council Report for 1886–87, presented at the Manchester meeting:—
"That the Council be requested to consider the advisability of calling the attention of the proprietor of Stonehenge to the danger in which several of the stones are at the present time from the burrowing of rabbits, and also to the desirability of removing the wooden props which support the horizontal stone of one of the trilithons; and in view of the great value of Stonehenge as an ancient national monument, to express the hope of the Association that some steps will be taken to remedy these sources of danger to the stones."
The Council have carefully considered the question, and, having had the advantage of perusing the detailed report recently prepared by a deputation of the Wilts Archæological and Natural History Society on the condition of the whole of the stones constituting Stonehenge, are of opinion that the proprietor should be approached with the expression of a hope that he will direct such steps to be taken as shall effectually prevent further damage.

practical shape, and that it now seemed to be time that a Committee of the British Association should be formed for taking the investigation in hand. Through the advocacy of Mr. Symons, who was unable to be present at the Conference, Sections A and G had that morning agreed to recommend the appointment of such a Committee in conjunction with Section C, which Section would be approached next day. The work to be done was of a preliminary character, and its object was rather to inquire into the best methods of conducting observations on earth tremors than to actually cause such observations to be made. The North of England Institute of Mining and Mechanical Engineers had, since the Birmingham meeting, carried on a series of seismoscopic observations at Marsden in the county of Durham; and the daily results, extending over several months and compared with a barometric curve, were shown to the meeting in the form of a diagram by Mr. Walton Brown, the Secretary of the Newcastle Institute Committee. The Institute possessed also a more elaborate instrument, made after a pattern supplied by Prof. Ewing, which registered the intensity and direction of the tremors. Prof. Lebour stated that, although such instruments as the last mentioned were probably too costly to be placed at all desirable stations, this would not be the case with the simpler seismoscope, which recorded merely the fact of earth tremors having taken place and the time of their occurrence. Such records would be valuable, though limited. The Corresponding Societies, if they would interest themselves in the matter, might be the means of establishing a great network of seismoscopes with a few seismographs in suitable localities, and results of value would by this means be in all probability obtained. These results would be valuable altogether in proportion as well-equipped seismometrical observing stations were numerous. The expense must in any case be considerable in the aggregate, but need not be great in individual cases. A sufficiently good seismoscope might be had for about £2, a seismograph for £14 to £15, and the cost of keeping them in order would not be great. Prof. Lebour hoped the Delegates present would help in establishing such a network of observing stations all over the country, and he stated, in concluding, that he would be happy to communicate with anyone interested in the subject.

Prof. Ewing, in response to the Chairman, said that from his experience of earth-tremor observations in Japan he could concur in the remarks of Prof. Lebour. To investigate fully the character of the motion, even at one station, required delicate and costly apparatus, and the cost was greatly increased when it was attempted to bring a number of stations into correspondence so as to determine the motion over a large area. It was possible, however, to record the fact that a tremor had occurred, and even to learn something of its character by means of inexpensive seismoscopes; and it certainly seemed to him that no bodies could more appropriately undertake that work than the local Societies represented at the Conference acting in conjunction with a Committee of the Association. From recent observations it appeared probable that tremors would be found wherever they were tested for with sufficient delicacy, so that a Society undertaking the search was not likely to be disappointed.

At the second Conference the chair was taken by Prof. Boyd Dawkins, F.R.S., who was succeeded by Mr. W. Topley, the Corresponding Societies Committee being further represented by Mr. G. J. Symons, F.R.S., Dr. Garson, Mr. William White, and Prof. R. Meldola, F.R.S., as Secretary.

The Chairman invited discussion on the recommendations received from the various Sections.

Section A.

Temperature Variation in Lakes, Rivers, and Estuaries.—The following resolution was forwarded to the Secretary of the Conference by the Secretary of this Section:—

"That Mr. John Murray, Prof. Chrystal, Dr. A. Buchan, Rev. C. J. Steward, Hon. R. Abercromby, Mr. J. Y. Buchanan, Mr. David Cunningham, Mr. Isaac Roberts, Dr. H. R. Mill, and Prof. Fitzgerald be appointed a Committee to arrange for an investigation of the seasonal variations of temperature in lakes, rivers, and estuaries in various parts of the United Kingdom in co-operation with the local Societies represented at the Association; and that Mr. John Murray be Secretary."

Dr. H. R. Mill, as representing this Committee, stated that the question proposed had not been fully worked out, but that the few observations made showed relations of a very interesting kind. As a branch of meteorology, this research was particularly promising, and was one in which the co-operation of local Societies would be valuable. He proposed that the Societies situated in the neighbourhood of rivers and estuaries which were willing to undertake this work should appoint some member to observe the temperature daily or weekly, as the case might be, in accordance with the rules to be drawn up by the Committee. It was first proposed to ascertain how many observers would offer themselves in various parts of the country, then to draw up a scheme of observations and arrange for this being adopted.

Mr. G. J. Symons pointed out the necessity in such observations for having a well-considered scheme drawn up, as well as for having absolutely reliable thermometers, without which no observations would be of value. He also asked whether it was proposed that the cost of the instruments should be met by a grant from the British Association, or whether the Societies taking part in the observations should provide their own thermometers.

Mr. De Rance remarked that in the case of the Committee which had been formed for the observation of underground temperatures, and of which Prof. Lebour was a member, the thermometers had been supplied by the Association.

Mr. J. W. Davis raised the question as to whether it would be of use to extend the observations to the streams in manufacturing districts. He also asked what the Committee proposed to consider as an estuary.

The Rev. H. Winwood remarked that it would be necessary in all cases to record the depth at which the thermometer reading was taken. As a point of interest bearing upon the proposed observations, he stated that it had been observed that the temperature of the lakes in the Hebrides had been unusually high this year.

Prof. Lebour stated that the thermometers used by the Underground Temperature Committee had been supplied by the Association, but these instruments were very costly, and only a few observers had taken part in the work. He was of opinion that, if numerous Societies took part in the observations, these should in each case bear the expense.

Dr. Garson expressed a hope that the temperatures would be recorded on the Centigrade scale.

Dr. Mill, in reply, said that he understood that the fact of the investigation being sanctioned by a Committee of Section A was a sufficient guarantee that it should be carried out in a thoroughly scientific manner with properly verified instruments of a uniform pattern, and employed in the same way. The experience of the Scottish Marine Station for three years suggested many precautions which should be adopted in this work. The temperature of streams in manufacturing districts should certainly be ascertained in as many cases as possible, in order to find whether the increase of temperature of a river passing through a manufacturing town is in any sense permanent. The term "estuary" should in his opinion be used as meaning all parts of a tidal river between the upper limit of the tide and the open sea. Each local Society should be asked to supply its own thermometers, but all these should be verified at Kew, or compared by some person appointed by the Committee. The observations would, of course, be made on a uniform plan, and it would, probably, be found more convenient to use the Fahrenheit scale, but the readings could be easily converted, if necessary.

Section C.

Mr. C. E. De Rance, who represented this Section, referred to the work of the three Committees which he had brought under the notice of the Delegates on former occasions, viz.: (1) The Underground Waters Committee; (2) The Erratic Blocks Committee; and (3) The Sea Coasts Erosion Committee. (See last Report.)[1]

The first of these Committees requires information as to the depth of wells, the sections passed through, the height at which the water stands before and after pumping, daily records of the height and chemical analyses of the waters.

The *Erratic Blocks Committee* wants information as to the position, size, and character of boulders of foreign origin that

[1] The constitution of these Committees remains as last year. The Secretaries are:—
Underground Waters, C. E. De Rance, 28 Jermyn Street, London, S.W.
Erratic Blocks, Rev. H. W. Crosskey, 117 Gough Road, Edgbaston, Birmingham.
Sea Coasts Erosion, Wm. Topley, 28 Jermyn Street, London, S.W.
The schedules and all other information will be furnished on application at the above addresses.

may occur in drift-covered areas, and are anxious that the position of the same should be noted on the 1-inch map of the Ordnance Survey.

The *Sea Coasts Erosion Committee*, like the other two Committees, has a circular form of inquiry, which can be obtained on application to Mr. Topley.

With reference to the work of this last Committee, Mr. Topley stated that but little assistance had as yet been received from the local Societies. The Natural History Society of the Isle of Man had undertaken to collect information; and all similar Societies in maritime counties might greatly assist the Committee by local observation as to present changes, and by researches as to past conditions of the coast.

With respect to the work of the Erratic Blocks Committee, Prof. Meldola said that he had been authorized to state, on behalf of the Manchester Geological Society, that several members of that Society had been interesting themselves in the distribution of boulders in their district, and it was expected that their results would be available by the next meeting of the Association. It was also mentioned that Mr. Adamson had been rendering assistance to this Committee on behalf of the Yorkshire Naturalists' Union.

Mr. Ralph Richardson, as the representative of the Edinburgh Geological Society, pointed out that Scotland had been omitted from the localities dealt with by the Erratic Blocks Committee. He stated that much work in this field had already been carried out under the auspices of the Royal Society of Edinburgh, and he hoped the Committee would be able to utilize their results.

Earth Tremors Committee.—Prof. Lebour stated that since the last meeting of the Conference the formation of a Joint Committee by Sections A, C, and G has been agreed to, and the resolution forwarded to the Committee of Recommendations. The resolution was the following:—

"That Sir F. J. Bramwell, Mr. E. A. Cowper, Mr. G. J. Symons, Prof. G. H. Darwin, Prof. Ewing, Mr. Isaac Roberts, Mr. Thomas Gray, Dr. John Evans, Prof. Lebour, Prof. Prestwich, Prof. Hull, Prof. Meldola, and Prof. Judd be a Committee for the purpose of considering the advisability and possibility of establishing in other parts of the country observations upon the prevalence of earth tremors, similar to those now being made in Durham in connection with coal-mine explosions, and that Prof. G. A. Lebour be the Secretary."

Mr. Symons and Mr. Topley made some remarks on the work of this Committee.

Mr. De Rance remarked that the proposed observations might possibly under certain circumstances become connected with the work of the Underground Waters Committee. Thus the Essex earthquake of April 22, 1884, had caused a rise in the level of the water in Messrs. Courtauld's well at Bocking, which had reached its maximum in June of the same year. Since then the level had been gradually falling, and at its present rate it might be expected that the water would be at the same level as it was before the earthquake about next August.

Section D.

Life-Histories of Plants.—Prof. Meldola said that during a recent visit to Oxford he had had an opportunity of hearing a suggestion in the course of a conversation with Prof. Bayley Balfour, which had appeared to him as likely to be of use to the members of local Societies. He had therefore invited Prof. Balfour to attend the Conference and explain his views on the suggested subject, but as that gentleman was prevented from being present he had forwarded the following communication:—

"It appears to me that much good scientific work might be done by members of local Societies in a direction which has not attracted so much attention in Great Britain as it deserves. The discovery and description of new forms, and the distribution of our indigenous plants, are in botany the lines upon which most of the energies of local Societies are principally spent, whilst habit, construction, and generally the features of life-history of plants come in for attention in quite a secondary way. This arises, I think, in great part from the prevalent notion that the facts of the life-history of our common plants are all well known, and that there is little, if anything, more to find out about them. That this is an erroneous idea may easily be shown—witness, for example, the interesting observations recently published by Sir John Lubbock—and there is a field for a great deal of sound work upon plants growing at our doors.

"Within recent years Mr. Darwin's work, followed up by that of such men as Hermann Müller, Kerner, Ogle, and others, has given a stimulus to observations of adaptations between the vegetable and animal kingdoms in connection with pollination in flowers; and many interesting facts about British plants have been brought to light by workers in local Societies. Bnt little has been done for the subject of the vegetative organs of these plants—I mean the arrangement, true nature, and structure of the members that carry on plant-life. In Germany, many years ago, Wydler and Irmisch published a splendid series of contributions to the knowledge of these features in indigenous German plants—why has this not been done for Britain?

"Now, I venture to think that good results would follow if you would bring before the Delegates at the meeting to-day the importance of encouraging the members of their Societies to study the life-histories of indigenous plants in their entirety, *i.e.* from the stage of embryo in the seed up to the production of fruit and seed again. Anyone who will take up this line of study will assuredly derive great pleasure from it, and will be able to add a great deal to the sum of our knowledge of plant-life. Such work can be well combined with the more usual systematic work; it can be easily accomplished, and it will be found to give much additional interest to the study of British botany."

Mr. C. P. Hobkirk considered that Prof. Balfour's letter was a very important one, and that, as therein suggested, the time and energies of the members of local Societies would be far more usefully employed by following the lines indicated by Prof. Balfour than, as at present, in simply collecting, naming, and registering local plants. As far as he was concerned, he was prepared personally, and also on behalf of the Yorkshire Naturalists' Union, which he represented, to do everything in his power to assist in carrying out practically Prof. Balfour's most useful proposition. Although the compilation of local floras was most useful and necessary work, yet the actual life-history of individual forms was now of really paramount importance, and members of local Societies should be urgently requested to carry on this work without delay.

Section H.

Ancient Monuments Act.—The Secretary read the following communication from General Pitt-Rivers:—

"I am much afraid I shall not be able to be present at the meeting of Delegates of local Societies on Tuesday; but the subject is so important for the preservation of these monuments that in case I am not there I write in order that you may know what my view of the matter is.

"Perhaps I cannot do better than state in a few words what the work of the Inspector of Ancient Monuments is, and you will then see what kind of progress is likely to be made without some assistance such as has been proposed,[1] and in what way the assistance of local Societies can be given.

"You are probably aware that in the original Act of 1882 fifty ancient monuments in Great Britain were scheduled as monuments to which the Act could apply at once if the owners were willing. Some persons suppose that by scheduling these monuments they were actually placed under the Act, but this is not the case. The scheduling was done without the knowledge or consent of the owners, and their consent had to be obtained both for these and for every other monument that has been since added to the list. This has entailed the examination and survey of all these monuments which are distributed over England, Scotland, and Wales. The addresses of the owners had to be obtained, and this could only be done on the spot. After that the owners had to be visited personally, for I soon found an official letter, without a verbal explanation, almost invariably produced a refusal. On this account I have of late found it advisable never to approach an owner without a personal introduction, or without doing it in such a way as to induce him to consider the matter favourably. This mode of procedure for the whole country has, of course, taken a long time, and the result has been that about half of these fifty monuments have been voluntarily put under the Act by their owners, and of the remainder some of the proprietors have refused, whilst in the case of others it has been found impracticable owing to peculiarities in the ownership. All the monuments have, however, been carefully surveyed, planned, and drawn, and in every case in which there has been a refusal the owners have stated their intention of taking good care of the

[1] This refers to the work of the Prehistoric Remains Committee of the British Association.

monuments themselves. In one case only a camp has been partly damaged, and this owing to mining operations involving a question of a large sum of money which made it impossible for the Government to interfere. Other non-scheduled monuments have since been added to the list, and the number is steadily but not rapidly increasing.

"The Government makes no allowance for an assistant; not even so much as a man to hold the end of the tape in measuring, without which no proper survey of the monuments can be made, and I have to employ a private assistant, whom I take about with me at my own cost. With his assistance, and by dividing the work with him—I making the necessary notes and measurements while he is drawing—each monument takes on an average about one day; without an assistant the time would be about doubled. After this the owner has to be visited, and as he generally lives at a distance from the monument, this frequently takes another day or more. A great deal of this time might be saved by the assistance of persons living in the localities and with better chance of success.

"I issued a circular to a number of local Societies inviting them to co-operate, but few responded. One instance, however, shows what may be done in this way. Sir Herbert Maxwell has not only sent me the addresses of several owners in Wigtonshire and Kirkcudbrightshire, but, by using his influence with these, has been the means of placing several monuments under the Act. I would suggest that the same course might well be followed by others.

"The recommendation I would make is this:—Local Societies should (1) report to me what monuments in their district they think worthy of being put under the Act; (2) they should send me the names and addresses of the owners; (3) they should communicate with the owners, and, if possible, obtain their consent to have the monuments placed under the Act, subject, of course, to their subsequent acceptance by the Office of Works; and (4) they should report to me any damage that they find being done or contemplated either to the monuments under the Act, or to others not so protected. With such assistance I think that much more rapid progress may be made."

Prehistoric Remains Committee.—Mr. J. W. Davis stated that this Committee had been recommended for reappointment by the Committee of Section H. The recommendation is as follows:—

"That Sir John Lubbock, Dr. John Evans, Prof. Boyd Dawkins, Dr. R. Munro, Mr. Pengelly, Dr. Hicks, Mr. J. W. Davis, Prof. Meldola, and Dr. Muirhead be reappointed a Committee for the purpose of ascertaining and recording the localities in the British Islands in which evidences of the existence of prehistoric inhabitants of the country are found; and that Mr. J. W. Davis be the Secretary."

Prof. Lebour suggested that it would be convenient if, in registering prehistoric remains, the Committee would adopt a uniform scheme of signs—if possible, an international one.

Mr. William Gray stated that the work of registering ancient remains had been carried on for twenty-five or thirty years by members of their Society (Belfast Naturalists' Field Club) and others in Ireland, and they had long felt the want of some central organization such as that of the present Committee. He also alluded to the necessity for a uniform system of signs.

Mr. William White remarked upon the difficulty which private individuals often experienced in approaching the proprietors of ancient remains, and pointed out that individual efforts would be likely to be more successful if members of local Societies could make overtures backed up by the sanction of a British Association Committee such as the present one.

Work of the Corresponding Societies Committee.—The Secretary stated that during the present meeting of the Association an important resolution had been framed at the instigation of Sir Douglas Galton, with the object of extending the powers of their Committee. According to the present rules the Committee was nominated by the Council and appointed by the General Committee, but they had no power of submitting resolutions or recommendations to the Committee of Recommendations or to the General Committee. The present resolution, which was calculated to give them the necessary power, and thus to put them on the same footing as the Committees of the Sections, was as follows:—

"That the Conference of Delegates of Corresponding Societies be empowered to send recommendations to the Committee of Recommendations for their consideration, and for report to the General Committee."

The Secretary had succeeded that morning in getting this resolution passed by the Committees of Sections B and C, and it had been forwarded by them in due form to the Committee of Recommendations, by whom it had also been accepted. It was subsequently submitted to the General Committee, and accepted by them on the understanding that the recommendations so forwarded should not clash with the recommendations sent up by the Sectional Committees.

The Secretary remarked that he would take the present opportunity of explaining away a misunderstanding that had arisen on the part of some of the local Societies. Some of these had nominated Delegates to attend the Manchester meeting without having previously submitted any claim for election as Corresponding Societies. Such Delegates could not be officially recognized by the Association, as it was only those Societies which had been admitted as Corresponding Societies, and which were still on the list, that were thus entitled to be officially represented. According to the Rules no Society can be admitted without first sending in a formal application, accompanied by a specimen of its publications; this application would be considered by the Corresponding Societies Committee, and only in the event of the Society being recommended for election by this Committee, and this recommendation confirmed by the General Committee, would it be admitted to the privileges of a Corresponding Society.

At the termination of the meeting a vote of thanks was passed to Prof. Meldola, on the motion of Prof. Lebour, for the services which he had rendered as Secretary to the Committee and to the Conferences.

THE METEOROLOGY OF OXFORD.[1]

THE forty-second volume of the Observations of the Radcliffe Observatory has recently been published, and is in nearly all respects a continuation of the previous publications. The Radcliffe takes precedence of all our British Observatories as regards the length of time over which the published hourly observations of atmospheric pressure and temperature extend; to which is to be added a commendable fullness, far from common, with which many other observations have been made and given to the public for a long term of years.

At Oxford, atmospheric pressure attains the maximum, 29·760 inches, in June, and falls to the minimum, 29·677 inches, in March, to which the mean of October, 29·680 inches, closely approximates. The annual mean is 29·720 inches; the highest during the previous thirty years being 29·785 inches in 1858, and the lowest 29·572 inches in 1872, the year to be long remembered for its excessive rainfall. Temperature rises to the maximum, 61°·7, in July, and falls to the minimum, 38°·8, in January, the annual mean being 49°·2. The warmest year was 1868, with a mean of 51°·4, and the coldest mean 45°·5 in 1879. Of individual months, the warmest was July 1859, the mean of which was 66°·5, while the mean for February 1855 was only 29°·5, giving thus a mean monthly range of 37°·0. The rainfall reaches the maximum, 2·81 inches, in October, and falls to the minimum, 1·62 inch, in March, and the mean annual amount is 26·42 inches. The extreme annual amounts were 40·42 inches in 1852 and 17·56 inches in 1870. The month of heaviest rainfall was October 1875, when 7·53 inches fell, and the lightest fall was 0·18 inch in September 1865, when temperature was unusually high for the season.

The diurnal curves of pressure approach closer than those of any other British Observatory of which we have records to the seasonal phases of these curves for continental situations. On the mean of the year, the first minimum occurs about 4 a.m., and the maximum at 9 a.m.; and the second minimum at 3.30 p.m. and maximum at 10 p.m.,—the former being earlier in summer and later in winter, whereas the afternoon phases are the reverse of this. In June the time between the first and second maximum is 14½ hours, but in winter only 12 hours.

Of quite exceptional interest are some of the other diurnal phenomena at Oxford, notably the diurnal distribution of thunderstorms, sheet lightning, and auroras. We have compiled the following table showing the sums of the times of occurrence

[1] "Results of Meteorological Observations made at the Radcliffe Observatory, Oxford, in the Year 1884, under the Superintendence of E. J. Stone, F.R.S." (Oxford, 1887.)

of these phenomena during the several hours of the day for the twenty years ending 1884:—

Hours.	Thunder-storms.	Sheet lightning.	Auroras.	Hours.	Thunder-storms.	Sheet lightning.	Auroras.
	Summer—June, July, August.		Year.		Summer—June, July, August.		Year.
Midt. to 1 a.m.	9	14	10	Noon to 1 p.m.	26	0	0
1 to 2 a.m.	5	4	2	1 to 2 p.m.	24	0	0
2 to 3 a.m.	5	1	2	2 to 3 p.m.	21	2	0
3 to 4 a.m.	6	1	1	3 to 4 p.m.	29	2	0
4 to 5 a.m.	4	0	0	4 to 5 p.m.	17	2	0
5 to 6 a.m.	6	0	1	5 to 6 p.m.	22	4	5
6 to 7 a.m.	4	0	1	6 to 7 p.m.	22	3	10
7 to 8 a.m.	7	1	0	7 to 8 p.m.	5	12	26
8 to 9 a.m.	5	0	0	8 to 9 p.m.	3	22	31
9 to 10 a.m.	8	0	0	9 to 10 p.m.	5	41	27
10 to 11 a.m.	7	1	0	10 to 11 p.m.	5	40	25
11 to noon.	21	1	0	11 to midnight	5	26	16

Thus the daily maximum for thunderstorms is from about noon to 7 p.m., being the period of the day covered by the afternoon minimum of atmospheric pressure in summer; but the maximum for sheet lightning is from 8 p.m. to midnight, being the period embraced by the afternoon maximum of pressure. The absolute daily maximum for sheet lightning, it will be observed, does not occur till from 9 to 11 p.m., or till some time after dusk, and cannot therefore be accounted for by increased visibility as darkness sets in. The opinion is widespread that sheet lightning is merely the reflection of a distant flash of lightning. The Oxford observations show, however, that only a small percentage of all the cases admit of being explained in this way. In connexion with the well-defined maximum from 9 to 11 p.m. it may be remarked that there is no region of the globe nearer Oxford than America where thunderstorms with the accompanying true lightning have the daily maximum at the same physical time, 9 to 11 p.m. G.M.T., when sheet lightning has its daily maximum at Oxford.

The curve for auroras has its diurnal maximum substantially at the same time as sheet lightning, or during the time of the evening maximum of pressure. The agreement of these two maxima with this portion of the daily curve of pressure is all the closer when it is considered that the evening maximum of pressure is from one to two hours later in summer when the sheet lightning was observed than in the autumn and spring months when the great majority of auroras occur. These results are of the greatest importance with respect to recent theories regarding thunderstorms, and to suggested connexions between the aurora in arctic and sub-arctic regions and the lightnings of low latitudes. The time of occurrence of the maxima of aurora and sheet lightning from 9 to 11 p.m. indicates, perhaps, a more direct connexion between these phenomena and the evening maximum of pressure than has been suspected. This maximum is mainly due to an overflow of upper aërial currents back to eastward from the longitudes to westward, where at the time the afternoon pressure is at the minimum ("Encyc. Britt.," *Meteorology*, p. 122); and hence at these hours there is more aqueous vapour spread through the higher regions of the atmosphere in its gaseous and fluid states, and also in the solid state of minute spicules of ice, even though no cloud in the finest pencilled forms of the cirrus be visible.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Drs. Routh and Glaisher, Prof. J. J. Thomson, and Mr. A. R. Forsyth have been appointed Examiners in Part II. of the Mathematical Tripos of 1888.

The following appointments of Natural Science Examiners have been made:—Physics: Profs. J. J. Thomson and W. G. Adams. Chemistry: Prof. H. E. Armstrong and Mr. H. J. H. Fenton. Mineralogy: Messrs. T. W. Danby and H. A. Myers (British Museum). Botany: Prof. I. B. Balfour and Dr. S. H. Vines. Physiology: Dr. W. H. Gaskell and Prof. G. F. Yeo. Zoology: Messrs. H. Gadow and W. F. R. Weldon. Geology: Prof. C. Lapworth and Mr. A. Harker. Human Anatomy: Prof. J. Cleland and Dr. A. Hill. Pharmaceutical Chemistry: Mr. Pattison Muir.

At a meeting of the Senate in the Arts School recently, general approval was expressed of the scheme for providing a new room for botanical microscopy. The scheme for new anatomical and physiological rooms was not so entirely approved, some persons wishing to retain the ugly old Anatomical Museum and buildings, and also considering that the requirements of the Medical School had not been sufficiently considered.

Mr. W. Bateson, M.A., Fellow of St. John's College, has been elected to the Balfour Studentship.

Group E (Natural Science), in the Higher Local Examination, attracts a diminishing number of candidates, we are sorry to see. Only 36 presented themselves this year as against 73 in 1879; but 10 candidates gained a first class this year, as against 4 in 1879: 35 failed then, only 5 this year. Elementary Biology is reported on fairly this year; but Elementary Chemistry does not seem to have been studied practically, and problems were not satisfactorily dealt with. Only four candidates passed in Physics. The Physiology, Zoology, and Geology papers were well answered; but in Botany the general standard was decidedly low.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, November 2.—Dr. D. Sharp, President, in the chair.—Mr. Stevens exhibited a specimen of *Acidalia immorata*, L., purchased by him some years ago at the sale of the collection of the late Mr. Desvignes. He remarked that specimens of the insect lately captured near Lewes had been described last month by Mr. J. H. A. Jenner as a species new to Britain.—Mr. Adkin exhibited, and made remarks on, a series of male and female specimens of *Arctia mendica* from co. Cork; also, for comparison, two specimens of *A. mendica* from Antrim, and a series of bred specimens from the London district.—Mr. Enoch exhibited a specimen of *Calocoris bipunctatus* containing an internal parasitic larva.—Dr. Sharp exhibited three species of Coleoptera new to the British list, viz. *Octhebius auriculatus*, Rey, found some years ago in the Isle of Sheppey, but described only quite recently by M. Rey from specimens found at Calais and Dieppe; *Limnius rivularis*, Rosenh., found by Dr. J. A. Power at Woking; and *Tropiphorus obtusus*, taken by himself on the banks of the Water of Cairn, Dumfriesshire.—Dr. Sharp also exhibited a *Goliathus* recently described by Dr. O. Nickerl as a new species under the name of *Goliathus atlas*, and remarked that the species existed in several collections, and had been supposed to be possibly a hybrid between *G. regius* and *G. cacicus*.—Mr. Eland Shaw exhibited two species of Orthoptera, which had been unusually abundant this year, viz. *Nemobius sylvestris*, and *Tettix subulatus*.—Mr. E. B. Poulton exhibited the cocoons of three species of Lepidoptera, in which the colour of the silk had been controlled by the use of appropriate colours in the larval environment at the time of spinning up. He said this colour-susceptibility had been previously proved by him in 1886 in the case of *Saturnia carpini*, and the experiments on the subject had been described in the Proc. Royal Society, 1887. It appeared from these experiments that the cocoons were dark brown when the larvæ had been placed in a black bag; white when they had been freely exposed to light with white surfaces in the immediate neighbourhood. Mr. Poulton stated that other species subjected to experiment during the past season afforded confirmatory results. Thus the larvæ of *Eriogaster lanestris* had been exposed to white surroundings by the Rev. W. J. H. Newman, and cream-coloured cocoons were produced in all cases; whilst two or three hundred larvæ from the same company spun the ordinary dark brown cocoons among the leaves of the food-plant. In the latter case the green surroundings appeared to act as a stimulus to the production of a colour which corresponded with that which the leaves would subsequently assume. Mr. Stainton suggested that larvæ should be placed in green boxes, with the view of ascertaining whether the cocoons would be green. It had been suggested that the cocoons formed amongst leaves became brown because the larvæ knew what colour the leaves would ultimately become. The discussion was continued by Mr. Waterhouse, Dr. Sharp, Mr. McLachlan, and others.—Mr. S. Klein read "Notes on *Ephestia kuhniella*," and exhibited a number of living larvæ of the species, which he said had been recently doing great damage to flour in a warehouse in the East of London.—Mr. A. G. Butler contributed a paper "On the species of the Lepidopterous genus *Euchromia*; with descriptions of new species in the collection of the British Museum."—Lord Walsingham communicated a note substituting the generic name *Homonymus* for the generic name *Ankistrophorus*—which was preoccupied—used in his "Revision of the genera *Acrolophus* and *Anaphora*," recently published by the Society.

PARIS.

Academy of Sciences, November 14.—M. Janssen in the chair.—Note on certain definitions in mechanics, and on the unities in current use, by M. de Freycinet. In supplement to the remarks already made in his treatise on mechanics, the author here deals more fully with the notions involved in such terms as *force*, *weight*, *mass*, *bulk*, and shows that considerable advantage might be gained by slightly modifying the generally accepted unities. Fresh definitions are suggested of the unities of length, volume, weight, force, velocity, &c.—On the state of the potassa present in plants and the soil, and on its quantitative analysis, by MM. Berthelot and André. In continuation of a previous communication on this subject, the authors here study the condition and process of analysis of the potassa in living plants, and in the humus produced by their disintegration.—On waterspouts, by M. D. Colladon. In reply to M. Faye's strictures, the author illustrates his views by means of an instantaneous photograph, showing that under certain conditions two waterspouts may be generated, one ascending, the other descending, and crossing each other.—On MM. Houzeau and Lancaster's "Bibliographie Générale de l'Astronomie," by M. Faye. A well-merited eulogium is passed on the authors of this great work, who have earned the lasting gratitude of astronomers for accomplishing their vast undertaking in such a thoroughly satisfactory manner. The Bibliography constitutes a systematized catalogue of all astronomical publications that have appeared from the remotest times down to the present day. Although not absolutely exhaustive, the omissions do not appear on examination to be very numerous; but unfortunately only 300 copies have been issued of a work which should find a place in every Observatory and in every scientific library in the world. M. Houzeau has enriched the first volume with a valuable philosophic history of astronomy, which will be found extremely interesting, especially to those students who have been unable to follow the recent discoveries of specialists on the state of astronomical science amongst the Egyptians, Assyrians, and other ancient peoples.—New nebulæ discovered at the Observatory of Paris, by M. G. Bigourdan. These discoveries have been made during the years 1884–87 with the equatorial of the West Tower. Most of the nebulæ are very weak, and some, indicated as more or less stellar, might, strictly speaking, be regarded as simple stars, it being often quite impossible to distinguish between a small nebula and a star of small magnitude. The positions are approximately given for the mean equinox of 1860·0, in order to facilitate comparison with J. Herschel's "General Catalogue of Nebulæ and Clusters of Stars," and its supplement by Dreyer.—On the theory of magnetism, by M. P. Duhem. From a comparative study of magnetic and diamagnetic bodies the theorem is deduced that all magnetic bodies are attracted from great distances by permanent magnets, but that nothing can be affirmed regarding diamagnetic bodies. A theorem is also established which sets forth the difference between magnetic and diamagnetic bodies, and some remarks are appended regarding the magnetizing of crystals.—Measurement of the heights and movements of clouds in Spitzbergen and Upsala, by M. Nils Ekholm. These comparative studies are based on fifty meteorological observations taken during the Swedish expedition of 1882-83 to Spitzbergen, conducted by the author.—On a new method of formation of safranines, by MM. Ph. Barbier and Léo Vignon. Having in a previous communication explained a special method of forming substituted safranines, the authors here describe a new process for producing phenosafranine and its homologues.—On a new artificial serum intended to dilute the blood for the purpose of counting its globules, by M. Mayet. For the serum here described it is claimed that it is free from the disadvantages of others in general use. It consists of distilled water, 100 gr.; neutral phosphate of anhydrous and pure sodium, 2 gr.; with cane sugar to raise the density of the liquid to 1·085.—On antipyrine as a remedy against sea-sickness, by M. Eugène Dupuy. The author declares that for some time back he has successfully employed this substance as a prophylactic against sea-sickness. He recommends a dose of 3 gr. to be taken daily for three days before sailing, to be continued if necessary during the voyage. Without claiming to have discovered an absolute specific, he considers that the success hitherto attending the use of antipyrine justifies the hope that in this substance we possess a more or less efficacious remedy against one of the chief terrors of travelling by sea.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Dictionary of Place-Names: C. Blackie, 3rd edition (Murray).—Report of the Commissioner of Agriculture, 1885 (Washington).—The Cremation of the Dead: Dr. H. Erichsen (Haynes, Detroit).—Down the Great River: Capt. W. Glazier (Hubbard, Philadelphia).—The Creator, and what we may know of the Method of Creation: Dr. W. H. Dallinger (Woolmer).—Primo Saggio sui Ragni Birmani: Prof. T. Thorell (Genova).—Le Pétrole: W. de Fonvielle (Hachette).—Ants, Bees, Dragon-Flies, Earwigs, Crickets, and Flies: W. H. Bath (Sonnenschein).—Through Central Asia: Dr. H. Lansdell (Low).—The Volcanic Origin of Epidemics; Are Epidemics Contagious? Dr. J. Parkin (Low).—Bulletin of the U.S. Fish Commission, vol. vi., 1886 (Washington).—Mineralogy: F. Rutley (Murby).—A Vertebrate Fauna of Sutherland, Caithness, and West Cromarty: Harvie-Brown and Buckley (Douglas).—Bulletin of the U.S. National Museum, No. 32, Catalogue of Batrachians and Reptiles of Central America and Mexico: E. D. Cope (Washington).—Archiv für Pathologische Anatomie und Physiologie, und für klinische Medicin, Hund. Bandes, Zweites Heft (Reimer, Berlin).—Botanische Jahrbücher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Neunter Band, ii. Heft (Engelmann, Leipzig).—Transactions of the County of Middlesex Natural History and Science Society, 1886-87 (Mitchell and Hughes).—Records of the Geological Survey of India, vol. xx. part 3.—Bulletin of the California Academy of Sciences, vol. ii. No. 7.—Annalen der Physik und Chemie, 1887, No. 11 (Leipzig).—Beiblätter zu den Physik und Chemie, 1887, No. 10 (Leipzig).—Transactions of the Asiatic Society of Japan, vol. xv. part 1.

CONTENTS.

PAGE

Charles Darwin. By **Prof. T. G. Bonney, F.R.S.** . . 73

Our Book Shelf :—

Roberts: "A Treatise on the Integral Calculus" . . . 75

Todhunter: "Solutions to Problems contained in a Treatise on Plane Co-ordinate Geometry" 75

De Bary: "Lectures on Bacteria" 75

Desmaux: "Mattie's Secret" 76

Letters to the Editor :—

Politics and the Presidency of the Royal Society.—**Prof. Balfour Stewart, F.R.S.; Prof. Alex. W. Williamson, F.R.S.** 76

"The Conspiracy of Silence."—**Prof. T. G. Bonney, F.R.S.** 77

Instability of Freshly-Magnetized Needles.—**Prof. Francis E. Nipher; Prof. Arthur W. Rücker, F.R.S.** 77

Greek Geometry.—**R. T.** 78

The Chromosphere.—**John Evershed, Jun.** 79

Perception of Colour.—**C. E. Stromeyer** 79

Swifts.—**C. B. Witchell** 79

Note on a Madras Micrococcus.—**Edgar Thurston** . 79

Catharinea undulata.—**J. Reynolds Vaizey** 79

Researches on Meteorites. II. (*Illustrated.*) By **J. Norman Lockyer, F.R.S.** 80

Sir Julius von Haast, F.R.S. 87

Notes 87

Our Astronomical Column :—

American Observatories 89

U Ophiuchi 90

The New Algol Variables 90

Astronomical Phenomena for the Week 1887 November 27—December 3 90

Geographical Notes 90

Meteorological Notes 91

The British Association and Local Societies 91

The Meteorology of Oxford 94

University and Educational Intelligence 95

Societies and Academies 95

Books, Pamphlets, and Serials Received 96