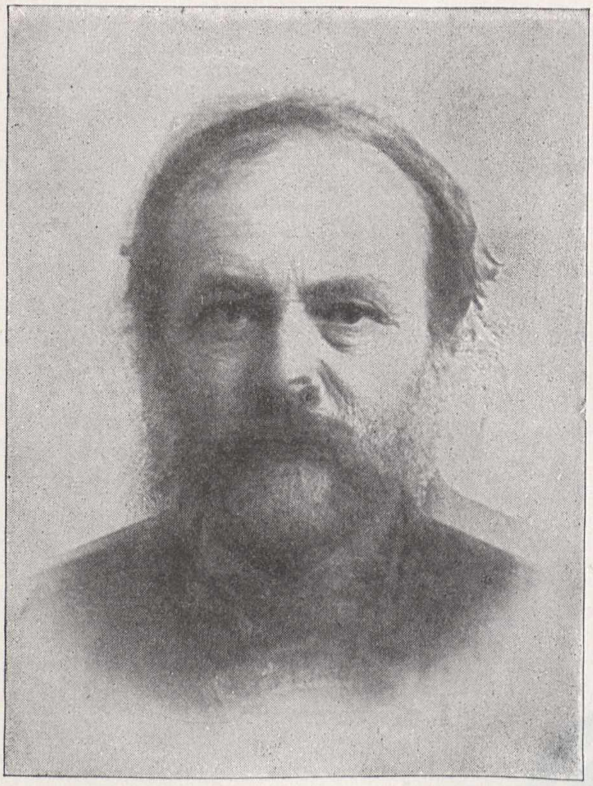

FIG. 1.—TWELVE MATHEMATICIANS.

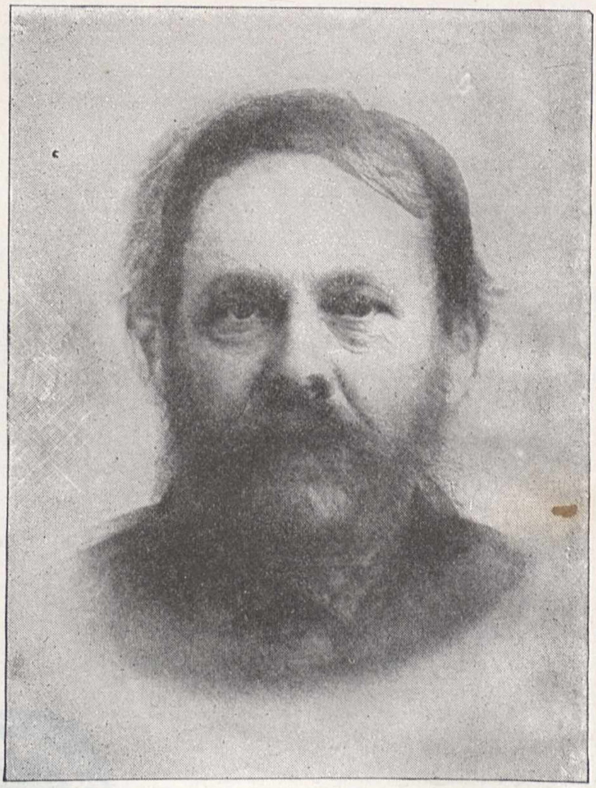

FIG. 2.—SIXTEEN NATURALISTS.

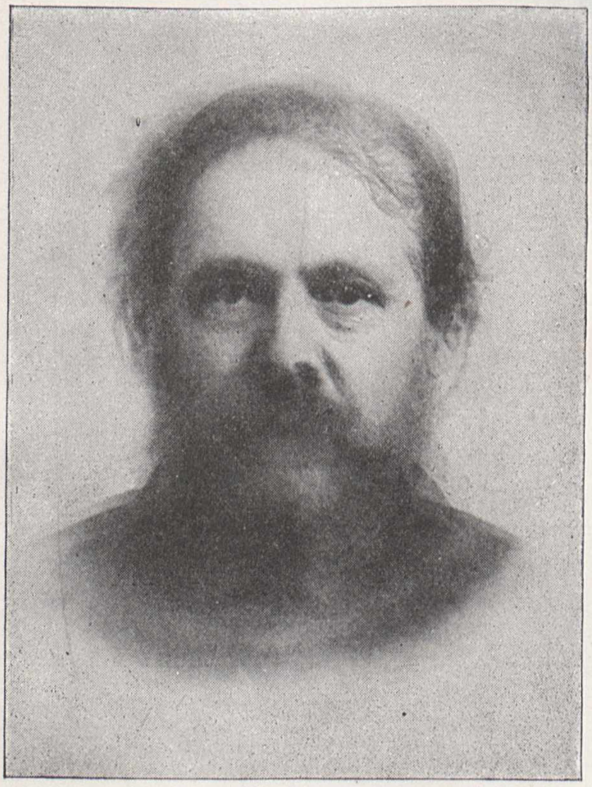

FIG. 3.—THIRTY-ONE ACADEMICIANS.

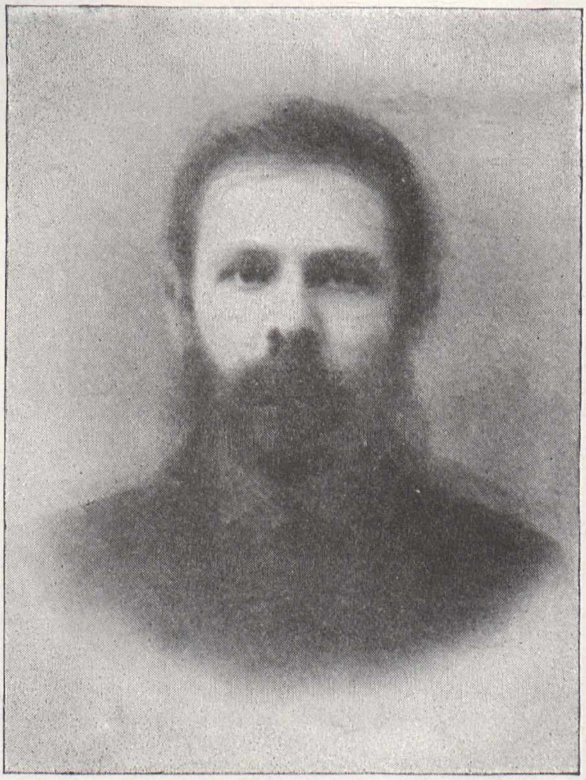

FIG. 4.—TWENTY-SIX FIELD-GEOLOGISTS, TOPOGRAPHERS, ETC.

COMPOSITE PORTRAITS OF AMERICAN SCIENTIFIC MEN.

Nature

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

THURSDAY, MAY 7, 1885

GREEK MATHEMATICS

A Short History of Greek Mathematics. By James Gow, M.A. (Cambridge University Press, 1884.)

"THERE are three classes of persons who, being mathematical students, require to know something of the history of their pursuit. The first want only a general view of leading points, such as can be furnished by one writer in a few volumes. The second wish to be able to compare the accounts given by different persons, and, up to a certain point, to examine the authorities used by those persons, or at least to keep watch upon their mode of using them. The third are desirous of being the critics of the historians, and of amending their works, if need be."[1] The catalogue, which the writer of this paragraph drew up, was intended for the second of the above classes. In some further remarks he arranges the histories under two heads—those which are written on the plan of Montucla, Bossut (we may now add M. Marie's "Histoire des Sciences mathématiques et physiques"), in which a general account is framed out of the writer's notes or remembrances of miscellaneous reading; or in that of Delambre, Woodhouse (we may add here the name of Todhunter, whose great historical treatises the late Henry Smith pronounced to be "so suggestive of research, and so full of its spirit"), in which the successive writings of eminent men are examined and described one after the other, so that each chapter or section is a description of the progress of science in the hands of some one person, and is complete in itself. The latter, De Morgan goes on still further to say, is the plan which is most favourable to accuracy and most interesting to the inquirers of the third class; the former, while it better suits the first and second class, leaves the writer open to many sorts of error which the latter avoids.[2]

[1] De Morgan, "References for the History of the Mathematical Sciences," Companion to "British Almanac" for 1843.

[2] Both M. Marie and Mr. Gow might profit by De Morgan's remarks on Indices. "No writer is so much read as the one who makes a good index, or so much cited." The former author may intend to give a thoroughly full index at the end of his seven volumes; the latter gives a fair index, but it is very far from being complete and satisfactory; for instance, "*et passim*" is not such a reference as one desires.

Mr. Gow's work being upon a special branch, viz. Greek mathematics—which he himself further limits to arithmetic, algebra, and geometry—comes under the second of the above two divisions, though for reasons which are more than once put forward, it is not so thorough a treatise as we could have wished. When, however, we learn that the book "represents part of a collection of notes which I have for many years been making with a view to a general history of the great City of Alexandria," and that "the materials for an account of the Alexandrian Mathematical School grew to exceed the reasonable limits of a chapter," we are glad that Mr. Gow determined to publish his results at an earlier date than he would otherwise have done. What of accuracy or perfection is sacrificed by a perhaps too early publication, he will have, we expect, an early opportunity of making good in a second edition, which we hope will be called for in the near future. It is a great reproach to English mathematicians that such books as this and M. Marie's have hitherto been conspicuous by their absence in this country. We can happily point to papers by De Morgan, to special treatises by Todhunter, to monographs by Allman, and to an interesting *résumé* by Dr. C. Taylor, but we look in vain for anything of the nature of a history of mathematical or physical science in the English language. A tendency of late years to give small historical notices of mathematical discoveries in our school text-books has been displayed, and we trust the time is not far distant when we shall have, if not a great original work, for which we can hardly look, yet a primer or primers founded upon the works of Bretschneider, Cantor, Hankel, Marie, and others.

Almost every page puts in evidence how greatly Mr. Gow is indebted to German and French writers; yet it is also evident, on a perusal of his work, that he is no blind follower of those predecessors in the field—he calls no one of them master—but when occasion arises he boldly differs from them, and gives good reasons for so differing. We note here that he does not appear to be acquainted with M. Paul Tannery's work in the same directions as his own. He refers to him but once (p. 101), and then he states he has not been able to find the article (quoted by Cantor). The journal in which the paper is published, viz. *Bulletin des Sciences Math. et Astronomiques*, is an easily accessible one, and we think from the analyses we have from time to time given in these columns of other papers by M. Tannery (in *Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux*) on Greek arithmetic and geometry, our author would have gathered useful material in the *Bulletin* paper and in the *Mémoires* also.[1] But this is the only case of omission we have come across; the reading is apparently most thorough, and the author's Greek scholarship enables him to improve upon the translations given by some of these foreign authorities.

The work consists of three parts. The first part, entitled "Prolegomena to Arithmetic," discusses the decimal scale and Egyptian arithmetic in a very thorough manner. Here, of course, much use is made of the "Rhind papyrus," a book written by one Ahmes (now put at 1700 B.C.), entitled "Directions for Obtaining the Knowledge of all Dark Things," which consists mainly of statements of results. One could wish some safe means could be discovered by the Museum authorities for unfolding the "olden leather-roll on a mathematical subject" which is "apparently too stiff to be opened"! (note, p. 16).

There are naturally statements in these early chapters which are fairly open to objection, but they are clearly put, and the results, as Mr. Gow gets them, are summarised on pp 20, 21.

The second part treats of Greek arithmetic under "Logistica, or Calculation and Arithmetica, or Greek Theory of Numbers." This part is very carefully done, and enables the reader to get a clear idea of the processes employed. Plato's appreciation of *logistic* may be inferred from his direction (Legg, 819 B) that "free boys shall be taught calculation, a purely childish art, by pleasant sports, with apples, garlands, &c."[2]

The third part treats of Greek geometry, and upon it we could expatiate at some length, but that is hardly our business on the present occasion. We need only say that there is much good work. Dr. Allman's powerful rectification of the position of Eudoxus did not appear in time to be of service to Mr. Gow (he mentions the fact of its publication on p. x. of the Addenda). Most of the geometers appear to have justice done them. We miss some of the touches which appear in M. Marie's work, but again we find a compensation in the fuller account given of Menelaus, and of the proposition now usually cited by the name of that geometer. Chapter V. discusses "prehistoric and Egyptian geometry," in which is given an account of Ahmes' work. Chapter VI. takes "Greek Geometry to Euclid" in five sections. Of the Pythagoreans, the Eudemian summary (which has in previous numbers been referred to in our notices of Dr. Allman's papers) says they made geometry "a liberal education;" and other writers, referred to by Mr. Gow, attribute to them the maxim, "A figure and a stride: not a figure and sixpence gained" (p. 153). In connection with this characteristic maxim we may give the story, which, in the Greek, forms the motto on the title-page of Mr. Gow's book, viz. "A youth who had begun to read geometry with Euclid, when he had learnt the first proposition, inquired 'What do I get by learning these things?' So Euclid called his slave, and said: 'Give him threepence, since he must make a gain out of what he learns.'" Many such boys there are, even in this nineteenth century, who are ever asking, "What is the use of learning Euclid?" We thank Mr. Gow for his story from Stobæus, which will possibly make us better prepared to answer the question the next time we are asked it. There is much other quotable matter, but we hasten to a close.[1] Chapter VII. gives an account of Euclid (what little is known of him, his writings, history of text of "Elements," and modern history of the book[2]), Archimedes, and Apollonius. Chapter VIII. is on "Geometry in Second Century B.C.;" Chapter IX., "From Geminus to Ptolemy;" and Chapter X., "Lost Years," principally occupied with an account of Pappus and his "Mathematicæ Collectiones."

Some matters of interest are illustrated, as the introduction of the *signs* in algebra, of the *sine* in trigonometry (it does not seem to be generally known that the first occurrence of "tangent" and "secant" is traced by De Morgan to a work by T. Finkius, "Geometriæ rotundi libri xiiii.," Basileæ, 1583), the derivation of "almagest" (cf. Chaucer's Clerk Nicholas, who had—

"His almageste and bokes grete and smáll,
His astrelabre, longing for his art,
His augrim stones, layen faire apart
On shelves couched at his beddes head")

and a few others.

On page 290, line 9 up, for λη read αη.

[1] In the *Bulletin* for March, 1885, there is a paper by M. Tannery, "Sur l'Arithmétique Pythagorienne" (pp. 69 88).

[2] It is curious to note that there was a Cocker before Cocker: cf. the Lucianic compliment, "You reckon like Nicomachus of Gerasa"; see also, in the opposite direction, "Budget of Paradoxes," p. 30.

[1] Hippocrates of Chios was one of the greatest geometers of antiquity; he lost his property, as a merchant, by piracy or chicanery. Aristotle speaks of him as "slow and stupid." "There seems to be no other ground for the criticism than that a Greek would call a man a fool who was cheated of his property. There are still extant mathematicians who are singularly deficient in ability for any studies but their own."

[2] In his "English Mathematical and Astronomical Writers" (companion to "British Almanac for 1837," p. 38), De Morgan made one of his shrewd guesses that Billingsley's (first English) Euclid was certainly made from the Greek, and not from any of the Arabico-Latin versions. This surmise has been found correct by G. B. Halsted in the *American Journal of Mathematics*, vol. ii. pp. 46-48. We notice that Mr. Gow gives the same reference in his Addenda. Mr. Gow gives a proof of a prop. (xxii.) of Euclid's optics which recalls a passage in the recent *brochure* "Flatland:" it is, "If a circle be described in the same plane as the eye, it will seem to be a straight line."

OUR BOOK SHELF

The Zoological Record for 1883, being Volume XX. of the Record of Zoological Literature. Edited by E. C. Rye, F.Z.S., &c. (London: John Van Voorst. 1884.)

ALTHOUGH bearing on its title-page the date 1884, it was not until the end of January in this year that the "Zoological Record for 1883" was, in its entire form, laid before the public. It comes to us with a melancholy interest, as being the last under the editorship of the late Mr. Rye, whose untimely death we have so recently recorded and deplored. Again in this volume we have to mention still further changes in the staff of the Recorders. Prof. Sollas takes Mr. S. O. Ridley's place as recording the sponges, and Prof. Haddon that of Mr. W. Saville Kent in recording the Protozoa. Other engagements have prevented the Rev. O. P. Cambridge recording the literature of the Arachnids for 1883, and it has been arranged that Mr. T. D. Gibson-Carmichael is to record the literature of this group for 1883 and 1884 in the next volume of the "Record."

A rapid glance over the contents of the volume brings to light the fact that in all the leading groups of the animal kingdom a goodly amount of work has been accomplished in the year 1883, and that many of the lacunæ in our knowledge are being steadily filled in. The Mollusc-oidea seem to have had more than ordinary attention paid to them, and the record of this group by Prof. E. von Martin appears to be extremely well done. As usual, Messrs. W. H. Kirby and R. McLachlan record the enormous section of Insecta, the lion's share falling to the former, the latter confining his attention to the Neuroptera and Orthoptera. In his treatment of the general subject (Insecta) the recorder frequently quotes memoirs relating to the structure, &c., of the groups recorded by Mr. McLachlan, and it is not without interest to note that, while some of these are the subjects of a double record, others are not. One interesting fact, showing the importance which a "Zoological Record," when complete, is to the working naturalist, is alluded to by Mr. McLachlan in his remarks introducing us to H. de Saussure ("Mémoires pour servir à l'Histoire naturelle du Mxeque des Antilles, et des États-Unis. Orthoptères de l'Amérique moyenne: Famille des Blattides." Genève, 1864):—"This very important memoir is noticed at the request of the author. It escaped notice in the early volumes of this 'Record' (which commenced with the year 1864), and also in the German *Bericht*. It would also appear to have escaped the notice of workers on Blattidæ generally, for none of the new terms employed therein for generic, &c., division are included in Scudder's just-published laborious 'Universal Index' which extends down to 1879." Scudder's New Index is, however, far from being a full record of generic names in any one group.

The new names proposed for genera or sub-genera, as recorded in this volume, amount, the editor informs us, to 1079, as against 1015 of last volume, and this without including any of the Arachnidæ. Of these, no less than 115 require re-naming, having been already in use. This number affords no clue to the amount of new species described, which is considerably larger, thus indicating for the present no lack of work for the systematic zoologist.

The British Association for the Advancement of Science still continues its grant of 100*l*., and the Government Grant Committee of the Royal Society renewed its vote of 150*l*., while the Zoological Record Association itself keeps up both the number of its members and subscribers.

A Treatise on Practical Chemistry and Qualitative Inorganic Analysis. By Frank Clowes, D.Sc. Lond. Pp. xv., 376. Fourth Edition. (London: J. and A. Churchill, 1885.)

THIS well-known manual has reached a fourth edition. It very thoroughly fulfils the aim which is set forth in the preface, viz. to place trustworthy and practical methods of qualitative analysis in the hands of the student. If the chemical student must still devote a large amount of his time to qualitative testing, then he certainly could not do better than follow the directions of this book. But the very excellence of the tables and methods of the book before us makes us more than ever doubt the wisdom of attempting to teach the science of chemistry by a course of "test-tubing." The art can be learnt by rules and formulæ, but the science comes not by such as these.

This book only includes what "directly bears on the ordinary requirements of the laboratory student"; its directions are those of a man who knows what he is writing about, and who has learnt what he teaches by good honest work in the laboratory. It contains many of those results of laboratory experience which are usually preserved in the private note-books of the teacher, and which may almost be regarded as trade secrets. The only fault we have to find is that the book tends too much in the direction of recipes. Were a student to work conscientiously through the book he would certainly be an accomplished analyst, but we are afraid he might have ceased to be a chemist. However excellent rules and tables may be in their own way, it is possible to have too much of them. In fact, the better they are the less one wants to be bound by them. The "tables of differences" given in the book are excellent; in the hands of a good teacher they might be made the basis of a really scientific training. But the ordinary student will not trouble to develope methods from the facts set before him in these tables; he will pass on to the systematic examination of simple salts, and be caught in the fatal whirlpool of "experiment," "observation," "inference." M. M. P. M.

Original Researches in Mineralogy and Chemistry. By J. Lawrence Smith. Edited by J. B. Marvin. (Louisville, 1884.)

IN a recent number (vol. xxxi. p. 220) we gave a statement of the life and work of the late Prof. J. Lawrence Smith condensed from a memoir prepared at the request of the National Academy of Sciences, Washington, by Prof. B. Silliman, who was so soon to follow his friend to his long rest. The papers containing the original investigations of Prof. L. Smith have now been collected together and reprinted as a memorial volume intended for presentation to his friends. Three memoirs prepared by Mr. Marvin, Mr. Michel, and Prof. Silliman respectively, form an appropriate introduction, and give one a good glimpse into his life and character. The work is clearly printed on good paper, and will be highly appreciated by his numerous friends, to each of whom a copy has been presented by his widow.

Lehrbuch der Mineralogie. Von Dr. Gustav Tschermak. Zweite, verbesserte Auflage. (Wien: Alfred Hölder, 1885.)

WE are glad to find that a second edition of this work is already called for, although the latter part of the first edition appeared so lately as 1884. In our notice of the first part of that edition (vol. xxiv. p. 355) we directed attention to the excellent character of the work, and gave a brief statement of its contents; we now need only remind our readers that the author is a thorough master of his subject, who has done a large amount of original and valuable work, and further, has had a long teaching experience as Professor of Mineralogy in the University of Vienna. The work is but slightly changed in the present edition; the length is increased by a few pages through the incorporation of the results of investigations made since the first part left the press in 1881; the contents are well up to date. If some University Professor would provide us with an equivalent work written in our own tongue the study of mineralogy in this country would begin to revive.

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.*]

Mr. Lowne on the Morphology of Insects' Eyes

I DESIRE to give an unqualified denial to the imputation made by Mr. Lowne in his letter to NATURE of April 9 (p. 528), that my opinion with regard to his paper on the structure of the eye in Arthropods was formed under the influence of my colleague, Prof. Lankester, or that any consultation upon the paper took place between us. References of papers for the Royal Society are strictly confidential, and I did not know the name of the second referee until after I had come to a conclusion upon the subject—a conclusion which was only arrived at as the result of a long and patient investigation of Mr. Lowne's preparations, and the nature of which may be inferred from the fact that I advised the author to withdraw his paper and submit the subject to a renewed investigation, with the aid of improved methods.

With regard to the question, which Mr. Lowne raises, as to my competency to form any opinion at all, on the ground that I had not myself devoted any special attention to the literature of the subject, I may remark that the points which had practically to be decided were (1) whether Mr. Lowne's statements were in themselves probable, and (2) whether they were corroborated by his preparations. Had I not felt myself qualified to form an opinion on these points I should not have accepted the reference. E. A. SCHÄFER

The Late Prof. Clifford's Papers

IN the "Mathematical Papers" (pp. 628–37) I was able to print the syllabuses of a series of ten lectures delivered by Prof. Clifford to a class of ladies at South Kensington in the spring and summer of 1869. Whilst turning over a collection of miscellaneous papers, in a box, Mrs. Clifford and I had the good fortune to light upon a manuscript quite ready for printing, and this ("Mathematical Papers," p. 628) subsequently formed part of the volume on "Seeing and Thinking;" but we could not find any trace of any more manuscript of the above-mentioned series of lectures. Just before the recent Easter holidays Prof. Karl Pearson returned to me a few pages of manuscript bearing on the International Scientific Series volume which I had lent him, and with them he sent me a large note-book which had been in the late Prof. Rowe's hands. On opening this book I at once saw that it contained very full notes of other lectures of the course. In fact, Lecture II. ("On Plane Surfaces and Straight Lines") is quite ready for press, as is also, I think, Lecture III. ("On the Rotation of Plane Figures"); Lecture IV. ("Of Similar Figures") is a fragment, and still more fragmentary is Lecture V. ("The First Principles of Calculation"). Of Lecture VI. ("The Theorem of Pythagoras") there are two loose sheets of figures: on one sheet is "the Bride's Chair," and the figures on this and the other sheet show that my information was correct, and that the remarks on pp. 633, 637 are *ad rem*. As Lecture IX. ("On the Shadows of a Circle") is very fully illustrated in the recent volume edited by Prof. Pearson, we see that we are in possession of a fairly complete presentment of Prof. Clifford's views on the subjects of the course of lectures.

Messrs. Macmillan have stated their willingness to publish the MS. of the second part of "The Elements of Dynamics," and I hope to be able, after a re-examination of it, to put the work into their hands for printing. When this book is got out, and the above lectures published in some shape yet to be determined, the mathematical world will be in possession of all that we can now look for from the hands of this great master.

University College School, April 25 R. TUCKER

Sir Wm. Thomson and Maxwell's Electro-magnetic Theory of Light

SHORTLY after writing my former letter I saw a copy of the verbatim report of Sir Wm. Thomson's lectures in Baltimore, and would have written to you to that effect and to apologise to Mr. Forbes for having doubted the accuracy of what I thought was his report, only that I met him in London about that time, and he then desired me not to do so. Sir William Thomson has now himself stated that the passage is correctly quoted, and I can only regret that he has expressed himself in the way he did.

I certainly think that anybody reading the passage would imagine that the velocity of propagation of electro-magnetic disturbances upon Maxwell's electro-magnetic theory of light, which he showed to be the same as the velocity of propagation of light, and to be a true velocity of wave-propagation—any one, I say, would suppose that this was the same thing as that Sir Wm. Thomson calculated in the year 1854.

Sir Wm. Thomson certainly says, "That is a very different case," but the rest of this sentence is rather ambiguous as to what the "it" after "putting" refers to, and I am afraid that many people will imagine that, in Sir William Thomson's opinion, Maxwell has made some unjustifiable assumption. I believe, however, that all he thinks is that Maxwell has not made a satisfactorily definite thing of the so-called electro-magnetic theory of light.

In Sir Wm. Thomson's article in Nichol's "Cyclopædia" he puts the matter very clearly indeed. He says:—"The law of this phenomenon [transmission of electric signals] is identical with that which Fourier . . . found as the law of propagation of summer heat and winter cold to different parts of the earth," *i.e.* it obeys the laws of a diffusion and not of a wave-propagation; and again:—"Now it is obvious from these results [experimental results] that the supposed velocity of transmission of electric signals is not a definite constant like that of light:" and afterwards he says that, when an initial current is started, the potential rises simultaneously at all points, and that the apparent velocity would depend on the delicacy of our instruments. All these obviously distinguish between the propagation of a variable current in a conductor and a true wave-propagation.

He has also clearly pointed out a direction in which to look for a true wave-propagation. It will make his position clearer, and also Maxwell's, to use his analogy between water in an elastic tube and a conductor of electricity. I will suppose the water contained in a tube bored out of a very large lump of india-rubber. He enumerates three electric qualities concerned, and their hydrodynamic analogues:—(1) "Charge" or electrical accumulation in a conductor subjected in any way to the process of electrification. (2) "Electro-magnetic induction" or electromotive force excited in a conductor by variations of electric current. (3) Resistance to conduction through a solid. The hydrodynamic analogues are:—(1) Accumulation of a greater or less quantity of water in any part of the canal or tube. (2) Inertia of the water. (3) Viscosity or fluid friction. He explains that a true wave-propagation arises from the compressibility of the water, combined with its inertia, and that if the tube be elastic, like india-rubber, there would also arise a wave-propagation. "Accordingly," he says, "a definite velocity of propagation of electric impulses, depending on the inertia and the capacity for charge, is to be looked for, as has been done in a first article, published by Kirchhoff, on the subject."

Now, in all this discussion Sir Wm. Thomson omits to mention the only thing that is at all analogous to Maxwell's propagation of wave disturbances in non-conductors, and it arises from his considering the water as contained in a tube like ordinary india-rubber tubes, instead of in a tube bored in an indefinitely large lump of india-rubber. If we consider this case it is evident that one of the conditions to be considered is the propagation of waves in this lump of india-rubber. In Sir Wm. Thomson's tube there would of course be a velocity of wave propagation in the india-rubber, but that is a very different matter from the propagation of disturbances away from the neighbourhood of the tube by which energy would be carried away from it. To do this Sir Wm. Thomson should have included the propagation of sound in the air or whatever he supposed surrounding the outside of his tube. Without including this, he was not including anything a bit analogous to Maxwell's electromagnetic theory of light. In Sir Wm. Thomson's tube the whole state of affairs at any time could be expressed in terms of variables that represented bodies near the tube, while in the other case it would be absolutely necessary to introduce variables representing every part of the india-rubber which I have supposed of indefinite extent. This is just the difference between Sir Wm. Thomson's and Maxwell's views. According to Maxwell's view there is a great deal more going on outside the conductor than inside it, and it is evident that the inertia of the water is a very bad analogue to electromagnetic induction, for this latter depends essentially upon the form of the circuit, and not only upon its section and length. Maxwell has shown that light may be a wave-propagation of what are on his theory *analogous*, though probably utterly *unlike* the distorsional waves propagated in the india-rubber, and has shown that a medium which would only transmit disturbances analogous to these would explain electric and magnetic phenomena. It is to be remembered that Maxwell's theory gets rid of all action at a distance, and that the only *experimentum crucis* between theories of action at a distance and of action through a medium is that in this latter case the energy may be propagated in time through the medium, while in the former it cannot.

I cannot conclude without protesting strongly against Sir Wm. Thomson's speaking of the ether as *like* a jelly. It is in some respects *analogous* to one, but we certainly know a great deal too little about it to say that it is *like* one. May be Maxwell's conceptions as to its structure are not very definite, but neither are any body's as to the actual structure of a jelly, and there is no real difficulty in supposing a medium whose condition is represented by symbols that obey the laws that Maxwell has shown should be the laws of symbols representing the condition of a medium that would explain electric and magnetic phenomena. It seems very unlikely that any jelly is at all *like* the ether that Maxwell supposes. It seems much more likely that what he called "electric displacements" are changes in structure of the elements of the ether, and not actual displacements of the elements. He guards against this being supposed a necessary part of his theory when he defines polarisation in terms that certainly require a change of structure rather than a change of position, so that I think the word "displacement" was unfortunately chosen. I also think that Sir Wm. Thomson, notwithstanding his guarded statements on the subject, is lending his overwhelming authority to a view of the ether which is not justified by our present knowledge and which may lead to the same unfortunate results in delaying the progress of science as arose from Sir Isaac Newton's equally guarded advocacy of the corpuscular theory of optics. GEO. FRAS. FITZGERALD

40, Trinity College, April 25

The April Meteors

IN 1882 the Lyrid meteor shower (epoch, April 19–20) was noticed to be far more conspicuous than usual. The display had been quiescent for some years; it appeared to have degenerated into a third rate shower, scarcely deserving the trouble of observation. But in the year mentioned the stream gave distinct intimation of greater intensity, and showed the necessity of continuing annual observations of periodical showers such as this, even though they may exhibit, during a comparatively long interval, but a very feeble sustenance of the richness recorded in former times.

On April 20, 1882, Mr. Corder, at Chelmsford, watching for three hours between dusk and 12h. 30m., counted 26 Lyrids and 8 meteors belonging to other contemporary radiants of minor character. He regarded the horary rate of apparition on that occasion as about two or three times as high as on any other occasion since 1877, when he had been able to watch for the display. He found the radiant at 268° + 37°, and remarked that four of the meteors seen were as brilliant as first-magnitude stars, but he had never found them a very interesting species in respect to their visible appearances.

No observations of the shower were obtained in 1883, the moon being near the full at the time of its occurrence; but in 1884 the conditions were more favourable. On the night of April 19, in the hour preceding midnight, 17 Lyrids were observed by the writer at Bristol. During the last quarter of an hour of the watch the sky was much clouded, and only one Lyrid seen. The horary number was computed as 22 for one observer—evidently, therefore, the display of 1884 was a notable one, and it was very unfortunate that a clouded sky prevented the development of the shower being watched through the morning hours of April 20, when possibly it may have attained a richness without parallel in late years. On the evening of April 20 it had evidently become exhausted, for in a watch of twenty minutes not a single shooting-star appeared, though the sky was very clear.

The present return of the Lyrids occurred under very auspicious circumstances. The moon offered no impediment to morning observations, which is far the best time to watch for these meteors, as the radiant, west of α Lyræ, is very low in the evening hours. The nights of April 17, 18, 19, and 20 were cloudless throughout, and on the three latter dates observations were made here with the following results:—

Date		Period h. m. h. m.		Duration of Watch Hours		Meteors seen		Lyrids seen		Radiant Point ° °
April 18	...	12 0 to 14 30	...	$2\frac{1}{2}$	...	16	...	6	...	260 + $33\frac{1}{2}$ [1]
,, 19	...	10 30 to 14 0	...	$3\frac{1}{2}$	...	26	...	10	...	$267\frac{1}{2}$ + 33 [2]
,, 20	...	11 30 to 15 30	...	4	...	39	...	14	...	274 + $33\frac{1}{2}$ [3]
Ap. 18-20	...	10 30 to 15 30	...	$9\frac{1}{2}$	...	81	...	30	...	267·2 + 33·3

After April 20 moonlight and cloudy weather effectually prevented further work.

The table furnishes us with some interesting facts. It shows that during the three nights the proportion of Lyrids to unconformable meteors was very nearly the same, namely, as 3 to 5, and that the horary rate of their apparition was little more than 3. It also shows a very marked displacement of the radiant (in the direction of east longitude) from night to night. I regard this as the most interesting and certain feature observed. The three centres resulting from the paths of short meteors, observed with the utmost care, may each be relied on as very accurate. It therefore appears most conclusive that the radiant point of the Lyrids, similar to that of the August Perseids, increases in right ascension from night to night, and the extent of this displacement is even greater for the Lyrids than for the Perseids.

[1] Sky clear; slight haze. [2] *Ib.* [3] Sky very clear.

On April 19, 1884, $11\frac{1}{2}$h., I found the radiant at 269° + 33°, and on April 19, 1885, $12\frac{1}{4}$h. (the middle time of the observation), at $267\frac{1}{2}$° + 33°. Allowing for the difference (about 12′) in the sun's longitude at the two epochs in applying the correction for the displacement of the radiant (7° of R.A. daily) observed this year, we shall find that the two positions are in *exact* agreement.

The recent display has been decidedly meagre in point of numbers. There is a great falling off since last year, when the horary rate was nearly eight times as great. But some of the meteors observed this year were very bright and in a great measure compensated for their scanty apparitions. It is curious that three of the most brilliant Lyrids, equal to or exceeding Jupiter, appeared in nearly the same region of the western boundaries of Virgo. On April 18, at 12h. 57m., one of these fell, with a bright flash and streak, 10° east of Spica Virginis. Another on April 20, at 13h. 14m., came out very suddenly 7° above that star; and a third, at 14h. 1m., descended, with a swift, diving motion, about 13° east of the star, so that the path was nearly similar to that of the first of the three. These five meteors gave transient

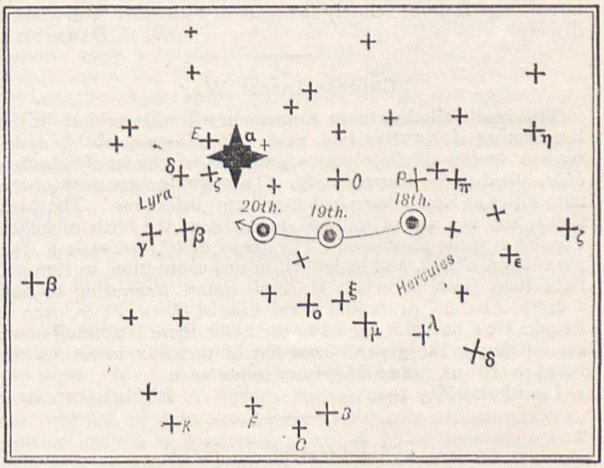

Diagram exhibiting the displacement in the radiant point amongst the stars of Hercules and Lyra on the nights of April 18, 19, and 20, 1885.

flashes of yellowish-white light, which brightly illumined the mist lying along the horizon, and left short streaks of very brief duration. I saw four other Lyrids, quite equal to first-magnitude stars, and these, together with the fainter members of the same shower, were nearly all registered amongst the stars surrounding Lyra, especially those of Hercules, Draco, and Aquila. Altogether I regard the April display of this year as one of considerable importance, for though much less rich than in 1884, it furnished meteors of greater interest and brilliancy. The low position of the radiant during observations last year may have influenced the visible aspect of the meteors from Lyra, especially as regards the swift, flashing characteristic so invariably noticed this year in the brighter members, or, possibly, the explanation may be that near the richer portion of the stream the corpuscles are smaller in almost the same ratio as the increased condensation. It will be advisable to regard this question during future observations of this shower, and especially the new feature detected this year as to the evident shifting of the radiant point from night to night. I believe the maximum of the shower is usually attained with the radiant at $269\frac{1}{2}$° + 33° (the radiant point of its allied Comet I. 1861 is $270\frac{1}{2}$° + 32°). If the display extends over as long as seven days, and the radiant shifts 7° in R.A. every day, it is curious that the meteors will be *Herculids* on April 17, 18, and 19, *Lyrids* on April 20, 21, 22, and 23, and then *Cygnids* on April 24.

Contemporary with the special periodical display of this epoch there are vast numbers of feeble systems which annually give some indication of their presence. This year I ascertained the positions of several of these showers with great care. The number of meteors from them averaged from 3 to 5 only, but the paths intersect nearly at a point in the individual cases, so that the centres are entitled to the same value as positions resulting from a large number of tracks. I give the best of these co-Lyrid showers, and the nearest confirmations from previous observations:—

Observed 1885		Previous Observations	Authority
April 18	181° + 35¼°	184° + 35°	March 31-April 12, D.S., 1872.
19	236 + 62	240 + 55	April 14, Schiaparelli and Zezioli.
20	226 + 41	223 + 40	March 12-April 30, Greg and Herschel.
18-20	296 ± 0	294 ± 0	April 16-19, 1877, D.
19-20	230 + 17	230 + 26	April 20-24, Heis.
18-20	299 + 24	298 + 25	April 16-19, 1877, D.
		300 + 20	April-May, Corder.
18-20	213 + 9	206 + 13	April 13-May 11, Heis.
18-20	226 ± 0	228 − 2	April 16-19, 1877, D.

The two radiants observed here in 1877 and 1885, with mean position at 295° ± 0° and 298°·5 + 24°·5, are very interesting. The former, just preceding η Aquilæ on the equator, supplies meteors of very great velocity, the latter in Vulpecula gives swift, streak-leaving meteors. This pair of showers, directed from points near the apex of the earth's way, are now, I believe, very exactly determined in regard to their centres of radiation. That they have hitherto evaded frequent detection is not surprising, as they only become well visible in the morning hours. It will be useful to watch for these special streams during future exhibitions of the Lyrids, as well as to note the several other interesting features closely associated with this well-known display.

W. F. DENNING

Chinese Insect Wax

THE beetle alluded to in connection with this subject in the last number of NATURE (vol. xxxi. p. 615) is a probably undescribed species of *Brachytarsus*, a genus of the family *Anthribidæ*, allied to the *Curculionidæ*. Through the courtesy of the authorities at Kew I have had specimens before me. The idea that it acts as a sort of midwife to assist at the birth of infant *Coccidæ* is quite erroneous. The genus *Brachytarsus* is a true parasite on *Coccus*, and its habits, in this connection, in Europe, have long been known. It is of course interesting to find "unity of habit" prevailing in the case of *Coccus Pé-la*, even to its parasite; but with regard to the latter there is nothing new; some points in the general economy of the wax insect, in the notes published, are of far greater importance.

Lewisham, May 1 R. McLACHLAN

The New Bird in Natal

THERE can be little doubt from the description given by Mr. Turnbull in your issue of April 16 (p. 554) that the bird lately obtained by him in Natal is the Standard-winged Nightjar, *Cosmetornis* (seu *Macrodipteryx*) *vexillarius*, Gould. It has not been met with in Cape Colony, which accounts for Mr. Turnbull's inability to find mention of it in Layard's "Birds of South Africa;" but in Mr. Sharpe's new edition of Layard's work (which Mr. Turnbull would do well to procure) he will find an account of this bird given at p. 89. It appears to have a wide geographical range, being found both on the west and east coasts of Africa; in Angola and Damaraland, in Natal, on the Zambesi (where 300 miles up the river Dr. Kirk found it quite common), in the islands of Bourbon and Madagascar, along the Red Sea shore, and on the island of Socotra. With this extended range it is somewhat remarkable that it has not yet been met with in Cape Colony. According to the observations of Dr. Kirk the singular prolongations of the primaries are peculiar to the males, and a seasonal peculiarity observed only during the months from October until January. The habits of this bird, like those of other nightjars, are crepuscular. An excellent coloured figure of the male is given in Gould's "Icones Avium."

J. E. HARTING

Wild Bees

A FEW words respecting a colony of wild bees (a species of *Andrena*) which I have just discovered in our garden, may interest your entomological readers. A day or two ago, on walking beside a low-turfed mound which supports two trees on one of our towns, I noticed that the grassy surface on the south—therefore the sunny—side was covered with little hillocks of earth, such as ants throw up after rain. On examination each little heap showed the circular hole which denotes a bee's nest, and the bees themselves were seen in many places going in and out. Some holes were level with the ground, but most had the tiny mound of soil cast up in the process of excavation. The peculiarity of the case seems to me to lie in the great number of nests forming a complete colony. It is difficult to count them, but there cannot be less than eighty or ninety in an area—roughly calculated—of about sixty square feet. Have any of your readers noticed a similar city of these busy people? and can any one supply the specific name? E. BROWN

Further Barton, Cirencester, May 2

ON M. WOLF'S MODIFICATION OF FOUCAULT'S APPARATUS FOR THE MEASUREMENT OF THE VELOCITY OF LIGHT

NO one who has the true interests of scientific accuracy at heart can fail to welcome any innovation whereby the elements of a research may be varied, for thereby the ever-lurking constant error is most readily eliminated. It seems, therefore, that this in itself is sufficient reason for the interesting paper communicated by M. Wolf to the Académie des Sciences (*Comptes Rendus*, 9 Février), describing a very ingenious arrangement of Foucault's experiment, and that there was no occasion for disparaging other work in order to justify its publication. It is to be hoped that this was done rather through inadvertence than design, but I feel called upon to correct some of the misapprehensions under which the author labours, and particularly those concerning the appearance and distinctions of the image of the slit in my work on the velocity of light.

M. Wolf remarks that, under the conditions which I selected, this image, even under the most favourable circumstances, must be bordered with very large diffraction fringes, which the atmospheric disturbances transform into a badly-defined "*tache lumineuse*." In reply to this, though I grant that the fringes *ought* to be present, yet I can affirm as a matter of fact that they were not to be seen. Possibly M. Wolf and others may have been somewhat misled by a drawing of the appearance of the image given in my work (p. 124, *Astr. Papers*, *American Ephemeris*, and *Nautical Almanac*, vol. i. Part 3) where the image proper, which is quite clear, is surrounded by a luminous haze, from which, however, it is very easily distinguished.

I hardly think that if M. Wolf had given the "specimen observations" (p. 133 of my work) due consideration, he would have characterised as a "*tache lumineuse*" an image whose position was measured with the following results (each result is the mean of ten observations made by one observer, and recorded without divulging the result by another):—

No. 1	112·801 mm.	0·020 mm.
,, 2	112·773 ,,	0·006 ,,
,, 3	112·769 ,,	0·010 ,,
,, 4	112·772 ,,	0·007 ,,
,, 5	112·779 ,,	0·000 ,,

Average difference from mean = 0·0086 ,,

These are measurements of the *deflected* image, so that the differences are not merely errors of linear measurement, but include errors in the estimate of the speed of the revolving mirror.

Now, M. Wolf, in his most sanguine statement, does not hope for a greater degree of accuracy than one part in 3500 in this particular measurement, whereas the above results are on the average closer than one part in 10,000.

But let us examine the data on which he bases this most favourable estimate. In the first place, the image whose position is to be measured to within one-hundredth of a millimetre is the result of seventy-nine reflections from concave mirrors !

Secondly, one of these mirrors is to be 2 decimetres in diameter. Such a mirror used in a reflecting telescope would show signs of distortion if not carefully mounted—even at rest. But this mirror is required to make fifty revolutions per second, and the distortion is multiplied by forty reflections from its surface !

Finally, notwithstanding the avowed purpose of diminishing the path of the light ("*sans augmenter le trajet de la lumière*"), the distance required is *greater* than in my own experiments in the proportion of 1600 to 1200, and hence atmospheric disturbances would come into play in the same proportion—unless especial precautions were taken to guard against them.

And here, I am free to concede, is an important advantage, but one which is by no means limited to M. Wolf's arrangement, but is universally applicable—for by repeated reflection by plane or by concave reflectors the whole path, either in Fizeau's method or in Foucault's, may be confined to a limited space. But I think the chief object of such an arrangement—namely, to control easily the homogeneity of the air-column—could be more advantageously effected by a long underground tunnel containing a pipe, surrounded, if necessary, by running water, or, better still, exhausted of air.

At Prof. Newcomb's request I have repeated, with some alterations, the experiments described in the paper referred to, and occasionally the appearance of the image was better than in that work. On one occasion the width of the image was carefully measured, and found to be 0·25 mm. Evidently there is nothing remarkable in measuring the position of the centre of an image of this width within a hundredth of a millimetre.

Again, the "probable error" of my final result, 5 kilometres, would seem to show a somewhat greater degree of consistency than would be possible had I only a "*tache lumineuse*" to bisect.

I cannot forbear remarking that by astronomical methods—if M. Wolf entirely mistrusts the results obtained by Cornu, Newcomb, and myself—the velocity of light is known certainly within 1 per cent., and that it would, therefore, denote rather an excess of caution to deduce a formula for the elimination of a possible uncertainty of from 5 to 10 per cent., as M. Wolf does in determining "*l'ordre* M *de cette déviation*."

In conclusion, I think M. Wolf is to be congratulated on the very happy combination he has devised for the solution of this most fascinating problem—a problem which, notwithstanding its difficulties, will ultimately yield a result correct not merely to one part in 3500, but, I firmly believe, one in 300,000—perhaps one in 1,000,000.

ALBERT A. MICHELSON

SELF-INDUCTION IN RELATION TO CERTAIN EXPERIMENTS OF MR. WILLOUGHBY SMITH, AND TO THE DETERMINATION OF THE OHM

IN a lecture delivered by Mr. Willoughby Smith before the Royal Institution in June last (see *Proceedings*) some experiments are detailed, which are considered to afford an explanation of discrepancies in the results of various investigators relating to the ohm, or absolute unit of electrical resistance. As having given more attention than probably any one else in recent years to this subject, I should like to make a few remarks upon Mr. Willoughby Smith's views, which naturally carry weight corresponding to the good service done by the author in this branch of science.

In the first series of experiments a primary circuit is arranged in connection with a battery and interrupter, and a secondary circuit in connection with a galvanometer and commutator of such a character that the make and break induced currents pass in the same direction through the instrument. Under these circumstances it is found that at high speeds the insertion of a copper plate between the primary and secondary spirals entails a notable diminution in the galvanometer deflection, and this result is regarded as an indication that the molecules of copper need to be polarised by the lines of force—an operation for which there is not time at the higher speeds. The orthodox explanation of the experiment would be that currents are developed by induction in the copper sheet, which thus screens the secondary spiral from the action of the primary, and the result is exactly what might have been anticipated from known electrical principles. I have the less hesitation in saying this, because as a matter of fact I did anticipate from theory the action of a combination very similar in character. The experiment is described in the *Philosophical Magazine* for May, 1882, and differs from Mr. W. Smith's only in the substitution of a telephone for the galvanometer, and of a microphone for the interruptor, no reverser in the secondary circuit being required. By the interposition of a thick copper sheet the sound is greatly enfeebled.

The second series of experiments were made with Faraday's "new magneto-electric machine," in which a copper disk rotates about its centre between the poles of a horse-shoe magnet. The currents developed are examined with a galvanometer whose electrodes touch two points upon the disk—in Mr. W. Smith's experiments, one at the centre, and the other at the circumference. At low speeds the distribution is symmetrical with respect to that diameter of the disk which is passing at any moment between the poles; but, as the speed is increased, a certain "drag" is observed, disturbing the symmetry. This drag, or lagging, was noticed by Nobili in a very similar arrangement as long ago as 1833 ("Wiedemann's Electricity," third edition, vol. iv., § 374), and is no doubt to be attributed to the induction of the currents upon themselves.

This question of self-induction is indeed a very important one in respect of certain methods for determining the ohm; but it certainly cannot be said to have been neglected, as Mr. W. Smith seems to suggest. Both in the original experiments of the British Association Committee with a coil revolving about a vertical axis, and in my own recent repetition of them, the self-induction of the coil is a most important feature, and may cause a displacement of the position of maximum current from the plane of the magnetic meridian through as much as 20°. In my paper (*Phil. Trans.*, 1882, p. 661) I thought I had discussed the question at almost tedious length.

It is possible that Mr. W. Smith had in his mind rather determinations by the method of Lorenz, in which Faraday's disk is used. The arrangement here, however, differs in one very important respect from that of Mr. W. Smith's experiments in that the lines of force are symmetrically arranged in relation to the axis of rotation. The consequence is that, however great the speed of rotation, there are no currents circulating in the disk, and therefore no question arises as to the self-induction of such currents. What is observed is simply the difference of electrical potential between the centre and the circumference. It is impossible to discuss the matter fully here, but the reader will find all that is necessary by way of explanation in the paper published in the *Phil. Trans.* ("Experiments by the Method of Lorenz for the further Determination of the Absolute Value of the British Association Unit of Resistance," &c.). My object in writing is to correct the inference, suggested by W. Smith's remarks, that the question of self-induction has been neglected by workers upon this subject.

RAYLEIGH

THE INVENTIONS EXHIBITION

IN the presence of a crowded and distinguished assembly the Inventions Exhibition was opened by the Prince of Wales on Monday. The Exhibition is, as usual on first days, still in a somewhat chaotic condition, and we can do no more this week than refer to the leading incidents of the opening ceremony. Sir Frederick Bramwell, Chairman of the Executive Council, in his address to the Prince of Wales, gave a sketch of the progress and objects of the Exhibition, which, he pointed out, is intended to illustrate the progress of inventions since the year 1862, and that of musical instruments and appliances since the commencement of the present century. The labours of the different committees were, he stated, rendered extremely onerous by the vast number of applications received—a number far greater then we had space to accommodate. Influential Commissions have been nominated by Austria-Hungary, France, China, Greece, Italy, Japan, Russia, Siam, and Switzerland, from which countries interesting and valuable exhibits have been received or are promised. Arrangements have been made with the Council of the Royal Albert Hall by which that building forms an integral portion of the Exhibition, with the National Fish Culture Association for the maintenance of the Aquarium, and with the Council of the Royal Horticultural Society for the holding of the usual periodical flower and fruit shows. The Old London Street, which was so popular a feature in last year's Exhibition, has been maintained. Many small annexes have been swept away, and in their places spacious galleries have been erected. Not only has greater exhibiting space been thus obtained, but the gardens, which are so great a source of attraction to visitors, have actually been enlarged. Notwithstanding the fact that the gallery used last year for machinery has been greatly extended to meet the requirements of exhibitors, it proved to be inadequate for the many important inventions for which motive power was desired; indeed, it has been found necessary to furnish such power in no less than three other galleries. "The employment of electricity for the purposes of lighting," Sir Frederick said, "is undoubtedly one of the most striking instances of the application of science to the purposes of daily life; we have, therefore, not hesitated to give this subject special prominence. The method we have adopted will, it is believed, render any sudden failure of the lights impossible, and will favourably display the most recent and improved apparatus, and the advances that up to this date have been made in electric lighting. After most careful experiments we have ventured to employ, for the garden illumination, the incandescent electric lamp, and we have done so in a manner and on a scale which, we believe, has never before been attempted. As a division of the Exhibition is devoted to music, we have set apart an important portion of the buildings to the illustration of instruments and appliances appertaining to that art; and we have invited the formation of a historical loan collection of musical instruments, which we believe is of a deeply interesting character. In requesting your Royal Highness to declare this Exhibition open we desire to express the hope that it may, on the one hand, be the means of bringing valuable and meritorious inventions prominently before the general public, to the benefit and credit of the exhibitors, and that it may, on the other hand, be the means by which that public may, within the area of one exhibition, be enabled to appreciate the marvellous progress which during the past quarter of a century every industry has achieved."

The Prince of Wales, in reply said: It is with much pleasure that I have listened to the report of the Executive Council, and I fully appreciate the labours which you have bestowed upon this great undertaking. At the closing of the International Fisheries Exhibition I took the opportunity of expressing a hope that an International Inventions Exhibition might be held in these buildings during the present year; and I am sincerely gratified to find that this hope has been realised. The scope of this Exhibition is, indeed, vast, and I can readily comprehend the difficulties which must have beset you and the Committee of Advice in your endeavours to secure adequate representation for each branch of industry. I have observed with much pleasure that the classification originally adopted has been made the practical basis of the allotment of space in the Exhibition, and that the exhibits in each group have, as far as possible, been placed together. I am convinced that by following this plan you have materially increased the educational value of the Exhibition. I readily echo the sentiments of gratitude which you have expressed for the invaluable aid rendered by the guarantors; and I join with you in welcoming the representatives of those foreign countries who are present here to-day.

The Prince of Wales, after declaring the Exhibition open, made a tour of the galleries in company with the Princess of Wales and many others of the distinguished visitors who were present.

THE FLORA OF BANK-NOTES

"LA Flore des Billets de Banque" is the title of an article in *Science et Nature*, an article which, in spite of an amusing tendency inseparable from all things savouring of sensational science, may suggest thoughts more or less alarming in view of recent discoveries in bacteriology. It is no new fact that books, coins, and other articles of a durable nature which pass much from hand to hand may be the means of transmitting

FIG. 1

infectious diseases, and if these infectious diseases are caused by visible and tangible agents, it is not going far to expect that the agents should be discoverable on the transmitting media by means of the microscope, and by other methods employed by the specialists who devote themselves to tracing the awful bacterium to its home. Of course it is now matter of fact that "bacteria" (using the term in its widest sense) can be and are causal agents in disease, and the writer of the article referred to shows that bacteria and other minute organisms always occur on bank-notes; there is, perhaps, no more in this observation than that it demonstrates a fact in a particular case which scientific biologists have shown to be true much more generally. All objects exposed to the air and passed from hand to hand are apt to have minute organisms settling upon them, and we should expect such things as bank-notes, which pass through many hands, to be favoured by more than their usual share of "germs," knowing that simple abrasion is no satisfactory means for removing such minute bodies. Nevertheless it is interesting to see what really have been found on European bank-notes. M. Reinsch some time ago undertook to examine the money in circulation, with the result that two very small algæ, which were named as species of *Chroococcus* and *Pleurococcus* respectively, proved to be not uncommon on coins. M. Jules Schaarschmidt has since undertaken to examine the paper currency of various States, with the result that such living organisms and other objects as those in the annexed woodcut were discovered. According to the statements to hand, the notes examined were particularly those of Austro-Hungary and Russia, and new as well as old ones furnished "an abundant cryptogamic vegetation," as well as "microbes," and objects such as grains of starch, particles of hair, &c.

The entire list comprises *Bacterium termo*, the common bacterium of putrefaction; *Saccharomyces cerevisiæ*, the

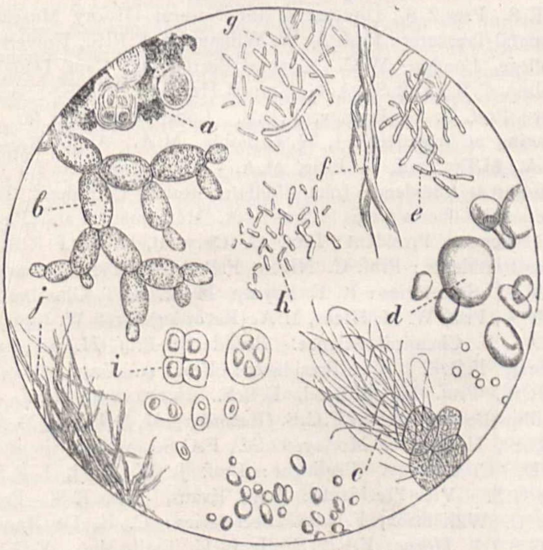

FIG. 2.—*a* and *i*, minute algæ; *b*, yeast cells; *c*, *Leptothrix*; *d*, starch-granules; *e*, *g*, *h*, various *Schizomycetes*; *f* and *j*, fibres.

yeast plant; various species of *Micrococcus*, *Leptothrix*, and *Bacillus*, as well as the two minute green algæ described by Reinsch.

We presume, in the absence of definite statements, that the groups of organisms sketched in Fig. 2 were obtained at different times, and on different notes; otherwise the "flora" is indeed a rich and abundant one, and may probably have been an isolated one, to allow the species of *Saccharomyces* to form such a fine growth.

There is obviously a very serious side to all this, however, if further researches prove that, as appears possible, our most minute and dreaded enemies are always in our midst on such apparently welcome visitants as coin and bank-notes: money will have earned a worse name even than it has heretofore! *En revanche*, there are two points which no doubt will be insisted on: in the first place, the observers named have not, so far, described any organism on the money investigated which is known to be inimical to us; and secondly, precautions have been taken from time immemorial against the transmission of currency passing from a plague-stricken community to a healthy one. Possibly the facts derived from these observations will be made use of to bring more forcibly before the minds of our less careful brethren the dangers of handling "filthy lucre" in times of disease.

STANDARD PITCH[1]

M. SORET raises the question of musical pitch, and advocates A 432, long ago proposed by M. Meerens, of Belgium. It is rather curious that in Belgium itself M. Meerens's proposal was considered and rejected by a Commission appointed in 1877, upon whose report the French pitch A 435 was adopted by Royal decree on March 19 of this year. There seems to be very little difference between the two; it amounts, in fact, to exactly 12 cents or hundredths of an equal semitone, of which $21\frac{1}{2}$ make a comma. Hence there is no practical reason for making the change as affecting singers. But no instruments made for A 435 would be available for A 432, so that the advantage of uniformity would be lost, without any advantage to the voice or the quality of instruments. The arguments in its favour are almost entirely arithmetical. To begin with the inaudible 1 vibration and proceed by exact doubling to 64 is an arithmetical dream. It is true that König, by a most ingenious adaptation of a large tuning-fork acting in place of a pendulum to a clock going in a room at 20° C. (for about five days in a year), has succeeded in making a fork of that precise number of vibrations at that precise temperature. But at 15° C., the temperature adopted for the French *diapason normal* (standard fork), the pitch of this would not be 64, but, to take König's numbers, 64·036. The charm of the arithmetic vanishes, therefore, with a slight alteration of temperature, and the pitch has become fully 1 cent (hundredth of a semitone) sharper. Granted that this is an imperceptible amount, yet it is enough to alter the whole of the arithmetic. Then the arithmetic is itself founded on just intonation, which is not adopted anywhere. If we take the equal temperament, now generally accepted, we should get for A 432 the values C 256·9, C♯ 272·2, D 288·3, D♯ 305·5, E 323·6, F 342·9, F♯ 363·3, G 384·9, G♯ 407·8, A 432, A♯ 457·7, B 473·9, C 513·8. There is nothing charming here. M. Soret, in his table, quietly ignores the chromatic notes and the equal temperament. If, however, we took C 256 as the starting-point, the A of C major in just intonation would not be 432, but, as he owns, a comma flatter, 426·67. He bases everything physically on the violin, which is tuned in D and not in C, or the viola and violoncello, which are both tuned in G, not in C, and hence even for these instruments, with the great assumption of just intonation, his use of the major scale of C is incorrect.[2] The reasons that are to guide us in the choice of a pitch must certainly not be arithmetical. For more than two centuries up to 1813, when the Philharmonic Society was founded, all Europe used a pitch within a comma either way of Handel's fork A 422·5. Then, owing to the presentation of new instruments by the Emperor of Russia to a Vienna regiment at the Congress of Vienna, pitch rose gradually but slowly. In 1826 our Philharmonic Society, under Sir G. Smart, adopted A 433, between M. Soret's and French pitch, and this was known for many years in London as the Philharmonic pitch. France adopted A 435 in 1859. Under Costa our pitch rose to its present height, A 454·7. But our army pitch, used at Kneller Hall, and adopted for the forthcoming Exhibition, is A 452. Now, the trouble is that our classical composers wrote their music for Handel's pitch, while since 1860 Continental composers

[1] J.-L. Soret, "Sur le Diapason" (*Archives des Sciences physiques et naturelles*. January, 1885. Geneva).

[2] Savart, whom M. Soret quotes, was in error with regard to the pitch of the resonance of Cremona violins. It was not 256 vibrations. A series of instruments examined by Mr. A. J. Ellis in 1880 gave about 270 as the primary maximum, and 252 as the secondary. But the main character was the great uniformity of reinforcement for different pitches.—See his "History of Musical Pitch."

have used French pitch, and English composers our high pitch. The first and last may compromise with the second, but are incompatible with each other. To sing Handel in modern English pitch is to unduly strain voices and spoil the effect originally intended. But we submit to it even in Handel festivals. There is a greater difficulty in altering pitch in England than on the Continent. We have no subsidised Conservatoires or theatres to which we can say: "Use this standard of pitch, or go without subsidy." Even regimental bands are not supplied at the expense of the State. A new set of instruments is very costly, and more than that, it is long before makers learn how to manufacture correctly to a new pitch. The question is therefore beset with difficulties. But the solution is certainly not to be found in the arithmetic of M. Soret.

THE SCIENCE AND ART MUSEUM, EDINBURGH

WE understand that Col. Murdoch Smith has been appointed by the Lords of the Committee of Council on Education to the Directorship of this Museum, in succession to the late Prof. Archer. As Lieut. Smith he was associated with Prof. Newton in the discoveries at Helicarnassus, and, subsequently, with Commander Pacher, R.N., undertook the explorations in the Cyrenaica which resulted in the acquisition by the nation of the valuable collection of sculptures now in the British Museum. Latterly, Col. Smith, while employed at Teheran, has acquired for the South Kensington Museum the large and valuable collection of Persian art and manufactures which is so well known there. We believe Col. Smith obtained his first Commission in the Royal Engineers direct from a Scottish University, and is one of the very few officers in that Corps who did not pass through the Royal Military Academy at Woolwich or Addiscombe.

NOTES

WE take the following from the *Times*:—The following is the list of selected candidates recommended by the Council of the Royal Society for the election to the Fellowship:—A. W. Baird, Major R.E., P. Herbert Carpenter, D.Sc., Sir Andrew Clark, M.D., Mr. A. A. Common, F.R.A.S., E. W. Creak, Staff-Commander, R.N., Prof. E. Divers, H. Hicks, M.D., W. M. Hicks, M.A., F. R. Japp, Ph.D., A. M. Marshall, M.D., Prof. H. N. Martin, D.Sc., C. O'Sullivan, Prof. J. Perry, Prof. Sydney Ringer, and Sidney H. Vines, D.Sc.

OF the fifteen candidates who have thus been selected no less than five are Cambridge men. Mr. W. M. Hicks was bracketed seventh wrangler in the Mathematical Tripos of 1873. Prof. H. N. Martin, Prof. Milnes Marshall, and Dr. Vines were the seniors in the Natural Science Triposes of 1873, 1874, and 1875 respectively, while Dr. Herbert Carpenter obtained a First Class in the Tripos of 1874, together with Mr. J. N. Langley, who was elected to the Royal Society in 1883. The name of the late Prof. F. M. Balfour follows that of Dr. Martin in the Tripos list of 1873; while the late Prof. A. B. Garrod was senior in 1871, and the Tripos list of 1870 contains the names of Francis Darwin and E. J. Romanes. The Natural Science Triposes from 1870 to 1875, inclusive, have thus furnished no less than nine Fellows of the Royal Society, either actual or elect. The names of seven more occur in the Mathematical Tripos lists from 1871 to 1880 inclusive, viz.: J. Hopkinson (1871), J. W. L. Glaisher (1871), H. Lamb (1872), A. B. Kempe (1872), W. M. Hicks (1873), Rr. T. Glazebrook (1876), and J. J. Thomson (1880). To these may be added the name of Dr. W. H. Gaskell, who obtained mathematical honours in 1869, but has since devoted himself to physiology. All who know Cambridge will recognise how largely these results are due to the influence and example of the late Prof. Clerk Maxwell and of Prof. Michael Foster respectively.

WE are informed that Dr. Frankland, F.R.S., has intimated his intention to resign the Professorship in Chemistry in the Normal School of Science and Royal School of Mines at the end of the current session. Applications for the post should be addressed to the Secretary, Science and Art Department.

THE Fifty-fifth Annual Meeting of the British Association will commence on Wednesday, September 9, 1885, at Aberdeen. The President-Elect is the Right Hon. Sir Lyon Playfair, K.C.B., M.P., Ph.D., LL.D., F.R.S. L. & E., F.C.S., who will take the place of Lord Rayleigh. The Vice-Presidents are His Grace the Duke of Richmond and Gordon, K.G., Chancellor of the University of Aberdeen, the Right Hon. the Earl of Aberdeen, LL.D., Lord-Lieutenant of Aberdeenshire, the Right Hon. the Earl of Crawford and Balcarres, F.R.S., James Matthews, Lord Provost of the City of Aberdeen, Prof. Sir William Thomson, F.R.S., Alexander Bain, M.A., LL D., Rector of the University of Aberdeen, the Very Rev. Principal Pirie, D.D., Vice-Chancellor of the University of Aberdeen, Prof. W. H. Flower, F.R.S., Pres.Z.S., Director of the Natural History Museum. General Treasurer: Prof. A. W. Williamson, F.R.S., University College, London, W.C. General Secretaries: Capt. Douglas Galton, C.B., F.R.S., A. G. Vernon Harcourt, F.R.S. Secretary: Prof. T. G. Bonney, F.R.S. Local Secretaries for the meeting at Aberdeen: J. W. Crombie, M.A., Angus Fraser, M.A., M.D, Prof. G. Pirie, M.A. Local Treasurers for the Meeting at Aberdeen: John Findlater, Robert Lumsden. The Sectional Officers are as follows:—A. Mathematical and Physical Science. President: Prof. G. Chrystal, M.A., F.R.S.E. Vice-Presidents: Prof. C. Niven, F.R.S., Prof. A. Schuster, F.R.S. Secretaries: R. E. Baynes, M.A., R. T. Glazebrook, F.R.S., Prof. W. M. Hicks, M.A. (Recorder), Prof. W. Ingram, M.A. B. Chemical Science. President: Prof. H. E. Armstrong, F.R.S. Vice-Presidents: Prof. A. Crum Brown, F R.S., Prof. H. McLeod, F.R.S. Secretaries: Prof. P. Phillips Bedson, D.Sc., F.C.S. (Recorder), H. B. Dixon, M.A., F.C.S., H. Forster Morley, D.Sc., F.C.S., W. J. Simpson, M.D. C. Geology. President: Prof. J. W. Judd, F.R.S., Sec.G.S. Vice-Presidents: John Evans, Treas.R.S., Prof. W. C. Williamson, F.R.S. Secretaries: C. E. De Rance, F.G.S., J. Horne, F.R.S.E., J. J. H. Teall, M.A., F.G.S., W. Topley, F.G.S. (Recorder). D. Biology. President: Prof. W. C. McIntosh, F.R.S. Vice-Presidents: Prof. I. Bayley Balfour, F.R.S., Prof. J. S. Burdon Sanderson, F.R.S. Secretaries: W. Heape, J. Duncan Matthews, F.R.S.E., Howard Saunders, F.L.S., F.Z.S. (Recorder), H. Marshall Ward, M.A. E. Geography. President: Lieut.-General J. T. Walker, C.B., R.E. F.R.S., F.R.G.S. Vice Presidents: Prof. James Donaldson, F.R.S.E., John Rae, M.D., F.R.S. Secretaries: J. S. Keltie, F.R.G.S., J. S. O'Halloran, F.R.G.S., E. G. Ravenstein, F.R.G.S. (Recorder), Rev. G. A. Smith. F. Economic Science and Statistics. President: Prof. Henry Sidgwick, M.A., Litt.D. Vice-Presidents: Prof. R. Adamson, M.A., LL.D., Sir Rawson W. Rawson, K.C.M.G., C.B., Pres.S.S. Secretaries: Rev. W. Cunningham, D.Sc., F.S.S., Prof. H. S. Foxwell, F.S.S. (Recorder), C. McCombie, M.A., J. F. Moss, F.R.G.S. G. Mechanical Science. President: Benjamin Baker, M.Inst.C.E. Vice-Presidents: Prof. W. C. Unwin, M.Inst.C.E., Prof. H. C. Fleeming Jenkin, F.R.S., M.Inst.C.E. Secretaries: A. T. Atchison, M.A., M.Inst.C.E. (Recorder), F. G. Ogilvie, M.A., E. Rigg, M.A., H. T. Wood, M.A. H. Anthropology. President: Francis Galton, F.R.S., President of the Anthropological Institute. Vice-Presidents

W. Pengelly, F.R.S., Prof. W. Turner, F.R.S. Secretaries: G. W. Bloxam, F.L.S. (Recorder), J. G. Garson, M.D., Walter Hurst, B.Sc., A. MacGregor, M.B. The First General Meeting will be held on Wednesday, September 9, at 8 p.m. precisely, when the Right Hon. Lord Rayleigh, M.A., D.C.L., LL.D., F.R.S., F.R.A.S., F.R.G.S., will resign the chair, and the Right Hon. Sir Lyon Playfair, K.C.B., M.P., Ph.D., LL.D., F.R.S. L. & E., F.C.S., President-Elect, will assume the Presidency, and deliver an address. On Thursday evening, September 10, at 8 p.m., there will be a Soirée; on Friday evening, September 11, at 8.30 p.m., a discourse by Prof. W. Grylls Adams, M.A., F.R.S., F.G.S.; on Monday evening, September 14, at 8.30 p.m., a discourse on "The Great Ocean Basins," by John Murray, F.R.S.E., Director of the *Challenger* Expedition Commission; on Tuesday evening, September 15, at 8 p.m., a Soirée; on Wednesday, September 16, the concluding General Meeting will be held at 2.30 p.m. The lecture to working men will be on the "Nature of Explosions," by Mr. H. B. Dixon, M.A., F.C.S., Fellow of Trinity College, Oxford.

WE understand that the Marquis of Lorne is likely to succeed Lord Aberdare as President of the Royal Geographical Society.

SIR JOHN LUBBOCK responded to the toast of "Science" at the Royal Academy dinner on Saturday evening, and in doing so adduced one more argument on behalf of science as a training and discipline even from the standpoint of art. He claimed for the workers in science that the careful habit of observation and study in which they are necessarily trained enable them to derive peculiar enjoyment from the creations of artistic genius; and he might have suggested in this connection the great advantage to the artist himself of a preliminary training in practical scientific work.

SIR FREDERICK BRAMWELL has evidently a very high ideal of the training necessary to qualify a civil engineer for the performance of the duties of his calling. At the anniversary dinner last week he told his audience that the ideal engineer—"I am glad to say in many cases the real engineer—of the present day is one who has a scientific knowledge as the foundation for his technical training, and frequently that scientific knowledge is of a very extended character. Mechanics, it need hardly be said, are essential, but, in addition, many branches of physical science, such as heat, light, sound, hydraulics, pneumatics, magnetism, electricity, are all now within the knowledge of the accomplished engineer. Moreover, although I do not suggest that every engineer should be a chemist, it is quite certain that he should not be without some chemical instruction, even if it be confined to that which is needed to warn him that the time has arrived when he should seek sound chemical advice."

DR. NOETLING, of the University of Königsberg, has been despatched by the Prussian Academy of Sciences to Lebanon, to study the geology of the Greater Hermon.

THE Italians have lost no time in erecting a meteorological station at Massowah, which they have occupied quite recently.

EARLY in the afternoon of the 2nd a loud detonation was heard from Mount Vesuvius, and two new craters, from which lava issued abundantly, were opened on the southern side at a height of about 200 metres above the upper station of the funicular railway. The lava flowed in the direction of Pompeii and Torre del Greco. The stream descends in a straight line for about half a kilometre, and then, turning sideways, is directed towards the crater of 1872. The new craters present the appearance of a great cleft. The lava has not spread beyond the side of the mountain, and according to the latest telegram the eruption is not increasing.

AT half-past 1 o'clock on the morning of the 1st inst. two or three rather violent shocks of earthquake were felt at Vienna, accompanied by a rolling noise, and causing a great clattering of furniture. Shocks of far greater violence were experienced in Styria, where many houses were damaged and some persons were killed. In the western districts the shocks were of a slight character. The phenomenon appears to have extended southward as far as Grätz and westward to Bavaria. A shock was also felt at Monte Carlo at 10 minutes to 3 on the morning of the 2nd. The shock was strongest in the districts of Condamone and the Cap d'Aile.

THE Annual General Meeting of the members of the Iron and Steel Institute commenced yesterday. The Bessemer medal for the year was presented to Prof. Richard Äkermann by Dr. Percy, F.R.S., the newly-elected President, who gave his inaugural address. The meeting will be continued to-day and to-morrow. The following is a list of some of the principal papers:—On the blast furnace value of coke from which the products of distillation have been collected, by Mr. I. Lowthian Bell, F.R.S.; on the manufacture of steel, by Sir Henry Bessemer, F.R.S.; on the mechanical properties of steel, by Dr. H. Wedding; on the microscopic structure of steel, by Dr. Sorby; on the causes of failures in steel plates, by Mr. W. Parker, of Lloyd's; on a new description of wrought-iron castings, by Mr. T. Nordenfelt; on natural gas, and its utilisation for manufacturing purposes in the United States, by Mr. A. Carnegie; on a modified type of the Siemens gas-producer, whereby the gases are enriched and the bye-products recovered, by Mr. J. Head. We propose to draw attention to the scientific points in some of these papers next week.

PROF. W. ODLING will give the first of two lectures on Organic Septics and Antiseptics, at the Royal Institution, on Saturday, May 16.

THERE is an excellent programme for May at the Royal Victoria Hall and Coffee Tavern, Waterloo Bridge Road, S.E. The science lectures on Tuesdays will be given by Dr. Dallinger, on wonderful things we do not personally see, on the 12th; and by Prof. Perry on the spinning tops of Japan and other countries, on the 19th. Owing to the depression in trade and wishing to put enjoyable entertainments within every one's means, the management have decided to lower the prices of admission during May.

THE Russian Geographical Society has awarded, this year, its great Constantine medal to M. A. S. Woeikoff for his important work, "The Climates of the Globe, and especially of Russia." Analysing this work in the "Annual Report for 1884" of the Society, Dr. Robert E. Lenz shows how original it is in its fundamental idea. Instead of representing the climates as they result from the averages of climatological elements, as is usually done in meteorological works, M. Woeikoff, like Dr. Hann in his "Handbuch der Klimatologie," but with much more fullness and details, tries to explain the local alterations which the general meteorological laws are submitted to in various countries in consequence of the topographical features of these last; and he verifies his conclusions with regard to each country by comparing them with those arrived at as to the climates of neighbouring countries, and establishes thus the elements of a comparative meteorology. The extensive travels of the author in Asia and America have enabled him to recognise the leading meteorological features of the climates he describes and to become acquainted, by personal knowledge, with the topographical features of each separate region. The first twenty-two chapters of this volume, 640 pages, are devoted to a detailed analysis of the chief meteorological elements: the heat received from the sun; the dynamical and thermical consequences of the rising and falling of masses of air; the hydrometeors and their influence on the climates of separate regions—many quite new and original remarks and observations being embodied in these five chapters; the influence of snow and ice-coverings—two chapters again where the meteorologist and geologist will find a series of most interesting suggestions; the temperature of lakes, seas, and oceans; the influence of wind; the variations of temperature with the height—very carefully discussed; the diurnal changes and the unperiodical ones; and finally, the influence of climate on vegetation, and *vice versâ*—again two chapters full of new appreciations. The climates of eight separate regions—Atlantic, North and Middle America, Tropical America, Middle and South Africa, Mediterranean basin, North-West and Middle Europe, South-Eastern Asia, and finally Russia and Northern Asia, are discussed with great detail and with a richness of quite new data in ten separate chapters. Needless to add that the author, well acquainted with so many foreign languages, has embodied in his work all that is worthy of notice in meteorological literature. The work is illustrated by ten maps and fourteen drawings, and contains very numerous tables.

DURING the opposition of Neptune, just passed, we learn from *Science*, Prof. Pickering continued the observation of the planet's magnitude with the meridian photometer of the Harvard College Observatory in the same method as previously employed. Nine series of observations extend from December 16, 1884, to January 21, 1885, the final result from which, when corrected for atmospheric absorption, instrumental error, and reduction to mean opposition, becomes 7·63. The residual difference for only one series is as great as two-tenths of a magnitude. The corresponding results for two previous seasons are 7·71 and 7·77. Contrary to the experience of Mr. Maxwell Hall, of Jamaica, who found evidence for a rotation-period of Neptune in small variations of the planet's light according to his own observations, Prof. Pickering regards it as improbable that there is any variation in the light of Neptune of a strictly periodic character, and further calls attention to the influence, much neglected by observers, upon the observed brightness of objects when seen east and west of the meridian on the same night. This has to be taken account of in the observations of maxima and minima of many variable stars, and may to some extent account for the variations of Neptune's light detected by Mr. Hall.

THE report of the Post Office, Telegraph, and Observatory Departments of South Australia for the past year contains a detailed account *inter alia* of the work of the Observatory Department in that colony since its foundation in 1867. It would be impossible to do more than refer generally here to numerous details given in the ten closely-printed foolscap pages devoted to the subject. Since its establishment the department appears to have kept pace with the strides of the colony to which it belongs. The astronomical observatory at Adelaide is now well supplied with meteorological appliances, having self-recording and other instruments necessary to constitute it a first-class station as defined by the Meteorological Congress at Vienna. There are also fourteen well-equipped stations of the second order, scattered all over the colony, from Port Darwin, in the extreme north of the continent, to Cape Northumberland in the extreme south. Rain-gauges are kept at every telegraph office in the colony; in 1870 there were reports from forty-six stations; in 1883 from 254, and there are still large gaps to be filled up. A system of weather-telegraphy has been arranged between the Australasian colonies, these being divided into districts or aspects to facilitate the transmission of the messages, and to afford the necessary data for laying down the isobars. The important operations undertaken to determine Australian longitudes are also described in detail. Mr. Todd, the head of the combined departments—post, telegraph, and observatory—anticipates great assistance in the inter-colonial meteorological part of his work from Mr. Clement E. Wragge's high-level meteorological station on Mount Lofty, "as he brings to his work great practical experience and almost unbounded enthusiasm."

THE last number (vol. v. No. 4, 1885) of the *Proceedings* of the Bath Natural History and Antiquarian Field Club contains papers on the group of stones at Stanton Drew, in Somersetshire, by Mr. J. Allen Tucker, in which he favours the theory that these huge monuments are the remnants of a temple, either erected by the Druids or by some primeval or prehistoric race, and only used by the Druids, and were not intended to commemorate a battle, which was too common an event in early times; by Rev. L. Blomefield, on a second specimen of the rare Longicorn beetle found in Bath; by Mr. Williams, on the natural history of British owls; and by Mr. Morgan, on water-supply, principally as applied to domestic purposes. There are also several minor contributions noticed in the summary of proceedings at the meetings.

WE have received the prospectus of a Field Club for Hampshire, the Honorary Secretary to which is Mr. E. Westlake, Fordingbridge, Salisbury. The first meeting is to be held at Winchester on May 28. The marvel is that a county as varied as any in England in this respect should have been so long without its Naturalists' Field Club. White of Selborne on the east, and Kingsley on the north, have made the county a classic one for students of nature. With these examples to live up to, and such a field as Hampshire (including the Isle of Wight) the Proceedings of the new club should be interesting and successful.

THE Report of the Committee of the Kelvingrove Museum of Glasgow for the past year illustrates the truth of a remark of Mr. Higgins in his pamphlet on museums recently noticed in these columns, viz. that the number of museums in which a sum of money could be best spent in making additions is very small; that is, as a rule, arrangement is more needed than acquisition. The Committee of the Kelvingrove Museum report that the establishment has been overcrowded for years, that the enormous amount of specimens of all kinds stored away out of sight is constantly increasing, and that the labour and unremitting watchfulness required to keep such stored specimens from deteriorating grows in proportion, and withdraws from essential and more useful museum work much of the time of the small staff, and it has thus become from year to year increasingly difficult to undertake any considerable project for improving the order, classification, or labelling of any section of the museum. This is certainly a grave evil, for it threatens to destroy the main object of such a museum, viz. public instruction. It is to be hoped that a wealthy and public-spirited town such as Glasgow will not permit this state of things to continue; for, as the Report points out, from the stores already within the museum, supplemented in some departments by inexpensive and easily acquired objects, a natural history museum could be equipped which would satisfactorily illustrate all the range of the animal kingdom, and prove at once of great value to the student of zoology, and a popular attraction to the public. For the rest, there has been "a large, steady, and well-maintained flow of visitors, which does not show any indication of waning."

THE Lyceum of Natural History of Williams College, Williamstown, Mass., the oldest natural history society but one connected with any college in the United States, will celebrate its fiftieth anniversary on the 24th of this month, at which a former member, Dr. W. K. Brooks, of the class of 1870, now Associate of Johns Hopkins University, will deliver an address. The Lyceum proposed to take advantage of the occasion to raise funds to enable it to undertake expeditions to some spot, similar to those which it has undertaken in former years to Labrador, Florida, &c.

We learn from *Science* that the Leander McCormick Observatory of the University of Virginia was inaugurated on April 13, the ceremonies taking place in the public hall of the institution, and Prof. Asaph Hall, of the Naval Observatory, Washington, delivering the address. The principal instrument is the great Clark refractor of 26 inches' aperture. The Observatory has a house adjoining for the director, Prof. Stone, and is possessed of a considerable endowment fund, the gift of Mr. W. H. Vanderbilt, of New York.

Capt. L. U. Herendeen, of San Francisco, communicates the following notes on prehistoric structures in Micronesia to *Science*:—A few years ago I visited Ponapé Island in the Pacific, in E. longitude 158° 22′, and N. latitude 6° 50′. The island is surrounded by a reef, with a broad ship-channel between it and the island. At places in the reef there were natural breaks, that served as entrances to the harbours. In these ship-channels there were a number of islands, many of which were surrounded by a wall of stone five or six feet high; and on these islands there stood a great many low houses, built of the same kind of stone as the walls about them. These structures seem to have been used as temples and forts. The singular feature of these islands is that the walls are a foot or more below the water. When they were built, they were evidently above the water, and connected with the mainland; but they have gradually sunk until the sea has risen a foot or more around them. The natives on the islands do not know when these works were built: it is so far back in the past, that they have even no tradition of the structures. Yet the works show signs of great skill, and certainly prove that whoever built them knew thoroughly how to transport and lift heavy blocks of stone. Up in the mountains of the island there is a quarry of the same kind of stone that was used in building the wall about the islands; and in that quarry to-day there are great blocks of stone that have been hewn out, ready for transportation. The natives have no tradition touching the quarry—who hewed the stone, when it was done, or why the work ceased. They are in greater ignorance of the great phenomena that are going on about them than the white man who touches on their island for a few hours for water. There is no doubt in my mind that the island was once inhabited by an intelligent race of people, who built the temples and forts of heavy masonry on the high bluffs of the shore of the island, and that, as the land gradually subsided, these bluffs became islands.

A correspondent recently referred to the use of artificial teeth by the ancient Romans, as shown by a passage from Cicero, where one of the laws of the Twelve Tables is quoted. The law in question belongs to the Tenth Table (*de jure sacro*), which deals mainly with funerals, with the object of limiting the display and ceremonies attending them. Thus the body must not be burnt in more than three robes, or be attended to the grave by more than ten musicians; women must not tear their faces in time of mourning, nor must the bones be collected to make a new funeral with them, the bodies of slaves could not be embalmed, and the like. Section IX. of Table X., which is the one relating to teeth, reads as follows in Ortolan's text ("Histoire de la Législation Romaine," p. 121): "*Neve aurum addito. Quoi auro dentes vincti escunt, ast im cum illo sepelire urereve se fraude esto*—Add no gold; but if the teeth are bound with gold, then that gold may be buried or burnt with the corpse." The date of the Twelve Tables is put about 450 B.C., and it is thought possible by some writers that some of the provisions relating to funerals were taken from the laws of Solon. It would therefore appear that dentistry was known and practised to some extent in the earliest period of their history by the Romans—to an extent, at any rate, that they used gold for binding the teeth. How the artificial teeth were made, or whether they had artificial teeth at all, is not apparent. In the case of the Etruscan skull mentioned recently in Nature, the artificial teeth are made from the teeth of animals.

M. Soroloff, who continues his regular analyses of the water of the Neva, has come to the conclusion that the differences between the average monthly content of solid mixture in the water and the yearly average may be expressed by a curve whose characteristics are the opposite to those of the curve for the average monthly temperatures. The solid inorganic deposit remaining after the evaporation of a given amount of water is also inversely proportionate to the amount of organic matter contained by the water of the Neva. When comparing these curves for the Neva with that showing the amount of solid matter contained by the Thames (as given in the *Journal* of the London Chemical Society for 1880), it appears that both rivers give the same curves, notwithstanding the wide difference of their origins, which coincidence may lead to the supposition that the above might be considered as a law for the rivers.

The additions to the Zoological Society's Gardens during the past week include a Yellow Baboon (*Cynocephalus babouin* ♀) from West Africa, presented by Mrs. Wilson; a Lesser White-nosed Monkey (*Cercopithecus petaurista* ♀) from West Africa, presented by Mr. James S. Jameson; a Crested Pigeon (*Ocyphaps lophotes*) from Australia, presented by Mr. J. Harrison; a Glaucous Gull (*Larus glaucus*), European, presented by Mr. G. Edison; a Common Viper (*Vipera berus*), British, presented by Mr. W. H. B. Pain; four Common Lizards (*Lacerta vivipara*), British, presented by Mr. H. Hanauer; a Common Squirrel (*Sciurus vulgaris*), British, three Wigeons (*Mareca penelope* ♀ ♀ ♀), three Pintails (*Dafila acuta* ♀ ♀ ♀), two Shovellers (*Spatula clypeata* ♀ ♀), five Common Teal (*Querquedula crecca* ♂ ♂ ♂ ♀ ♀), European, purchased; a Yak (*Pœphagus grunniens*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

Tuttle's Comet.—This comet returns to perihelion in the present year under circumstances which are not favourable for its re-observation, without an ephemeris deserving of some degree of reliance. For the last return in 1871 the perturbations during the previous revolution were very accurately determined by Tischler, of Königsberg, who lost his life before Metz in the Franco-German war; and the comet was detected at Marseilles about seven weeks before the perihelion passage, and was followed at the Cape of Good Hope for a still longer period after it. So far it does not appear that the observations of 1871-72 have been brought to bear upon the predicted elements, nor has it been notified that any one is occupied in ascertaining the effect of planetary attraction since the comet was last observed. Tischler's mean motion for 1871, neglecting perturbation, would bring the comet to perihelion again about 1885, September 23·5 G.M.T., and under this condition the comet's position will by readily commanded during the absence of moonlight in August, but unfortunately the theoretical intensity of light will be below the least value with which it has been thus far observed. Assuming the perihelion passage to fall on September 23, the following would be the rough places of the comet:—

At Greenwich Midnight

	R.A. °	Decl. °	Distance from Earth	Distance from Sun	Intensity of Light
August 10	106·5	+33·8	1·89	1·22	0·19
14	110·1	31·6	1·85	1·19	
18	113·6	29·3	1·82	1·17	0·22
22	117·0	26·7	1·78	1·14	
26	120·4	+24·0	1·75	1·12	0·26

In 1871, when the comet was detected by Borrelly at Marseilles with the aid of Tischler's ephemeris, the intensity of light was 0·54, and at the last Cape observation, 0·33. On August 10 the effect of an acceleration of eight days in the time of perihelion passage would be to increase the comet's right ascension rather more than 3°, and to diminish the declination about 4°·4.

In the middle of September it will have attained the value at which observations have been already made.

This comet was first observed by Méchain on January 9, 1790. It was rediscovered by Mr. Tuttle at Cambridge, U.S., on September 5, 1858: from the observations made in this year its period was ascertained to be about 13·8 years, so that the comet had completed five revolutions since it was found by Méchain, without having been perceived. According to Clausen's calculations it was in perihelion on the following dates:—1803, November 7; 1817, May 18; 1830, December 6; and 1844, June 28. It approaches nearest to the orbit of Jupiter in heliocentric longitude 264°, or at a true anomaly of about 144°, when its distance from the planet's orbit is 0·8. At the comet's last passage through this point in July, 1873, the distance from Jupiter was as great as 8·9.

Subjoined are the elements of the orbit for 1871, expressed as usual in the catalogues, and with a slight correction to the predicted time of perihelion passage, which the observations showed to be required:—

Perihelion passage, 1871, December 1·7974 G.M.T.

	° ′ ″	
Longitude of perihelion	116 4 36	Mean Equinox, 1870
,, ascending node ...	269 17 12	
Inclination	54 17 0	
Eccentricity	0·8210540	
Log. perihelion distance	0·0128823	
,, semi-axis major	0·7601603	

The corresponding period of revolution is 5044·7 days, or 13·812 years.

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, MAY 10–16

(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 May 10

Sun rises, 4h. 18m.; souths, 11h. 56m. 12·1s.; sets, 19h. 35m.; decl. on meridian, 17° 44′ N.: Sidereal Time at Sunset, 10h. 50m.

Moon (New on May 14) rises, 2h. 28m.; souths, 8h. 24m.; sets, 14h. 31m.; decl. on meridian, 1° 30′ S.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury ...	3 55 ...	10 51 ...	17 47 ...	10 7 N.
Venus ...	4 25 ...	12 3 ...	19 41 ...	17 38 N.
Mars ...	3 42 ...	10 44 ...	17 46 ...	11 23 N.
Jupiter ...	11 25 ...	18 41 ...	1 57* ...	13 49 N.
Saturn ...	6 7 ...	14 15 ...	22 23 ...	22 15 N.

* Indicates that the setting is that of the following day.

Occultations of Stars by the Moon

May	Star	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image ° °
11 ...	44 Piscium	... 6 ...	3 27	near approach ...	160 —
16 ...	130 Tauri	... 6 ...	18 31	.. 19 27 ...	137 298

Phenomena of Jupiter's Satellites

May	h. m.		May	h. m.	
10 ...	20 46	II. occ. disap.	14 ...	19 46	I. tr. ing.
13 ...	1 17	I. tr. ing.		22 6	I. tr. egr.
	21 58	IV. occ. disap.	15 ...	20 27	I. ecl. reap.
	22 25	I. occ. disap.			
	22 35	III. ecl. reap.			

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

May	h.	
10 ...	10 ..	Mercury stationary.
11 ...	9 ..	Mercury at greatest distance from the Sun.
13 ...	4 ...	Mercury in conjunction with and 0° 21′ south of the Moon.
13 ...	4 ..	Mars in conjunction with and 2° 4′ north of the Moon.
13 ...	9 ...	Mercury in conjunction with and 2° 27′ south of Mars.
14 ...	18 ...	Venus in conjunction with and 3° 47′ north of the Moon.
16 ...	15 ...	Saturn in conjunction with and 4° 2′ north of the Moon.

GEOGRAPHICAL NOTES

WE regret to learn that the well-known African explorer, Dr. Gustav Nachtigal, died on April 24, on board the German gunboat *Möwe*, off the west coast of Africa. Dr. Nachtigal was born at Eichstadt in 1834, and was trained to the medical profession. In 1862 he went to Algiers on account of his health, and in 1868 he started on his memorable journey into the heart of Africa, visiting the territories of Bornu, Baghirmi, Wadai, Darfur, Kordofan, emerging at Khartoum in 1874. This work placed Dr. Nachtigal in the front rank of African explorers. At the time of his death, it is well known, he was looking after the interests of Germany in connection with its recent annexations in West Africa.

THE death is announced from Königsberg of Dr. Karl Zöppritz, Professor of Geography in the University there, and well known as the author of the reports on the progress of geophysics and of African exploration in Behm-Wagner's Geographical Year-Book.

ON Monday last a lecture was delivered at the Alexandra Palace by Prof. A. H. Keane, on "The Lapps: their Origin, Type, Affinities, Social Usages," in connection with the company of Lapps who have recently arrived in London and pitched their tents on the slopes of Muswell Hill for the summer months. The group consists of seven persons, mostly members of one family, from the Karasjok district in Finmark, Norway, and belongs to the "mountain" or nomad division of the race. As these "Mountain Finns," as the Norwegians call them, are of much purer descent than the "River" and "Sea" Lapps, who have given up their nomad life and now reside in settled communities either along the riverain tracts or on the sea coast, they afford ethnologists a favourable opportunity of studying the type of these primitive inhabitants of the Arctic regions of North-West Europe. They are accompanied by six reindeer, and two native dogs used for tending the herds, and have also brought with them specimens of the three kinds of sledges still in use, and some of the famous snow-shoes with which they travel with such surprising velocity over the frozen surface of the ground. The average height appears to be about five feet (extremes, 4·10 to 5·4 in.); but in some other respects the type seems to have deviated considerably from the Mongolic, to which it fundamentally belongs, and has been assimilated, especially in the colour of the hair and eyes, in the complexion and shape of the nose, to the surrounding Norse populations. But the lecturer seemed disposed to attribute this assimilation rather to like climatic influences than to actual intermixture, of which there is no direct evidence.

AT the last meeting of the Geographical Society of Paris, M. Thouar described in detail the observations made by him during his recent journey in search of the remains of the unfortunate Crévaux expedition in the Northern Chaco. These were divided into notes on the flora and fauna, with specimens of the dialects of the Tobas, Matacos, and Chiriguanos, a description of the dress, ornaments, implements, utensils, &c., of the Tobas and Chiriguanos, anthropological details of the same tribes, an itinerary from Tarija to Asuncion, with numerous meteorological observations, and two documents belonging to the Crévaux mission. He added a study on the Peruvian and Bolivian affluents of the Amazon. With reference to the statement (recently quoted in NATURE) that certain members of the mission were still alive amongst the Indians, M. Thouar stated that this information should be received with the greatest reserve, because the date to which it referred was prior to his own sojourn on the frontier, which he traversed from north to south when no one gave him any such information. Since his return to France he has received directly from Bolivia numerous communications from Dr. Gianneccini, who lives on the frontier; in the e nothing is said on the subject. A monument to Dr. Crévaux is to be unveiled at Nancy on June 13.

THE Geographical Society of Paris has awarded a gold medal (La Rouquette's prize) to the well-known Danish Government publication on Greenland, "Meddelelser om Grönland," in which are from time to time published the results of the Danish scientific expedition exploring that continent. The same honour has befallen Danish subjects twice before, viz. Lieut. Graak, for his explorations of the east coast of Greenland; and Messrs. Bredsdorff and Olsen, for their map, "Esquisse d'une Carte orographique de l'Europe."

GEOGRAPHICAL education in Sweden has for years left much to be desired, but of late steps have been taken for its improvement. In the so-called "Elementär-lärovarken" (classical schools) geography has hitherto been classed as an appendix to history, and at the "Lektor" (candidate) examinations in history and geography questions are only asked about the former study. And while the hours and parts of history-teaching in the schools are detailed, no such arrangement has been made as regards geography; the hours of teaching are, in some cases, even not fixed. However, at the congress of teachers held in Stockholm last year, a resolution was adopted to the effect that geography ought to form a separate study of the school education. The University of Lund is the only institution which possesses an eminent geographer for this Board of Science, viz. Baron von Schverin, who, last year, represented Sweden at the Geographical Congress in Toulouse.

THE last Annual Report of the Russian Geographical Society contains extracts from letters addressed by M. Prjevalsky to the Grand Duke Alexander Alexandrovitch, which contains some further interesting details about his Hoang-ho journey. About the end of May he reached, as known, the foot of the Burkhan-budda Mountains, which inclose the high Thibet plateau separating it from Tsaidam. Leaving there his baggage, he went with only thirteen men to the sources of the Yellow River. The climbing on the 15,700 feet high passage of the Burkhan-budda ridge took three days. The descent, on the contrary, was very short, the plateau of Thibet being there 14,000 to 15,000 feet high. Further 60 miles across the desert plateau brought the traveller to the sources of the Yellow River. They are 13,600 feet above the sea-level, and consist of two rivers coming from the south and west and rising in the hills scattered on the plateau. A wide marshy valley, Odon-tala, 40 miles long and 20 miles wide, feeds numerous springs. The Hoang-ho itself is only a rivulet dividing into two or three branches, each of them but 80 to 100 feet wide, and only 2 feet deep at low water. Some 13 miles below this place the Hoang-ho enters a broad lake, colouring its southern part with its muddy water, and, after leaving it on the east, it enters again another lake, whence it flows out as a large river; further down it makes a great curve to avoid the snow-covered Amis-matchin range, and breaks through, in a wild course, the parallel ridges of the Xuen-lun. On the Thibet plateau the expedition experienced dreadful cold. In the second half of May snow-storms were as strong as in winter, and the night frosts reached $-23°$ Celsius. Still the thin grass covering did not perish and a few flowers reappeared every day under the sun-rays. Even in June and July the thermometer fell during bright nights as low down as $-5°$. As to rain, it poured every day, sometimes several days without interruption. The amount of vapour brought by the south-west monsoon and deposited there is so great that, during the summer, Northern Thibet becomes an immense marsh. Needless to say that the advance was difficult for camels. Though uninhabited by man, these deserts were full of herds of yakes, khoulans, antelopes, and mountain sheep; even bears were seen in groups, sometimes of more than ten at once; some thirty pairs were shot down; they are altogether very cowardly, and fly even when wounded. After having spent a few days at the source of the Hoang-ho, M. Prjevalsky went south to the Blue River, called there Dy-tchou by the Tangoutes. The plateau remained hilly, mostly covered with marshes, where the Thibet rush, hard as iron wire, grows freely. The water-divide between the two rivers has an altitude of 14,500 feet. Further south the region takes the characters of an Alpine country, still devoid of forests, but with a richer and more varied grass vegetation. Tangoutes, of the Kam branch, were met with, and received the travellers, though not friendlily, yet not as enemies. Some 70 miles across a mountain region brought M. Prjevalsky to the Dy-tchou River, at an altitude of 12,700 feet. The river, deep and very rapid, is 350 to 420 feet wide. To ford it with camels was quite impossible, so that a further advance to the south had to be renounced. So it was decided to stay there a week and then return to explore the great lakes of the Hoang-ho. During this stay the Tangoutes fired once from the opposite bank of the Dy-tchou. Returning to the Hoang-ho, M. Prjevalsky took another route to reach the lakes of this river, finding his way without guides. The Tangoutes closely followed the party, and on July 13 suddenly attacked them. This attack, as also another one, were repulsed, and the only further difficulties were in the rains and snow-storms (end of July). On the southern foot of the Burkhan-budda Mountains a party of gold-washers was met with. They did not dig the soil deeper than one or two feet, and their washing was most primitive. Still they showed handfuls of gold, mostly in corns as large as a pea, or twice and thrice the size. After having thus laid over more than 670 miles the party returned to Tsaidam, which appeared to them, as desert as it is, a real Eldorado in comparison with the Thibet plateau.

BESIDES the special medals awarded to M. Woeikoff and M. N. J. Zinger, the other medals of the Russian Geographical Society have been awarded as follows:—Small gold medals to the members of the last Pamir expedition, Col. Putyata; M. Ivanoff, geologist; and M. Bendersky, topographer, as also to M. Gavriloff for a manuscript on the religious beliefs of the Votyaks, and to Prof. Zomakion for magnetic measurements at Kazan. The great gold medals were awarded this year by the Sections of Ethnography and Statistics to M. Shein for his "Materials for the Study of the Customs and Language of the Russian Population in the North-West Provinces of Russia," and to M. Yanjul on the manufactures of the Government of Moscow. Sixteen silver medals have been awarded for several papers published in the publications of the Society, for observations extended over more than ten years on thunderstorms and rainfall, to those students who helped Prof. Zomakion in his magnetic measurements, and so on.

THE eccentricities of the European nomenclature of distant regions is well exemplified in the case of the eastern portion of the Indo-Chinese peninsula to which so much attention is attracted just now by the political events in progress there. On some English maps we find four separate divisions: starting from the north, Tonquin placed next to China; then Annam; then Cochin-China, and finally French Cochin-China. In the map accompanying Mr. Colquhoun's recent work, "Amongst the Shans," territory inhabited by independent tribes is inserted between Tonquin and China, which gives five divisions. This latter, however, is wholly incorrect, as the Tonquin frontier proper marches with that of China. In other maps (chiefly in those published in France) Annam and Cochin-China are thrown in together and called indifferently Annam or Cochin-China; while in others, mainly those of from ten to twenty years old, the whole coast from the Chinese frontier to the French colony of Saigon is called sometimes Cochin-China, sometimes Annam. We derive the name Cochin-China from the early navigators, who applied it to the whole coast round from Siam to China; and various generations, in search of trade rather than of geographical accuracy, have added to the confusion. Since the beginning of the present century, when the rulers of Annam imposed their yoke on Tonquin, there has been only one political power on this coast, viz. Annam. As the territories of this State stood twenty-five years ago, it was bounded by China, the Shan States, Siam, Cambodia, and the ocean, and, with the exception that France obtained three small States at the extreme south in 1861, so it stands at present. Tonquin was a feudatory State of Annam when the present war broke out. In a history of Annam recently published by Abbé Launay, a missionary in these regions, we find his title-page runs thus: "Histoire Ancienne et Moderne de l'Annam—Tong-King et Cochin-Chine—depuis, &c., &c.;" and in some interesting preliminary observations on these names, he explains that the titles Tonquin and Cochin-China are relatively recent, and are employed only by Europeans, and never by the Annamites. Tonquin comes to us from *Dong-kinh*, formerly the name of the capital, now called Hanoi; while Cochin-China comes from *Chen-chin*, the name given to the ancient State of Ciampa, situated to the extreme south of the peninsula. *Chen-chin* was probably preceded at one time by *Cao*, an abbreviation of Cao-tchi (*Giao-chi*), and from *Cao-chen-chin* Europeans have made Cochin-China. The name Annam was first given by the Chinese in the third century of our era. It was never used in the official documents between the two countries, but it is that by which the Annamites now call their country. It was at first applied to Tonquin only, but it was extended by conquest to Cochin-China, the ancient Ciampa. It should not, says Abbé Launay, be used for Cochin-China as distinct from Tonquin, but to the two united. The term *Giao-chi*, above alluded to, was that employed in the earliest epochs for the people inhabiting Annam, and was extended to the country. Their historians record that when the Emperor of China, Hoang-ti, formed the Chinese Empire in the twenty-sixth century before our era, he took Giao-chi as his boundary in the south-west. An ancient sacred book of the Chinese, the "Chou-king," which was collected in the sixth century before our era by Confucius from the remnants of still earlier works, refers to a tribe south of the Chinese frontier as the Giao-chi, which means "toes spread out," or "far apart," a term which points to a wide separation between the great toe and the others. This curious distinctive racial mark exists to-day, notwithstanding the lapse of time and the social revolutions of twenty-five or thirty centuries amongst the Annamites. We might therefore adopt the native distinctions as stated by Abbé Launay *en bloc*, and call the whole region Annam, with sub-divisions Tonquin and Cochin-China; or, making a sacrifice of strict accuracy to long habit, we might call the whole Cochin-China, with sub-divisions Tonquin and Annam. But it is probably as hopeless at present to expect strict uniformity in these names as it is to expect it in the orthography of Tonquin, although uniformity even in doing wrong would be better here than the present confusion.

AT the meeting of the Dutch Geographical Society on April 18, Mr. Robidée Van der Aa delivered a lecture on "Papuans and Melanesians, and their Relation to the Malay-Polynesian Race." Succinctly stated, the opinions expressed in the lecture were these:—The opinion once prevailed that the Papuans were the autocthones of the Malayan Archipelago, but that they were conquered by the Malays. There is, however, no support for this supposition, since in the interior of none of the Sunda Islands has a tribe been found bearing any resemblance to the Papuans. Since the researches and discoveries of Miklucho-Maclay we may not consider their hair or their dark skin as a decisive distinction with regard to other tribes. Moreover, it is now stated that their language is related to the Malayan tongue; there are still many customs and usages found amongst them similar to those met with among Malays. From all this Mr. Van der Aa concludes that the Papuans are one of five families, all of which have descended from one "insular race," and were separated from each other at an early date.

THOUGH nothing was said at the Dutch Geographical Society on April 18 about the expedition undertaken to the West Indies by Prof. Martin and Prof. Suringar, we now learn that they left Curaçoa in March. The former, accompanied by Mr. Van de Poel, arrived at Paramaribo and intended to make an excursion to the "Boven Suriname" on March 30; the latter intends to go to Venezuela, and after that to some of the Windward Islands, viz. St. Martin's, St. Eustathius, and Saba.

WE take from the Annual Report of the Russian Geographical Society the following figures giving the average temperatures for twenty-two months at the Sagastyr Polar Station at the mouth of the Lena. The following figures are on the Centigrade scale, and the first of them gives the average of the corresponding month for the year 1882–1883, while the second is the average of the same month for the year 1883–1884:—September, 0°·1 and 0°·6; October, −15°·1 and 14°·1; November, −27°·9 and −25°·7; December, −33°·5 and −33°·3; January, −37°·2 and −35°·8; February, −41°·3 and −34°·0; March, −31°·5 and −35°·2; April, 20°·7 and −21°·8; May, −8°·1 and −9°·7; June, 0°·9 and −0°·2; July, 5°·1; August, 3°·8. Average of the first year. −17°·1; of the second (incomplete), −16°·7. As seen, both years are closely similar; the exceedingly low temperatures of February, 1883, are most remarkable, the average of the month being only −41°·3, and the lowest temperature observed having been −52°·3 for the first year and −48°·0 during the second. The auroras were also less frequent, and the magnetic perturbances feebler. The number of hours during which auroras were observed is seen from the following figures:—September, 13 hours in 1882–1883, and 23 hours in 1883–1884; October, 87 and 69; November, 179 and 83; December, 191 and 178; January, 194 and 151; February, 197 and 126; March, 137 and 118; April, 10 and 8; none in May to August. Total for the first year, 1008; for the second, 756.

IT results from the same report that the delta of the Lena extends, by nearly one-half a degree, further north than on our best maps. The northern cape of the Danube (Dounay) Island is under 73° 55′ north latitude. This determination does not correspond with the Vega map, where Sagastyr, being under 73° 21′, the northern extremity of the island is under 73° 27′, and the course of the Vega in this longitude is under 74° 8′. At any rate, M. Yurgens has been compelled to go for twenty miles north of Sagastyr before reaching the extremity of the Dounay Island.

SOME EXPERIMENTS ON THE VISCOSITY OF ICE

THAT ice will change its form under the influence of pressure is exemplified at large in glaciers, and may be illustrated by experiments in the laboratory. How far this is due to a true viscosity, and how far to a rearrangement of the particles by melting and regelation, is a question the discussion of which among physicists has been of long continuance, though there there may now perhaps be some signs of permanent yielding under the influence of continuous pressure.

In the first volume of NATURE (p. 534) Mr. Wm. Matthews describes experiments (1870) in which planks of ice, supported at each end, but free in the middle, become permanently bent. In the first of these experiments the plank was 6 inches wide, 2⅜ inches thick, and supported by bearers 6 feet apart. The temperature of the air was above the freezing-point of water. The plank bent rapidly, so that the total deflection was 7 inches in about as many hours. "At its lowest point it appeared bent at a sharp angle, and was rigid in its altered form." Its lower surface showed minute fissures. In a second experiment a plank of somewhat similar dimensions (1⅞ inch thick, 6¼ to 6½ inches wide, 6 feet between the supports) became permanently bent. The amount of deflection was 3⅝ for the upper surface and 3⅛ for the lower surface. The time was 64½ hours. The temperature "never rose above the freezing-point"; but the fact that the thermometer registered 29°·5 F. one morning at 9·30 a.m., and 30° F. the next morning at the same time, would lead us to suppose that the midday temperature was not far from the freezing-point. Similar experiments were subsequently carried out (1871) by Prof. Tyndall, in Switzerland, and are mentioned in NATURE (vol. iv. p. 447).

In NATURE, vol. vi. p. 396, Mr. John Aitken describes experiments in which weighted shillings were caused to sink into blocks of ice. But when the block of ice was previously cooled to about 1° below the freezing-point, a shilling weighted with 90 lbs. and left for three and a half hours, "was found not to have entered in the slightest degree into the ice." Subsequently, in 1873 (NATURE, vol. vii. p. 287), Mr. Aitken described experiments which showed that ice bends the more readily the more air-bubbles it contains. "Temperature," he says, "seemed to have some influence on the rate of bending of these beams, but this point was difficult to determine on account of the different beams bending at different rates at the same temperature; but, so far as could be ascertained from the experiments, the beams bent slower the lower the temperature. The lowest temperature used in these experiments was rather more than 3° F. below freezing."

In 1875 Prof. Pfaff described in *Poggendorff's Annalen* (civ. p. 169, reported in NATURE, vol. xii. p. 317) a carefully conducted experiment in which a paralellopiped of ice 52 cm. long, 2·5 cm. wide, and 1·3 cm. thick, was supported in such a way that 5 mm. at each end rested on the bearers. This was left for seven days, from February 8 to February 15, the temperature varying between −12° and −3·5° C. The total bend was 11·5 mm. That is to say, to translate these measurements into inches for the sake of comparison with the other results, in a bar 20 inches in length between the supports, 1 inch in width, and ½ inch in thickness; the total bending was a little over ·45 of an inch. When the temperature rose to slightly under 0° C. the bending increased, and amounted to 9 mm. (·34 inches) in 24 hours. Other experiments are described by Prof. Pfaff in the same paper, and the general conclusion to which he is led is, "that even the smallest pressure is sufficient to dislocate ice-particles if it act continuously, and if the temperature of the ice and its surroundings be near the melting point."

In the current volume of NATURE (p. 329) there is a report of a paper recently read before the Royal Society by Mr. Coutts Trotter (to whom I am indebted for references on this subject) "On some physical properties of ice, &c.," in which were described some experiments on the shearing of ice, carried out in a glacier grotto at a nearly uniform temperature of about 0° C. In that report we learn that in the paper itself "reasons are given for supposing that the range of temperature through which ice is sensibly viscous is small."

So far as I know no experiments on the viscosity of ice at very low temperatures have been recorded. It is the object of the present communication to describe some such experiments which I have recently conducted, through the kindness and courtesy of Messrs. J. S. Fry and Sons, of Bristol, in the snow chamber of the refrigerator, at their well-known Cocoa Works.

In this chamber the air, which has been previously condensed and cooled, is allowed to deposit, in the form of snow, the moisture which it can no longer retain owing to the great diminution of temperature due to expansion. George Punter, whose business it is to look after this snow chamber, rendered the most intelligent assistance in preparing the bars of ice, and in conducting the experiments. In this mode of experimentation the great variation of temperature, namely, between $-30°$ C., when the engines are stopped in the evening, and $-12°$ C., as a maximum when they begin work in the morning is an unavoidable drawback. Still, I think that the experiments, although they give uniformly negative results, are worth putting on record.

Experiment 1.—A cylinder of ice was cast with a diameter of 3 inches. Over it was hung, as in the well-known Bottomley experiment (NATURE, vol. v. p. 185), a wire loaded with a total weight of 5 lbs. It was left in the freezing-chamber 6½ hours. No dent was traceable on the surface of the cylinder.

Experiment 2.—With a similar cylinder and wire the load was increased to 10 lbs. and the time to 8 hours, with like negative results.

Experiment 3.—With a similar cylinder and wire the load was further increased to 14 lbs. and the time to 17½ hours, with the same result or absence of result. This experiment would seem to show that the ice refused to yield to a pressure of 20 to 30 atmospheres, or probably more, applied in this way and for this time.

Experiment 4.—A bar of ice 1½ inches thick, 2½ inches wide, and supported on bearers 13½ inches apart, was left in the chamber from 12 noon on Monday until 12 noon on Saturday. It showed no sign of bending under its own weight.

Experiment 5.—A similar bar similarly supported was weighted in the middle with 7 lbs., and left for the same time. No sign of bending.

Experiment 6.—A similar bar similarly supported was weighted with 18 lbs., and left for the same time. There was no bending perceptible to the eye; but, on removing the apparatus, the bar broke with the jar occasioned by setting it down somewhat carelessly, so that no exact measurement was taken.

Experiment 7.—A bar of the same length and width, but thinner, tapering somewhat from ⅞ to ⅝ of an inch in thickness, was weighted with 7 lbs., to which, during the last two days, seven additional pounds were added, and left for the same time. No bending by measurement.

Such negative results are just what one would expect on theoretical grounds, and as an inference from previous experiments conducted at temperatures nearer the melting-point. But it is well not to rely on theory or on inference where direct experiment is practicable.

The matter, then, would appear to stand at present somewhat thus. The viscosity of ice, due to whatever cause, is—

(1) At temperatures at and above the melting-point...considerable.
(2) ,, ,, below but near ,, ,, ...much less.
(3) ,, ,, between $-3°.5$ C. and $-12°$ C....very slight.
(4) ,, ,, below $-12°$ C. ... ...*nil*.

What seems now to be wanted is an experimental determination of the lower temperature-limit of viscosity, which would appear to lie somewhere between $-12°$ C. and $-3°.5$ C., but probably nearer the latter temperature.

University College, Bristol — C. LLOYD MORGAN

BEN NEVIS

AT the meeting of the Royal Society of Edinburgh held on Monday last, Mr. John Murray, Vice-President, in the chair, Mr. R. T. Omond, Superintendent of the Meteorological Observatory on Ben Nevis, delivered, at the request of the Council, an address on two years' residence and work there. Mr. Omond, at the outset, recalled the advantages which Ben Nevis presented as a high-level meteorological station, the services of Mr. Clement S. Wragge, and the chief steps that led up to the erection and equipment of the existing permanent observatory. Glancing at some of their daily experiences during last summer and autumn, he mentioned that some 3000 or 4000 tourists climbed the mountain—sometimes at least 100 in a single afternoon. Since the middle of October, however, not more than half a dozen strangers had ventured up. Some came for information; others were disappointed at finding they could not be fed as well as sheltered; others came to spend the night, but were disappointed at finding they could not do so. Most of the visitors, however, were satisfied, though a little astonished, by the explanation that the building on Ben Nevis was primarily a scientific observatory, and not a hotel. Storms of exceptional and terrific violence were described. Beautiful optical phenomena that had been witnessed, and the comparative scarcity of animal life on the mountain, were next alluded to. Rainbows are seldom seen. Thunderstorms are very rare. The temperatures during winter are not so low as many people think—10° F. is about the lowest recorded as yet, and the ordinary winter temperatures ran from 15° to 25°. Observing that much must yet be done in the work of the discussion and interpretation of the observations made on Ben Nevis, before the observations could be safely used, he proceeded to state some of the more interesting points which Mr. Buchan had already succeeded in approximately establishing: (1) The normal or average temperature and barometric pressure for each month, and the normal differences between these averages and those at sea-level. (2) The daily variation of temperature and pressure during each month. (3) The daily variation in the average velocity of the wind—this being shown to be greater at night than during the day, exactly the reverse of what holds good at sea-level. (4) Variations in the direction of the winds as compared with those prevalent over Scotland at any given time. A comparison of the Ben Nevis winds with those at low-level stations sometimes shows that both are part of one system, whether cyclonic or anti-cyclonic; but the direction is almost always different, and in the case of cyclonic storms, coming from the west. The observed differences in direction seem to give an indication as to whether the storm centre is to pass to the north or south of Ben Nevis. If this point can be definitely made out, it will obviously be of immense value in forecasting weather. (5) The hygrometric observations indicate that the atmosphere on the Ben shows that during ordinary weather a state of persistent saturation, usually accompanied by fog or mist, prevails; but occasionally a sudden and extraordinary drought sets in, the temperature rises, and the sky clears, not merely of fog, but often of every vestige of cloud, and at the same time the valleys and lower hills are often shrouded in mist, showing that this dryness coming from above is not able to penetrate right down to the sea-level. The thorough investigation of these phenomena is one of the most important pieces of work connected with the Observatory, and may be expected to throw great light on the question of atmospheric circulation. (7) The rainfall of Ben Nevis is greatly in excess of what several theories of the distribution of rain led them to expect—a result possibly due to the great vertical movements of the atmosphere indicated by the hygrometric indications referred to above. Though there are many high-level stations in different parts of the world, none, perhaps, are so favourably situated as Ben Nevis for the investigation of what he had explained is the present great problem in meteorology, namely, the vertical movements of the atmosphere. If the Scottish Meteorological Society were possessed of sufficient funds to establish a completely-equipped observatory at the foot of Ben Nevis as well as on the summit, he was convinced that the science of meteorology would advance far more in a few years than it would by a generation of ordinary work with low-level stations alone.

SUNLIGHT AND THE EARTH'S ATMOSPHERE[1]

THERE is, we may remember, a passage in which Plato inquires what would be the thoughts of a man who, having lived from infancy under the roof of a cavern, where the light outside was inferred only by its shadows, was brought for the first time into the full splendors of the sun.

We may have enjoyed the metaphor without thinking that it has any physical application to ourselves who appear to have no roof over our heads, and to see the sun's face daily; while the fact is that if we do not see that we have a roof over our heads in our atmosphere, and do not think of it as one, it is because it seems so transparent and colourless.

Now, I wish to ask your attention to-night to considerations in some degree novel, which appear to me to show that it is not transparent as it appears, and that this seeming colourlessness is a sort of delusion of our senses, owing to which we have never

[1] Lecture delivered at the Royal Institution, April 17, 1885, by S. P. Langley. Communicated by the author.

in all our lives seen the true colour of the sun, which is in reality blue rather than white, as it looks, so that this air all about and above us is acting like a coloured glass roof over our heads, or a sort of optical sieve, holding back the excess of blue in the original sunlight, and letting only the white sift down to us.

I will first ask you, then, to consider that this seeming colourlessness of the air may be a delusion of our senses, due to habit, which has never given us anything else to compare it with.

If that cave had been lit by sunshine coming through a reddish glass in its roof, would the perpetual dweller in it ever have had an idea but that the sun was red? How is he to know that the glass is "coloured" if he has never in his life anything to compare it with? How can he have any idea but that this is the sum of all the sun's radiations (corresponding to our idea of white or colourless light); will not the habit of his life confirm him in the idea that the sun is red; and will he not think that there is no colour in the glass so long as he cannot go outside to see? Has this any suggestion for us, who have none of us ever been outside our crystal roof to see?

We must all acknowledge in the abstract, that habit is equally strong in us whether we dwell in a cave or under the sky, that what we have thought from infancy will probably appear the sole possible explanation, and that, if we want to break its chain, we should put ourselves, at least in imagination, under conditions where it no longer binds us.

The *Challenger* has dredged from the bottom of the ocean fishes which live habitually at great depths, and whose enormous eyes tell of the correspondingly faint light which must have descended to them through the seemingly transparent water. It will not be as futile a speculation as it may at first seem, to put ourselves in imagination in the condition of creatures under the sea, and ask what the sun may appear to be to them; for if the fish who had never risen above the ocean floor were an intelligent being, might *he* not plausibly reason that the dim greenish light of his heaven—which is all he has ever known—was the full splendour of the sun, shining through a medium which all his experience shows is transparent?

We ourselves are, in very fact, living at the floor of a great aërial sea, whose billows roll hundreds of miles above our heads. Is it not at any rate conceivable that we may have been led into a like fallacy from judging only by what we see at the bottom? May we not, that is, have been led into the fallacy of assuming that the intervening medium above us is colourless because the light which comes through it is so?

I freely admit that all men, educated or ignorant, appear to have the evidence of their senses that the air is colourless, and that pure sunlight is white, so that if I venture to ask you to listen to considerations which have lately been brought forward to show that it is the sun which is blue, and the air really acts like an orange veil or like a sieve which picks out the blue and leaves the white, I do so in the confidence that I may appeal to you on other grounds than those I could submit to the primitive man who has his senses alone to trust to; for the educated intelligence possesses those senses equally, and in addition the ability to interpret them by the light of reason, and before this audience it is to that interpretation that I address myself.

Permit me a material illustration. You see through this glass, which may typify the intervening medium of air or water, a circle of white light, which may represent the enfeebled disk of the sun when so viewed. Is this intervening glass coloured or not? It seems nearly colourless; but have we any right to conclude that it is so because it seems so? Are we not *taking it for granted* that the original light which we see through it is white, and that the glass is colourless, because the light seems unaltered, and is not an appeal to be made here from sense to reason, which, in the educated observer, recalls that white light is made of various colours, and that whether the original light is really white and the glass transparent, or the glass really coloured and so *making* the white, is to be decided only by experiment, by taking away the possibly deceptive medium? I can take away this glass, which was not colourless, but of a deep orange, and you see that the original light was not white, but intensely blue. If we could take the atmosphere away between us and the sun, how can we say that the same result might not follow? To make the meaning of our illustration clearer, observe that this blueness is not a pure spectral blue. It has in it red, yellow, blue, and all the colours which make up white, but blue in superabundance; so that, though the white is, so to say, latent there, the dominant effect is blue. The glass coloured veil does not put anything *in*, but acts I repeat like a sieve straining *out* the blue, and letting through to us the white light which was there in the bluishness, and so may not our air do so too?

I think we already begin to see that it is at any rate conceivable that we *may* have been hitherto under a delusion about the true colour of the sun, though of course this is not proving that we have been so, and it will at any rate, I hope, be evident that here is a question raised which ought to be settled, for the blueness of the sun, if proven, evidently affects our present knowledge in many ways, and will modify our present views in optics, in meteorology, and in numerous other things. In optics, because we should find that white light is *not* the sum of the sun's radiations, but only of those dregs of them which have filtered down to us; in meteorology because it is suggested that the temperature of the globe and the condition of man on it, depend in part on a curious selective action of our air, which picks out parts of the solar heat (for instance, that connected with its blue light), and holds them back, letting other selected portions come to us, and so altering the conditions on which this heat by which we live, depends; in other ways, innumerable, because, as we know, the sun's heat and light are facts of such central importance, that they affect almost every part of scientific knowledge.

It may be asked what suggested the idea that the sun may be blue rather than any other colour.

My own attention was first directed this way many years ago when measuring the heat and light from different parts of the sun's disk. It is known that the sun has an atmosphere of its own which tempers its heat, and, by cutting off certain radiations and not others, produces the spectral lines we are all familiar with. These lines we customarily study in connection with the absorbing vapours of sodium, iron, and so forth, which produce them; but my own attention was particularly given to the regions of absorption, or to the colour it caused, and I found that the sun's body must be deeply bluish, and that it would shed blue light except for this apparently colourless solar atmosphere, which really plays the part of a reddish veil, letting a little of the blue appear on the centre of the sun's disk where it is thinnest, and staining the edge red, so that to delicate tests the centre of the sun is a pale aqua-marine, and its edge a garnet. The effect I found to be so important, that if this all but invisible solar atmosphere were diminished by but a third part, the temperature of the British islands would rise above that of the torrid zone, and this directed my attention to the great practical importance of studying the action of our own terrestrial atmosphere on the sun, and the antecedent probability that our own air was also and independently making the really blue sun into an apparently white one. We actually know then, beyond conjecture, by a comparison of the sun's atmosphere, where it is thickest, and where it is thinnest, that an apparently colourless atmosphere *can* have such an effect, and analogous observations which I have carried on for many years, but do not now detail, show that the atmosphere of our own planet, this seemingly clear air in which we exist like creatures at the bottom of the sea, does do so.

We look up through our own air as through something so limpid in its purity that it appears scarcely matter at all, and we are apt to forget the enormous mass of what seems of such lightness, but which really presses with nearly a ton to each square foot, so that the weight of all the buildings in this great city, for instance, is less than that of the air above them.

I hope to shortly describe the method of proof that it too has been acting like an optical sieve, holding back the blue; but it may naturally be asked, "Can our senses have so entirely deceived us that they give no hint of this truth, if it be one? is the appeal wholly to recondite scientific methods, and are there no indications, at least, which we may gather for ourselves?" I think there are, even to our unaided eyes, indications that the seemingly transparent air really acts as an orange medium, and keeps the blue light back in the upper sky.

If I hold this piece of glass before my eyes, it seems colourless and transparent, but it is proved not to be so by looking through it edgewise, when the light, by traversing a greater extent, brings out its true colour, which is yellow. Every one knows this in every-day experience. We shall not get the colour of the ocean by looking at it in a wine-glass, but by gazing through a great depth of it; and so it is with the air. If we look directly up, we look through where it is thinnest; but if we look horizontally through it towards the horizon, through great thicknesses, as at sunrise or sunset, is it not true that this air, where we see its real colour most plainly, makes the sun look very plainly yellow or orange?

We not only see here, in humid English skies, the "orange sunset waning slow," but most of us in these days of travel can perfectly testify that the clearest heavens the earth affords, the rosy tint on the snows of Mont Blanc, forerunning the dawn, or the warm glow of the sun as he sets in Egyptian skies, show this most clearly—show that the atmosphere holds back the blue rays by preference, and lets the orange through.

If, next, we ask, "What has become of the blue that it has stopped?" does not that very blue of the midday sky relate the rest of the story—that blue which Prof. Tyndall has told us is due to the presence of innumerable fine particles in the air, which act selectively on the solar waves, diffusing the blue light towards us? I hope it will be understood that Prof. Tyndall is in no way responsible for my own inferences; but I think it is safe at least to say that the sky is not self-luminous, and that, since it can only be shining blue at the expense of the sun, all the light this sky sends us has been taken by our atmosphere away from the direct solar beam, which would grow both brighter and bluer if this were restored to it.

If all that has been said so far renders it possible that the sun may be blue, you will still have a right to say that "possibilities" and "maybes" are not evidence, and that no chain of mere hypotheses will draw truth out of her well. We are all of one mind here, and I desire next to call your attention to what I think is evidence.

Remembering that the case of our supposed dweller in the cave who could not get outside, or that of the inhabitants of the ocean-floor who cannot rise to the surface, is really like our own, over whose heads is a crystalline roof which no man from the beginning of time has ever got outside of, an upper sea to whose surface we have never risen; we recognise that if we could rise to the surface, leaving the medium whose effect is in dispute wholly beneath us, we should see the sun as it is, and get proof of an incontrovertible kind; and that, if we cannot entirely do this, we shall get nearest to proof under our real circumstances by going as high as we can in a balloon, or by ascending a very high mountain. The balloon will not do, because we have to use heavy apparatus requiring a solid foundation. The proof to which I ask your kind attention, then, is that derived from the actual ascent of a remarkable mountain by an expedition undertaken for that purpose, which carried a whole physical laboratory up to a point where nearly one-half the whole atmosphere lay below us. I wish to describe the difference we found in the sun's energy at the bottom of the mountain and at the top, and then the means we took to allow for the effect of that part of the earth's atmosphere still over our heads even here, so that we may be said to have virtually got outside it altogether.

Before we begin our ascent, let me explain more clearly what we are going to seek. We need not expect to find that the original sunlight is a pure monochromatic blue by any means, but that though its rays contain red, orange, blue, and all the other spectral colours, the blue, the violet and the allied tints were originally there in disproportionate amounts, so that, though all which make white were present from the first, the refrangible end of the spectrum had such an excess of colour that the dominant effect was that of a blueish sun. In the same way, when I say briefly that our atmosphere has absorbed this excess of blue and let the white reach us, I mean, more strictly speaking, that this atmosphere has absorbed *all* the colours, but, selectively, taking out more orange than red, more green than orange, more blue than green; so that its action is wholly a taking *out*—an action like that which you now see going on with this sieve, sifting a mixture of blue and white beads, and holding back the blue while letting the white fall down.

This experiment only rudely typifies the action of the atmosphere, which is discriminating and selective in an amazing degree, and as there are really an infinite number of shades of colour in the spectrum, it would take for ever to describe the action in detail. It is merely for brevity, then, that we now unite the more refrangible colours under the general word "blue," and the others under the corresponding terms "orange" or "red."

All that I have the honour to lay before you, is less an announcement of absolute novelty than an appeal to your already acquired knowledge and to your reason as superior to the delusions of sense. I have, then, no novel experiment to offer, but to ask you to look at some familiar ones in a new light.

We are most of us familiar, for instance, with that devised by Sir Isaac Newton to show that white light is compounded of blue, red, and other colours, where, by turning a coloured wheel rapidly, all blend into a grayish white. Here you see the "seven colours" on the screen; but, though all are here, I have intentionally arranged them, so that there is too much blue, and the combined result is a very bluish white which may roughly stand for that of the original sun-ray. I now alter the proportion of the colours so as to virtually take out the excess of blue, and the result is colourless or white light. White, then, is not necessarily made by combining the "seven colours," or any number of them, unless they are there in just proportion (which is in effect what Newton himself says); and white, then, may be made out of such a bluish light as we have described, not by putting anything to it, but by taking away the excess which is there already.

Here, again, are two sectors—one blue, one orange-yellow with the blue in excess, making a bluish disk where they are revolved. I take out the excess of blue, and now what remains is white.

Here is the spectrum itself on the screen, but a spectrum which has been artificially modified so that the blue end is relatively too strong. I recombine the colours (by Prof. Rood's ingenious device of an elastic mirror), and they do not make a pure white, but one tinted with blue. I take out the original excess of blue, and what remains combines into a pure white. Please bear in mind that when we "put in" blue here, we have to do so by straining out other light through some obscuring medium, which makes the spectrum darker; but that, in the case of the actual sunlight, introducing more blue, introduces more light, and makes the spectrum brighter.

The spectrum on the screen ought to be made still brighter in the blue than it is—far, far brighter—and then it might represent to us the original solar spectrum before it has suffered any absorption either in the sun's atmosphere or our own. The Frauenhofer lines do not appear in it, for these, when found in the solar spectrum, show that certain individual rays have been stopped, or selected for absorption by the intervening atmospheres; and though even the few yards of atmosphere between the lamp and the screen absorb, it is not enough to show.

Our spectrum, as it appears before absorption, might be compared to an army divided into numerous brigades, each wearing a distinct uniform, one red, one green, one blue, so that all the colours are represented each by its own body. If, to represent the light absorbed as it progresses, we supposed that the army advances under a fire which thins its numbers, we should have to consider that (to give the case of nature) this destructive fire was directed chiefly against those divisions which were dressed in blue, or allied colours, so that the army was thinned out unequally, many men in blue being killed off for one in red, and that by the time it has advanced a certain distance under fire the proportion of the men in each brigade has been altered, the red being comparatively unhurt. Almost all absorption is thus selective in its action, and often in an astonishing degree, killing off, so to speak, certain rays in preference to others, as though by an intelligent choice, and destroying most, not only of certain divisions (to continue our illustration), but even picking out certain files in each company. Every ray, then, has its own individuality, and on this I cannot too strongly insist; for just as two men retain their personalities under the same red uniform, and one may fall and the other survive, though they touch shoulders in the ranks, so in the spectrum certain parts will be blotted out by absorption, while others next to them may escape.

To illustrate this selective absorption, I put a piece of didymium glass in the path of the ray. It will, of course, absorb some of the light, but instead of dimming the whole spectrum, we might almost say it has arbitrarily chosen to select one narrow part for action, in this particular case choosing a narrow file near the orange, and letting all the rest go unharmed. In this arbitrary way our atmosphere operates, but in a far more complex manner, taking out a narrow file here and another there, in hundreds of places, all through the spectrum, but on the whole much the most in the blue, the Frauenhofer lines being merely part of the evidence of this wonderful quasi-intelligent action which bears the name of selective absorption.

Before we leave this spectrum, let us recall one most important matter. We know that here beyond the red is solar energy in the form of heat which we cannot see, but not on that account any less important. More than half the whole power of the sun is here invisible, and if we are to study completely the action of our atmosphere, we shall have to pay great attention to this part, and find out some way of determining the loss in it, which will be difficult, for the ultra-red end is not only invisible, but compressed, the red end being shut up like the closed pages of a book, as you may notice by comparing the narrowness of the red with the width of the blue.

Now refraction by a prism is not the only way of forming a spectrum. Nature furnishes us colour not only from the rainbow, but from non-transparent substances like mother-of-pearl, where the iridescent hues are due to microscopically fine lines. Art has lately surpassed nature in these wonderful "gratings," consisting of pieces of polished metal, in which we see at first nothing to account for the splendid play of colour apparently pouring out from them like light from an opal, but which, on examination with a powerful microscope, show lines so narrow that there are from 50 to 100 in the thickness of a fine human hair, and all spaced with wonderful precision.

This grating is equal in defining power to many such prisms as we have just been looking at, but its light does not show well upon the screen. You will see, however, that its spectrum differs from that of the prism, in that in this case the red end is expanded, as compared with the violet, and the invisible ultra-red is expanded still more, so that this will be the best means for us to use in exploring that "dark continent" of invisible heat found not only in the spectrum of the sun, but of the electric light, and of all incandescent bodies, and of whose existence we already know from Herschel and Tyndall.

Now we cannot reproduce the actual solar spectrum on the screen without the sun itself, but here are photographs of it, which show parts of the losses the different colours have suffered on their way to us. We have before us the well-known Frauenhofer lines, due, you remember, not only to absorption in the sun's atmosphere, but also to absorption in our own. We have been used to think of them in connection with their cause, one being due to the absorption of iron-vapour in the sun, another to that of water-vapour in our own air, and so forth; but now I ask you to think of them only in connection with the fact that each is due to the absorption of some part of the original *light*, and that collectively they tell much of the story of what has happened to that light on its way down to us. Observe, for instance, how much thicker they lie in the blue end than in the red—another evidence of the great proportionate loss in the blue.

If we could restore all the lost light in these lines, we should get back partly to the original condition of things at the very fount, and, so far as our own air is concerned, that is what we are to ascend the mountain for—to see, by going up through nearly half of the atmosphere, what the rate of loss is in each ray by actual trial; then, knowing this rate, to be able to allow for the loss in the other part still above the mountain-top, and, finally, by recombining these rays to get the loss as a whole. Remember, however, always, that the most important part of the solar energy is in the dark spectrum which we do not see, but which, if we could see, we should probably find to have numerous absorption-spaces in it corresponding to the Frauenhofer lines, but where heat has been stopped out rather than light. To make our research thorough, then, we ought not to trust to the eye only, or even chiefly, but have some way of investigating the whole spectrum; the invisible in which the sun's power chiefly lies, as well as the visible, and both with an instrument that would discriminate the energy in these very narrow spaces, like an eye to see in the dark; and if science possesses no such instrument, then it may be necessary to invent one.

The linear thermopile is nearest to it of any, and we all here know what good work it has done, but even that is not sensitive enough to measure in the grating spectrum, in some parts of which the heat is 400 times weaker than in that of a prism, and we want to observe this invisible heat in very narrow spaces. Something like this has been provided since by Capt. Abney's most valuable researches, but these did not at the time go low enough for my purpose, and I spent nearly a year before ascending the mountain in inventing and perfecting the new instrument for measuring these, which I have called the "bolometer" or "ray-measurer." The principle on which it is founded is the same as that employed by my late lamented friend, Sir Wm. Siemens, for measuring temperatures at the bottom of the sea, which is that a smaller electric current flows through a warm wire than through a cold one.

One great difficulty was to make the conducting wire very thin, and yet continuous, and for this purpose almost endless experiments were made, among other substances pure gold having been obtained by chemical means in a plate so thin that it transmitted a sea-green light through the solid substance of the metal. This proving unsuitable, I learned that iron had been rolled of extraordinary thinness in a contest of skill between some English and American iron-masters, and, procuring some, I found that 15,000 of the iron plates they had rolled, laid one on the other, would make but one English inch. Here is some of it, rolled between the same rolls which turn out plates for an iron-clad, but so thin that, as I let it drop, the iron plate flutters down like a dead leaf. Out of this the first bolometers were made, and I may mention that the cost of these earlier experiments was met from a legacy by the founder of the Royal Institution, Count Rumford. The iron is now replaced by platinum, in wires or rasher tapes, from 1-2000 to 1-20,000th of an inch thick, one of which is within this button, where it is all but invisible, being far finer than a human hair. I will project it on the screen, placing a common small pin beside it as a standard of comparison. This button is placed in this ebonite case, and the thread is moved by this micrometer screw, by which it can be set like the spider line of a reticule; but by means of this cable, connecting it to the galvanometer, this thread acts as though sensitive, like a nerve laid bare to every indication of heat and cold. It is then a sort of sentient thing: what the eye sees as light it feels as heat, and what the eye sees as a narrow band of darkness (the Frauenhofer line) this feels as a narrow belt of cold, so that when moved parallel to itself and the Frauenhofer lines down the spectrum it registers their presence.

It is true we can see these in the visible spectrum, but you remember we propose to explore the invisible also, and since to this the dark is the same as the light, it will feel absorption lines in the infra-red which might remain otherwise unknown.

I have spent a long time in these preliminary researches; in indirect methods for determining the absorption of our atmosphere, and in experiments and calculations which I do not detail, but it is so often supposed that scientific investigation is a sort of happy guessing, and so little is realised of the labour of preparation and proof, that I have been somewhat particular in describing the essential parts of the apparatus finally employed, and now we must pass to the scene of their use.

(*To be continued.*)

THE INSTITUTION OF MECHANICAL ENGINEERS

A VERY interesting discussion on the merits of the Maxim automatic machine-gun, which was described in NATURE, vol. xxxi. p. 414, took place at the special meeting of the Institution held on the 30th ult. In reply to Mr. Carbutt, M.P., the inventor explained that the recoil of the gun, which is utilised for loading and firing, did not interfere with the accuracy of aim, and instanced the circumstance that as good target-practice was made in firing from the shoulder as with fixed rifles; whilst the energy of recoil was sufficient to carry on the automatic action, whether the muzzle of the gun was elevated or depressed. As regarded keeping the barrel cool, he found that the water-jacket around the body of the gun acted most efficiently, as gunpowder in exploding produced very little heat-energy, or, as he put it, "he should not buy gunpowder to raise steam." During all his experiments he had used only Government cartridges, and had never found one to fail; he thought it would be an advantage if cartridges were packed in cases containing calcium chloride or other hydroscopic material, so that they might not be injured by moisture.

The gun was frequently fired during the meeting, and its automatic action was thoroughly shown, as well as its freedom from danger should a cartridge hang fire. Mr. Maxim had a most enthusiastic reception, the general feeling of the speakers and of the meeting being in favour of the gun being taken up by the British Government, the President and Mr. Adamson giving it as their opinion that, if the necessity should occur, 1000 of these guns could be produced weekly at a month's notice, when their use might have as material an effect on a campaign as the needle-gun had at Sadowa. Mr. Maxim is now experimenting with a more recent form of gun of his invention, which fires a projectile 3 lbs. weight at the rate of 120 shots a minute; in this the cartridges are fed from above, which much simplifies the mechanical arrangement, as no apparatus has to be arranged for lifting the cartridges into the magazine of the gun; in this new form the total length of firing apparatus is 22 inches. This arrangement was not described, as the various patents for which the inventor had made application, amounting to over a hundred in all, were not yet complete.

On the 1st inst. was read Prof. Kennedy's abstract of the work of the Research Committee of the Institution on Riveted Joints. There can be no question of the value of a series of experiments of this character, covering in all 290,—64 on perforated (punched and drilled) plates, 97 on actual joints, 44 on the tenacity of the plates used in the joints, 33 on the tenacity and shearing resistance of the rivet steel used in the joints, and the rest on various other matters connected with them. The whole of the experiments were made upon soft steel supplied from the Landore Siemens-Steel Works, which was found to have a tenacity of from 28 to 30 tons per square inch, with an extension of 23 to 25 per cent. in a length of 10 inches. The limit of elasticity of the metal was generally about 60 per cent. of its ultimate resistance, the percentage of carbon in the plates was given as about 0·18.

The main conclusions drawn from these experiments are the following:—The metal between the rivet holes has a considerably greater tensile resistance per square inch than the same metal unperforated, the excess tenacity varying from 20 to about 8 per cent. The shearing resistance of rivet steel is a much more variable quantity than the tenacity of steel plate or of the rivet steel itself—a result due, Prof. Kennedy thinks, in some manner to the want of attention directed to this point, or of experiments specially upon it. The size of the rivet heads and ends plays a most important part in the strength of the joints, at any rate in the case of single riveted joints; an increase of about one-third of weight of metal in the heads and ends increased the resistance of the joint $8\frac{1}{2}$ per cent., the additional strength being no doubt due to the prevention of so great tensile stress in the rivets through distortion of the plates. The strength of a joint made across a plate is equal to that of one made in the usual direction. The intensity of bearing pressure on the rivets exercises, with joints proportioned in the ordinary way, a very important influence on their strength. The value of hydraulic as compared with hand riveting, in the case when sound hand-riveting is possible, lies in the increased security and stiffness it gives at ordinary working loads rather than in any actual raising of the breaking load.

The experiments point to very simple rules for proportioning joints of maximum strength. Assuming a bearing pressure on the rivet of 43 tons per square inch, and an excess tenacity of the plate of 10 per cent. of its original strength, the diameter of the rivet-hole should be $2\frac{1}{4}$ times the thickness of the plate, and the pitch of the rivets $2\frac{5}{8}$ times the diameter of the holes for single riveted joints, while for double-riveted lap-joints with the same ratio of diameter to thickness the ratio of pitch to diameter should be from 3·64 to 3·82. If a smaller rivet be used than that here specified, the joint will not be of uniform, and therefore not of maximum, strength; but with any other size of rivet the best result will be obtained by using a pitch calculated from the following formula, viz.:—

$$p = a\frac{d^2}{t} + d,$$

where p is the pitch, d the diameter of the hole, and t the thickness of the plate, whilst the mean value of the constant a is 0·56. By use of this formula for double-riveted lap-joints it is likely that the prescribed size of rivet may be inconveniently large in practice. In this case the diameter of the rivet should be taken as large as possible, and the above formula will give the pitch, by making the constant $a = 1·15$ in the mean. For double-riveted butt-joints of maximum strength the diameter of the rivet hole should be 1·8 times the thickness of the plate, and the pitch should be 4·1 times the diameter of the hole.

In a boiler the plate is much more affected by time than the rivets, and it is therefore not unreasonable to estimate the percentage by which the plates might be weakened by corrosion before the boiler would be unfit for use at its proper steam-pressure, and to add correspondingly to the plate area. In this case the joint should be proportioned not for the actual thickness of the plate, but for a nominal thickness less than the actual by the assumed percentage. The joint will thus be approximately one of uniform strength by the time it has reached its final workable condition, up to which time the joint as a whole will not really have been weakened, the corrosion only gradually bringing the strength of the plates down to that of the rivets.

There is an interesting point to which we propose reference on a future occasion, viz. the probable causes to which an increase of tensile strength in the remaining material of perforated plates may be due.

The President, at the conclusion of an interesting discussion, referred to the circumstances that the paper was an abstract of three years' work, the experiments having been carried on by Prof. Kennedy free of charge to the Institution, whilst the material supplied and the work of preparing the various joints tested had been performed at prime cost.

The main feature in connection with the Blooming Mill designed by Mr. C. B. Holland, of Ebbw Vale, a paper describing which was read, was the application of hydraulic power to all the work performed, except that of actually driving the rolls.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—On Tuesday last an effort was made to relieve Honour men in the non-classical final schools from the drudgery entailed by the present Pass Classical Moderations. Council brought forward a scheme substituting a Preliminary Honour Examination in each Honour School in lieu of "Pass Mods." Under this scheme the study of the classical languages would not be required of any candidate for honours in Natural Science or Mathematics after he had passed the present Responsions or "Smalls;" thus an extra year would be given to the study of the subjects chosen for the final schools. Unfortunately for the measure, the preamble—mainly of general character—contained one clause relating to Mathematics which would have exempted mathematical class-men from any literary work, and the preamble was therefore opposed by many who approved of its general policy bnt desired an opportunity of discussing or amending the Preliminary Examinations to be introduced into each Faculty. The preamble, after a long debate, was lost on a division by one vote only—71 voting for it and 72 against. No doubt a similar measure will be brought forward again.

SCIENTIFIC SERIALS

To the number of the *Journal of Botany* for March Mr. F. Townsend contributes an illustrated paper on the floral envelopes of Cyperaceæ and Gramineæ, in which he claims a closer homology between the corresponding parts in the two orders than has generally been allowed.—Mr. G. Murray has another and apparently a final word on the so-called "sclerotiets" found by Mr. A. S. Wilson and Mr. Worthington Smith in the leaves of diseased potatoes, and which he demonstrates not to have the organic structure attributed to them and to be unconnected with the potato disease.—In the number for April Mr. T. Hick gives a further observation on the continuity of protoplasm, which he finds to prevail throughout the frond of a seaweed belonging to a different group from those in which it had hitherto been obeerved, the common *Ascophyllum nodosum* (*Fucus noaosus*).—In addition to these papers there are in these two numbers others which are descriptive or refer to local botany.

THE most interesting article in the *Nuovo Giornale Botanico Italiano* for January, 1855, is by Signor Cugini on the anatomy of the inflorescence and of the flowers of *Diöon edule*. The intermediate position occupied by the Gymnosperms, and especially by the Cycadeæ, between Cryptogams and the more highly developed flowering plants, renders especially valuable any fresh contribution towards the knowledge of the structure of their reproductive organs. The present paper deals especially with the anatomy of the ovule and of the ovuliferous leaves.—The greater part of the same journal for April is occupied by a very elaborate paper by Signor J. Danielli, illustrated by a number of plates, on the structure, distribution, and uses of the American aloe, *Agave americana*.—A shorter article of interest in the same number is by Signor A. Piccone, on the part played by herbivorous (phytophagous) fishes in the distribution of marine algæ. An examination of the intestines of several species of fish showed that, in addition to *Zostera* and other flowering plants, they contained the remains of a number of seaweeds, some of them in a fruiting condition, the spores of which are in all probability voided in the excrements, and are then in a favourable state for germination.—The remaining papers in these numbers are descriptive or contributions to local floras.

SOCIETIES AND ACADEMIES

LONDON

Physical Society, April 25.—Prof. Guthrie, President, in the chair.—The following papers were read:—On the theory of illumination in a fog, by Lord Rayleigh. The paper dealt with certain theoretical results based upon the assumption that the medium in which the fog was formed and the substance composing the fog itself were perfectly transparent. The effect of such a fog surrounding a source of radiation would be to diminish the radiation, and in the case of a supply of energy from without, as with the carbon filament of an incandescent lamp, the temperature of the source would be increased by the fog. A spherical envelope of such a fog surrounding the lamp, and sufficiently thick to be impervious, would act as a perfectly reflecting surface. A problem closely related to the above, and which is easily worked out, is that of light incident normally upon a pile of glass plates. If m be the number of such plates, and ρ the fraction of incident light reflected by one plate, $\phi(m)$ the light reflected, and $\psi(m)$ that transmitted by a pile of m plates, we have—

$$\frac{\phi(m)}{2m\rho} = \frac{1}{1+(2m-1)\rho} = \frac{\psi(m)}{1-\rho}.$$

If the transmitted light be allowed to fall upon another pile consisting of n plates, we have an infinite amount of reflection between the plates, and as the final result if A denotes the radiation in the original direction, and B that in the opposite,

$$A = \frac{2n\rho + 1 - \rho}{2(m+n)\rho} \quad B = \frac{2n\rho}{2(m+n)\rho + 1}.$$

If m and n are large, we have—

$$A = B = \frac{n}{m+n},$$

which shows that by increasing n we can make the radiation between the plates as much as if the first pile did not exist whatever the number of plates in it.—On a monochromatic telescope, by Lord Rayleigh. This is a modification of Maxwell's colour-box. In this instrument, as is well known, light passes through a slit in the focus of a collimating lens; it traverses in succession this lens, a prism, and another lens by which it is brought to a focus upon a plane surface in which is a movable slit, the eye being placed behind which receives light approximately monochromatic. If, in addition, a lens be placed just behind the first slit, so as to bring some distant object into focus at a convenient distance from the eye, this object will be seen by the light that would enter the eye in the simple colour-box. The author suggested the use of this instrument to compare lights of different colours, and hinted at the possibility of choosing some colour towards the middle of the spectrum at which light might be compared for practical purposes.—On the self-regulation of the compound dynamo, by Prof. A. W. Rücker. If ϕ represents the current or electromotive force in the external circuit of either form of compound dynamo, it is given by means of an equation of the form

$$\phi = \frac{P}{A+x} - \frac{Q}{B+x},$$

where A, B, P, and Q are quantities which are different in different cases, but are always independent of the external resistance, and x is the conductivity or the resistance of the external circuit, according as ϕ represents the E.M.F., or current. The constant A in all cases depends only on the resistance of the various parts of the machine. If μ and m are respectively the largest and smallest values of x between which self-regulation is aimed at, then $\mu - m$ may be called the *range of x*. That value of x which corresponds to the resistance most frequently used may be called the *usual value* of x and indicated by ξ. The maximum efficiency η of the machine is connected with A and ξ by the relations

$$A = \xi\,(1+\eta)/(1-\eta) \text{ if } \phi \text{ be the external E.M.F.}$$
$$A = \xi\,(1+\eta)/(1+\eta) \text{ if } \phi \text{ be the external current.}$$

It can easily be shown that the function ϕ has two critical values, and that the value of x, corresponding to one of these, is necessarily negative, unless one of the inducing spirals is wound so as to diminish the magnetisation Various cases are considered, corresponding to different relations among the magnitudes of the constants A, B, P, and Q. The following indications of the method of treatment may suffice. If $A/B < 1 < \sqrt{P}/\sqrt{Q}$, ϕ is positive for all positive values of x, and the critical value of ϕ occurs for a negative value of x, so that ϕ diminishes as x increases. Hence, if we write

$$\frac{P}{A+m} - \frac{Q}{B+m} = \phi,$$

we must have

$$\frac{P}{A+\mu} - \frac{Q}{B+\mu} = \frac{\phi_1}{1+q},$$

where q is a positive quantity which will be less as the self-regulation is more perfect. These equations give

$$P = \frac{\phi_1}{1+q} \cdot \frac{\mu - m - q(B+m)}{(A-B)(\mu - m)}(A+\mu)\,(A+m)$$
$$Q = \frac{\phi_1}{1+q}\,\frac{\mu - m - q(A+m)}{(A-B)\,\mu - m)}(B+\mu)\,B+m).$$

Now since $A - B$ is negative, we must, if P and Q are positive, have

$$q < (\mu - m)/(A+m),$$

and *a fortiori*.

$$q < (\mu - m)/(B+m).$$

By similar methods inferior limits to q are found in other cases, and it is thus shown that for given values of μ and m, the limit is lower as A is larger. It has, however, been proved above that if the maximum efficiency of the machine is high, A will be large or small, according as it is taken from an expression that gives the external E.M.F. or the external current. Hence it is more difficult to combine high efficiency with good self-regulation if an approximately constant external current is desired than if an approximately constant external E.M.F. is aimed at. The equations do not lead to any simple rules for the relations which should hold between the various parts of compound dynamos; but if some of the constants are taken as given, the values which must be assigned to the others can be calculated if a given efficiency for the usual value of x and a given deviation from perfect self-regulation between given values of x are to be attained.—On the determination of the heat-capacity of a thermometer, by Mr. J. W. Clark. The method consists in the estimation of the masses of the mercury and glass of the thermometer by weighing the instrument in air and in water, and again in water when immersed to the extent usual in the thermal experiment. The specific gravity of the glass and mercury being known, the absolute masses immersed can be readily calculated, and consequently their thermal capacity.—A photometer which enabled a comparison to be made between the light of a lamp emitted at any angle and a standard was exhibited by Mr. Dibdin, and the action explained by Mr. Livingstone, who stated that the maximum amount of illumination took place at an angle of 45°.

Geological Society, April 15.—Prof. T. G. Bonney, F.R.S., President, in the chair.—John Rudd Leeson, M.D., was elected a Fellow of the Society.—The following communications were read:—A general section of the Bagshot strata from Aldershot to Wokingham, by the Rev. A. Irving, F.G.S. The author referred to earlier papers in the *Geological Magazine*, in which the green colouring-matter so common in the Middle and Lower Bagshot strata of the London Basin had been attributed to the presence of vegetable *débris* and the materials resulting from decomposition of vegetable matter. The marked difference in this respect between these strata and the higher members of the series furnishes a clue to the conditions under which they were respectively deposited, the former being delta- and lagoon-deposits, the latter the deposits of a marine estuary. This implies a transgressive overlap of the upper portions of the Bagshot series upon the London clay; and the present paper was devoted to a consideration of the stratigraphical evidence of this overlap. Sections were described in detail at Aldershot, Farnborough, Yateley, Camberley, Wellington College and the neighbourhood, and from the last-named place to Wokingham. From these a general section was constructed to exact scale, both as to thickness of strata and altitudes, showing a relation of the Bagshot formation to the London clay which was inconsistent with the generally received idea of their conformability and at variance with the mapping of the district as executed by the Geological Survey. The importance of the Bagshot pebble-bed as a basement-line of the upper division of the Bagshot strata was shown, as was suggested by the author so long ago as 1880. The synclinal arrangement of the London clay was shown to have been produced *before* the deposition of the Bagshot series, though a certain amount of movement (with a resultant amount of 150 feet of tilting in thirteen miles from south to north) has since taken place.—Notes on the Polyzoa and Foraminifera of the Cambridge greensand, by G. R. Vine. Communicated by Thomas Jesson, F.G.S.

Royal Meteorological Society, April 15.—Mr. R. H. Scott, F.R.S., President, in the chair.—The following papers were read:—Report of Committee on Decrease of Water-Supply. This Committee was appointed to take into consideration the question of the decrease of water in springs, streams, and rivers, and also the simultaneous rise of the flood-level in cultivated countries. As far as any inference can be drawn from the records collected by the Committee, it appears that the years 1820, 1821, 1824, 1835, 1838, 1845, 1847, 1850, 1854, 1855, 1858, 1859, 1864, 1865, 1871, 1874, 1875, and 1884 have been periods of marked low water. On the other hand, the years 1817, 1825, 1830, 1836, 1841, 1842, 1853, 1860, 1861, 1866, 1873, 1877, 1879, 1881, and 1883 have been periods when there has been exceptionally high water. In 1852 the water was very low in the early part of the year, while at the end of the year it was very high. In the intervening periods the water has been of moderate altitude. It does not appear from existing records that there is any diminution in the water-supply of this country, and the large quantity of water which has been stored or has flowed off the ground between 1876 and 1884 is confirmatory of this view. There appear, however, to be periods when there is exceptionally low water, and these are almost immediately followed by periods of exceptionally high water. With reference to the increase of floods, it does not appear from the records that there is any great increase in the height to which the floods rise in this country. Whether or not the height to which floods have risen in recent years has been affected by river improvements and the greater facility with which floods can be got rid of, or whether there is a diminution in the quantity of water, are questions upon which the Committee have not at present sufficient information to speak positively.—Report of Committee on the occurrences of the Helm-Wind of Cross Fell, Cumberland, from 1871 to 1884. In response to a letter inserted in the Penrith newspapers, the Committee has received a number of communications bearing on the subject of the helm-wind. With the view of ascertaining as far as possible the meteorological conditions which exist when the helm-wind is blowing, all the recorded occurrences that have been received have been chronologically arranged. The first systematic record commences in 1871, and in this report the Committee deals with all occurrences from that date to the end of 1884. Since that time more detailed records have been commenced at numerous stations in the locality at the instigation of the Royal Meteorological Society. Ninety-three instances of the helm-wind were recorded from 1871 to 1884; the months with the greatest frequency being February, March, April, and November. On examining the Daily Weather Reports it was clearly seen that, whenever the helm-wind was blowing there was an easterly wind, not only in the locality, but generally over the entire country. As the helm-wind seemed to occur so regularly with the easterly wind, the Committee further extended the inquiry with regard to the east wind. The Daily Weather Charts were consequently examined for each day from January 1, 1871, to December 31, 1884, and every occurrence of east wind tabulated; the instances with general easterly conditions over the whole country being kept separate from those instances in which the easterly wind was only partial, though of sufficient intensity to occasion the helm-wind. This examination showed that, although the wind over the United Kingdom is generally easterly when the helm occurs, yet the helm by no means occurs whenever the wind is easterly. Indeed, this step in the inquiry has not at all tended to the elucidation of the phenomenon in question, for it frequently happens that the conditions are, to all appearances, precisely similar when the helm is on, and yet no such occurrence has been recorded. This may in part be due to the occasional omission to record the helm, although it cannot possibly be, in the main, attributable to such an omission; but it points to other conditions being necessary besides absolute agreement of wind direction and isobaric lines. Possibly the different hygrometric qualities of the air with the existing easterly winds may be an important factor in deciding whether or no the helm will be formed, but it is not readily conceived why, even in this case, the helm-wind should not blow. It must, however, be borne in mind that the surface-winds can only be examined, whilst those at a comparatively small elevation may be intimately connected with the phenomenon. From the observations made prior to those started at the beginning of 1885, no idea can be formed of the behaviour of the upper currents, even at the time of the occurrence of the helm-winds, far less with the occurrence of each east wind experienced. The Society has, however, provided for the extension of the inquiry in this direction in the records which are now being collected, the observers supplying observations of the upper currents by means of the clouds, as well as the direction of the winds at the surface of the earth. As soon as a sufficient number of these observations have been received, the Committee hopes to present a further report, which will tend to explain the phenomenon of the helm-wind.—Results of meteorological observations made at Asuncion, Paraguay, by R. Strachan, F.R.Met.Soc.

PARIS

Academy of Sciences, April 27.—M. Bouley, President, in the chair.—Experimental researches regarding (1) Attacks of an epileptic character excited by the electrisation of the excito-motor regions of the brain properly so-called; (2) the duration after death of the excitability so produced in the brain, by M. Vulpian. The main object of these experiments, made chiefly on dogs, is to confirm the conclusion already arrived at and communicated by the author in a previous paper, that the grey cortical substance of the cerebral regions known as motor centres does not play the indispensable part hitherto supposed in the production of epileptic attacks caused by the faradisation of those regions. The inference is also confirmed that amongst the higher mammals under normal conditions the cerebral substance proper loses its excitability as soon as the circulation has completely ceased in the nerve-centres.—Nebula discovered, observed, and tabulated at the Observatory of Marseilles, by M. E. Stephan.—Results of the boring recently carried out at Ricard, in the Grand'-Combe Valley, Gard, in search for coal, by M. Grand'-Eury. These borings tend to confirm the conclusion, already arrived at on other grounds, that no parallelism exists between the St. Barbe and Grand'-Combe geological systems, and as the former are unquestionably the older, they must, in the normal state, necessarily underlie the latter.—Report on the relation between the phenomena presented by the recent earthquakes in Andalusia, and the geological constitution of the region comprised within the area of disturbance, by M. Fouqué.—Remarks on an instrument analogous to the sextant, by means of which angles projected on the horizon may be directly measured, by M. E. H. Amagat.—Note on the calculations made to determine the solar parallax from the daguerrotypes taken by the French Commission during the transit of Venus in 1874, by M. Obrecht. The calculations have been carefully checked, and the definite result is represented by

$$\pi = 8''\cdot81 - 0''\cdot004\, dL \pm 0''\cdot06,$$

where π is the solar parallax, and dL the correction to be made for the longitude of Pekin.—Elements and ephemerides of the planet 246, deduced from the observations made on March 9 at Marseilles, Vienna, and Düsseldorf, on March 18 at Marseilles and Vienna, on March 31 at Berlin, and on April 9 at Marseilles, by M. Andoyer.—On a general law in the theory of the partition of numbers, by MM. Bougaieff.—A short and simple demonstration of M. de Sperre's theorem regarding Poinsot's "herpolhodie" curve, by M. A. de Saint-Germain.—Note on a method of regulating the velocity of electric motors, by M. M. Deprez.—*Régime* of combustion of explosive mixtures formed with illuminating gas, by M. A. Witz.—Description of the solar corona, the so-called "Bishop's ring," observed subsequently to the Krakatoa eruption in 1883, 1884, and 1885, by M. F. A. Forel.—Researches on the phosphates: a method of reproducing at pleasure a large number of crystallised phosphates and oxides, by M. H. Debray.—On the oxidation of iodine during the process of natural nitrification, by M. A. Müntz. The object of this paper is to determine the natural conditions under which were produced the extensive deposits of nitrates in certain tropical regions.—On the ammoniacal sulphate of copper, and on a basic sulphate of copper, by M. G. André.—On the dimorphism of telluric anhydride and on some of its combinations, by MM. D. Klein and J. Morel.—On the chemical constitution of cocaïne, by MM. G. Calmels and E. Gossin.—Studies on the inhalation of bichloruretted formene (chloride of methylene) and of tetrachloruretted formene (perchloride of carbon), by MM. J. Regnauld and Villejean.—On the effects produced on man and animals by the stomachic ingestion and hypodermic injection of the microbes associated with the diarrhœic liquid of cholera, and cultivated in peptonised gelatine, by M. Bochefontaine. Experiments made by the author on himself and on the guinea-pig tend to show that these preparations, when swallowed or injected in small doses, produce no morbid symptom, although large doses may give rise to more or less serious local inflammation. He infers that the physiological disorders observed in cholera patients are due, not to the development of the microbe germs, but to the presence of a special substance not yet determined; further, that in its normal state the blood of man and other animals is destructive to the choleraic microbes artificially prepared in gelatine.

BERLIN

Physiological Society, March 27.—Prof. Ewald spoke on the occurrence of lactic acid in human gastric juices, which was now universally regarded as a pathological formation, *i.e.* a product of fermenting processes which did not obtain under normal conditions. In conformity with this opinion he had, in a former investigation, clearly demonstrated the absence of lactic acid, even after milk had been partaken. On the other hand, he had regularly found hydrochloric acid in the gastric juice. Two cases of hysteric vomiting, which had come under his observation in the infirmary, induced him to resume this investigation, one of the cases especially inviting such inquiry. The female patient was able to retain on her stomach and normally digest solid food, but whenever she swallowed anything fluid the whole contents of the stomach were at once vomited. Opportunity was, therefore, here offered at any time to examine the contents of the stomach after food had been received. Prof. Ewald mentioned the different chemical reactions by means of which the presence of lactic acid might be easily detected in the gastric juice, and among them he deemed carbolic acid and chloride of iron the most trustworthy. He then described the experiments he had carried out on the female patient above referred to, which had yielded the following results:—After a mixed meal (of bread, vegetables, and meat), lactic acid was found 26 times out of 31 in the contents of the stomach in the space of 10 to 100 minutes after the reception of the food; in 5 cases, however, not till 120 minutes or more after that point of time. Hydrochloric acid was found in the contents of the stomach only in the second hour and later, after the lactic acid had disappeared. Out of 26 cases in which white bread was alone eaten, lactic acid was demonstrated in 17 cases, occurring in 10 to 100 minutes from the time of eating. Out of 15 cases in which cooked albumen was administered, lactic acid was found only in one case, within one-and-a-half hours from the time of its being taken; while, on "schabefleisch" (scraped raw meat) being administered, lactic acid became again demonstrable; in the majority of cases in 10 to 100 minutes' time. From these experiments it was to be inferred that lactic acid occurred normally in the contents of the stomach, namely, in the first period of digestion. It was, however, in the opinion of Prof. Ewald, no normal constituent of the gastric juice, but in the case of a mixed and meat diet originated in the carno-lactic acid obtained from the meat and, in the case of white bread being taken, from the fermentation of the starch. On albumen being taken, lactic acid was, therefore, not found, because it occurred in the stomach only when it was introduced with the food—in the case of meat, for example—or when it arose from a fermentive aliment. With reference to the ulterior issues of the lactic acid, the speaker adopted the view of Prof. Maly, that it was employed towards the formation of the free hydrochloric acid afterwards appearing in the gastric juice.—Dr. Blaschko reported some observations he had made on sensations of pressure. In the course of investigations into the development of the skin, he had found that the hair-roots were provided with a rich nerve plexus in the same manner as the touch corpuscles in the touch balls of the hands and feet, and this induced him to examine the hairs in respect of their sensibility to pressure. When he took a stiff hair a little curved at the tip, and stroked the skin with it, he had only then a sensation when he touched a lanugo hair. By this and other means he became convinced that the hair papillæ possessed such a high degree of sensibility as entitled them to be placed in a series with the touch papillæ. While, however, the touch corpuscles had to be drawn hither and thither over the object to be touched, in the case of the touch hairs the body to be felt had, on the contrary, to be waved over it. Dr. Blaschko was therefore of opinion that a direct and an indirect, or a papillary and a ciliary feeling of the skin had to be distinguished. The first performed its functions at the unhaired cutaneous spots; the touch balls of the hand, and the foot, and at the lips; by means of the touch corpuscles. The indirect or ciliary sensations, on the other hand, were performed by the lanugo hairs covering the whole body, which were properly, therefore, touch hairs. If at a limited spot of the skin, such as the forehead, the lanugo hairs were shaved away, then would the fine sensations of pressure likewise disappear, and on waving that part of the skin with the stiff hair above referred to, a correspondingly large hiatus would become perceptible, at which nothing would be felt. In the course of this investigation the speaker had failed to convince himself of the existence of special points of pressure, and controverted the doctrine set up by Dr. Goldscheider in the former sitting of the Society respecting the specific energies of the nerves of feeling, and their punctiform distribution over the surface. In the discussion which followed, Dr. Goldscheider maintained the accuracy of his former statements, and invited Dr. Blaschko to convince himself of their correctness according to the method prosecuted by him.

VIENNA

Imperial Academy of Sciences, February 5.—Contributions to general nerve and muscle physiology (seventeenth communication): on the electric stimulation of the sphincter of Anodonta, by W. Biedermann.—Experiments on the oxidation of albumen by potassium permanganate, by R. Maly.—On *Clemmys sarmatica*, nov. spec., from the Hernalstegel, near Vienna, by C. A. Purschke.—Remarks on the velocity of light in quartz, by K. Exner.—Histological and embryological researches on the uro-genital apparatus, by T. Tanosik.—On a new vegetable parasite of the human body, by R. von Wettstein.

February 12.—On the bloodless-vessels in the tail of Batrachian larvæ, by S. Mayer.—On the constitution of isutivinic acid, by T. Schreder.—On the isogyric plane of double-refracting crystals, by H. Pitsch.—On the geographical distribution of the Jurassic formation, by M. Neumeyr.

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