THURSDAY, OCTOBER 22, 1891.

RUDOLF VIRCHOW AND HIS COUNTRYMEN.

THE German people are to be congratulated on the brilliant way in which the seventieth birthday of Prof. Virchow was celebrated last week in Berlin. We say the German people, because the entire nation associated itself with the scientific societies in doing honour to the illustrious investigator of whose achievements it has for many a day been so justly proud. Everyone who devotes the slightest attention to science is aware that Prof. Virchow occupies a prominent place among the foremost intellectual leaders of the present age. As the *Times* has said, "So much has he done, and so thoroughly has he done it, that it is difficult for this generation to apprehend the full magnitude of his work. Open a book on medicine, and especially any volume on pathology, composed, it matters not much where, before Virchow began his observations, and compare it with one composed with the light of his endless investigations to guide the author: a veritable revolution in conceptions and terminology has taken place; at every turn you read, 'All this is understood since Virchow wrote,' or words to that effect; and you are referred to his multifarious 'epoch-making' articles scattered through many professional and technical periodicals." By his great principle, "Omnis cellula ex cellula," he made a contribution of the highest importance to biological science; and his conception of cellular processes introduced wholly new and most fertile ideas as to all the phenomena of disease. The science of pathology as it is now understood and taught we owe, indeed, mainly to his insight and labour, and the recent advances which have been made in it by other explorers have been made on the lines he has traced. If Prof. Virchow had done nothing else for science, this alone would have secured for him imperishable fame; but his energies are so varied that it has been impossible for him to content himself with one department of research. As a student of archæology, ethnology, and anthropology, he is hardly less eminent than as a pathologist. In all these sciences he has marked an era by his writings, and by the personal influence he has exerted on the Berlin Gesellschaft für Anthropologie, Ethnologie, und Urgeschichte, which he founded in 1869. In practical life, too, as a member of Parliament and of the Municipal Council of Berlin, Prof. Virchow long ago made himself a great power in Germany. He has missed no opportunity of expounding the laws of public health, and of insisting upon their importance; and a striking testimony to the value of his work in this direction may be seen in the improved sanitary condition of the German capital.

To the Germans it seemed perfectly natural that, when so illustrious a man of science completed his seventieth year, the nation should offer its congratulations on the splendid results he had accomplished. Would an English man of science of corresponding intellectual rank have received similar tokens of popular gratitude and respect? Unfortunately, the question answers itself; and it would be well worth the while of Englishmen to consider carefully the causes which have led to the contrast in this respect between them and their German kinsfolk. It may be said that Germans are more demonstrative than Englishmen, but this by no means accounts for the very different ways in which scientific discoverers are treated in the two countries. The real root of the difference lies in the fact that the importance of science is much more highly estimated in Germany than in England, and especially by the Governments. For several generations, the various German Governments have done everything in their power to foster scientific investigation. With this object in view, they have spent money freely and wisely, allowing themelves to be guided, not by impulse or caprice, but by the advice of men of wide experience and knowledge. They were quick to note the influence which might be exerted on industrial development by technical education; and the result is that Germany has for some time had as many technical schools and colleges, adequately equipped, as are necessary for her wants. We need scarcely say how very different is the spirit that has hitherto animated our own Government. The idea of most English statesmen about science seems to be that it is a bore and a nuisance, and that the less they have to do with it the better for themselves and the public. Even for technical instruction they declined to make provision, until, by an accident, the present Government found itself in possession of a fund which it did not know how to get rid of except by giving the County Councils authority to use it for the establishment of technical schools and classes. Is it surprising that when their rulers act in this way the mass of the British people should be utterly indifferent to scientific progress? The Germans have been accustomed all their lives to see science encouraged, and all classes learn therefore to regard it as an essential factor in the evolution of their national life. This week they have had a fresh example of the respect in which science is held, the Emperor having appointed Prof. Helmholtz a member of the Privy Council, with the title of Excellency. In the telegram announcing to Prof. Helmholtz the honour conferred on him, the Emperor took occasion to refer with pride to the lustre shed on Germany by his scientific achievements. Nothing of the kind is ever done here.

The influence of education must also, of course, be taken into account. There is still some dispute in Germany, as in other countries, about the exact place which properly belongs to science in general education; but there is no dispute at all as to the importance of training children to recognize the benefits which science in all its branches has conferred on mankind. Moreover in the "Realschulen" an excellent scientific training is provided for those who either have little power of appreciating classical literature, or who are likely to be best fitted for their future work by the study of science. And in elementary schools an effort is everywhere made to interest children in the facts and laws of nature, and to give them some conception of the objects and methods of scientific inquiry. How far we lag behind the Germans in these respects all true "educationists" know. We have made only a beginning in the use of science as an instrument of popular culture, and many years, we fear, may pass before we shall have applied it sufficiently to render scientific conceptions a really vital element in the intellectual life of the community.

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It is not for the sake of men of science that we desire to see more widely diffused an intelligent appreciation of their work. A celebration like that of last week necessarily brings with it sad as well as happy reflections. "After all," said Bluntschli, the famous jurist, on a like occasion, "it is an end, not a beginning." Prof. Virchow is fresh and vigorous, and the world may still reasonably expect from him much sound work; but we may be sure that, in responding to congratulations, he had a little of Bluntschli's feeling; and it is possible that, if he had consulted his own wishes only, he would have preferred to celebrate his seventieth birthday more quietly. But it is good for a nation to express on such occasions the admiration and reverence excited by a long and great career. The mere fact that men desire to honour one whose title to distinction is that he has advanced human knowledge proves that they have interests higher than those of a material character; and it inevitably tends to deepen and strengthen the best and most enduring of their impulses. We should be glad, therefore, if Englishmen had as strong a wish as Germans to display a hearty appreciation of the triumphs achieved by their great scientific thinkers. That would be the most effectual of all proofs that they had begun, as a people, to understand how momentous is the part which science has played, and must continue to play, in the modern world.

ELECTRIC LIGHT FITTING—GOOD AND BAD WORK.

Electric Light Fitting: a Hand-book for Working Electrical Engineers. By John W. Urquhart. (London: Crosby Lockwood and Son, 1890.)

THIS book is exactly what it professes to be—a practical book for practical men—and is vastly superior to "Electric Light," by the same author. The detailed instructions given in the first 42 pages, on the erecting, managing, and repairing dynamos, are admirable, and are not to be found in any other book in the English language. The young electrical engineer will find just the information he needs: how to fit up a large dynamo when received in parts from the makers; how to prevent the commutator becoming rough in use; exactly what to do if it be rough; how to prevent sparking at the brushes; how to attach a new commutator and make joints in the armature wires; what to do if the dynamo heats; and how to get over the various other difficulties met with in the dynamo-room.

The author, in these early chapters, and indeed throughout the book, uses the expression "constant current" for direct current; and although the action of the regulators of the Brush and of the Thomson-Houston constant current dynamos is correctly described, and clear illustrations given of their construction, the reader is left in the dark as to the exact use of these regulators. Or, rather, the only definite statement as to the function of the Thomson-Houston regulator, that it is "for causing the machine to evolve more or less current as required," is certainly much more likely to lead the reader wrong than right. Further, to say that "in Siemens's alternator, or the Ferranti dynamo, 'lead' must be given to the brushes" (an instruction, of course, quite impossible to carry out, as alternate machines have no commutators, but only collecting rings), will probably destroy the correct impression about lead which the practical man may have derived from reading the previous page.

In spite of these defects, however, chapter i. is excellent, but we cannot speak quite as highly of chapter ii., "On Localizing Dynamo Faults, and Observations respecting Accumulators." In describing the test for the existence of leakage between the iron framework and the earth, the author makes an error that we have met with before, in stating that a deflection of a galvanometer whose ends are connected respectively with the iron framework and the earth indicates leakage between these two. This is equivalent to saying that a conductor not having the potential of the earth proves that it is in connection with the earth. In the "Hints to Accumulator Attendants" there are some very useful suggestions, but the instructions for deciding when an accumulator is charged confirm the impression we gave when reviewing the author's "Electric Light," that the author had not derived his knowledge of storage cells from a practical acquaintance with them. For he says that they must not be so much discharged that they cease to give any current; and in the chapter on "Switch Board and Testing Work," that the E.M.F. of accumulators, in discharging, should never be allowed to fall below 0·5 volt per cell. Such instructions are about as useful as saying that a horse should not be worked until he dropped, for if accumulators were to be regularly discharged until their E.M.F. fell to a value even three times as great as the limit prescribed by Mr. Urquhart, they would be speedily ruined.

Why these two statements about the discharge limit of storage cells should be given in different parts of the book, with information about "Running Dynamos in Parallel," the "Periodicity of Alternators," &c., inserted between, we do not know. In a somewhat similar way, the author returns again and again in different parts of the book to the subject of insulation resistance. Each time, no doubt, valuable information is given; but why not have put it all together, so that the working electrical engineer could have at once read up the subject, without having to turn up a number of references? This sort of scattering of information runs through the whole book, and rather suggests the idea that no very serious attempt was made to sort out information written down by the author as it occurred to him at different times.

We do not think that the explanation on p. 54 "alternators work according to a 'phase,'" is very lucid. Further on, the author says the number of phases per second is the periodicity, and later that periodicity and phase are the same thing. On p. 51 we are told "a fall of five volts in a hundred affects the brightness of the lamps," from which a person might easily obtain the wrong impression that a fall of two or three per cent. was not observable, and be astonished when he read, on p. 72, "that a fall of five volts in a hundred in the working pressure will cause lamps which burn brightly at a hundred volts to become very dull." He would also not be able to reconcile the statement, "upon well-conducted systems the pressure upon the mains is never allowed to vary more than one-half per cent.," with the variation of 2 per cent. up and 2 per cent. down, which is allowed by the Board of Trade. Nor is it possible to understand the rule with reference to the wiring of a house, "It should show an insulation resistance of at least ·1 megohm per lamp," since this would make the insulation of an installation the higher the greater the number of lampholders, whereas of course, as a matter of fact, the very reverse is the case.

Chapter iv., on "Arc Light Wiring and Fitting," is full of practical suggestions; the instructions on the trimming of arc lamps, and the precautions that ought to be adopted in order to keep arc lamps in good working order, will greatly help the young engineer when he is first put in charge of arc lamps. It is a pity, however, that when the author is speaking of supplying constant current to a variable number of arc lamps running in series, he should say, "but the shunt or compound-wound machines are supposed to regulate themselves, which they very often fail to do." For we never heard of a compound-wound machine, still less of a well-made shunt machine, which professed to produce a constant current when the external resistance was varied. And this mistake is emphasized in the next section, on running arc lamps in parallel, since, although it is quite rightly said of the attendant, that "his chief care is to keep the *potential difference* between the leads the same," Mr. Urquhart states, "This is usually effected in part by the dynamo itself when a shunt-wound machine is used, or by regulating the speed"; and he makes no reference here to the use of a compound-wound machine, as if it were not the special function of this type of machine to keep the potential difference between the mains constant.

There is a good illustration on p. 107 of the Thomson-Houston lightning arrester, with an explanation of its construction, but no hint is given that the electric arc produced by the lightning flash is magnetically blown out and thus extinguished. And in the large perspective illustration of a Thomson-Houston transformer, given in this chapter, the thickly-insulated leads are shown with a thick copper conductor inside them, while the lightly-insulated leads have a thin conductor, and since, in the description of a transformer, it is not stated that, besides transforming from a high to a low *potential difference*, this apparatus also transforms from a small to a large *current*, it would be quite possible for a beginner to read this book, and wonder why people went out of their way to construct dynamos to produce one or two thousand volts, and then had to employ special apparatus at the consumers' premises to lower this high potential difference. "It is usual to put the secondary circuit to earth," probably expresses the author's view (as it also does the reviewer's) of the proper way to guard against accidents being produced by a contact between the primary and secondary circuits of a transformer, but it certainly does not represent the ordinary practice.

The name "impedance coils" is suggested for inductive coils used to diminish a varying or an alternating current; but the necessity for this name arises from the expression "choking coils," which is commonly used in this sense, having been wrongly employed by the author for any kind of resistance coils, such as, for example, a non-inductive resistance used with a steady current.

Chapter v., on "Wiring for Incandescent Lamps," abounds in useful hints, and is illustrated with several well-executed woodcuts. Admirable, however, as may be the switches, fuses, &c., constructed by Messrs. Woodhouse and Rawson, the succession of illustrations with the names of that firm underneath tends to give the impression that there are no other manufacturers of such apparatus. Surely the weighted fuses made by the Acme Works, the switches of Messrs. Siemens—which provide a metallic circuit for the current but expend the flash, produced by opening the circuit, on carbon contacts—and the S switches of Messrs. Crompton, were worthy of a reference.

If the well thought out precautions detailed in "Methods for Running Wires" had been followed in all the wiring of houses that has been carried out during the past few years, we should not have heard of those very justifiable complaints of occupiers who, after taking the lease of a house, temptingly described in the agent's list as fitted throughout with the electric light, find that they have to entirely re-wire the house before the insurance office will allow the current to be turned on. We thoroughly agree with the author that "There is one leading maxim for a contractor putting in electric light, and it is to avoid contracts that do not allow of the best class of material and labour being used throughout." We should also like to impress on the general public that the plummer, or the carpenter's handy man, is not, as they seem to think he is, any more capable of fitting up an electric installation than he is of setting a broken leg.

We do not understand why, as a definition of "cleat wiring," Mr. Urquhart says, "This means uncovered wires run &c.": surely cleats are frequently employed to hold down covered as well as uncovered wires. On p. 185 the temperature is not stated at which "the ohm is the resistance offered by a column of mercury 1 square millimetre in cross-section and 106 centimetres long." Power and work are said to be synonymous, and foot-pounds said to be analogous with volt-amperes. The output of 1000 watts "is called under the Board of Trade regulation a *kilowatt*," whereas the late Sir William Siemens, and not the Board of Trade, originated this name. "As lamps are now made, each would probably give a light of 20 candle-power, the watts per candle-power being 2·5." Would that we could buy glow lamps which had a decent life, while needing only 2·5 watts per candle.

Sir William Thomson's rule about the right sectional area to give to a conductor "is only a suggestion made for the protection of buildings from fire." We thought everyone knew that it was a rule for settling the thickness of the conductor with which maximum economy could be obtained.

The rules about jointing leads are exact and valuable; we do not, however, like the general rule of using the body of a chandelier itself to serve as the return, and we think this rule ought to be followed only when the return wire is throughout the installation an uninsulated one.

Chapter vi. gives a good *résumé* of the *pros* and *cons* regarding the use of the body of an iron ship as the return for ship lighting; while chapter vii. gives the substance of the rules issued by the Institution of Electrical Engineers, in connection with fire risks and danger to life.

MORE SUGGESTIONS FOR COUNTY COUNCILS.

County Councils and Technical Education. By J. C. Buckmaster. (London: Blackie and Sons.)

UNDER the above title Mr. Buckmaster, who for many years has been connected as teacher, lecturer, and organizer with the Science and Art Department, gives some statistics relating to technical education, and his views on the best way of utilizing the funds in the hands of County Councils. We need hardly say that, backed as they are by so long an experience, his opinions deserve the most careful and respectful consideration.

Briefly stated, Mr. Buckmaster believes in class teaching as opposed to lectures, and in utilizing as far as possible existing elementary and science and art teachers. "Unless," he says, "the sympathy of teachers and other educationists can be enlisted, the most carefully considered schemes of County Councils can only end in partial or complete failure." Again,

"Lectures by themselves are never to be highly valued as a means of education. In a lecture on science, to create and sustain an interest, you must be popular, and to do this you avoid the complex difficulties of the science, which are often the only intellectual parts of it. . . . Lectures, unless followed up by thought and reading on the part of those who hear them, fail as a means of education, &c., &c."

All this is excellent, and the warning is useful. But when Mr. Buckmaster comes to the application of these principles he is not quite so happy. For example, he is unjust to the University Extension system, which he does not clearly understand, and treats as though it were mere popular lecturing, like the work of the old Mechanics' Institutes. Now, though we have no belief that the University Extension machinery can fill the place of elementary class teaching, we cannot accept the implied suggestion that courses of ten or twelve lectures (often arranged in sequences of two or three sets of twelve lectures), each lecture followed by a class for the more serious students, and by written paper work corrected by the lecturer, and the whole course tested by independent examination, form an engine of instruction scarcely above the level of a clever conjuror's performance.

His constructive suggestions are, first, to use elementary teachers to give object-lessons in simple science—a most useful proposal, about to be carried out in various counties as soon as the teachers themselves can be properly trained for the work; and secondly, to multiply science and art classes. "The best technical instruction for some time will be a wider development and extension of the educational work of the Science and Art Department by means of night classes and continuation science and art schools." This depends, of course, on the meaning to be attached to "development." If it merely means multiplication, the statement is open to serious question. No one can know better than Mr. Buckmaster the special dangers attaching to the system which he advocates—the abuses which grow up round a system which makes the financial success of the class, and usually the salary of the teacher, depend on the result of an examination. In our opinion, the machinery of the Science and Art Department will long continue to be a most useful and important factor (though not to the exclusion of other agencies) in the development of technical instruction. But the present is the great chance to consolidate and improve, rather than merely extend the work. If the County Council funds are so granted as to correct the evils which inevitably arise out of such a system of payments on results as is adopted by the Department—if its control is used to render more effective the *inspection* as opposed to the mere examination of science and art classes—then the portion of the grant given to promote the work aided by the Science and Art Department will be well spent. But no claim on the part of this or any other single agency to a monopoly of all technical instruction above the rank of that which can be given by the village teacher can be conceded. Mr. Buckmaster does not in so many words make the claim, but he sometimes seems to imply it by minimizing the value of most other experiments which County Councils are attempting. It is virtually a plea for educational bureaucracy against local experiment. But we have not yet reached the stage, if, indeed, we ever do so, when variety of experiment can be dispensed with. Some of the experiments will probably fail. But it is only by wide and free experimenting that the "fittest" will be discovered. Mr. Buckmaster has confined himself, probably on purpose, to the elementary branches of technical instruction, and is silent on its higher developments. Manual work he only just mentions, and not with much sympathy. His criticisms on the wood-carving taught by ladies in villages is not, perhaps, too severe; but it is strange that he does not give a hint that systematic manual training may be (as it has been for a long time in other countries, and lately in our own) made of real educational value. Not a word is said of the worst defect of all in our educational system—the want of good, cheap, secondary schools, which the present grant may do so much to remedy.

Though, however, Mr. Buckmaster takes a rather cramped and narrow view of the outlook, his pamphlet is full of valuable, if rather partial, ideas.

The pamphlet opens and concludes with some useful statistical and other information taken from various publications of the National Association for the Promotion of Technical and Secondary Education. Readers who do not know the source from which these pages are derived may be puzzled by a reference to "the Committee" (p. 41), which by some error in editing has been left still standing, without explanation, in Mr. Buckmaster's pamphlet.

THE MISSOURI BOTANICAL GARDEN.

Missouri Botanical Garden: Second Annual Report. By William Trelease. Pp. 188; Plates 48, reproduced Photographs 5, and Plan of Garden. (St. Louis, Missouri: Published by the Board of Trustees, 1891.)

THE Board of Trustees of the Missouri Botanical Garden have instructed the Director to edit for publication each year a volume setting forth the objects of the Garden and the School of Botany, and the results accomplished by each. The first volume of this series was issued in December 1890, and contained an account of the Garden and School. The present volume, therefore, really begins the series of annual reports, and together with the reports we have a revision of the North American species of *Epilobium*. In the earlier part of the book details are given of the appointment of six garden pupils to scholarships in accordance with a resolution adopted by the trustees at a meeting held in November 1889. Each scholarship conferred may be held by the recipient for a period not exceeding six years, subject to certain conditions. The holders of scholarships are repaid for their services to the Garden, and at the expiration of the six years are entitled to examination by the Garden Committee. On passing such examination to the satisfaction of the Committee and Director, they receive a certificate of proficiency in the theory and practice of gardening. The only scientific paper in the volume is, as we have just mentioned, a revision of the genus *Epilobium*, the American species occurring north of Mexico being those studied. This genus differs from all the other capsule-bearing *Onagraceæ*, except the Californian *Zauschneria*, in having its seeds provided with an ample coma at the apex. While it reaches great development in New Zealand, *Epilobium* is essentially a genus of temperate and cold climates, and the most widely distributed species are those of Arctic and Alpine regions. In Alaska a few such species occur, which are otherwise confined to the adjacent part of Asia. More widely distributed Arctic-Alpine immigrants from the Old World to the New are *E. spicatum*, *E. latifolium*, *E. palustre*, *E. alpinum*, &c. *E. hirsutum*, *E. parviflorum*, and *E. adnatum*, also occur as accidental waifs. The genus passes into South America along the backbone of the continent; few members of this family extend very far across the Mexican boundary in either direction. The most interesting biological features of the genus are those connected with the means of vegetative propagation, pollination, and dissemination. The contrivances by which species survive the winter, and are vegetatively propagated, in this respect attain an extreme degree of differentiation, one in particular having acquired aërial bulblets. The large-flowered species appear to be regularly proterandrous, the duration of the dichogamy being brief in most of them, and the smaller-flowered seem to be always synacmic and self-fertile, although with the probability of frequent intercrossing by aid of insects attracted by the nectar which is secreted within the calyx tube. The genus is of no striking economic value. The North American Epilobia have been mostly described by De Candolle, Torrey and Gray, Haussknecht and Barbey; the more notable works of more limited range being Hooker's "Flora Boreali-Americana," and Brewer, Watson, and Gray's "Botany of California." Prof. Trelease in his revision enumerates 38 species, which number includes the following novelties: *E. holosericeum*, *E. delicatulum*, and *E. clavatum*. The well-known sections *Chamænerion* and *Lysimachion* are still adhered to, the latter, of course, being by far the larger. In the analytical key the main divisions depend on whether the stigma is deeply 4-lobed or 4-cleft, or entire or only notched. Subdivisions are founded on whether the seeds are smooth, or papillately roughened. The name *E. spicatum*, Lam., is used instead of *angustifolium*, the typical *angustifolium* of Linnæus being, according to Prof. Haussknecht, what is commonly known as *E. Dodonæi*, Vill. We are glad to see that Prof. Trelease differs from Prof. Haussknecht in not adopting a new name for what is left of the original *E. alpinum* The *E. alpinum* of Linnæus included with this *E. Hornemanni* and *E. anagallidifolium*, but we think that the name may well stand for one of the segregates. The genus *Epilobium* has always proved a difficult subject; and Prof. Trelease is to be congratulated on his careful treatment, and successful arrangement, of the North American members. The 48 plates will be found of great help to students of these plants; they are not quite of uniform merit, but, taken as a whole, they give the essential details, stress being laid on the varied form of the stigma and seed. Additional illustrations are some well-reproduced photographs taken in the Garden, and a plan of the grounds (scale $\frac{1}{120}$) in five sections.

E. G. B.

OUR BOOK SHELF.

The Story of the Heavens. By Sir Robert Stawell Ball. Eighteenth Thousand. (London: Cassell and Company, 1891.)

IN the preface to this edition, Sir Robert Ball remarks that he has taken the opportunity to "revise the work in accordance with the progress of astronomy during the last four years," and, generally speaking, new facts and theories are briefly referred to. A few points, however, are hardly brought up to date. For example, the spectrum of the Andromeda nebula is said to be "a faint continuous band of light" (p. 462), although it is now definitely known that this continuity does not exist. We also find no reference to the many stars now known to have bright lines in their spectra. The author thus misses a chance of exercising his well-known descriptive ability in an account of the connection between such stars and nebulæ; the similarity of the two being so considerable that Pickering has followed Lockyer in arranging them in a single group. Dr. Huggins's old view as to the coincidence of the nebula line with nitrogen is mentioned merely to be dismissed as erroneous. Why, therefore, is no notice taken of the suggested magnesium origin of the line—for, on any published evidence, the edge of the magnesium fluting is nearer the proper position than the nitrogen double? We would also point out that, according to recent observations, the apex of the sun's way is much nearer Lyra than Hercules. Telescopic changes in comets are fully described, but the accompanying changes in their spectra are not touched upon. Motions of stars in the line of sight are considered; but not those of nebulæ, although Mr. Keeler's observations have been published for some time. In fact, it may be said that there is a tendency to eschew spectroscopic questions, and hence much of the most beautiful part of the story of the heavens is left untold.

Notes on Elementary Physiography. By Horace C. Martin. (London and Manchester: John Heywood, 1891.)

THE author has collected a lot of scraps of information from standard writers on physiographical matters, and has strung his gleanings together to form this book. And if he were an adept at compilation, and knew how to best arrange and connect facts, this plan of printing extracts *verbatim* might be commended. But when Mr. Martin selects notes which by themselves are incorrect, and interpolates in others crude statements which render them ridiculous, he does an injustice to the authors to whom he acknowledges his indebtedness, and he shirks responsibility by saying that "these notes do not lay claim to originality." Could anything be more misleading than the following description of sun-spots on p. 148? "They seem to rise suddenly to a great height, cool, and then sink back into the photosphere. They are due to uprushes of incandescent hydrogen, and are identical with the red flames seen during an eclipse." And the figure that accompanies this text cannot be a sun-spot at all, but must be something else inserted by mistake. Another blunder occurs on p. 59, where a section of an intermittent spring is shown upside down. The figures are mostly very coarse and poor, especially the moraines on p. 62, the section through a cinder cone on p. 89, and one of a volcano on p. 90; whilst the two figures of ocean bottoms on pp. 102 and 103 give a very wrong idea of their nature. There is, of course, a deal of information in the book, but no attempt is made to give it interest. In fact, although the author is a teacher of physiography, it is very evident from his work that he has not paid attention to the practical side of his science, or verified any of the phenomena he essays to describe. As a book of reference the work before us is untrustworthy; and as a work for students of elementary physiography it is useless and much to be condemned.

Thomas Sopwith, M.A., C.E., F.R.S.; with Excerpts from his Diary of Fifty-seven Years. By B. Ward Richardson, F.R.S. (London: Longmans, Green, and Co., 1891.)

MR. SOPWITH died in 1879 at the age of seventy-six. He was not eminent as an original scientific investigator, but he was a man of great vigour and freshness of mind, and had won the affection of a wide circle of friends by his genial and happy temper. For many years he resided at Newcastle as an engineer and railway surveyor. Afterwards he removed to Allenheads, where he served as the chief agent of Mr. T. W. Beaumont's lead-mines in Northumberland and Durham. Dr. Richardson's book will recall Mr. Sopwith vividly to the minds of his friends, and it contains many things which will be of interest even to readers who were not personally acquainted with him. During the long period of fifty-seven years he kept a diary regularly; and of this, of course, Dr. Richardson has made liberal use. The extracts show that Mr. Sopwith studied closely the currents of scientific opinion, and formed his own judgment about them in a shrewd and independent spirit.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Electric Transmission of Power.

YOUR article of the 1st inst. on the International Electrical Exhibition (p. 522), says: "In those days (before 1879) two wrong notions misled people—the one, that the maximum efficiency of a perfect electromotor could be only 50 per cent.; the other, quoting the remarks of Sir W. Siemens, 'in order to get the best effect out of a dynamo-electric machine, there should be an external resistance not exceeding the resistance of the wire in the machine.'"

These two notions are really one: the first follows by immediate inference from the second.

Your article says a little further on: "At the British Association in 1879, Prof. Ayrton exposed the fallacy of assuming that 50 per cent. was the maximum efficiency theoretically obtainable from an electromotor. . . . This was perhaps the first time that it had ever been suggested that the efficiency in electric transmission of power could be more than 50 per cent."

This is a mistake as to historical fact. Many years ago, I am not sure of the date, but it was long before the dynamo was invented, I had some conversation with the late Prof. Joule about mechanical equivalents and motive power, in which he told me that an electromotor (worked, of course, by a voltaic battery) had shown a very high percentage of efficiency—I think he said 79 per cent., and I am sure it was far above 50. I said, "How is that compatible with Ohm's demonstration that the efficiency of an electric circuit is at a maximum when the resistance of the battery is equal to that of the rest of the circuit?" to which he replied, "The maximum effect, in Ohm's theorem, does not mean the maximum work done by the oxidation of a given quantity of zinc, but the maximum effect obtainable from a given surface of zinc plates." "I see," said I, "just as in the case of the steam-engine, the problem of getting the maximum of useful effect from a given weight of coals is a different one from that of getting the maximum of power from a given area of piston."

This appears to be an instance of a truth being grasped by one of the great masters of science long before it passed into general teaching. And it is also an instance of a truth being so mistaken as to mislead: Ohm's law was evidently understood to bear a significance that it did not really bear.

Belfast, October 13. JOSEPH JOHN MURPHY.

[That Joule had clear and correct views regarding the efficiency of an electromotor driven by a voltaic battery was pointed out some years ago, being mentioned, for example, by Prof. S. P. Thompson in his book on "Dynamo-Electric Machinery." But in the paragraph quoted by Mr. Murphy from NATURE of October 1, the expression "electric transmission of power" had reference to the combination of apparatus exhibited at th lecture in question—had, in fact, the meaning usually attache to this expression, viz. the employment of a dynamo to conver mechanical energy into electric energy at one end of a pair o wires of some length, and the employment of a second dynamo at the other end of the wires to convert the electric energy back again into mechanical energy.

Now, not only would it have been somewhat difficult to foretell what would be the combined efficiency attainable by the employment of two dynamos as generator and motor, at a period "long before the dynamo was invented," but even down to 1879 no one had succeeded in practically transmitting power by means of this combination with an efficiency of as much as 50 per cent. over a distance of even one mile.

The only direct-current dynamo in common use at that date was the series-dynamo, and that machine, as is well known, differs radically in its behaviour from a voltaic battery. For while it is when a voltaic battery is developing a very small current that it gives power most economically to the outside circuit, the series-dynamo, when only a very small current is passing through it, develops practically no electromotive force, no power, and therefore has a very low efficiency. Hence, although electricians were undoubtedly mistaken in fancying that there was a theoretical limit of 50 per cent. in the efficiency when two dynamos were employed in the transmission of power, neither the error, nor its correction, were of that obvious character in 1879 that one might imagine from reading Mr. Murphy's letter.—W. E. A.]

Rain-making.

IN 1883 I published in NATURE (vol. xxviii., p. 83) an account of some experiments which I made to explain the curious phenomenon commonly seen at the Bocca of the Solfatara of Pozzuoli: paper or brushwood is kindled near the fumarole, and the action of the flame, even when its duration has been very brief, is observed for some time after in the relatively great increase of cloudy vapour that appears to roll out of the Bocca and to rise from the surrounding minor fumaroles. According to Prof. Arcangelo Scacchi, this increasd condensation of vapour is due to the carbon dioxide produced in the combustion; this gas causing condensation from the highly saturated medium in the same way as fumes become visible when concentrated hydrochloric acid is exposed to ordinary air. My experiments of 1883 tend to show that not only carbon dioxide, but (in accordance with the views of Dr. Aitken on the formation of cloud or mist) the increase of solid corpuscles made to float in the vapour-laden air inside or near the fumarole, might be the cause of a rapid and continuous condensing of the invisible vapour. I noticed that the "powdering" of the air with any kind of dust increased the cloudy column issuing from the Bocca of the Solfatara. I am therefore led to believe that the action of a paper- or faggot-flame in causing the increase of visible vapour from the Bocca of the Solfatara is due both to the production of carbon dioxide and to the increase of solid particles of soot and of light unburnt fragments made to rise and float in the air.

These experiments may help in explaining the action of explosives in causing a downfall of rain. Not only does the explosion produce a certain amount of carbon dioxide, but dust is widely scattered in the air, and carried upwards by the hot gases produced in the explosion. If the results of the experiments in Texas and Kansas by General Dyrenfurth and Prof. Curtis be confirmed, it would be interesting to see if the condensation of vapour in the atmosphere could be better insured by purposely increasing the quantity of dust produced in each explosion. The effect would perhaps be enhanced if the dust were of a markedly hygroscopical nature: the scattering in high air of very minute particles of calcium chloride should help in the making of cloud and rain. ITALO GIGLIOLI.

Laboratory of Agricultural Chemistry,
Royal Agricultural College, Portici, near Naples,
October 12.

Weather Cycles and Severe Winters.

THE following view of the relations of severe winters is one which I do not remember to have seen stated.

Consider the 79 years 1812–90 (at Greenwich), and let us take, as a measure of winter cold, the mean temperature of the three months December, January, and February. Divide the series of years at 1860; giving a first series of 48 years (1812–59), and a second of 31 years (1860–90).

Now consider the first series. The coldest winter in it is 1813 (meaning, by that, 1813–14). The coldest of the following winters is 1829; the coldest of the following, 1840; then come (reckoning similarly) 1844 and 1846 (equal), 1854, 1859. The absolute order of decreasing severity is to some extent the same, but at certain points the order of time is reversed.

Next take the second series. The coldest winter in this is 1890 (*i.e.* 1890-91); the coldest of those preceding, 1878; the coldest of those preceding, 1870; then come (similarly) 1864 and 1860.

Thus we have a succession of severe winters of decreasing severity, and another, after it, of growing severity.

We may tabulate the data:—

Severe winters with lessening severity.	Mean temperature.	Severe winters with growing severity.	Mean temperature.
1813	31·9°	1860	37·4°
1829	33·2	1864	37·1
1840	33·9		
1844	34·9 }	1870	36·4
1846	34·9 }	1878	34·6
1854	35·6		
1859	37·4	1890	34·1

These data, put into the form of a graphic diagram, give a wave whose crest (mildest of the severe winters) we seem to have passed in the sixties. And it would appear judging by the past, that we have not yet reached the bottom of the hollow; but that after some years' interval we may have a winter even more severe than last, possibly we may have more than one, of growing severity.

It is right to state that, as far as 1856, the values of mean temperature used are those of Mr. Belleville, reduced to sea-level, as given in a paper by Mr. Eaton to the Royal Meteorological Society (Quarterly Journal, January 1888); after that date, those of Greenwich Observatory, published annually. The slight difference in kind does not materially affect the result.

In the *Meteorologische Zeitschrift* for September, M. Woeikof considers the question whether winters in Russia have been growing warmer, and his examination of the St. Petersburg records, from 1744 to 1890 (noting the number of cold days), leads to an affirmative answer. The number of very cold days has, on the whole, fallen off considerably in the later sixty-three years compared with the earlier, and in the second half of our century, as compared with the eighteenth and the earlier half of the nineteenth.

This, he finds, corresponds with popular opinion for Northern and Central Russia, according to which intense frosts have become more rare; but in the south, in the Crimea, the Caucasus, and Turkestan, there have been complaints of colder winters of late.

Mr. Glaisher some time ago expressed the view that our winters had been becoming milder. I have seen a criticism of this view, to the effect that the proximity of Greenwich to such a rapidly growing city as London might have to do with such a result. If the facts are as I have suggested above, a growing severity has taken the place of growing mildness, and the criticism referred to would fail to apply. A. B. M.

A Lunar Rainbow.

ON the evening of Saturday, October 17, at about 6.30 p.m., the rare and interesting phenomenon of a lunar rainbow was observed from Patterdale, Westmoreland. On the south-east, the moon, which had just risen, brightened the sky behind the mountains, while on the north-west there hung a uniformly dark and unbroken screen of haze or rain-cloud, which lightened off somewhat and was more scattered on the extreme west. With its highest point lying almost exactly north-west, a semicircle of pale whitish light was projected against this vapoury curtain. The bow was quite complete, but much brighter and sharper on its northern arc than on that falling south. The brighter portion fell over weird and clear into Glenridding (a favourite haunt of sun-painted rainbows), and as seen striped against the dark hill-sides of that valley, appeared to emit a pale-blue phosphorescent glare. At one time a shred of the dark smoky haze scudded over, but did not completely obscure the highest reaches of the spectral light. The radius appeared smaller than in the case of an ordinary solar rainbow, and the breadth of beam was about one-half thereof, or perhaps rather less. The spectacle having lasted for about eight minutes, light rain began to fall, and then the sky in a very short time became quite clear and star-lit, and all was over.

P. Q. KEEGAN.

Patterdale, Westmoreland, October 17.

The Destruction of Mosquitoes.

THE recent mention of this subject in your pages reminds me that I was told a few years ago by an English gentleman who has a most beautiful place on the Riviera that he had freed his property from this pest.

The property in question is a peninsula, and for that reason is exceptionally open to separate treatment. On the Riviera, as many of your readers will know, fresh water is a somewhat rare commodity, and all of it that the inhabitants can lay hold of is stored for future use in tanks or small receptacles.

The larva of the mosquito lives, as I understand, only in fresh water. Consequently, on the Riviera he is found in the tanks I have named.

The carp is, I am told, passionately fond of the larva of the mosquito, and the Englishman I refer to had extirpated the insect by putting a pair of the fish in every tank.

The plan is not one that could be adopted everywhere, but it is worth bringing under the notice of those whose circumstances are like those of the Riviera. S. A. M.

Law of Tensions.

POSSIBLY many science teachers find some little difficulty in satisfactorily demonstrating to a class the "law of tensions" for vibrating strings. In practice, unless the sonometer is fixed vertically, the error introduced by friction at the pulley (especially with heavy weights) is so great that the real tension is very different from that represented by the weight attached. Even if the apparatus be thus fixed, the changing of the weights occupies time, and a comparison wire is necessary, which must first be tuned to exact unison. The following admirable and very simple method was suggested to me by one of my students, and possibly there are some teachers to whom the idea is new.

Instead of applying tension by attaching weights, the result may be effected much more readily by means of an ordinary spring suspension-balance, such as is often used for weighing parcels. By this method the tension may be regulated to within half a pound, and increased or decreased so rapidly that the heightening of pitch is clearly recognized without the use of an auxiliary wire. H. G. WILLIAMS.

Congregational School, Caterham.

The Koh-i-Nur: a Reply.

IT is a far from pleasant task for me to set about replying to Prof. Maskelyne's criticism of my history of the Koh-i-Nur. I desire to say what must be said with all respect for him, but the tone of some of his remarks renders this a task of exceeding difficulty. All I care about is to get at the truth, and in order to do so I have spared neither time nor labour. I cannot suppose that you would grant me space sufficient for answering in detail all the statements in Prof. Maskelyne's article; nor do I seek for such space, because I deem it to be sufficient for those, several of them experts, who have accorded my views their hearty support and approval—Firstly, to state here in a general way that having very carefully studied Prof. Maskelyne's long article it has not, in my opinion, in the very smallest degree shaken the facts I have quoted, and the deductions from them which are to be found in my appendix to "Tavernier's Travels," and in the article published in the April number of the *English Illustrated Magazine* of the present year. Indeed, I might go further, and say that this attack very materially confirms the strength of the position upon which I have taken my stand. Secondly, I shall select a few points only which afford clear issues without any mystification, as to which side the balance of evidence lies upon, and invite readers to draw their own conclusions.

Before going further I think I should recall to notice the review of my edition of "Tavernier" which appeared in NATURE last February (vol. xliii. p. 313), and the *English Illustrated Magazine* for April, from which it will be seen that a suggestion made in the review has since been acted upon, with the result that was anticipated.

Prof. Maskelyne states that there is an absence of novelty in my facts. Just so, it is the old facts that I rely upon, not the misquoted and distorted variants which are to be found in so many writings. In my earliest allusions to this subject, many years ago, I made some mistakes, from blindly following authorities whom I now know to have been misled as to their facts. Since then I have learnt how necessary it is to check all statements as of fact in reference to this subject, and not to place too implicit a trust on quotations, no matter how eminent the authority who makes them may be.

Is it conformable to the judicial position which Prof. Maskelyne claims to occupy, to say that I dismiss Prof. H. H. Wilson, and what he narrates, "by the somewhat flippant remark that 'it has afforded sundry imaginative writers a subject for highly characteristic paragraphs'"? the facts being these—I never referred to Prof. H. H. Wilson; I did not even know before that he was the writer of the anonymous note in the official catalogue; and more than that, I had not that particular contribution to the subject in my mind when writing the above words.

Still further, with regard to the judicial position, I do not think it is apparent in any of Prof. Maskelyne's subsequent remarks. They are those of an advocate who smites his opponent in season and out of season, and seeks to disparage him by implying that he has assaulted the reputation of men (whom all must honour), when he has merely pointed out misquotations in their writings and expressed dissent with their conclusions.

I yield to no one in my admiration for the late Mr. King's work, but this cannot and should not restrain me from pointing out misquotations and misprints in his books when treating of the subjects with which he has dealt. To justify this I shall quote but a few instances which I have noticed, out of many. On pp. 78 and 82 ("Natural History of Precious Stones," Bohn's edition, 1870) the weight of the Mogul's diamond is stated as on Tavernier's authority to have been 240 carats and on the plate 208 carats, instead of $279\frac{9}{16}$ carats.

The Koh-i-Nur is stated on p. 82 to have weighed 184 carats instead of $186\frac{1}{16}$, and, strangest of all, when recut, that is to say in its present condition, its weight is given, pp. 75 and 347, as $102\frac{1}{2}$ and on the plate as $102\frac{1}{4}$ carats, whereas its true weight is $105\frac{1}{16}$ carats.

On p. 68 he deduces an argument from the note by Clusius, which is referred to by Prof. Maskelyne, and given in the original in my paper; the whole force of his argument depending, however, on the change of the word Belgium of the original to Europe in his, Mr. King's, own rendering of it.

I might add to this list, but sufficient has been stated to show that such statements require the most careful scrutiny, by whomsoever they may have been made.

On pp. 81–82 will be found Mr. King's dissent from Prof. Maskelyne's theory about the identity of Babar's diamond with the Mogul's; the difference of opinion between them being very wide indeed, though Prof. Maskelyne does not think it necessary to refer to it in his article.

With reference to what Prof. Maskelyne writes about De Boot and Garcia de Orta, I shall only say that I am very well acquainted with both authors' works, and that I assert again that the statement wrongly attributed to Monardes, and quoted as from Mr. King by Prof. Maskelyne, was an unsound and dangerous link in the chain by which it was proposed to connect Babar's diamond with the Koh-i-Nur.

It was a statement convenient to use, but what if I had used it first, and had also misquoted the authority? Would the terms Prof. Maskelyne employs about my aberration, &c., have been considered strong enough? There was, however, no aberration whatever on my part, and Prof. Maskelyne has himself now fully demolished, as anyone may read, the authenticity of the link he formerly used as a very material element in his chain. How can he, then, still cling to the fragments of this shattered link, while he dismisses so peremptorily Malcolm's statement about the weight of the Darya-i-Nur? Will he ever again use that link, or quote Monardes as his authority? (*Edinburgh Review*, vol. cxxiv., 1866, p. 247.)

I still venture to think that my conclusion as to the kind of carat used by Tavernier is a legitimate one. At the end of chapter xviii., book ii., he says, where computing from their weights the values of diamonds to a *liard*, "le Diamant du Grand Mogol pese $279\frac{9}{16}$ carats" (*sic*); and in the very next paragraph, "le Diamant du Grand Duc de Toscane pese $139\frac{1}{2}$ carats."

True it is, as pointed out by Prof. Maskelyne, that Tavernier in some other passages defines the carats as "nos carats"; he does not say, however, "carats de France," and the meaning therefore I take to be the carats employed by himself and his confraternity as contrasted with Indian measures of weight.

The value of the *abbas* or pearl *ratti* of 2·66 grains, or seven-eighths of the Florentine carat, has also been approximately arrived at by other relations given by Tavernier; conversely, therefore, it proves his carat to have been the Florentine.

I know of several early writers who have written about the Grand Duke's diamond, and by them Tavernier is referred to as *the* authority for its weight, which, as even Prof. Maskelyne admits, was given in Florentine carats. I think all the circumstances justify the belief that it was probably weighed by Tavernier himself with his own weights and scales. Now as to the weighment of the Mogul's diamond, in one passage Prof. Maskelyne (p. 557) states that Tavernier does not say he weighed any of the stones, and, in another, on the same page, "The diamond Tavernier saw, weighed, he said (was he merely told so or did he really weight it?), $319\frac{1}{2}$ ratis."

The pages of Tavernier give the following very explicit answer to this query. He says, "Ce diamant appartient au Grand Mogol, lequel me fit l'honneur de me le faire montrer avec tous ses autres joyaux. On voit la forme où il est demeuré étant taillé, *et m'ayant esté permis de le peser jay trouvé qu'il pese $319\frac{1}{2}$ ratis qui sont $279\frac{9}{16}$ de nos carats.*"

This is precise evidence enough that he did weigh the stone himself, and if the carats were French instead of the lighter Florentine carats, which I believe them to have been, the stone was so much the heavier, and therefore still more removed in weight from Babar's stone.

Tavernier, I must remind the reader, besides Bernier, is our only authority for what is known about the Mogul's stone, as such, and what I have protested against and still protest against is, the suppression or rejection of such precise statements as the above, while others of his which fit in with particular theories are accepted.

In various directions I have been enabled to show Tavernier's minute accuracy about matters not connected with his trade as a jeweller, and when he speaks as an expert, in the practice of his own profession, he deserves, and proves that he deserves, a very different treatment from that which he has received. It is for this reason, and not because I am blind to his faults, that I give him my loyal support. I have already, in vol. ii. of "Tavernier's

Travels," stated that some corrections of values given in vol. i. are required in consequence of the identification, made too late for their correction, of the value of Tavernier's carat, but the present discussion as to the Koh-i-Nur is quite independent of that.

With regard to the mutilated condition of the Koh-i-Nur, I have nothing to add ; the statement as to its condition, quoted by me, and the figures and models of the stone appear to be sufficient proof that portions had been removed by cleavage, which would account for the difference between its weight and the Mogul, as described by Tavernier, and I still retain that opinion.

It is not of the least importance as regards the main question, whether my suggestion should prove correct or not, that if Babar's stone has survived it may be identical with the Darya-i-Nur, to which Malcolm attributed a weight of 186 carats. Prof. Maskelyne, upon a system of calculation which I cannot admit as applicable to the case, as we do not know the thicknesses of the stones which he compares, gives to the Darya-i-Nur an estimated weight of 210 carats. For the present, therefore, I prefer Malcolm's definite statement to Prof. Maskelyne's theory about the attributed weight being the "echo associated with the Koh-i-Nur."

I shall have something to say about the Golconda table diamond, and about a great many other diamonds and other precious stones too, on a future occasion. In that work I shall be as careful to give, as I have hitherto been, chapter and verse for every statement of fact quoted, and I shall trust the histories so supported will find acceptance from those who care to investigate the evidence in favour of the conclusions connected therewith.

I am not quite sure that I appreciate the full force of the phrase "verisimilitude of a true history"—the last words of Prof. Maskelyne's article—but of this I am certain, that if ever I should see a history of the Koh-i-Nur following the lines of that article, I shall feel bound to make another and special "incursion" into the subject in defence of Tavernier if not of myself.

Dublin, October 12. V. BALL.

THE NAUTICAL ALMANAC.

IT has been known for some little time that Dr. John Russell Hind, F.R.S., who for many years past has been responsible for the production of the national ephemeris, would soon seek that retirement to which his long services and his distinguished career entitle him. At the end of the year, he will relinquish the office of Superintendent of the "Nautical Alamanac," and the good wishes and kindly sympathy of the astronomers of many nations will follow him in the retirement he is seeking.

His successor has been appointed, and in Mr. A. M. W. Downing we have not the slightest doubt that the Admiralty have made a happy selection, and that under his auspices the high character and reputation of the "Nautical Almanac" will be fully maintained. Mr. Downing has long been associated with meridian astronomy in its best traditions; and in his position of greater responsibility and greater freedom we entertain the hope that his astronomical reputation will be fully maintained and extended. He may be said to enter on his office at a time when the "Nautical Almanac" is on its trial. The arrangement of the book, and the information it conveys, were practically settled by a Committee some sixty years since. How efficiently that Committee performed its task is shown by the fact that so little alteration has been needed for so long a period. But the outcry for change has gone forth : new committees are deliberating and reporting, and it will be among Mr. Downing's first duties to give shape, alike to the suggestions of irresponsible authorities, as well as to incorporate the recommendations of recognized committees in a new and improved "Nautical Almanac."

One great difficulty which has to be encountered, and of which it is not easy to see the proper solution, is due to the fact that the "Nautical Almanac" seeks to supply the wants of two very different classes of persons—namely, astronomers properly so called, and nautical men. The former demand very considerable detail in the exhibition of the several computations, the latter are satisfied with a very few final results. The former class is a small one, and a very moderate edition would satisfy their demands. The latter class is a very large one, and necessitates the printing, it may be, of thirty or forty thousand copies. The first question therefore, it seems, which must claim the attention of any Committee, or of any Superintendent, is, whether it be desirable to separate the "Nautical Almanac" into two, or it may be more, sections—one circulating among astronomers, the other among mariners. Private enterprise, anxious to minister to the wants of a rapidly increasing mercantile marine, has long supplemented the "Nautical Almanac" with a smaller and pirated edition, valuable to sailors, but detrimental to the circulation of what may be considered the legitimate ephemeris. Would it not be better if the Admiralty could see their way to publish an ephemeris with other nautical information, entirely for the use of the marine? Such a course is followed by the Governments of other countries. The German Government publish at Berlin a compact "Nautisches Jahrbuch," admirably adapted for naval purposes. This example is followed in Austria and in America, and we believe that the sale of our "Almanac" to the naval men of those countries has fallen off in the last years, or at least has not kept pace with the increase of foreign tonnage.

Such questions are of importance, as concerning not only the financial position of the work, but its influence in our own and foreign navies. There are, however, others touching the scientific and purely astronomical side of the compilation. Such, for instance, is the vexed question of the introduction of empirical terms in the final positions of the moon. Astronomical purists will maintain that the position of the moon should be that assigned by a purely gravitational theory, to facilitate the comparison of that theory with observation. Others demand that the place of the moon should coincide, as accurately as possible, with observation ; and looking at the large portion of the "Nautical Almanac" devoted to "lunar distances," it would seem (if this section is ever used) that it is desirable that the distances given should represent observed facts. After a naval man has been at the trouble of observing and reducing a lunar distance, to ask him to apply a correction for the error of moon's place seems wanton and irritating. And if the amount of the empirical correction is clearly ascertainable, it can be easily removed before instituting a comparison between observation and that theory from which the moon's place has been computed. But to satisfy the demands of both classes of astronomers will try the tact and ability of the new Superintendent to the utmost.

The section devoted to the apparent places of the stars has also been submitted to considerable criticism. No doubt here enlargement is needed, and possibly improved places of the stars, particularly of circumpolar stars in the southern hemisphere, are much wanted. But on this point the new Superintendent is himself a weighty authority. He has worked much and successfully in the determination and removal of systematic differences from star catalogues, and their reduction to known and recognized standards. So that, under his influence, we may hope that this section will take and maintain a foremost position.

Mr. Downing has undertaken a very important duty, of great national importance, at a very critical period. We fully believe that he will grapple with this task successfully, and that, in his efforts to improve our ephemeris, he will have the assistance and support of all classes of astronomers.

RAIN-MAKING IN TEXAS.

In Nature of September 17 (p. 473), Mr. H. F. Blanford has discussed at considerable length the rain-making experiments in Texas, on the basis of such information as was attainable from newspaper reports. Inasmuch as these telegraphic reports have not only been inadequate, but also frequently inaccurate and misleading, the writer, who was the meteorologist of the Expedition, is led to give the following brief summary of the experiments and their results.

The experiments, which have been quite independent of the direction or patronage of the Weather Bureau, have been carried on by the Hon. R. G. Dyrenforth, Special Agent appointed by the Department of Agriculture. The plan of exploding oxy-hydrogen balloons was adopted as one of the principal methods to be employed, and several months were spent in preparing the necessary materials and apparatus. Preliminary experiments made in Washington demonstrated that a tremendous concussion could be produced by the explosion of balloons 10 feet in diameter, filled with a mixture of hydrogen and oxygen in the ratio of two to one. In addition to the explosion of balloons, preparations were made to fire sticks of dynamite carried up in the air by kites, and to explode rackarock (an explosive consisting of three parts of potassium chlorate to one part of nitrobenzol) and dynamite on the ground.

With materials for carrying out these three lines of experiment, the party went to an isolated ranch twenty-three miles north-west of Midland, Texas (lat. 32° 14′, long. 102° 12′). The inauguration of the experiments attracted great attention throughout the whole south-western section of the country, and, locally, people went from all the surrounding counties to witness the operations. Actual trial in the field soon developed the fact that the preparations for the balloon experiments were entirely inadequate. Accidents occurred to the furnaces for generating the gas, which took much time to repair, windy weather prevented the filling of the balloons, and a combination of other sources of delay rendered this line of experiment a practical failure. One or two balloons were exploded on several days, but these were too few in number and too infrequent to serve the purpose of an adequate experiment. Similarly it was found impossible with the small available force to operate the kites to advantage, and in windy weather they were quite unmanageable; so that, although, in all, quite a number of dynamite sticks were fired in the air in this way, yet as a line of effective experiment this also proved a failure. The only explosions that were made on a scale even approximately commensurate with the requirements were those of rackarock, and it may be stated that all the effective operations essential at Midland can be duplicated in every essential particular with 1500 pounds of rackarock together with 500 feet of wire and a small portable dynamo.

The first rain that occurred after the party reached Midland began shortly after noon on August 10, and continued at intervals until evening. The amount of rainfall was not measured, but it was stated in the language of the country to be a good "grass rain." The writer, who was *en route* to Midland, met similar sharp showers in the latter part of the afternoon near Sweetwater, 100 miles to the eastward. On the preceding evening some preliminary explosions had been made, but only on a small scale, and no result was anticipated. In the telegraphic despatch that was sent reporting the rainfall, no causative action was claimed—in fact, such action was explicitly disclaimed in the telegraphic report, which stated "we do not think the explosions actually produced the storm, as they were not on a large enough scale. The preliminary trial was made simply to test the efficiency of the special blasting powder." The firing, which was not over half-a-dozen blasts, was, then, simply a preliminary trial of material, and not in any sense an experiment to produce rain.

On August 16, 17, 18, and 20, cloudy weather very largely prevailed, and numerous thunderstorms were seen on the horizon that did not visit the ranch. On each of these days blasts of rackarock and of dynamite were fired while heavy cumulus or dense storm-clouds were in the field. In several instances, when a dense threatening cloud was overhead, a sharp detonating explosion of rackarock or of dynamite was followed at an interval of 30 to 40 seconds by a spatter of rain, or, if it was already sprinkling, the blast was followed by a very noticeable increase of the drops. This interesting result occurred a sufficient number of times to indicate that the phenomenon was a real effect of the explosions. On none of these days, however, was the amount of rainfall appreciable, except on the 18th, when it was two-hundredths (0·02) of an inch. The 18th opened cloudy, and old settlers predicted rain for the afternoon, whether the experiments should be made or not. To what extent, therefore, the explosions that were made were influential in producing the 0·02 inch that fell is obviously very difficult to determine, and as an evidence of the efficacy of the explosions it is practically valueless.

The next explosions were on the evening of August 21, when 156 pounds of rackarock were fired in 14 blasts. During the night a genuine norther came on, the wind blew from the north, the barograph curve rose rapidly, the temperature fell rapidly, and during the next forenoon a fine mist prevailed. This change of weather was quite extraordinary and unexpected, and with its accompanying mist was attributed to the heavy firing of the evening previous; but the norther had been on its way for several days, and the fine mist was evidently due to the uplifting by the cold north wind of the warm moist air of the plains. At numerous points in the State where the air was more humid a heavy rainfall occurred.

The last experiment, which in magnitude was the greatest of all, took place on the evening of August 25, after the writer had departed. The conditions were thought to be extremely unfavourable for rain, and the party was advised to wait for a more propitious occasion. The firing, however, was carried on until 11 p.m., when the party retired for the night. It is reported that "at 3 a.m. the heavy rolling of thunder disturbed the sleepers, heavy banks of clouds were seen advancing, almost constantly lighted by most brilliant lightning. An hour later the rain began to fall in torrents on the ranch, and did not cease till 8 a.m." Unfortunately, records of the *amount* of rainfall have not yet been received, but I am informed, by a gentleman who was present, that "it was nothing but a sprinkle." Further light is thrown on this rainfall by the weather map for 8 p.m. eastern time, of August 25. Rainfall is shown in New Mexico to the north-west of Midland, Texas, and the forecast officer made the following prediction: "For Eastern Texas, generally fair, except *local showers* on the extreme south-east coast *and the north-west*." Here we have an official prediction made in Washington City of probable showers over the district in which the experimenters were operating, and for the very night in which the thunderstorm followed the last of the explosions to produce rain.

In view of these facts, it is scarcely necessary for me to state that these experiments have not afforded any scientific standing to the theory that rain-storms can be produced by concussions. But, if the adherents of the theory maintain that "no experiment has been tried that is worthy of the name, and that no results ought to be looked for," it will be difficult to take opposite ground.

George E. Curtis.

Smithsonian Institution, October 9.

COLOUR-BLINDNESS GENERALLY CONSIDERED.

COLOUR-BLINDNESS has now passed from the category of ailments denominated interesting, and is recognized as a visual infirmity the importance of which cannot be over-estimated. Before entering upon a discussion of the subject it will be well to lay down a definition of colour-blindness that shall run on all fours with the latest scientific findings in the matter. Colour-blindness is merely the inability of the eye to recognize the quality of the light that falls upon it, *i.e.* to discriminate between ether waves of varying refrangibility, the impingement of which upon the retina conveys to us the sensation of colour. Total colour-blindness is the inability to distinguish any colours. To a person so afflicted all bodies are either black, or white, or grey, according to the intensity of the light reflected from them. This form of the disease is very rare. Colour-blindness in ordinary is merely a question of degree, no two persons having exactly the same colour perception. A popular, but erroneous, belief respecting human vision is that good eyesight, *i.e.* accurate perception of form and distance, carries with it a keen perception of colours. This belief is deeply rooted, the impression that colour perception is an integral part of *good* eyesight being of almost universal adoption. The eye, however, that has the most perfect appreciation of form and distance *may* utterly fail to discriminate between two differently coloured objects of the same shape, and placed at equal distances from the observer. In this case a variation in the intensity of the light reflected from the objects under view would enable the colour-blind to discriminate between them, for along with colour ineptitude there generally exists the most delicate sense of discrimination as to the relative intensities of two sources of light.

The majority of people are undoubtedly afflicted with a mild form of colour-blindness. They are physically incompetent to differentiate exactly between the nicer shades of the more composite colours, such as browns, greys, and neutral tints. Yellow would appear to be the colour that gives least trouble to the colour-blind, and blue, if strongly illuminated, is readily recognized.

Red would appear to be the colour the want of the sense of which may be said to be characteristic of colour-blindness; and as a person blind to red is usually blind to its complementary colour, green, ordinary colour-blindness may thus be defined as the inability to discriminate between red and green. The normal eye would appear capable of analyzing white light into three coloured elements, one of which is red; the colour-blind eye, on the other hand, analyzes white light into two elements, neither of which is red. Why this visual defect should manifest itself in inability to distinguish that part of the spectrum which is the result of the slowest of the series of ethereal undulations is by no means clear. Physiological knowledge as to the exact relationship between external colour factors and our mental idea of colour is yet in its infancy. A consensus of opinion would, however, appear to obtain, that, of the rods and cones to which the nerve terminals of the retina are generally compared, the latter are responsible for the processes of analysis by which a compound ether wave is decomposed into its constituent elements, each of which produces an influence upon a corresponding nerve fibre. That the rods and not the cones are least responsible for our sensations of colour would appear to be borne out by the fact that among predatory animals, to whose nocturnal habitat a colour percipient apparatus would be an unnecessary adjunct, the cones are wanting, while the rods are very highly developed. Which theory may be ultimately accepted as best explaining the varied phenomena of colour-blindness is at present matter of speculation.

Authorities on colour-blindness are, however, agreed that in the majority of cases it is congenital; that to a great extent it is amenable to the same laws that govern the transmission of other hereditary tendencies; and while in some very few cases where it is induced by accident, such as concussion of the brain, or is the residual product of some malady or alcoholic excess, it may be palliated, yet colour-blindness is absolutely incurable.

The knowledge that something like 3 to 4 out of every 100 of our adult population are afflicted with colour-blindness is of serious importance, and statistics show that this is no over-estimation of the case. The following table shows the percentage obtained from a large number of cases:—

Examiner.	Number Examined.	Number Colour-blind.	Percentage.
Holmgren ...	... 32,165 men ...	1,019 ...	3·168
Dr. Joy Jeffries	... 18,556 ,, ...	764 ...	4·117
London Committee ..	14,846 ,, ...	617 ...	4·156
Total ...	65,567 men	2,400	3·66%

The percentage of female colour-blinds is much less. My own findings show 0·162; Dr. Joy Jeffries, however, found a lower proportion than this, as among 14,557 females tested only 11 were colour-blind.

This great disparity between the numbers of the colour-blind in the two sexes has been long known, and various causes have been assigned as accountable for it. As far back as 1855, Prof. Wilson, while admitting the superiority of colour perception in the female, could not believe that the number of colour-blind women were so few as compared with the number of men similarly afflicted. He took up the view that women were not so willing to be tested as men, so that unless they were members of some public institution it was quite a voluntary matter whether they were tested or not. He argued, too, that women attached greater importance to perception for colour than men do, and would consequently strive to screen their defect from others. Thus the only women who would voluntarily submit to be tested for colour would be those who had no doubt but they were possessed of perfect colour-vision.

Unfortunately, however, this method of reasoning is based upon an hypothesis altogether fallacious. Colour-blind people do *not* of themselves realize their condition. They cannot tell that there is any difference between red and green as they see them, and red and green as viewed by the normal eye.

The fact that females have more practice in handling colours than males is not sufficient to account for this disparity, unless we assume that the present condition of the female colour percipient is the resultant of the gradual development and training transmitted through ages of time. The superior colour percipience of the female must, we believe, be regarded as an inherent quality of the sex, which no amount of individual artificial training and practice can attain to.

There is just one thing, however, which may discount a little this feminine superiority. As colour-testing was first applied, too much importance was attached to the correct *naming* of colours, and as this is a province in which the masculine section of humanity is decidedly inferior, the ratio of male to female colour-blinds may have been increased in consequence. Assuming that the percentage of 3·66 of adult male colour-blinds is correct, we are confronted with the fact that there are over 4000 colour-blind seamen in the British mercantile marine. This number is exclusive of pilots, canal or lightermen, and firemen. Now, all of the 4000 are liable to be called upon to officiate as look-outs, *i.e.* they may be placed in circumstances where it is necessary they should distinguish instantaneously between the colours of the regulation side-lights of an approaching vessel. As far back as 1877, the Board of Trade, acting in accordance with the recommendation of the ophthalmic section of the British medical profession, came to the conclusion "that all candidates for masters' or mates' certificates shall pass a test examination as to their ability to distinguish the following colours, which enter largely into combination of signals by day or night used at sea; viz. black, white, red, green, yellow, and blue"; and they state that "the Board have been led to this decision because of the serious consequences which might arise from an officer of any vessel being unable to distinguish the colour of the lights and flags which are carried by vessels."

So far so good. But there the matter stopped. An officer failing to pass in colours is not deterred from going to sea; his certificate is simply endorsed "*failed to pass in colours*," and then it is optional with the owners, *if* they know of a man's colour imperfectness, to engage him or not. In the majority of cases they do not know. Wishing to obtain accurate information as to the views of the Liverpool shipowners upon this subject, I submitted to them the following queries:—

(1) Do you consider a colour-blind officer, mate, or captain, competent to have command of a vessel, steam or sailing?

(2) Would you consider a colour-blind man fit to be a look-out man?

In reply, 110 firms answered both questions in the negative, while *one* answered both in the affirmative.

Six said "Yes," to the first query, and "No," to the second.

Six expressed the opinion that no colour-blind officer should have command of a vessel; but that colour-blindness was not a barrier to a seaman officiating as look-out.

The language of the firms that answered both questions in the negative was such as to show that there was not the slightest hesitancy in the minds of the writers as to the utter undesirability, not to say danger, of employing a colour-blind man in any capacity in which he was responsible, in part or whole, for the safe navigation of the vessel.

Such expressions as "emphatically no," "absolutely unfit," "not fit to serve on a ship," "very unsuitable," &c., show in unmistakable terms the views held by Liverpool shipowners on the subject.

Liverpool shipowners certainly seem alive to the dangers of colour-blind *employés*. The practice of private examination would seem to be coming into common practice among first-class firms. But the Board of Trade have still to realize that look-out men, as well as officers, should not suffer from colour-blindness. If shipowners themselves deem it necessary for their own interests, and the safety of the *voyageurs* and property intrusted to their care, to debar colour-blind seamen from their service, it is surely incumbent upon the Board of Trade, in the interests of the travelling community over whose welfare they are supposed to preside, to make perfect colour-vision a *causa sine quâ non* that shall apply to *all* seamen of our mercantile marine. It is but fair, however, to that complex and overburdened instrument of government to add that they have introduced a so-câlled voluntary test, whereby a seaman, on payment of a fee of 1*s*., may be tested as to the perfectness of his vision for colour. Such a test must, from the very necessities of the case, be absolutely worthless. What A.B. would be likely, had he the slightest suspicion of his colour-blindness, to seek that confirmatory evidence which would debar him from following his calling? Sailors may be pardoned if they prefer to remain in a state of blissful ignorance as to their colour-vision, since they have nothing to gain, and possibly everything to lose, by undergoing an examination in colours. It must be admitted, however, that there are not wanting those who aver most positively that colour-blindness is not responsible for maritime disaster of any description whatever.

Rear-Admiral P. H. Colomb is of this opinion. In discussing the action of the Washington International Maritime Conference relative to colour-blindness, he stated, "I never knew myself a case of collision where colour-blindness was in question. The statements were generally perfectly clear that wrong helm was given deliberately in the face of the colour seen, and as no authoritative teaching had existed to show that it mattered what colour was seen as long as danger was denoted, I have never been able to lay stress on the colour-blind question."

Again, Admiral Colomb expressed the opinion "that collisions at night occurred through the helm being ported to the green light, and starboarded to the red light."

Undoubtedly this is a fertile source of disaster, but seamen, unless we assume them wilfully negligent, or astoundingly nervous, could hardly fail to act correctly at the critical moment in so many instances, if there were not some other factor at work which brought them to grief. I admit the truth of Admiral Colomb's statement as to collisions at night occurring through the helm being ported to the green light, and starboarded to the red. But I would go further, and inquire why such a wrongful procedure should be adopted in so many cases. I cannot believe it is done wilfully with the intent of causing collision, I cannot accept nervousness on the part of men, many of whom have spent a lifetime at sea, as the sole, or even a likely cause. I believe that in many cases the reason why the helm is ported to the green light and starboarded to the red light is that the persons responsible for the porting and starboarding are visually incapable of differentiating between one colour and the other.

Admiral Colomb's cause is undoubtedly the immediate means of effecting the collision; but that cause traced to its original source will, in the majority of cases, show neither negligence nor nervousness, but will stand revealed as the inevitable resultant of eyesight that cannot distinguish red from green. Pronouncements such as those quoted above, coming from those in high places, and pregnant with the weight of authority that usually attaches to such utterances, are mainly responsible for the general laxity and half-heartedness which are so characteristic of the Board of Trade's officials in respect to colour-blindness. A perusal of the records of inquiries into collisions at sea, or of the courts which settle questions of maritime and commercial law arising therefrom, reveals an astounding amount of conflictory evidence as to the relative positions of the colliding vessels as judged by their side-lights. It would be more charitable to suppose that the witnesses examined were colour-blind, rather than guilty of wilful and deliberate perjury. In such cases the question of a look-out's colour percipience is never discussed. An examination of the witness *on the spot*, as to his capability of discriminating between the port and starboard lights of a ship, would set at rest the question of his physical competence to assist in elucidating the problems under consideration.

The Dutch Government has long been alive to the dangers accruing from *induced* colour-blindness—I use the term induced in contradistinction to congenital—and adopt the most drastic measures to prevent a colour-blind officer from holding a position in their mercantile marine. Among other qualifications necessary to procure a warrant empowering a man to act as mate in the merchant marine, the royal order requires:—

"Colour perception perfect for transmitted light in one eye, and at least one half in the other, according to Donders's method."

Also that "the report and declaration of the expert, as required in the above, shall be considered valid for one month only from the time the test is made."

In Holland the tests are made by experts. In England they are applied by persons who, however well they may be qualified to examine candidates in navigation and seamanship, have certainly no *locus standi* in the matter of reporting upon the perfectness, or otherwise, of a man's visual organs.

The tests themselves that these navigation examiners have to apply are far from being perfect. They are established upon a wrong principle. Candidates are made to name colours, and according to the Parliamentary Report of 1887 "the only reasons for which they are reported as having failed are inability to distinguish red from green, and either from black by daylight, and red from green and either from ground glass by artificial light."

Candidates are first required to give correct colour names to a series of eight cards coloured black, red, green, pink, drab, blue, white, and yellow, respectively. A candidate is passed, however, if he names correctly the first three.

The second test consists in naming the colours of glasses some eleven in number, viz. ground glass, standard red, pink, three shades of green, yellow, neutral tint, two shades of blue, and white. The candidate need, however, only name the ground glass, the standard red, and the standard green.

Clearly, with such tests as these, the colour-blind may easily escape detection.

The Board of Trade return relative to colour tests for the year ending May 31, 1891, shows that out of 4688 candidates who presented themselves for masters' and mates' certificates, 31 were rejected on account of deficient colour sense. That these should be rejected after serving an apprenticeship to the sea, is manifestly unfair. The test should be applied at the commencement of their nautical career, and not when the initial stage is passed. Four of the 31 were reported as passing on subsequently undergoing examination, although medical expert opinion is emphatic in stating that colour-blindness is absolutely *incurable*. Perhaps it may be that the examiners were disposed, by their leniency in passing young men whose previous "failure in colours" proved them colour-blind, to atone in some slight form for the bad system which allows lads to spend the best years of their life in mastering the irksome details of a profession, before it informs them that they are visually unfitted for it. It is to be hoped that the investigation into the whole system of colour-testing at present being conducted by a committee appointed by the Royal Society, may lead to thorough and effective reforms.

T. H. BICKERTON.

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: A LETTER TO PROF. TAIT.

MY DEAR PROF. TAIT,—I gather from your letter of September 28 (NATURE, October 8, p. 546) that you admit the correctness of Van der Waals's deduction from the virial equation (1) when the particles are infinitely small, in which case

$$\left(p + \frac{a}{v^2}\right) v = \tfrac{1}{3}\Sigma m V^2 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

a representing a cohesive force, whose range is great in comparison with molecular distances; and (2) when, in the absence of a cohesive force, the volume of the particles is small in comparison with the total volume v, in which case the virial of the repulsive forces at impact gives

$$p(v - b) = \tfrac{1}{3}\Sigma m V^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For hard spherical masses, the value of b is four times the total volume of the sphere. But you ask, "How can the factor $(v - b)/v$, which Van der Waals introduces on the left (in the first case) in consequence of the finite diameters of the particles, be justifiably applied to the term in K (or a/v^2) as well as to that in p?"

In my first letter I desired to avoid the complication entailed by the consideration of the finite size of the particles; but it appears to me that the argument there given (after Van der Waals) suffices to answer your question. For, if the cohesive force be of the character supposed, it exercises no influence upon any particle in the interior, and is *completely* accounted for by the addition to p of a/v^2. In so far, therefore, as (2) is correct when there is no cohesive force, the effect of such is properly represented by

$$\left(p + \frac{a}{v^2}\right)(v - b) = \tfrac{1}{3}\Sigma m V^2 \quad . \quad . \quad . \quad . \quad (3)$$

in which b is to be multiplied by a/v^2, as well as by p.

Yours very truly,

October 13. RAYLEIGH.

NOTES.

AT the Royal College of Physicians, on Monday, when the Harveian Oration was delivered by Dr. W. H. Dickinson, the Baly Medal was given to Prof. Michael Foster for distinction in physiology; the Morgan Medal to Sir Alfred Garrod for distinction in clinical medicine.

DR. DICKINSON, in the Harveian Oration, presented an admirably clear and vigorous account of Harvey's great discovery, and of the scientific results to which it has led. The earliest and most important of these results was the completion of Harvey's work by the discovery of the capillary system by Malpighi, who was born in the year in which Harvey published his famous treatise. "Harvey," said Dr. Dickinson, "had never seen a capillary, nor did the state of the microscope in his time allow of it. He was fain to conclude that the blood passed from the arteries to the veins partly by anastomoses but mainly by percolation, as water, to quote his own illustration, percolates the earth and produces springs and rivulets. Had it been possible, we may imagine the delight with which he would have witnessed the completion by vessels of his circular route." Dr. Dickinson also referred, among other results of Harvey's discovery, to embolism, and to our knowledge of inflammation, or at least as much of it as concerns the capillaries. In conclusion, he said:—"Knowledge has been advancing since Harvey's time in many and independent lines; the achievements of Bell, Bright, and Addison had no direct connection with his, but it is not too much to assert that the medicine of to-day is scarcely less permeated with the results of Harvey's discovery than is the human body with the circulation he discovered. It does not make him small to say that what he found out must have come to light had he never lived. If Columbus had not discovered America some one else must have done so before now. The law of gravity might even have been revealed in the fulness of time to another if not to Newton. But the discoverer is before his time; in this lies one measure of his praise; another, and a more important one, is in the results of his discovery."

THE Electrical Exhibition, to be opened at the Crystal Palace on January 1 next, promises to be one of great interest and importance. The requests for space—which already exceed a total of 200—include electric lighting plants for country and town houses, for mines, for steamships, for railway trains, and even for private carriages. There are also included the newest forms of motors, generators, accumulators, and other machinery employed for producing and storing electricity. Several of the more important exhibits at the Frankfort Exhibition will be transferred to the Crystal Palace. The apparatus section will include a complete set of Sir William Thomson's standard electric instruments, new electro-medical and electro-thermic apparatus, the latest improvements in telephony and telegraphy, and also the most recent electrical appliances for war purposes, blasting, signalling, &c. Special buildings are now in course of erection for boilers and other heavy machinery.

THE Municipality of Genoa has voted the sum of 15,000 lire in aid of the International Botanical Congress which is to be held in that city in September 1892 to celebrate the fourth centenary of the discovery of America.

THE French Association for the Advancement of Science will meet at Besançon in 1893.

THE Russian Geographical Society has awarded its great Constantine Medal to Prof. Sludsky for his researches into the figure of the earth and his geodetical work generally. Another Constantine Medal has been given to Prof. Pontebnya for his researches into the ethnography and the languages of the Great Russians, the Little Russians, and other Slavonians. His two works on the Russian grammar far surpass all previous works of the kind, not only in the number of examples but in the novelty and importance of his conclusions as to the structure of the Russian and other Slavonian languages; while his works on Great and Little Russian folk-lore are full of new and profound observations. The Count Lütke's medal has been awarded to S. D. Rylke for an elaborate work on the determinations of longitudes in Russia by means of the telegraph; the probable error of the chief determinations does not exceed 0·016 of a second of time. Another work of the same geodesist deals with the possible errors of levellings, as dependent upon temperature; they appear considerably to exceed those admitted in the best treatises on this subject. We also learn from Mr. Rylke's researches that the level of the Baltic Sea, as deduced from long series of observations, regularly sinks in the direction from north to south. Other gold medals have been awarded to Rovinsky, for a work on the geography and history of Montenegro; to M. Filipoff, for researches into the changes of the level of the Caspian Sea; to M. Obrutcheff, for a geological and orographical sketch of the Transcaspian region; and to M. Priklonsky, for a work on the Yakutes. Some silver medals have been awarded for works, chiefly in ethnography, of minor importance.

DR. A. R. FORSYTH, F.R.S., and Dr. M. J. M. Hill have been nominated to fill up the vacancies caused by the retirement of Dr. Hirst, F. R.S., and Mr. Lachlan from the Council of the London Mathematical Society.

MR. JOSEPH THOMSON has returned to England from South Africa, where he has been at work on behalf of the British South Africa Company. Accompanied by Mr. Grant, a son of Colonel Grant, he crossed the plateau between Lake Nyassa and Lake Bangweolo, and we learn from the *Times* that he has been able to make important rectifications in the geography of the Bangweolo region. The lake, as shown in our maps, is incorrectly laid down, mainly because the one definite and precise observation taken by Livingstone has not been adhered to. The lake is really only a backwater of the Chambeze (the source of the Congo), which enters from the east, and issues from the west of the lake as the Luapula. The lake, in fact, lies in a very slight depression of the plateau to the north of the Chambeze-Luapula. Even in the rainy season Mr. Thomson believes the lake does not exceed 20 feet at its deepest. The southern shores are clothed with forests, and, as a matter of fact, Mr. Thomson encamped far within the bed of the lake as it is laid down in most maps. In the rainy season the water of the lake spreads out, and covers for some distance the ground on which the forest stands.

MR. W. L. SCLATER, the Deputy Superintendent of the Indian Museum, Calcutta, will proceed to Upper Assam in December next, upon a collecting expedition for the benefit of the Museum. From Makum he will ascend the Dibing river in boats to the mouth of the Dapha, one of its confluents from the north, and establish his camp at some convenient spot in the Dapha valley. At the head of the Dapha valley rises Dapha Búm, a mountain of some 15 000 feet in altitude, on the frontiers of Chinese territory, so that there is a good prospect of the occurrence of Chinese forms in the district. The Dapha valley has been described geographically by Mr. S. E. Peal, who visited it in 1882 (see J.A.S.B., lii., pt. 2, p. 7), but has not been much explored zoologically. Mr. Sclater will pay special attention to mammals and birds.

MR. FRANK H. BIGELOW, who has been acting as assistant in the U.S. Nautical Office, has been appointed to a newly-created professorship in the American Weather Bureau. His work will relate to terrestrial magnetism and solar physics, especially in their relation to meteorology.

NEWS has been received of M. Paul Maury, who started in March last year for a botanical expedition in Mexico, and of whom nothing had been heard since his departure. He appears to have made a successful exploration of the province of Huasteca.

DR. S. WINOGRADSKY, of Zürich, has been appointed director of the scientific bacteriological section of the new Bacteriological Institute at St. Petersburg.

A NOTICE which will be read with interest by owners of gems has been issued by Dr. A. Brezina, the Director of the Mineral Department of the Natural History Museum at Vienna. It relates to the doings of a young man who, on September 26, contrived to conceal himself in the Department just before the time for the closing of the Museum. He was caught, and found to be armed with a revolver, and to have in his possession files and other implements. He had also in his possession nearly 600 gems, some of them cut, but the majority in their natural state. He has a passport, in which he is described as Hugo Kahn, of Berlin; but he has also called himself Krony, Kronek, Kornak, Kronicsalsky. His age is twenty-four; he measures in height 170 cm.; he is slender, has a longish, handsome face, is of a brownish complexion, has dark hair, grey eyes, and a light-brown beard, which is of feeble growth. Upon the whole, he is an attractive-looking person. He has made several journeys in Germany, France, Switzerland, and Italy; and between the middle of July last and the beginning of September he travelled through Pyrmont, Ems, Strasburg, Basel, Milan, Genoa, Nice, Monaco, Genoa, Venice, to Vienna. Most of the gems (the names of which, with the exception of a rock crystal, he does not know) he professes to have bought from a barber in Marseilles. As it is important that the former owner or owners should be known, Dr. Brezina prints a list of the gems, with the request that anyone who has information about them will communicate with him.

ON Monday the centenary of the Royal Veterinary College in Great College Street, Camden Town, was celebrated by a luncheon given in a tent which had been erected in front of the new buildings. The Duke of Cambridge, President of the College, took the chair, and the Prince of Wales was among the guests. In proposing the toast, "Success and continued prosperity to the institution," the Prince of Wales contrasted the important position of the College at the present day with its humble beginnings a hundred years ago.

WE regret to record the death of the Rev. Percy W. Myles, of Bright's disease, at the comparatively early age of forty-two, at Ealing, on October 7. He was a man of great ability both in literary and scientific pursuits. He was a good botanist, and proved himself a most able editor of *Nature Notes*, the journal of the Selborne Society. The work with which his name will be identified is the "Pronouncing Dictionary of Botanical Names," appended to Nicholson's "Dictionary of Gardening"; it is now recognized as a standard work by botanists. Unfortunately his professional duties did not enable him to leave a margin; so that it is proposed to raise a "Myles Memorial Fund" on behalf of his widow; and any contributions will be thankfully received and at once acknowledged by the Rev. Prof. G. Henslow, Drayton House, Ealing, London, W.

THE Council of the Institution of Civil Engineers has issued a list of subjects on which it invites communications. The list is to be taken merely as suggestive, not in any sense as exhaustive. For approved papers, the Council has the power to award premiums, arising out of special funds bequeathed for the purpose. A detailed list is given of the awards made for original communications submitted during the past session.

MORE than ten earthquake shocks were felt in the island of Pantellaria, between Sicily and the Tunisian coast, between 5.30 p.m. and 4 a.m. on October 14–15. Some of the shocks were rather violent, and nearly all the inhabitants left their houses and passed the night in the streets or in the open country. According to intelligence received at the Central Meteorological Bureau, Rome, from Pantellaria on October 18, shocks of earthquake continued to be felt in the island. A remarkable phenomenon is announced in connection with these seismic disturbances. A new volcano has risen from the bed of the sea, not far from the coast of the island, and has been throwing up masses of stones and rubbish to a considerable height. A "slight eruption" from it was referred to in a telegram sent from Rome on October 20.

LAST winter there were some reports that sunset phenomena had greatly increased in brilliancy, as if something similar to the optical disturbance following the Krakatão eruption had occurred. Herr Busch has remarked (*Met. Zeit.*) how difficult it is to recognize gradual variations in such phenomena, or to say where they pass beyond the normal. Even the brown-red Bishop's ring may be regarded as quite normal in winter. A much more sure method of finding an optical disturbance of the atmosphere is measurement of the polarization of light. Herr Busch has carried this on systematically for some years with a Savart polariscope, and a simple instrument for measuring angles, determining the height of the two neutral points (Babinet's and Arago's) at sunset. Now, the values for this height, in February and May last, considerably exceed those obtained in the three previous years, and come near those in 1886, when the last traces of the great atmospheric disturbance were still everywhere perceptible. It would seem, then, that some optical disturbance has been really present, the beginning, extent, and cause of which, however, are in obscurity. The desirability of systematic observations in different places is pointed out.

IN our issue of October 8 (p. 549) we drew attention to three atlases issued by the Chief Signal Officer of the U.S. Army. We have now to record the publication, dated June 15 last, of an atlas containing seventy-two charts showing the normal temperature conditions in the United States and Canada by decades, three decades to each month, for 8h. a.m. and 8h. p.m., Washington time. Although the Signal Service has been in existence upwards of twenty years, it had not before been able to accumulate sufficient actual observations at any one hour, or set of hours, from which normal values could be derived. The values and isotherms contained in the present atlas are based upon nine years' observations, 1881–89. The charts have been carefully prepared, for the work of the Forecast Division, and will also be very useful in furnishing general information upon the average temperature of North America. The work has been prepared under the supervision of General Greely, although issued by the new Weather Bureau.

THE Ealing *Middlesex County Times* (October 17) prints the following account of an incident which occurred at "The Grange," the residence of Mr. Yates Neill, Ealing, on Wednesday, October 14:—"It appears that during Tuesday night a large branch of one of the magnificent chestnut trees standing in the ground was broken off by the force of the wind, and fell on two stripling chestnut trees near the wall. On Wednesday morning, the gardener, a man named Parker, was engaged in sawing the detached bough, Mr. Delancey Neill and Mr. Vertie Neill watching the operation. Just before noon, the first-named gentleman saw what appeared to him to be a ball of fire fall, and striking the tree in an oblique direction, alight on the ground within two or three yards of where the three were standing, whence it rebounded and exploded with a sound like dynamite. Although neither of them was struck, the shock was so great that for a time all three were dazed, Mr. Vertie Neill, indeed, being thrown down, and rolling over two or three times. His brother was the first to recover from the shock, and promptly went to his help, and he was removed to the house, where the feeling of dizziness speedily wore off; and beyond somewhat severe headaches, which lasted for some hours, neither of the gentlemen nor the gardener appeared to have suffered any ill effects. The trunk of the tree struck by the meteor presents the appearance of having been burned in a zigzag direction for a distance of some 20 or 30 feet."

MOST people who visit Greece devote their attention mainly to the remains of ancient art. Dr. Philippson, of Berlin, is of opinion that they might also with advantage spend some time in climbing the mountains of Greece. In the *Zeitschrift des Deutschen und Oesterreichischen Alpenvereins* he deals with the subject in a capital paper, which has been issued separately. He gives an attractive account of his own experiences in climbing Mount Chelmós, in the Peloponnese, describing admirably the impression produced upon him by the Styx. Dr. Philippson shows that in the Highlands of Greece there is still much good work to be done in topography, geology, and meteorology; and he sees no reason why some of it should not be accomplished by tourists.

MESSRS. W. H. ALLEN AND CO. have published a second edition of the late Mr. R. A. Proctor's "Other Suns than Ours."

THE new number of the *Internationales Archiv für Ethnographie* opens with a most interesting paper (in German) by Dr. I. Zemmrich on "The Islands of the Dead, and related Geographical Myths." The author shows how widely diffused is the belief that there are far-off happy islands, where all sorts of enjoyments are in store for the dead; and he suggests that Atlantis, about which so much has been written, was originally one of these mythical realms. Dr. J. Jacobs concludes his critical examination (in Dutch) of Dr. Ploss's view of the significance of circumcision.

MR. G. J. SYMONS, F.R.S., contributes to the current number of the Quarterly Journal of the Royal Meteorological Society a learned paper on the history of rain-gauges. It was read before the Society on March 18, in connection with the annual exhibition, and is one of the series in which hygrometers, anemometers, instruments for travellers, thermometers, sunshine recorders, barometers, marine instruments, apparatus for studying atmospheric electricity, solar radiation instruments, and the application of photography to meteorology, have been successively dealt with. Among the remaining contents of the number are papers on the following subjects: meteorological photography, by A. W. Clayden; on the variations of the rainfall at Cherra Poonjee, in the Khasi Hills, Assam (plate), by H. F. Blanford, F.R.S.; some remarkable features in the winter of 1890-91 (four illustrations), by F. J. Brodie; the rainfall of February 1891, by H. S. Wallis; "South-east Frosts," with special reference to the frost of 1890-91, by the Rev. F. W. Stow.

IN the latest record of the proceedings of the Philosophical Society, Philadelphia, Dr. Daniel G. Brinton gives some vocabularies from the Musquito Coast. He obtained them from the Rev. W. Siebärger, a missionary of the United Brethren, now resident in that region. The most important of the vocabularies is a list of words from the language of the Ramas tribe, the only specimen of their tongue Dr. Brinton has ever secured. These people live on a small island in Blomfield lagoon. There are at present about 250 of them. All of them have been converted to Christianity, and, with the exception of a few very old persons, are able to speak and read English. Their native language is rapidly disappearing, and in a few years, probably, no one will use it fluently and correctly. They are large and strongly built, and are described as submissive and teachable. Their language has always been regarded as wholly different from that of the Musquito Indians, who occupy the adjacent mainland; and this is shown to be correct by the specimen sent to Dr. Brinton. It bears no relation, he says, to any other tongue along the Musquito Coast. It does not, however, stand alone, constituting an independent stock, but is clearly a branch, not very remote, of a family of languages once spoken near Chiriqui lagoon, and thence across—or nearly across—to the Pacific.

THE Penang Administration Report for 1890 contains some interesting observations on the little-known aborigines of the Malay Peninsula. Observations made during the course of the year go to show that the Sakai (as distinguished from the Semang, or Pangan, as the Negrito tribes are called by the Malays of Perak and Pahang respectively) are far more numerous than was formerly supposed, and the President is of opinion that there may be more than 5000 men, women, and children in the district of Ulu Pahang alone. The country on both sides of the mountain range, which forms the watershed of the Jelai, Selom, Bidor, and Kampar rivers, is thickly inhabited by Sakai, who, although one or two large villages exist, live for the most part in groups of from two to three families. These Sakai are divided into two distinct tribes, called by themselves Sen-oi and Tem-be respectively, the former being the more civilized and more accessible tribe, while the latter are but little known to the Malays. Both the Tem-be and Sen-oi dialects, however, resemble one another so closely that it would seem to be evident that they originally sprang from the same source. Words to express any numerals higher than three are not found in the dialect of either tribe.

THE mareograph in the harbour of Pola, according to Lieut. Gratzl (*Met. Zeitsch.*), often shows, in addition to the ordinary tidal curve, certain more or less regular oscillations, generally with a period of about 15 minutes (some with one of 7 minutes). These appear to be of the nature of *seiches*, and to be caused by squalls, which drive water from the open sea into the partly inclosed basin of the harbour, where it rises as a wave, retires, rises again to a less height (as only part of the surplus water escapes), and so on. Thus, in the evening of July 6, 1890, after a stiff west-north-west squall, there were eight pronounced oscillations, the strongest showing about 1·4 inch difference of level in 16 minutes. In another case, the harbour level rose higher than it had done for 15 years. The latter squall (a strong south-west one) affected also the Trieste mareograph, which showed nine wide oscillations with a mean period of 1 hour 46 minutes. Lieut. Gratzl suggests observations as to whether sudden impulses of "bora" against the Italian coast might not heap up the water there, so that a return wave might affect the Austrian mareographs; also whether certain sudden currents which injure fishermen's nets in the Dalmatian canals may not be connected with those waves.

A CAT born with only two legs (the fore-legs being absent from the shoulder-blades) has been recently described by Prof. Leon of Jassy (*Naturw. Rundsch.*). It is healthy, and goes about easily, the body in normal position. When startled, or watching anything, it raises itself to the attitude of a kangaroo, using the tail as a support. This animal has twice borne kittens; in both cases two, one of which had four feet, the other only two.

WE learn from Dr. Woeikof's notes of a journey in the Caucasus published in the Russian *Izvestia*, that the Russian Ministry of Ways and Communications has issued a very interesting work on the snow-slips of the Kazbek glaciers, accompanied by an atlas of maps and plans. Careful measurements of the variations in the position of the lowest end of the Devdorak glacier since 1878 have been made, and the results are given in the atlas. A house has been recently built close to the glacier, and it is connected by a road (available for horses) with the villages beneath. An experienced guide, who is bound to accompany the men of science and tourists who may intend to visit the glacier, stays in the house.

A KIND of artificial honey which has lately been produced seems likely to become a formidable rival of natural honey. It is called "sugar honey," and consists of water, sugar, a small proportion of mineral salts, and a free acid; and the taste and smell resemble those of the genuine article. Herr T. Weigle brought the subject before a recent meeting of the Bavarian Association of the Representatives of Applied Chemistry, and there is a paragraph about it in the current number of the *Board of Trade Journal*.

RATS at Aden appear to have a vigorous appetite, and to adopt remarkable ways of gratifying it. Captain R. Light, writing on the subject from Aden to the Journal of the Bombay Natural History Society, says the rats in his house—which is overrun with them—demolish skins, braces, whips, &c.; and one night he awoke, feeling a rat gnawing at his toes. This happened in spite of a dog (a good ratter) being in the room. Captain Light was lately watching his pony being shod, and noticed the hoof apparently cut away all round the coronet, wherever it was soft. He accused the "nalband" of doing this in addition to the usual rasping of the hoof to suit the shoe. The "syce" said that the rats had done it, and that they came at night and ate away not only the pony's hoofs but those of the goat and kid, and that these animals were greatly tormented by the rats. Captain Light examined the hoofs, and found beyond doubt that such was the case, the marks of the teeth being plain; moreover, he found that the horns of the kid, which had been about half an inch high, were eaten flush with the head. Next morning, too, a large rat was discovered in the bedding under the horse. It had evidently been killed by a kick from him.

TWO new methods of preparing azoimide, N_3H, the hydride of nitrogen isolated last year by Prof. Curtius, of Kiel, have been discovered. As announced at the time in NATURE (vol. xliii. p. 21), Prof. Curtius prepared this remarkable compound by reacting with his previously isolated hydrazine hydrate, $N_2H_4.H_2O$, upon hippuric acid, converting the hydrazine derivative thus obtained into its nitroso-derivative, and decomposing an alkaline solution of the latter with sulphuric acid. An aqueous solution of azoimide was obtained upon distilling the product of the latter operation. In order to obtain the free compound itself, the silver salt was prepared by allowing the distillate to flow into a solution of silver nitrate, and the precipitated silver salt, after drying, was decomposed with sulphuric acid. In a subsequent communication (comp. NATURE, vol. xliii. p. 378), Prof. Curtius, in conjunction with Dr. Radenhausen, showed that the pure compound was a very volatile liquid, boiling at 37°, and of fearfully explosive properties. In the current number of the *Berichte*, Drs. Noelting and Grandmougin, of Mülhausen, publish a preliminary note, in which they describe a new, and from the point of view of its constitution most important, method of preparing the liquid. The phenyl ester of azoimide is the diazobenzene imide of Griess, $C_6H_5{-}N\left\langle\begin{array}{l}N\\ \| \\ N\end{array}\right.$, just as chlorbenzene is the phenyl ester of hydrochloric acid. In view of the great stability of the esters of aromatic radicles, it was hardly to be expected that diazobenzene imide wou'd yield azoimide upon saponification. But Drs. Noelting and Grandmougin considered that it might be possible to obtain the latter by decomposing a nitro-derivative of diazobenzene imide by means of alkalies, inasmuch as the introduction of nitro-groups generally effects a considerable increase in the mobility of the acid radicle, rendering its removal by processes of saponification much less difficult. They therefore prepared the dinitro-derivative of diazobenzene imide from dinitro-aniline by means of the usual diazo-reaction—conversion into the perbromide, and treating with ammonia. When treated with alcoholic potash, this dinitro-diazobenzene imide readily decomposes into the potassium salt of dinitro-phenol and azoimide. Upon acidifying the product of the reaction and subjecting it to distillation, an aqueous solution of azoimide passes over, which may be converted into the anhydrous liquid by the method described by Prof. Curtius. The properties of the anhydrous azoimide obtained by this new method agree completely with those detailed by Prof. Curtius.

THE second new method of preparing azoimide was communicated by Dr. Thiele, of Halle, at the Versammlung deutscher Naturforscher und Aerzte, held in that city in September last. In the course of an investigation of the compounds of guanidine, nitro guanidine was obtained, $C\begin{array}{l}\diagup NH_2\\ = N{-}NO_2\\ \diagdown NH_2\end{array}$. Upon treating this compound with acetic acid and zinc dust, it is reduced to amido-guanidine, a substance which forms well-crystallized salts. By boiling the latter with soda, decomposition ensues, with formation of free hydrazine, N_2H_4, which may be very conveniently prepared by this method. Upon subjecting the nitrate to the diazo-reaction, the diazo-nitrate of guanidine is obtained, $C\begin{array}{l}\diagup NH_2\\ = N{-}N{=}N{-}NO_2\\ \diagdown NH\end{array}$. This compound readily breaks up on warming into two compounds, one of which is azoimide, and the other a complex acid of the composition CN_5H_3, and the curious constitution $\begin{array}{lll}C & = N & - N\\ | & \;| & \;\; \| \\ NH_2 & NH & - N\end{array}$. The azoimide may be obtained by distillation in a manner similar to that described above.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mr. W. Harrow; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Cotton; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Trelawny; a Gannet (*Sula bassana*), British, presented by Mr. J. Hitchman; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. F. C. Adams; ten Smooth Snakes (*Coronella lævis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

DISTRIBUTION OF LUNAR HEAT.—Mr. Frank H. Very's essay on the distribution of the moon's heat and its variation with the phase, which gained the prize of the Utrecht Society of Arts and Sciences in 1890, has recently been published. A bolometer in connection with a very sensitive galvanometer was used in the research, and the plan has been to project an image of the moon about 3 centimetres in diameter by a concave mirror; and to measure, not the heat from the whole of this, but only that in a limited part of it, from $\frac{1}{25}$ to $\frac{1}{30}$ of the area of the disk, the observations being repeated at different points and at different phases. Measures made six hours after full moon show that the east limb was hotter than the west limb in the proportion of 92·2 to 88·9. In one observation, made a day after full moon, the excess of heat at the east limb was much larger. There is a regular decrement of heat in passing from higher to lower latitudes, and observations on this point appear to indicate that heat is accumulated after many days of continuous sunshine. The heat in the circumferential zone of the full moon differs from that of the centre by about 20 per cent. In this respect, therefore, the thermal image is like the visual one. There seems to be some evidence that bright regions radiate a little more than dark during the middle of the lunar day; but this is not quite proved, and with a low altitude of the sun the effect is reversed. A comparison of the curve drawn by Zollner for the moon's light with that deduced from Mr. Very's observations brings out the point that visible rays form a much larger proportion of the total radiation at the full than at the partial phases, the maximum for light being much more pronounced than that for the heat. The diminution of the heat from the full to the third quarter is shown to be slower than its increase from the first quarter to the full. This result agrees with that obtained at Lord Rosse's Observatory, and is direct evidence of the storage of heat by lunar rocks.

GEOLOGICAL SOCIETY OF AMERICA.

THE Geological Society of America met at Washington on August 24 and 25. Owing to the death of the President, Prof. Alexander Winchell, Vice-President Gilbert took the chair.

The meeting was opened with an address on the late President by his brother, Prof. N. H. Winchell. Alexander Winchell was born on December 31, 1824, in Dutchess County, N.Y., and died at Ann Arbor on February 19 last. His work was many-sided. He had studied to be a civil engineer; had a strong leaning towards theology. He also read medicine and was a fine mathematician. He loved music, wrote poetry, and modelled in clay and plaster. As a financial resource he became a teacher, and was very successful. He became famous by his arguments on "The Bible History of the Creation," and published in the *Christian Advocate* "Adamites and Pre-Adamites," an exposition of Scriptural and scientific harmony. For four years he lectured on geology at Vanderbilt University. During his long connection with the University of Michigan he wrote many scientific articles of a popular nature, and did a great deal to popularize geological science. The speaker spoke eloquently of his dead brother's long and splendid connection with the Ann Arbor University. His death was most touchingly described. Oddly enough the last words he uttered in public were these: "When I speak to you again it will be of the inhabitants of another world." He had just finished his weekly lecture, and referred in his closing sentence to the subsequent lecture that was never delivered. He discovered many new geological species, and many other geologists testified their admiration for him by naming after him species they discovered. His great work for the Geological Society was touched on, and the speaker expressed his conviction that the next generation would keenly feel the beneficent influence of his brother's work. At the conclusion of the memorial Prof. Edward Orton, Dr. C. A. White, and Mr. C. R. Van Hise were appointed a committee to draft resolutions expressive of the Society's regret at the death of its President.

Prof. Dr. Gustav Steinmann, of the University of Freiburg, Germany, read the first paper, which consisted of the description of a geological map of South America. A large copy of the map was hung up beside the platform, and small replicas were distributed among the audience. Dr. Steinmann, who is a young, bearded, spectacled, typical German student, was sent to

South America by the Strasburg University some ten years ago, and spent some two years making a most thorough research in the geology of the continent, the tangible result being the remarkably complete map exhibited. His researches in South America prove that there is a most remarkable similarity between the geology of the two Americas, and especially between the geology of the southern United States and the southern continent.

The second paper was by Dr. August Rothpletz, of the University of Munich, Germany, on the Permian, Triassic, and Jurassic formations in the East Indian Archipelago. The doctor's paper was devoted to the description of some Mesozoic and Palæozoic fossils collected in two of the Indian islands by his friend Dr. Wichmann, during a geological exploration of the islands. Dr. Wichmann being geologist of the University of Utrecht, Holland, the collections were of particular value, and Dr. Rothpletz's description and classification of them, to which he devoted his paper, was thorough and minute. He took occasion to ridicule some of the classifications of fossils which put them in one category when found in one place and in another when found somewhere else.

"Thermometamorphism in Igneous Rocks" was the title of the next paper presented. It was by Mr. Alfred Harker, of St. John's College, Cambridge, England, and dealt with the effects of high volcanic temperatures in the formation of rocks. He described the results of his researches in the lake region of England, where the volcanic forces of nature were particularly well marked.

Prof. Alexis Pavlow of the University of Moscow, Russia, presented a paper entitled "Sur les Couches Marines terminant le Jurassique et commençant le Crétace, et sur l'Histoire de leur Faune."

Another paper, also in French, presented by Prof. Max Lohest, of the University of Liége, Belgium, was entitled "Sur l'Homme contemporain du Mammouth en Belgique." The contemporaneous existence of man was supported by proofs additional to those heretofore given.

Baron Gerald de Geer, of Sweden, gave an interesting account of recent changes of level along the sea-board of the Scandinavian peninsula.

The most important new matter presented was a paper on "Fossil Fishes of the Lower Silurian Rocks of Colorado," by Mr. C. D. Walcott, of the United States Geological Survey. The discovery of the fossil fish remains is of recent date, and attracts great attention among zoologists and geologists from its carrying back into the past, over a great time interval, our knowledge of vertebrate life. They are the oldest vertebrate remains known, and appear to be the ancestral types of the great ichthyic fauna of the classic "old red sandstone" of Europe, and the Devonian group of America.

In the discussion, Profs. Von Zittel, Jaekel, and F. Schmidt compared the fish remains exhibited with those of the Devonian, and stated that the Upper Silurian types were not represented in the fauna.

Second Day.—From the committee appointed to draft appropriate resolutions relative to the death of Dr. Alexander Winchell, the President of the Society, Prof. Orton made a report which was adopted. The resolutions reported paid a just and touching tribute to the character of the deceased, and fittingly acknowledged the great services which he had rendered to the science in the course of the forty years of arduous and unremitting toil which he had devoted to its investigation. To his writings and lectures were attributed in a great degree the growing liberality and enlargement of thought of the more serious-minded portion of the community in regard to the theory of organic evolution as presented by Darwin and his successors. Dr. Winchell, the report affirmed, stated and defended with marked ability and courage and persuasive power this the most characteristic and far-reaching doctrine of modern geological science. "The first enunciation of this doctrine," the report stated, "was sure to awaken distrust and even bitter hostility among a large class of people because of its apparent incompatibility with some of their most fundamental convictions and beliefs. To disregard the sincere apprehension of this great class, comprising, as it does, so much of the moral and intellectual force of the body politic, would be heartless. To mock at its fears, ill founded though they were, would be worse. What worthier service to science and the community than to disarm hostility by showing that the evolutionary philosophy, so far from degrading and dishonouring man, makes him in a peculiar sense the head and crown of the creation?"

In seconding the resolutions Dr. C. A. White paid a warm tribute to Dr. Winchell, with whom he had been on terms of intimacy for many years. As a further mark of respect the resolutions were adopted by a rising vote.

The first paper presented was by Dr. Frederick Schmidt, of St. Petersburg, Russia.

Prof. Gregoire Stefanescu, of the University of Bucharest, Roumania, presented "Sur l'Existence du Dinotherium en Roumanie," the next paper. The Professor read it in French, illustrating it by drawings on the blackboard, and after he had finished, Prof. Dr. Charles Barrois read it over again in English, so that those who did not know French might not lose it. Though quite short the paper was very interesting. It briefly described a large number of bones of the Dinotherium found widely distributed over Roumania, which indisputably pointed to the existence of this almost unknown extinct animal in that land countless years ago. This was probably the largest mammal that ever inhabited the earth, its epoch being the Tertiary period. It had enormous tusks, that curved downward and backward in such a way that it could only hurt itself with them, and probably had a massive trunk. In character it more nearly resembled the elephant and rhinoceros of modern ages than any other known animal.

Prof. A. N. Krassnof, of the Charkow University, Russia, read the next paper on the black earth of the steppes of Southern Russia, its origin, distribution, and points of resemblance with the soils of the prairies of America. The paper traced the resemblance between the Russian steppes and the American prairies to their similar origin in the layers of the vegetables of years. Their remarkable fertility was touched on generally, and a technical account of the origin of the two plains was given.

TECHNICAL CHEMISTRY.

In his Cantor Lectures on Photographic Chemistry, delivered last spring before the Society of Arts, and just issued by the Society in a separate form, Prof. Meldola opens with some remarks on the special position of technical training in chemistry, which should be carefully conisdered in connection with the present widespread movement in the direction of technical education throughout the country. He says:—

"There are many who identify technical instruction with the teaching of some handicraft, a notion which has no doubt arisen from the identification of technical skill with manual dexterity in some mechanical industry. By the adoption, either tacitly or openly, of this narrow definition, the chemical industries have suffered to a very large extent in this country, because their progress is more dependent on a knowledge of scientific principles, and much less dependent on manual dexterity than any of the other subjects dealt with in schemes of technical instruction. Now, in order to give technical instruction in a subject like photography, which is so intimately connected with chemistry, we may adopt one of two courses. The student may become a practical photographer in the first place, and may then be led on to the science of his practice by an appeal to the purely chemical principles brought into operation. This may be called the analytical method. The other method is to give the student a training in general chemistry first, and then to specialize his knowledge in the direction of photography. This may be regarded as a synthetical method.

"In other departments of technology, and especially in those where the underlying principles are of a mechanical nature, the analytical method may be, and has been, adopted with success. It is possible to lead an intelligent mechanic from his every-day occupations to a knowledge of the higher principles of mechanical science by making use of his experience of phenomena which are constantly coming under his notice. From this it is sometimes argued by those who are in the habit of regarding technical instruction from its purely analytical side, that technical chemistry can be taught by the same method. Some teachers may possibly succeed in this process, but my own experience, both as a technologist and a teacher, has led me to the conclusion that, for chemical subjects, the analytical method is both too cumbersome and circuitous to be of any real practical use. No person engaged in chemical industry in any capacity—whether workman, foreman, manager, or proprietor—can be taught the principles of chemical science out of his own industry, unless he has some considerable knowledge of general principles to start with. No person who is not grounded in such broad principles can properly appreciate the explanation of the phenomena with which his daily experience brings him into contact, and if his previous training is insufficient to enable him to understand the nature of the changes which occur in the course of his operations, he cannot derive any advantage from technical instruction. These remarks will, I hope, serve to emphasize a distinction which exists between technical chemistry and other technical subjects, and I have thought it desirable to avail myself of the present opportunity of calling particular attention to this point, because it is one which is generally ignored in all discussions on technical education.

"The reason for this difference in the mode of treatment of chemical subjects is not difficult to find. The chemical technologist—the man who is engaged in the manufacture of useful products out of certain raw materials—is, so far as the purely scientific principles are concerned, already at a very advanced stage, although he may not realize this to be the case. The chemistry of manufacturing operations, even when these are of an apparently simple kind, is of a very high order of complexity. There are many branches of chemical industry in which the nature of the chemical changes undergone by the materials is very imperfectly understood; there is no branch of chemical industry of which the pure science can be said to be thoroughly known. For these reasons I believe that I am justified in stating that the chemical technologist is working at a high level, so far as the science of his subject is concerned, and this explains why he cannot be dealt with by the analytical method.

"The general considerations which have been offered apply to the special subject of photography with full force. A person may become an adept as an operator without knowing anything of physics or chemistry; there are thousands of photographers all over the country who can manipulate a camera and develop and print pictures with admirable dexterity, who are in this position. If we adopt the narrow definition of technical instruction, we should appoint such experts in our Colleges, and through them impart the art of taking pictures to thousands of others. But would our position as a photographing nation be improved by this process? I venture to think not. We might be carrying out the ideas of certain technical educators by adopting this method, but I do not imagine that in the long run the subject itself would be much advanced; our position in the scale of industry would not be materially raised by the wholesale manufacture of skilful operators. And so with all other branches of applied chemistry; it is technologists whose knowledge is based on a broad foundation that are wanted for the improvement of our industries. These are the men who are raised in the technical high schools of the Continent, and whose training the Continental industries have had the wisdom to avail themselves of."

AN ASTRONOMER'S WORK IN A MODERN OBSERVATORY.[1]

THE work of astronomical observatories has been divided into two classes, viz. astrometry and astrophysics. The first of these relates to astronomy of precision, that is to the determination of the positions of celestial objects; the second relates to the study of their physical features and chemical constitution.

Some years ago the aims and objects of these two classes of observatories might have been considered perfectly distinct, and, in fact, were so considered. But I hope to show that in more recent years their objects and their processes have become so interlaced that they cannot with advantage be divided, and a fully equipped modern observatory must be understood to include the work both of *astrometry* and *astrophysics*.

In any such observatory the principal and the fundamental instrument is the transit circle. It is upon the position in the heavens of celestial objects, as determined with this instrument or with kindred instruments, that the whole fair superstructure of exact astronomy rests; that is to say, all that we find of information and prediction in our nautical almanacs, all that we know of the past and can predict of the future motions of the celestial bodies.

[1] Friday Evening Discourse delivered at the Royal Institution by Dr. David Gill, F.R.S., Her Majesty's Astronomer at the Cape of Good Hope, on May 29, 1891.

Here is a very small and imperfect model, but it will serve to render intelligible the photograph of the actual instrument which will be subsequently projected on the screen. [Here the lecturer described the adjustments and mode of using a transit circle.]

We are now in a position to understand photographs of the instrument itself. But first of all as to the house in which it dwells. Here, now on the screen, is the outside of the main building of the Royal Observatory, Cape of Good Hope. I select it simply because, being the observatory which it is my privilege to direct, it is the one of which I can most easily procure a series of photographs. It was built during the years 1824-28, and like all the observatories built about that time, and like too many built since, it is a very fair type of most of the things which an observatory should not be. It is, as you see, an admirably solid and substantial structure, innocent of any architectural charm, and so far as it affords an excellent dwelling-place, good library accommodation, and good rooms for computers, no fault can be found with it. But these very qualities render it undesirable as an observatory. An essential matter for a perfect observatory should be the posssibility to equalize the internal and the external temperature. The site of an instrument should also be free from the immediate surroundings of chimneys or other origin of ascending currents of heated air. Both these conditions are incompatible with thick walls of masonry and the chimneys of attached dwelling houses, and therefore, as far as possible, I have removed the instruments to small detached houses of their own. But the transit circle still remains in the main building, for, as will be evident to you, it is no easy matter to transport such an instrument.

The two first photographs show the instrument, in one case pointed nearly horizontally to the north, the other pointed nearly vertical. Neither can show all parts of the instrument, but you can see the massive stone piers, weighing many tons each, which, resting on the solid blocks 10 feet below, support the pivots. Here are the counter-weights which remove a great part of the weight of the instrument from the pivots, leaving only a residual pressure sufficient to enable the pivots to preserve the motion of the instrument in its proper plane. Here are the microscopes by which the circle is read. Here the opening through which the instrument views the meridian sky. The observer's chair is shown in this diagram. His work appears to be very simple, and so it is, but it requires special natural gifts, patience and devotion, and a high sense of the importance of his work to make a first-rate meridian observer. Nothing apparently more monotonous can be well imagined if a man is "not to the manner born."

Having directed his instrument by means of the setting circle to the required altitude, he clamps it there and waits for the star which he is about to observe to enter the field. This is what he sees. [Artificial transit of a star by lantern.]

As the star enters the field it passes wire after wire, and as it passes each wire he presses the key of his chronograph and records thé instant automatically. As the star passes the middle wire he bisects it with the horizontal web, and again similarly records on his chronograph the transit of the star over the remaining webs. Then he reads off the microscopes by which the circle is read, and also the barometer and thermometer, in order afterwards to be able to calculate accurately the effect of atmospheric refraction on the observed altitude of the star; and then his observation is finished. Thus the work of the meridian observer goes on, star after star, hour after hour, and night after night; and, as you see, it differs very widely from the popular notion of an astronomer's occupation. It presents no dreamy contemplation, no watching for new stars, no unexpected or startling phenomena. On the contrary, there is beside him the carefully prepared observing-list for the night, the previously calculated circle setting for each star, allowing just sufficient time for the new setting for the real star after the readings of the circle for the previous observation.

After four or five hours of this work, the observers have had enough of it; they have, perhaps, observed fifty or sixty stars, they determine certain instrumental errors, and betake themselves to bed, tired, but (if they are of the right stuff) happy and contented men. At the Cape we employ two observers—one to read the circle, and one to record the transit. Four observers are employed, and they are thus on duty each alternate night. Such is the work that an outsider would see were he to enter a working meridian observatory at night, but he would find out, if he came next morning, that the work was by no means over. By far the largest part has yet to follow. An observation that requires only two or three minutes to make at night, requires at least half an hour for its reduction by day. Each observation is affected by a number of errors, and these have to be determined and allowed for. Although solidly founded on massive piers resting on the solid rock, the constancy of the instrument's position cannot be relied upon. It goes through small periodic changes in level, in collimation, and in azimuth, which have to be determined by proper means, and the corresponding corrections have to be computed and applied; and, also, there are other corrections for refraction, &c., which involve computation and have to be applied. But these matters would fall more properly under the head of a special lecture upon the transit instrument. I mention them now, merely to explain why so great a part of an astronomer's work comes in the daytime, and to dispel the notion that his work belongs only to the night.

One might very well occupy a special lecture in an account of the peculiarities of what is called personal equation—that is to say, the different time which elapses for different observers between the time when the observer believes the star to be upon the wire, and the time when the finger responds to the message which the eye has conveyed to the brain. Some observers always press the key too soon; some always too late. Some years ago I discovered, from observations to which I will subsequently refer, that *all* observers press the chronograph key either too soon for bright stars or too late for faint ones.

Other errors may, and I am sure do, arise both at Greenwich and the Cape, from the impossibility of securing uniformity of outside and inside temperature in a building of strong masonry. The ideal observatory should be solid as possible as to its foundations, but light as possible as to its roof and walls—say, a light framework of iron covered with canvas. But it would be undesirable to cover a valuable and permanent instrument in this way.

But here is a form of observatory which realizes all that is required, and which is eminently suited for permanent use. The walls are of sheet iron, which readily acquire the temperature of the outer air. The iron walls are protected from direct sunshine by wooden louvres, and small doors in the iron walls admit a free circulation of air. The revolving roof is a light framework of iron covered with well-painted *papier-maché*.

The photograph now on the screen shows the interior of the observatory, and this brings me to the description of observations of an entirely different class. In this observatory the roof turns round on wheels, so that any part of the sky can be viewed from the telescope. This is so, because the instrument in this observatory is intended for purposes which are entirely different from those of a transit circle. The transit circle, as we have seen, is used to determine the *absolute* positions of the heavenly bodies; the heliometer, to determine with greater precision than is possible by the absolute method the *relative* positions of celestial objects.

To explain my meaning as to absolute and relative positions. It would, for example, be a matter of very little importance if the absolute latitude of a point on the Royal Exchange or the Bank of England were one-tenth of a second of arc (or 10 feet) wrong in the maps of the Ordnance Survey of England—that would constitute a small *absolute* error common to all the buildings on the same map of a part of the city, and common to all the adjoining maps also. Such an error, regarded as an *absolute* error, would evidently be of no importance if every point on the map had the same absolute error. There is no one who can say at the present moment whether the absolute latitude of the Royal Exchange—nay, even of the Royal Observatory, Greenwich—is known to 10 feet. But it would be a very serious thing indeed if the relative positions on the same map were 10 feet wrong *here* and *there*. For example, if of two points marking a frontage boundary on Cornhill, one were correct, the other 10 feet in error, what a nice fuss there would be! what food for lawyers! what a bad time for the Ordnance Survey Office! Well, it is just the same in astronomy.

We do not know, we probably never shall know with certainty, the absolute places of even the principal stars to one-tenth of a second of arc. But one-tenth of a second of arc in the measure of some relative position would be fatal. For example, in the measurement of the sun's parallax an error of one-tenth of a second of arc means an error of 1,000,000 miles, in round numbers, in the sun's distance; and it is only when we can be quite certain of our measures of much smaller quantities than one-tenth of a second of arc, that we are in a position to begin seriously the determination of such a problem as that of the distances of the fixed stars. For these problems we must use differential measures—that is, measures of the relative positions of two objects. The most perfect instrument for such purposes is the heliometer.

Lord McLaren has kindly sent from Edinburgh, for the purposes of this lecture, the parts of his heliometer which are necessary to illustrate the principles of the instrument.

This instrument is the same which I used on Lord Crawford's expedition to Mauritius in 1874. It was also kindly lent to me by Lord Crawford for an expedition to the Island of Ascension to observe the opposition of Mars in 1877. In 1879, when I went to the Cape, I acquired the instrument from Lord Crawford, and carried out certain researches with it on the distances of the fixed stars.

In 1887, when the Admiralty provided the new heliometer for the Cape Observatory, this instrument again changed hands. It became the property of Lord McLaren. I felt rather disloyal in parting with so old a friend. We had spent so many happy hours together, we had shared a good many anxieties together, *and we knew each other's weaknesses so well.* But my old friend has fallen into good hands, and has found another sphere of work.

The principle of the instrument is as follows. [The instrument was here explained.]

There is now on the screen a picture of the new heliometer of the Cape Observatory, which was mounted in 1887, and has been in constant use ever since. It is an instrument of the most refined modern construction, and is probably the finest apparatus for refined measurement of celestial angles in the world.

[Here were explained the various parts of the instrument in relation to the model, and the actual processes of observation were illustrated by the images of artificial stars projected on a screen.]

Here, again, there is little that conforms to the popular idea of an astronomer's work; there is no searching for objects, no contemplative watching, nothing sensational of any kind. On the contrary, every detail of his work has been previously arranged and calculated beforehand, and the prospect that lies before him in his night's work is simply more or less of a struggle with the difficulties which are created by the agitation of the star images, caused by irregularities in the atmospheric refraction. It is not upon one night in a hundred that the images of stars are perfectly tranquil. You have the same effect in an exaggerated way when looking across a bog on a hot day. Thus, generally, as the images are approached, they appear to cross and recross each other, and the observer must either seize a moment of comparative tranquillity to make his definitive bisection, or he may arrive at it by gradual approximations till he finds that the vibrating images of the two stars seem to pass each other as often to one side as to the other. So soon as such a bisection has been made, the time is recorded on the chronograph, then the scales are pointed on and printed off, and so the work goes on, varied only by reversals of the segments and of the position circle. Generally, I now arrange for thirty-two such bisections, and these occupy about an hour and a half. By that time one has had about enough of it, the nerves are somewhat tired, so are the muscles of the back of the neck; and if the observer is wise, and wishes to do his best work, he goes to bed early and gets up again at two or three o'clock in the morning, and goes through a similar piece of work. In fact, this must be his regular routine night after night, whenever the weather is clear, if he is engaged, as I have been, on a large programme of work on the parallaxes of the fixed stars, or on observations to determine the distance of the sun by observations of minor planets.

I will not speak now of these researches, because they are still in process of execution or of reduction. I would rather, in the first place, endeavour to complete the picture of a night's work in a modern observatory.

We pass on to celestial photography, where astrometry and astrophysics join hands. Here on the screen is the interior of one of the new photographic observatories, that at Paris. [Brief description.]

Here is the exterior of our new photographic observatory at the Cape. Here is the interior of it, and the instrument. [Brief description.]

The observer's work during the exposure is simply to direct the telescope to the required part of the sky, and then the clockwork *nearly* does the rest—but not quite so. The observer holds in his hand a little electrical switch with two keys; by pressing one key he can accelerate the velocity of the driving-screw by about 1 per cent., and by pressing the other he can retard it 1 per cent. In this way he keeps one of the stars in the field always perfectly bisected by the cross-wires of his guiding telescope, and thus corrects the small errors produced partly by changes of refraction, partly by minute unavoidable errors in cutting the teeth of the arc into which the screw of the driving-shaft of the clockwork gears.

The work is monotonous rather than fatiguing, and the companionship of a pipe or cigar is very helpful during long exposures. A man can go on for a watch of four or five hours very well, taking plate after plate, exposing each, it may be, forty minutes or an hour. If the night is fine, a second observer follows the first, and so the work goes on the greater part of the night. Next day he develops his plate, and gets something like this. [Star-cluster.]

Working just in this way, but with the more humble apparatus which you see imperfectly in the picture now on the screen, we have photographed at the Cape during the past six years the whole of the southern hemisphere from 20° of south declination to the South Pole.

The plates are being measured by Prof. Kapteyn, of Groningen, and I expect that in the course of a year the whole work, containing all the stars to 9½ magnitude (between 200,000 and 300,000 stars) in that region, will be ready for publication. This work is essential as a preliminary step for the execution in the southern hemisphere of the great work inaugurated by the Astrophotographic Congress at Paris in 1887, the last details of which were settled at our meeting at Paris in April last. What we shall do with the new apparatus, perhaps I may have the honour to describe to you some years hence, after the work has been done.

We now come to an important class of astronomical work, more purely astrophysical, for the illustration of which I can no longer appeal to the Cape, because I regret to say that we are not yet provided with the means for its prosecution. I refer to the use of the spectroscope in astronomy, and especially to the latest developments of its use for the accurate measurement of the velocity of the motions of stars in the line of sight.[1]

It is beyond the province of this lecture to enter into history, but it is impossible not to refer to the fact that the chief impulse to astronomical work in this direction was given by Dr. Huggins, our Chairman to-night—nay, more, except for the early contributions of Fraunhofer to the subject, Dr. Huggins certainly is the father of sidereal spectroscopy, and that not in one but in every branch of it. He has devised the means, pointed the way, and, whilst in many branches of the work he still continues to lead the way, he has of necessity left the development of other branches to other hands.

From an astronomer's point of view the most important advance that has been made in spectroscopy of recent years is the sudden development of precision in the measures of star motion in the line of sight. The method remained for fifteen or sixteen years quite undeveloped from the condition in which it left the hands of Dr. Huggins, and certainly no progress in the accuracy attained by Dr. Huggins was made till the matter was taken up Dr. Vogel at Potsdam. At a single step Dr. Vogel has raised the precision of the work from that of observations in the days of Ptolemy to that of the days of Bradley—from the days of the old sights and pinnules to the days of telescopes. Therefore I take a Potsdam observation as the best type of a modern spectroscopic observation for description, especially as I have recently visited Dr. Vogel at Potsdam, and he has kindly given me a photograph of his spectroscope, as well as of some of the work done with it.

A photograph of the Potsdam spectroscope attached to the equatorial is now on the screen. [Description.]

The method of observation consists simply in inserting a small photographic plate in the dark slide, directing the telescope to the star, and keeping the image of the star continuously on the slit during an exposure of about an hour; and this is what is obtained on development of the picture.

If the star remained perfectly at rest between the jaws of the slit the spectrum would be represented by a single thread of light, and of course no lines would be visible upon such a thread; but the observer intentionally causes the star image to travel a little along the slit during the time of exposure, and so a spectrum of sensible width is obtained.

You will remark how beautifully sharp are the faint lines in this spectrum. Those who have tried to observe the spectrum of Sirius in the ordinary way, know that many of these fine lines cannot be seen or measured with certainty. The reason is that on account of irregularities in atmospheric refraction, the image of a star in the telescope is rarely tranquil, sometimes it shines brightly in the centre of the slit, sometimes barely in the slit at all, and the eye becomes puzzled and confused. But the photographic eye is not in the least disturbed; when the star image is in the slit, the plate goes on recording what it sees, and when the star is not in the slit the plate does nothing, and it is of no consequence whatever how rapidly these alternate appearances and disappearances recur. The only difference is that when the air is very steady and the star's image, therefore, always in the slit, the exposure takes less time than when the star is unsteady.

That is one reason why the Potsdam results are so accurate. And there are many other reasons besides, into which I cannot now enter. What, however, it is very important to note is this, that we have here a method which is to a great extent independent of the atmospheric disturbances which in all other departments of astronomical observation have imposed a limit to their precision. Accurate astrospectroscopy, therefore, may be pushed to a degree of perfection which is limited only by the optical aid at our disposal and by the sensibility of our photographic plates.

And now I think we have sufficiently considered the ordinary processes of astronomical observation to illustrate the character of the work of an astronomer at night. The picture should be completed by an account of his work by day; but to go into that matter in detail would certainly not be within the limits of this lecture. It is better that I should in conclusion touch upon some recent remarkable results of these day and night labours. It is these after all that most appeal to you; it is for these that the astronomer labours; it is the prospect of them that lightens the long watches of the night and gives life to the otherwise dead bones of mechanical routine.

Let us take first some spectroscopic results. To explain their meaning let me remind you for a moment of the familiar analogy between light and sound.

The pitch of a musical note depends on the rapidity of the vibrations communicated to the air by the reed or string of the musical instrument that produces the note, a low note being given by slow vibrations and a high one by quick vibrations.

Just in the same way red light depends on relatively slow vibrations of ether, and blue or violet light on relatively quick vibrations. Well, if there is a railway train rapidly approaching one, and the engine sounds its whistle, more waves of sound from that whistle will reach the ear in a second of time than would reach the ear were the train at rest. On the other hand, if the train is travelling at the same rate *away* from the observer, fewer waves of sound will reach his ears in a second of time. Therefore an observer beside the line should observe a distinct change of pitch in the note of the engine whistle as the train passes him, and as a matter of fact such a change of pitch can be and has been observed.

Just in the same way, if a source of light could be moved rapidly enough towards an observer it would become bluer, or if away from him it would become more red in colour. Only it would require a change of velocity in the moving light of some thousands of miles per second in order to render the difference of colour sensible to the eye. The experiment is, therefore, not likely to be frequently shown at this lecture table!

But the spectroscope enables such changes of colour to be measured with extreme precision. Here on the screen is the most splendid illustration of this that exists at present, viz. copies of three negatives of the spectrum of α Aurigæ, taken at Potsdam in October and December of 1888, and in March 1889.

The black line (the picture being a negative) represents the bright line Hγ given by the artificial light of hydrogen, the strong white line in the picture corresponds to the black absorption line which is due to hydrogen in the atmosphere of the star.

Why is it that the artificial hydrogen line does not correspond

[1] The older methods enabled us to measure motions at right angles to the line of sight, but till the spectroscope came we could not measure motions in the line of sight.

with the stellar line in all these pictures? The answer is, either the star is moving towards or from the earth in the line of sight, or the earth is moving from or towards the star. But in December the earth in its motion round the sun is moving at right angles to the direction of α Aurigæ: why then does not the stellar hydrogen line agree in position with the terrestrial hydrogen line? The simple explanation is that α Aurigæ is moving with respect to the sun.

In what way is it moving? Well, that also is clear; the stellar line is displaced towards the red end of the spectrum—that is to say, the star light is redder than it should be in consequence of a motion of recession; this proves that the star is moving away from us, and measures of the photograph show the rate of this motion to be 15½ miles per second. We also know that in October the earth, in its motion round the sun, is moving towards α Aurigæ nearly at the same rate as we have just seen that α Aurigæ is running away from the sun. Consequently, at that time, their relative motions are nearly insensible, because both are going at the same rate in the same direction, and we find accordingly in October that the positions of the stellar and artificial hydrogen lines perfectly correspond. Finally, in March, the earth, in its motion round the sun, is moving away from α Aurigæ, and as α Aurigæ is also running away from the sun, the star-light becomes so much redder than normal that the stellar hydrogen line is shifted completely to one side of the hydrogen and artificial line.

The accuracy of these results may be proved as follows:—

If we measure all the photographs of α Aurigæ which Dr. Vogel has obtained, we can derive from each a determination of the relative velocity of the motion of the star with respect to our earth.

Of course these velocities are made up of the velocity of motion of α Aurigæ with respect to the sun (which we may reasonably assume to be a uniform velocity) and the velocity of the earth due to its motion round the sun. But the velocity of the earth's motion in its orbit is known with an accuracy of about one five-hundredth part of its amount, and therefore, within that accuracy, we can allow precisely for its effect on the relative velocity of the earth and α Aurigæ. When we have done so we get the following results for the velocity of the motion of α Aurigæ with respect to the sun. You see by the following table how beautifully they agree in the Potsdam results, and how comparatively rough and unreliable are the results obtained by the older method at Greenwich:—

α *Aurigæ—Potsdam.*

Date.	Observed relative motion of earth and star. Miles per sec.	Motion of earth.	Concluded motion. Star relative to the sun.
1888.			
October 22 ...	+ 2·5	− 13·0	+ 15·5
,, 24 ...	+ 3·1	− 12·4	+ 15·5
,, 25 ...	+ 3·1	− 12·4	+ 15·5
,, 28 ...	+ 2·5	− 11·8	+ 14·3
November 9 ...	+ 6·8	− 8·7	+ 15·5
December 1 ...	+ 11·8	− 3·1	+ 14·9
,, 13 ...	+ 14·9	+ 0·6	+ 14·3
1389.			
January 2	+ 20·5	+ 6·8	+ 13·7
February 5 ...	+ 32·9	+ 14·3	+ 18·6
March 6	+ 34·2	+ 16·8	+ 17·4

α *Aurigæ—Greenwich.*

Date.	Observed relative motion of earth and star. Miles per sec.	Motion of earth.	Concluded motion. Star relative to the sun.
1887.			
January 25 ...	+ 16·4	+ 12·6	+· 3·8
February 16 ...	+ 34·4	+ 15·9	+ 18·5
October 22 ...	+ 39·8	− 13·5	+ 52·3
,, 25 ...	+ 25·4	− 13 0	+ 38·4
,, 29 ...	+ 40·6	− 12·1	+ 52·7
1888.			
December 7 ...	+ 29·0	− 1·2	+ 36·2
1889.			
February 15 ...	+ 23·8	+ 16·0	+ 7·8
March 5	+ 20·3	+ 17·1	+ 3 2
September 17 ...	+ 18·6	− 13·3	+ 33·3
,, 19 ...	+ 21·8	− 16·7	+ 38·5
,, 25 ...	+ 24·8	− 16·5	+ 41·3
November 25 ...	+ 24·5	− 4·9	+ 29·4

I believe that in a few years—at least, in a period of time that one may hope to see—we shall not be content merely to correct our results for the motion of the earth in its orbit only, and so test our observations of motion in the line of sight, but that we shall have arrived at a certainty and precision of working which will *permit the process to be reversed*, and that we shall be employing the spectroscope to determine the velocity of the earth's motion in its orbit, or, in other words, to determine the fundamental unit of astronomy, the distance of the sun from the earth.

I will take as another example one recent remarkable spectroscopic discovery.

Miss Maury, in examining a number of photographs of stellar spectra taken at Harvard College, discovered that in the spectrum of β Aurigæ certain lines doubled themselves every two days, becoming single in the intermediate days. Accurate Potsdam observations confirmed the conclusion.

The picture on the screen shows the spectrum of β Aurigæ photographed on November 22 and 25 of last year. In the first the lines are single; in the other every line is doubled. Measures and discussion of a number of these photographs have shown that the doubling of the lines is perfectly accounted for by the supposition of two suns revolving round each other in a period of four days, each moving at a velocity of about 70 miles a second in its orbit.

When one star is approaching us and the other receding, the lines in the spectrum formed by the light of the first star will be moved towards the blue end of the spectrum, those in the spectrum of the second star towards the red end of the spectrum. Then, as the two stars come into the same line with us, their motions become at right angles to the line of sight, and their two spectra, not being affected by motion, will perfectly coincide; but then, after the stars cross, their spectra again separate in the opposite direction, and so they go on.

Thus by means of their spectra we are in a position to watch and to measure the relative motions of two objects that we can never see apart—nay more, we can determine not only their period of revolution, but also the velocity of their motions in their orbits. Now, if we know the time that a body takes to complete its revolution, and the velocity at which it moves, clearly we know the dimensions of its orbit; and if we know the dimensions of an orbit we know what attractive force is necessary to compel the body to keep in that orbit, and thus we are able to weigh these bodies. The components of β Aurigæ are two suns, which revolve about each other in four days; they are only between 7 and 8 millions of miles (or one-twelfth of our distance from the sun) apart, and if they are of equal weight they each weigh rather over double the weight of our sun.

I have little doubt that these facts do not represent a permanent condition, but simply a stage of evolution in the life-history of the system, an earlier stage of which may have been a nebular one.

Other similar double-stars have been discovered both at Potsdam and at Cambridge, U.S., stars that we shall never see separately with the eye aided by the most powerful telescope; but time does not permit me to enter into any account of them.

I pass now to another recent result that is of great cosmical interest.

The Cape photographic star charting of the southern hemisphere has been already referred to. In comparing the existing eye-estimates of magnitude by Dr. Gould with the photographic determinations of these magnitudes, both Prof. Kapteyn and myself have been greatly struck with a very considerable systematic discordance between the two. In the rich parts of the sky—that is, in the Milky Way—the stars are systematically photographically brighter by comparison with the eye-observations than they are in the poorer part of the sky, and that not by any doubtful amount, but by half or three fourths of a magnitude. One of two things was certain—either that the eye-observations were wrong, or that the stars of the Milky Way are bluer or whiter than other stars. But Prof. Pickering, of Cambridge, America, has lately been making a complete photographic review of the heavens, and, by placing a prism in front of the telescope, he has made pictures of the whole sky like this. [Here two examples of the plates of Pickering's spectroscopic *Durchmusterung* were exhibited on the screen.] He has discussed the various types of the spectra of the brighter stars, as thus revealed, according to their distribution in the sky. He finds thus that the stars of the Sirius type occur chiefly in the

Milky Way, whilst stars of other types are fairly divided over the sky.

Now, stars of the Sirius type are very white stars, very rich, relative to other stars, in the rays which act most strongly on a photographic plate. Here, then, is the explanation of the results of our photographic star-charting and of the discordance between the photographic and visual magnitudes in the Milky Way.

The results of the Cape charting further show that it is not alone to the brighter stars that this discordance extends, but it extends also, though in a rather less degree, to the fainter stars of the Milky Way. Therefore, we may come to the very remarkable conclusion that the Milky Way is a thing apart, and that it has been developed perhaps in a different manner, or more probably at a different and probably later epoch, from the rest of the sidereal universe.

Here is another interesting cosmical revelation which we owe to photography.

You all know the beautiful constellation Orion, and many in this theatre have before seen the photograph of the nebula which is now on the screen, taken by Mr. Roberts.

Here is another photograph of the same object, taken with a much longer exposure. You see how over-exposed, in fact burnt out, the brightest part of the picture is, and yet what a wonderful development of faint additional nebulous matter is revealed.

But I do not think that many persons in this room have seen *this* picture, and probably very few have any idea what it represents. It is from the original negative taken by Prof. Pickering, with a small photographic lens of short focus, after six hours' exposure in the clear air of the Andes, 10,000 feet above sea-level.

The field embraces the three well-known stars in the belt of Orion, on the one hand, and β Orionis (Rigel) on the other. You can hardly recognize these great white patches as stars; their ill-defined character is simply the result of excessive over-exposure. But mark the wonders which this long exposure with a lens of high intrinsic brilliancy of image has revealed. Here is the great nebula, of course terribly over-exposed; but note its wonderful fainter ramifications. See how the whole area is more or less nebulous, and surrounded as it were with a ring fence of nebulous matter. This nebulosity shows a special concentration about β Orionis.

Well, when Prof. Pickering got this wonderful picture, knowing that I was occupied with investigations on the distances of the fixed stars, he wrote to ask whether I had made any observations to determine the distance of β Orionis, as it would be of great interest to know, from independent evidence, whether this very bright star was really near to us or not. It so happens that the observations were made, and their definitive reduction has shown that β Orionis is really at the same distance from us as are the faint comparison stars. β Orionis is, therefore, probably part and parcel of an enormous system in an advanced but incomplete state of stellar evolution, and that what we have seen in this wonderful picture is all a part of that system.

I should explain what I mean by an elementary or by an advanced state of stellar evolution. There is but one theory of celestial evolution which has so far survived the test of time and comparison with observed facts, viz. the nebular hypothesis of Laplace. Laplace supposed that the sun was originally a huge gaseous or nebulous mass, of a diameter far greater than the orbit of Neptune. I say *originally*—do not misunderstand me. We have finite minds; we can imagine a condition of things which might be supposed to occur at any particular instant of time however remote, and at any particular distance of space however great, and we may frame a theory beginning at another time still more remote, and so on. But we can never imagine a theory beginning at an infinite distance of time or at an infinitely distant point in space. Thus, in any theory which man with his finite mind can devise, when we talk of *originally* we simply mean at or during the time considered in our theory.

Now, Laplace's theory begins at a time, millions on millions of years ago, when the sun had so far disentangled itself from chaos, and its component gaseous particles had by mutual attraction so far coalesced, as to form an enormous gaseous ball, far greater in diameter than the orbit of the remotest planet of our present system. The central part of this ball was certainly much more condensed than the rest, and the whole ball revolved. There is nothing improbable in this hypothesis. If gaseous matter came together from different parts of space, such coalition would unquestionably occur, and as in the meeting of opposite streams of water or of opposite currents of wind, vortices would be created, and revolution about an axis set up, such as we are familiar with in the case of whirlpools or cyclones. The resultant would be rotation of the whole globular gaseous mass about an axis.

Now this gaseous globe begins to cool, and as it cools it necessarily contracts. Then follows a necessary result of contraction, viz the rotation becomes more rapid. This is a well-known fact in dynamics, about which there is no doubt. Thus, the cooling and the contracting go on, and, simultaneously, the velocity of rotation becomes greater and greater. At last the time arrives when, for the outside particles, the velocity of rotation becomes such that the centrifugal force is greater than the attractive force, and so the outside particles break off and form a ring. Then, as the process of cooling and contraction proceeds still further, another ring is formed, and so on, till we have, finally, a succession of rings and a condensed central ball. If from any cause the cooling of any of these rings does not go on uniformly, or if some of the gaseous matter of the ring is more easily liquefied than others, then probably a single nucleus of liquid matter will be formed in that ring, and this nucleus will finally, by attraction, absorb the whole of the matter of which the ring is composed—at first as a gaseous ball with a condensed nucleus, and this will finally solidify into a planet. Or, meanwhile, this yet unformed planet may repeat the history of its parent sun. By contraction, and consequent acceleration of its rotation, it may throw off one or more rings, which in like manner condense into satellites like our moon, or those of Jupiter, Saturn, Uranus, or Neptune. Such, very briefly outlined, is the celebrated nebular hypothesis of Laplace. No one can positively say that the hypothesis is true, still less can anyone say that it is untrue. Time does not permit me to enter into the very strong proofs which Laplace urged in favour of its acceptance.

But I beg you for one moment to cast your imaginations back to a period of time long antecedent to that when our sun had begun to disentangle itself from chaos, and when the fleecy clouds of cosmic stuff had but commenced to rush together. What should we see in such a case, were there a true basis for the theory of Laplace? Certainly, in the first place, we should have a huge whirlpool or cyclone of cosmic gaseous stuff, the formation of rings, and the condensation of these rings into gaseous globes.

Remembering this, look now on this wonderful photograph of the nebula in Andromeda, made by Mr. Roberts. In the largest telescopes this nebula appears simply as an oval patch of nearly uniform light, with a few dark canals through it, but no idea of its true form can be obtained, no trace can be found of the significant story which this photograph tells. It is a picture that no human eye, unaided by photography, has ever seen. It is a true picture drawn without the intervention of the hand of fallible man, and uninfluenced by his bias or imagination. Have we not here, so at least it seems to me, a picture of a very early stage in the evolution of a star-cluster or sun-system—a phase in the history of another star-system similar to that which once occurred in our own—millions and millions of years ago, when our earth, nay, even our sun itself, "was without form and void," and "darkness was on the face of the deep."

During this lecture I have been able to trace but very imperfectly the bare outlines of an astronomer's work in a modern observatory, and to give you a very few of its latest results—results which do not come by chance, but by hard labour, and to men who have patience to face dull daily routine for the love of science—to men who realize the imperfections of their methods, and are constantly on the alert to improve them.

The mills of the astronomer grind slowly, and he must be infinitely careful and watchful if he would have them, like the mills of God, to grind exceeding small.

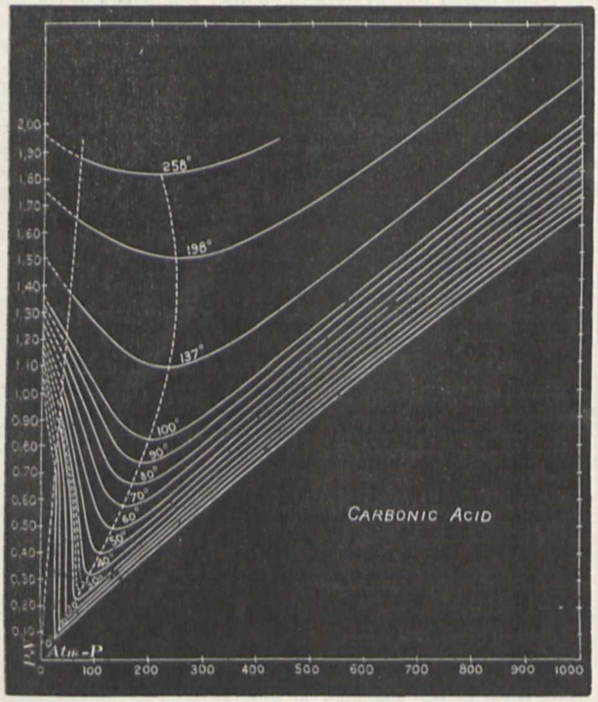
I think he may well take for his motto these beautiful lines—

"Like the star
Which shines afar,
Without haste,
Without rest,
Let each man wheel
With steady sway,
Round the task
Which rules the day,
And do his best."

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 12.—M. Duchartre in the chair.—On the theory of the antagonism of visual fields, by M. A. Chauveau.—An apparatus for carrying out various experiments connected with the study of binocular contrast, by the same author. The instrument described is that used by M. Chauveau in the experiments the results of which were communicated to the Academy on September 7 and 21. In the main it consists of a stereoscope having arrangements by means of which exact equality of luminous impressions may be realized, and the colours of the two fields altered independently.—New *réseau* of isotherms for carbonic acid, by M. E. H. Amagat. The author has determined the isotherms of carbonic acid for every 10° from 0° to 100°, and also those corresponding to 32°, 35°, 137°, 198°, and 258°; the pressures having been taken up to 1000 atmospheres. The results obtained are graphically shown in the accompanying figure, in which the abscissæ represent pressures, and products of P × V furnish the ordinates.—Variation



of the composition of Jerusalem artichokes at different periods of their growth; *rôle* of the leaves, by M. G. Lechartier. Analyses of the dried black leaves which appear on Jerusalem artichokes in the autumn have been made, and the results compared with analyses of green and yellow leaves. The effect of different fertilizers on their composition has also been studied. It appears that the black leaves must have had the same composition as the green leaves, and the substances which they lose are utilized for the nutrition of the higher leaves of the plants. They preserve their vitality as long as the soil furnishes the plant with sufficient phosphoric acid and potash. But if either of these fertilizers be absent, the leaves begin to dry up.—Observations of Tempel-Swift's periodic comet, made at Paris Observatory with the West Tower equatorial, by M. G. Bigourdan. Observations for position were made on October 8 and 9. It is remarked: "The comet is an excessively feeble nebulosity, at the extreme limit of visibility: it is round, from 1'·5 to 2' in diameter, and slightly brighter towards the centre."—Observations of the same comet, made at Paris Observatory with the East Tower equatorial, by Mdlle. D. Klumpke. An observation for position was made on October 9.—Experimental researches on "personal equation" in transit observations, by M. P. Stroovant. The author has determined his "personal equation" for observations of stellar points and disks under different conditions of illumination of the field of the telescope employed. His equation was very different when the preceding edge was observed to transit than when the passage of the following edge was noted. It was also subject to a slight variation. Observations by the "eye and ear method" show a tendency to choose certain tenths of a second in preference to others.—On conjugate systems and on the deformation of surfaces, by M. E. Cosserat.—On turbo-machines, by M. Rateau.—Variation of the electromotive force of piles with pressure, by M. Henri Gilbault. Taking the formula $q\frac{dE}{dp} = dv$, in which E = electromotive force, q the quantity of electricity developed and producing a variation of volume v, and p the pressure, the author has calculated the variations of the electromotive force of different piles, and finds that the results agree extremely well with those arrived at experimentally up to a pressure of 100 atmospheres.—A multitubular electric accumulator, by M. D. Tommasi.—Calculation of the specific heats of liquids, by M. G. Hinrichs.—Melting-point of certain binary organic systems, by M. Léo Vignon.—Calorimetric researches on the state of silicium and aluminium in cast-irons, by M. F. Osmond.—Heat of formation of platinic bromide and of its principal compounds, by M. Léon Pigeon.—Contribution to the study of hematozoaires; on the hematozoaires of the frog, by M. Alphonse Labbe.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Life-Romance of an Algebraist: G. W. Pierce (Boston, Cupples).—Blackie's Science Readers, III. (Blackie).—Recenti Progressi nelle Applicazioni dell' Elettricitá; Parte Prima, Delle Dinamo: Prof. R. Ferrini (Milano, Hoepli).—A Manual of Logic: J. Welton, vol. i. (Clive).—Text-book of Comparative Anatomy: Dr. A. Lang; translated by H. M. and M. Bernard. Part 1 (Macmillan).—Photography Applied to the Microscope: F. W. Mills (Iliffe).—Photographic Pastimes: H. Schnauss, translated (Iliffe).—Handy List of Books on Mining: H. E. Haferkorn (Gay and Bird)—A Treatise on Nitrogen (J. Heywood).—A Contribution to the History of Rain-Gauges: G. J. Symons.—The Constitutional Development of Japan, 1853–81: T. Iyenaga (Baltimore).—Praktisches Taschenbuch der Photographie: Dr. E. Vogel (Berlin, Oppenheim).—Bulletin of the American Geographical Society, vol. xxiii. No. 3 (New York).—Encyklopaedie der Naturwissenschaften, 65 and 66 Liefg. (Williams and Norgate).

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