THURSDAY, JUNE 29, 1893.

ELECTRO DYNAMICS.

Dynamo Electric Machinery. Fourth Edition. Revised and enlarged. By Silvanus P. Thompson. (London: E. and F. N. Spon, 1892.)

A COMPARISON of the size of the fourth edition of this book with that of the first, which appeared in 1884, supplies a good illustration of the rate at which the use of dynamo electric machinery, and our knowledge of its laws, have advanced during the past eight years. The 408 pages in 1884 have grown to 832, with a collection of twenty-nine excellent plates in addition at the end of the book, representing another 60 pages; in fact, the book has now become so portly that it would have been well had the matter been put into two volumes.

Each of the last three editions has been bulkier than its predecessor, but the increase of size of the second and third represented the simple adding of new material without the pruning away of antique and practically obselete matter which is so necessary in a text-book of a rapidly advancing industry. The last edition, on the contrary, has been rewritten, and the author's well-known capacity for hard work and information collecting has enabled him to produce a treatise which contains the latest knowledge and the existing practice of the dynamo designer and the dynamo constructor of to-day.

The book opens with a wonderfully complete collection of historical notes, but we fear that the author's love of history has led him to give a little too much credit to the early workers. It is the fashion with some, and especially with those of classical tendencies, to credit the Chinese with the invention of gunpowder, the compass, and a variety of other useful commodities; to condemn Galileo, Columbus, and Harvey as plagiarists; and to extol Pliny or Aristotle, or other gentlemen of that somewhat remote period, as having foreseen and foretold every scientific principle and device.

But as these prophecies of the ancients were somewhat marred by their utter unsuggestiveness until the discoveries of the moderns had set the historian searching for a meaning which the writers of the prophecies were themselves probably quite ignorant of, we do not regard the trousered investigator as a dealer in second-hand articles.

Nobody knows better than Dr. Thompson that a knowledge of the properties of rubbed amber, or the discovery of the loadstone, was not all that was necessary to construct a 1000 horse-power dynamo with a commercial efficiency of 93 per cent., but nevertheless he fails, we think, to sufficiently distinguish between a chance mention of some notion and the subsequent independent recognition of an important commercial principle. If fame could really be achieved by a person's mingling a grain of wheat with a ton of chaff, what a temptation it would be to spend one's time recording every notion that struck one (no matter how improbable it looked), in the hopes that a hundred years hence some indulgent historian would search through the weary waste, in the hope of discovering with his rosy spectacles an apparent anticipation of some device that practice had then brought to a successful issue.

The historical notes are in fact not critical enough, and show a desire to make things comfortable all round for everybody. For example, the conventional illustration of the Pacinotti machine is given, but the author does not point out, indeed we do not remember to have seen it pointed out, that the original illustration of the Pacinotti generator differed from the conventional illustration in that the collecting-brushes were placed in the worst position, so as to make the machine as powerless as possible. May this have been the real reason why this machine "fell into temporary oblivion"?

If another example were wanted, we might take the following sentence, which, although not occurring in the section called "Historical Notes," enters as a note of an historical character on page 59, in the section, "Combinations to give Constant Pressure." The sentence is, "The combination of a permanent magnet with electro-magnets in one and the same machine is much older than the suggestions of either Deprez or Perry, having been described by Hjorth in 1854." Undoubtedly that is true, only Hjorth used the combination because, not being aware of the instability of the magnetism in a properly designed dynamo, he thought permanent magnetism was necessary to start the magnetic excitation; whereas Deprez and Perry superimposed what was practically a permanent field for a totally different reason and in a totally different way.

The chapters on "Magnetic Principles," the "Magnetic Circuit," "Forms of Field Magnets," are excellent. We do not, however, see much advantage in the introduction of what the author calls the *diacritical* current. The formulæ are thereby simplified, no doubt, but the simplification is effected at the sacrifice of accuracy; for first, the permanent magnetism in the field has to be ignored, and secondly, as there is no absolute maximum induction in iron, there can be no exact value of this *diacritical* current, which produces half the maximum induction.

The author is not quite happy in his choice of symbols. E is defined as representing the entire electro-motive force in the armature, e as the difference of potentials from terminal to terminal. Presumably, then, e_1, e_2, &c., applied to the separate coils of the armature, represent the potential differences at the terminals of the several coils. But that is exactly what e_1, e_2 do not mean, for they stand for the electro-motive forces of the coils. The suffix m attached to the letter for current or resistance denotes field magnet coils, but only when these are series coils. If the coils be shunt coils, the suffix s is attached to the letter. S, however, stands not for the number of shunt coils, as one would expect, but for the number of series coils, the former being called by a different letter altogether, viz., Z. In fact, Dr. Thompson's rules for the use of suffixes have the precision that is possessed by the rules for the spelling of the English language, the delight of every foreigner who studies them.

The description of "Combinations to give Constant Pressure" (pages 57, &c.), and of "Constant Potential Dynamos" (pages 277, &c.), might be well brought together, seeing that both parts of the book deal with the same contrivances, only a little mathematics is added when the subject is taken up the second time. The equations that are given were very useful in the early days when the combinations were first worked out, as they showed what combinations were theoretically possible to produce the desired result. But it is questionable whether these equations are of much use at the present time, or if they are given, it should be clearly explained why equations originally obtained on the assumption that the permeability of iron was constant led to conclusions distinctly valuable in the case of machines intended to produce constant pressure, but quite useless for suggesting a method of compounding a dynamo to produce constant current.

The chapter on lap, wave, and ring winding of armatures is most instructive. The original idea of cross connecting the coils of a gramme ring, so as to only require two brushes with a multipolar dynamo, the author attributes to Mr. Mordey, but we were always of opinion that Prof. Perry's patent of 1880 contained the first suggestion of this now well-known arrangement.

Chapters xiii., xiv., xv., xvi., xvii., and xviii., on "Practical Construction of Armatures," "Commutators, Brushes, and Brush Holders," "Mechanical Points in Design and Construction," "Elements of Dynamo Design," "Arc Lighting Dynamos," and "Examples of Modern Dynamos," taken in conjunction with the twenty-nine plates at the end of the book, contain a wonderfully compressed, and most admirable, *résumé* of British and foreign practice, and make one feel proud that they have been written by an Englishman.

In Chapter xx., on electromotors, the laws of maximum activity and maximum efficiency are carefully distinguished, and it is pointed out that, while Jacobi, Verdet, Müller, and even Weidemann stumbled, the true ideas of Thomson and Joule were put forth correctly by Achard in January 1879. In 1883 was advocated the proposal to employ a forward lead of the brushes with a motor, and a backward lead with a dynamo, so that the magnetisation of the armature might help instead of opposing that of the fields magnets. With reference to this idea the author says, "The fascinating notion of using the armature to magnetise has proved a failure in practice," a statement undoubtedly true historically, but lacking in prophetic inspiration, seeing that this proposal of May 1883, to utilise instead of counterbalancing the magnetism of the armature is now warmly welcomed in May 1893, after Mr. Sayers has shown how the "destructive sparking" can be annihilated.

Chapter xxii., on "The Principles of Alternate Currents," is much too meagre for any one who does not already know more of the subject than is contained in the chapter itself. A student reading the book would be inclined either to skip this chapter altogether and go on to the next, on "Alternators," or turn to some other book for what Chapter xxii. professes to give. The account of the construction of alternating current dynamos contained in Chapter xxiii. is as comprehensive as the description of the principles in the preceding chapter is meagre. The abstract of Dr. J. Hopkinson's investigation on the coupling of alternators is clearly given and easily understood, when the misprint of N A for N H on page 691 is corrected. The device of commutating the current round the field of an alternate current motor, so that when the motor synchronises the excitation is produced by a pulsating direct current, is due originally to Prof. Forbes, and not to Mr. Mordey, as the author states on page 702.

The author is very perplexing in his naming of alternating currents. He calls the current produced by an ordinary alternator a *two phase* current, but why we have no idea. At any one moment the current in all parts of the circuit is in *one phase;* at different times the current has, of course, every possible phase in succession. The current must therefore be called a *one phase* current, or a single current having every possible phase in succession, if the author prefers that; but there is no more reason to call such a current a *two phase* current than to call it a twenty-two phase current. When again we come to the arrangement devised by Ferraris, and illustrated in Fig. 455 (page 405), we have two distinct circuits, the currents in which always differ in phase. We have therefore a two phase arrangement. The author however calls this a "*four phase transmission.*" Lastly, however, when there are three circuits in which there are three distinct currents, the phases of which at any one moment are always all three different, the author, for some reason, is content to call this a "three phase current" like ordinary mortals.

Chapter xxv., on alternate and direct current transformers, is good, but might be made a little fuller, seeing that so very much work has been carried out on transformers during the past few years. The methods of testing transformers are becoming as important as those for "Testing Dynamos and Motors," which forms the subject of Chapter xxviii. The last chapter, on the "Management of Dynamos," contains many valuable hints, derived in some cases from the author's own experience.

The table at the end of the book, headed "Wire Gauge and Amperage Table," we have gazed at with feelings of admiration tempered with doubt. Admiration—because, if all the columns of numbers given in this table be correct, then, while we have spent much time experimenting and calculating in order to obtain information about the heating of one or two bobbins of wire traversed by a current, the problem for bobbins wound with all kinds of wire to all sorts of depths up to 4½ inches has in some way or other been solved. Doubt—because we fear that, in solving this problem, some sort of simple proportion may have taken the place of the complicated mathematics which it is necessary to employ on account of the flow of heat taking place across many layers of copper and cotton interspersed.

To say that this book is the best on its subject in the English language is to say too little, since we know of no book in any other language on the same subject that can be compared with it. The few peculiarities that we have drawn attention to must be regarded less in the light of blemishes than as giving the book individuality, for we recognise our best friends, when we meet them, by their characteristic peculiarities. Dr. Thompson's treatise should be, nay, probably is already, in the hands of every one who deals with dynamo machinery from an educational or from a manufacturing point of view.

It is interesting to notice how the author, in common with other writers, is unconsciously searching for a good abbreviation for the important, but somewhat cumbersom expression—difference of potentials. Sometimes he calls it pressure, but he apologises for that, as he says it is popular. Sometimes he calls it potential, which we think is rather the expression to be apologised for, since it is wrong, the potential of a body having years ago been defined as being the difference between its potential and that of the earth. Sometimes he calls it the volts, but to speak of the volts of a dynamo being too high is like telling your tailor that a coat has too many inches when you mean it is too long. Voltage again appears to us as bad as amperage, a name which, by the bye, enters into the heading of the last table in the book. If we talk of the amperage instead of the value of the current in amperes, why not speak of the microfaradage of a condenser instead of its capacity in microfarads, or of the feetage of a tall man as being 6½? The names current, resistance, capacity, &c., require a short analogous name for difference of potential. Years ago Mr. Latimer Clark suggested that the name *potency* was going a-begging. How would this do as short for potential difference if the industrial name, pressure, be objected to? But, the shortest abbreviation of all is the initials of the words potential difference and our own, P. D.

CAPTAIN COOK'S JOURNAL.

Captain Cook's Journal during his First Voyage round the World, made in H.M. Bark "Endeavour," 1768–1781. A literal transcription of the original MSS., with notes and introduction. Edited by Captain W. J. L. Wharton, R.N., F.R.S., Hydrographer of the Admiralty. Illustrated by maps and facsimiles. (London: Elliot Stock, 62, Paternoster Row, 1893.)

CAPTAIN WHARTON has rendered excellent service to naval and colonial history, and to geographical science, by editing a transcript of Captain Cook's journal of the voyage of the *Endeavour*, which was undertaken chiefly for the purpose of observing the transit of Venus across the sun's disk, and which led to the founding of the Australian Colonies by Great Britain As is well known, the published accounts of that voyage are two, and neither of them satisfactory. The only very complete one is that compiled by Dr. Hawkesworth, from the journals of Cook, and of Mr. (afterwards Sir Joseph) Banks, who accompanied the great navigator as a volunteer, taking with him an eminent scientific man, Dr. Solander, a pupil of Linné, two artists, and servants, all of his own providing. The other is a brief and defective journal kept by Mr. Parkinson, one of Banks's artists, who died before the expedition reached England. It contains rude illustrations of the scenery and peoples of the Pacific Islands, which, if faithful reproductions of the originals (which I doubt), would show that his artistic powers were contemptible. Parkinson's narrative, which was edited by his brother, was published surreptitiously. It was suppressed by authority, and is, happily, not frequently met with.

Dr. Hawkesworth, on the other hand, has been severely and justly censured for the method he adopted, namely, the fusion of the journals of Cook and Banks,[1] and for attributing to their authors inept reflections of his own, an operation for which, even had it been advisable, he had not the ability, from his obvious want of appreciation of the distinctive labours of the navigator and of the naturalist.[2] The result is a narrative in which the performances of the actors are inextricably confounded, and the records of Cook, and doubtless also of Banks, in some cases garbled. With regard to the reflections, they are comparatively of small account, and there is little difficulty in recognising and rejecting them; they were in keeping with much of the literary style of the age, and Dr. Hawkesworth assures the reader that his whole work was before publication submitted to and approved by the members of the expedition then in England.

Unsatisfactory as Hawkesworth's account of the voyage is, it has the inestimable advantage of in some measure filling what would otherwise be a lamentable void in the annals of science, for strange as it must appear, not even a meagre life of Banks has ever been written, and but for Hawkesworth's work and "Cook's Journal," there is no published account of his indefatigable labours during the expedition. Banks, no doubt aided by Solander, kept a full journal of many events that happened during the voyage, which the commander had not the opportunity of witnessing or recording; and the admirable observations on the physical features, populations, languages, economic products, manufactures, zoology, and botany, of the islands, and coasts visited, are presumably for the most part his. Cook, indeed, especially mentions the signal services which Banks rendered, especially in the management of the natives, in acquiring their languages, in provisioning the ships, and in collecting information and objects of interest; and it needs no reading between the lines of his concise narrative to prove his appreciation of his companion, who he invariably took with him wherever he landed.

The materials for the reproduction of the journal of which Captain Wharton has availed himself with great judgment, are a complete copy of "Cook's Journal" in the possession of the Admiralty; another belonging to the Queen, that was transmitted to England from Batavia, thus containing everything of importance; and thirdly, a duplicate of this last, which having been appropriated by the Secretary of the Admiralty, Sir Philip Stephens, passed to his descendants, and from them by sale first to Mr. Cosens in 1868, and in 1890 to Mr. John Corner. The latter gentleman was arranging for the publication of his copy, with the view of devoting the proceeds to the restoration of Hinderwell Church, the parish church of Staithes, whence Cook ran away to sea, when he suddenly died, and the carrying out of his project devolved upon his son, who completed the arrangements which led to Captain Wharton's under-

[1] Sometimes alluded to as the journals of Mr. Banks and of Dr. Solander, though there is no reason to suppose that the latter individual kept any journal independently of that of Banks, of whom he was probably the amanuensis, as Mr. Orton (the ship's clerk) was of Cook.

[2] A conspicuous example of this is Hawkesworth's omission of the passage in Cook's journal (Wharton, p. 322) dwelling on the unaccountable absence of the cocoa-nut (except of its shells cast upon the beach) on the east coast of Australia, which is a most remarkable feature in the geographical distribution of that plant. A few living specimens exist at Rockhampton and Keppel Bay, but in an unhealthy state, producing no fruit, and probably introduced by Europeans.

taking the editorship. The latter informs the reader that the text is from Mr. Corner's copy, so far as it goes, with additional matter, from the date of the arrival at Batavia up to reaching England, from the Admiralty copy.

In an interesting chapter of fifty pages Captain Wharton gives a spirited sketch of Cook's life and labours from his birth in 1728 to his murder in 1779. It contains a list of the antiscorbutics supplied to the *Endeavour*, and an account of the preventive measures adopted to ward off sickness in his ship. It is not mentioned in it that this led to his election, after his return from his second voyage, to the Fellowship of the Royal Society; before which Society he communicated a paper on the above measures, and another on the tides along the east coast of New Holland. Nor that for the former of these he was awarded the Copley medal by the President and Council, the highest honour in the gift of any scientific body, and the more honourable in the case of Cook, from the fact of the medal having been instituted as an award for discoveries or researches in experimental science. It is a melancholy fact that Cook's departure on his third voyage prevented his receiving in person this the sole public recognition of his still unparalleled services.

To dwell upon Cook's professional labours would be out of place here, are they not written in his own Report? which is a model of completeness and conciseness, recalling in these respects the Wellington despatches. There is a reason for the minutest detail, down to the naming of islands, bays, straits, and inlets, with the result of these being as appropriate as are Linné's names of animals and plants.

Captain Wharton has further illustrated his work with valuable footnotes and facsimiles of some of Cook's original charts, as of the Society Islands and New Zealand, making that of the Australian coasts specially interesting by placing on the same sheets parallel with Cook's chart of 1770 one corrected up to 1890, and reduced to the same scale, thus showing the marvellous approximate accuracy of the former. It is to be regretted that no list of the charts and plates is appended to that of the chapters into which, for convenience, Captain Wharton has divided the Report. It is difficult to find some of these in a work printed on thick paper with uncut edges; and without such a list there is no assurance that a copy is perfect.

In the preface, Captain Wharton says (p. vii) "that it has several times been in contemplation to publish Mr. (afterwards Sir Joseph) Banks' Journal, but this has never been accomplished," and again (p. xxvi) that the said Journal "cannot at the present time be traced." This was, till the other day, true. Captain Wharton had spared no trouble in his endeavours to trace it; and the writer of this notice had, at intervals, for many years past pursued the same object, he having a personal interest in its discovery, as being one of the few persons living who had seen it. Its history he believes to be the following. On Sir Joseph Banks' death, without issue, in 1820, his effects passed to the Knatchbull family, with the exception of his extensive Herbarium, Library, and the lease of his house in Soho Square, which were left to the late eminent botanist, Mr. Robert Brown, who had been for many years Banks' librarian, with the proviso that the Herbarium and Library were to be eventually deposited in the British Museum. The Banksian correspondence and papers, including the Report, were thereupon confided to Mr. Brown, with the object of his writing a Life of Banks. Age and infirmities interfered with the prosecution of the work; and the materials were for the same object transferred, in the year 1833, to my maternal grandfather, Mr. Dawson Turner, F.R.S., a naturalist, and man of high literary attainments, in whose house I aided in the collation of a copy of the Journal, which he had caused to be made, with the original. In Mr. Turner's case they met the same fate as in Mr. Brown's, and they were then placed in the hands of the late Prof. Thomas Bell, secretary of the Royal Society, and who succeeded Brown as President of the Linnæan, in the hope that he would undertake a Life of Banks. After retaining the materials for some time he declined the task, but before returning them (in 1857 or 1858) he submitted them to Mr. John Ball, F.R.S., who also declined. Nothing further was known to me or to Captain Wharton of their history until last week, when (having previously been misinformed on this point) I ascertained that the original of all Banks' correspondence and of his Journal of the *Endeavour's* voyage, were in the MS. Department of the British Museum, and the aforesaid copy in the Natural History Department of the same Institution. It only remains to add the hope that this gratifying intelligence may lead to the publication of Banks' Report uniformly with Captain Wharton's admirable edition of Cook's.

J. D. HOOKER.

OUR BOOK SHELF.

The Soil in Relation to Health. By H. A. Miers, M.A., F.G.S., F.C.S., and R. Crosskey, M.A., D.P.H. (London: Macmillan and Co., 1893.)

THE attractive title of this little book speaks for itself, indicating that it is one of those numerous endeavours which are being made at the present day to supply just such an amount of information in several different sciences as will satisfy the requirements of men engaged in some particular department of practical life. In the present case it is a combination of chemistry, geology, and bacteriology which is offered for the benefit of the sanitary officer. The task undertaken by the authors is obviously a difficult one, and, if the book be regarded as a mere outline stimulating the reader to more extended and special study, they may be said to have accomplished this task with a fair degree of success. Our knowledge of the chemical and biological changes taking place in the soil has, during recent years, been so much increased, and is in some respects so complete, that it might have been anticipated that much of this book would have been devoted to a clear exposition of such matters as nitrification and denitrification, the micro-organisms of water, their removal by filtration and other agencies, the purification of sewage, &c. As a matter of fact, the account given of nitrification is incomplete, whilst of the other subjects referred to above, and which are of such cardinal importance in connection with sanitary science, we find hardly any mention whatsoever. On the other hand, there are long passages devoted to such speculative matters as the causes of epidemic infantile diarrhœa, the connection between typhoid and the depression of ground-water, the relationship between soil and the prevalence of cancer and phthisis, &c. In the chapter on water-supply we are informed that the water from the magnesian lime-stone is "too permanently hard to be a wholesome drinking-water," whilst a few lines further on we are surprised to read that "the total solids rarely exceed 20 grains per gallon." The chapter on the atmosphere makes no mention of the numerous investigations which have been made both at home and abroad on the aërial microbes and their distribution. The authors almost apologise for the prominence they have given to the subject of micro-organisms, but we think they might more appropriately have tendered some excuse for their unfortunate frontispiece, which endeavours to represent the microscopic appearance of the typhoid and anthrax bacilli; for whatever the excellence of the original illustrations may have been, the reproductions in the copy before us do little credit to British printing.

Practical Astronomy. By P. S. Michie and F. S. Harlow. Second edition. (London: Kegan Paul, Trench, Trübner and Co., Ltd., 1893.)

IN this book the authors have brought together all those astronomical problems which are required for field work, limiting themselves simply to these, and dealing with them at sufficient length for practical work. The volume is intended especially for the use of cadets of the U.S. Military Academy, and as a supplement to Prof. Young's text-book, and several subjects not sufficiently discussed there for this special branch of practical work are here expanded. After a short discussion on the uses of the *American Ephemeris and Nautical Almanac*, and a few words on interpolation, the authors launch out into the usual methods of determining Time, Latitude, and Longitude on Land, explaining them concisely and deducing the requisite reductions formulæ. Corrections for refraction, parallax, &c., also receive a good share in their respective places, while the instrumental errors are fully explained and discussed. Excellent illustrations of instruments (those in use in the Field and Permanent Observatories of the Military Academy during the summer encampment) are inserted and described. In addition to a set of tables collected together at the end, a few well-arranged forms, showing the methods of computing several problems, are inserted, which should prove a great help to those not accustomed to such calculations.

W. J. L.

LETTERS TO THE EDITOR.

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

The Publication of Physical Papers.

THERE is little doubt that there is much to be done towards improving the machinery connected with the publication of papers on physical science. By publication of a paper I do not mean printing and binding and sending it to libraries in inconvenient places, which are open at inconvenient hours, but bringing it under the eyes of those interested in its subject. It is hardly possible to discuss this matter without being personal to journals and societies, so perhaps direct references may be allowed.

The present position is that as societies we have the Royal Society, which nominally embraces all branches of science, and the Physical Society, which is alone devoted entirely to physics, and several important general scientific societies scattered about the present kingdom. We have also some journals. Of these NATURE must here be put first, but NATURE is by no means purely physical, and is a scientific newspaper, and not a collection of scientific papers, and, owing to the nature of the case, incomplete as regards abstracts. The *Philosophical Magazine*, with its splendid record, fills its place alone. It contains a certain proportion of original papers, and a number of others communicated by the Physical Society, with which there is evidently an arrangement. Thereare also purely electrical paper like the *Electrician*, which covers most branches of electrical work, and the *Electrical Review*, which publishes filtrates of papers on electrolysis and kindred subjects editorially, with the names and references left out; an annoying proceeding. Coming to the societies, the Physical Society is alone devoted to physics. The Royal Physical Society in Edinburgh need not be considered, as it indulges in ornithology and things of that sort. The Physical Society publishes well. Abstracts of the papers and discussions appear in NATURE and in most scientific or technical, and in some literary journals. The papers are often published in the *Philosophical Magazine*, and again in the Society's own Proceedings. No doubt in time this society will be to physics as the Chemical is to theoretical chemistry, but at present it does not command by any means all the most important physical papers. There is also some waste in republishing in the *Philosophical Magazine* and the Proceedings, though this does not cost much. The arrangement with the *Philosophical Magazine* prevents the immediate publication of a Physical Society paper in the scientific and technical journals at home and abroad. This is a source of weakness. A society which objects to its papers being published everywhere before appearing in its own journal does much to defeat its own ends. The Physical may be unable to help this, but in the Royal, or other wealthy institution, it is defeating the main object of the society's existence for the sake of selling a few odd copies of the Proceedings. To go back to the Physical, the result is that its papers are never reprinted either from the *Philosophical Magazine* or from the Proceedings. The *Philosophical Magazine* is not very cheap, and the Proceedings are, I think, not sold to non-members.

The Royal Society gets physical papers. I believe they are sometimes read, but do not know, not being a Fellow. The best papers are published a long time afterwards in a form which is very expensive to buy, and those who are not Fellows generally know nothing about them until they find them by chance. Royal Society papers, again, are seldom reprinted in the journals.

Then there are various other societies, like the Royal Society of Edinburgh, and the Cambridge and Dublin societies, which shroud valuable papers of all sorts in their transactions, and bury them in public libraries. The result of the present state of things is that an English physicist—it is difficult to get on without this curious word—has no simple means of following the progress of his own special study.

There are several courses which would improve matters, but none of these is perfect. The most obvious is for all physical papers of any importance to be sent to the Physical Society, and published in its Proceedings. The advantages of this need hardly be enumerated. Of course the Physical Society would develop, and would at once become one of the most important in the world. The drawback is that if this principle were carried out in all branches of science we should have a number of special societies in London, and none anywhere else, which would be a very bad arrangement. Another plan would be for the various societies to join, so that one journal, say that of the Physical Society, contained all the important physical papers read at the various societies. A society would communicate its best papers to the Physical Society's Proceedings, these Proceedings being controlled partly by representatives of all the other societies. The papers would, of course, also appear in the Proceedings of the societies to which they really belonged. One drawback to this would be that the Royal Society might object to communicating its papers to the Physical; and this might lead to competition between a special and a more powerful general society.

Another course would be for the Royal Society to act as the central body. This would be rather hard on the Physical, and would tend to reduce its standing, so that we would have no first-rate society devoted specially to physics in a country where an enormous amount of work is done in a disorganised way. There would be another difficulty. The Royal Society standard of papers is supposed to be very high, and though it occasionally publishes papers of no value, the high standard generally maintained would exclude many papers of great importance which were hardly good enough for the Royal Society. Then the Royal Society is specially devoted to pure—that is unapplied science, and there are very many papers on applied physics which are of the highest importance.

Still another course would be for a firm of publishers to bring out a purely physical paper. The stumbling-block here is the question of advertisements. According to Mr. Thiselton Dyer, scientific men are supposed to beunbusiness-like, no doubt without reason; but it may be well to remind them that most journals live on their advertisements, the reading matter being a necessary evil. It would thus be commercially impossible to run a purely physical paper, as there is no trade, except to a certain extent electrical engineering, which has much to do with physics.

It might be better to abandon the idea of a central organ for physics, and to publish a complete set of abstracts. Abstracts to be useful must be very well made, and they must be complete. It is very difficult to get good abstracts. The work is laborious and costly when efficiently done. Abstracts are only a developed index, and it would still be necessary that separate papers should be obtainable. Incomplete abstracting is a very common vice. It is not enough to have a few papers brought under a reader's notice: that is good when one is reading for general information in an indolent way, but it is useless in the far more common case in which he wants to know either all that has been done on a given subject, or whether some discovery has been hit upon before. A scientifically worked out subject index is also essential, and, as said before, the abstracts must be practically complete. JAMES SWINBURNE.

4, Hatherley Road, Kew Gardens, June 25.

The Glacier Theory of Alpine Lakes.

I HAVE read with interest the discussion in NATURE on the "Glacier Theory of Alpine Lakes," and I feel constrained to write now, more especially as Mr. Wallace has cited Tasmania as a country, among others, where Alpine lakes are associated with "palpable signs of glaciation." Having recently, with Prof. Spencer, of Melbourne University, made a visit to the central lake district of Tasmania, a few words about the lakes may not be without interest in reference to the subject under discussion.

The lake district of Tasmania is situated about the centre of the island on the great central greenstone plateau, which attains to a height of 4000 ft. above sea level in places. We camped on the shores of Lake St. Clair, and remained there during the whole month of January of this year. Lake St. Clair is about 2500 ft. above the sea, and is about 11 miles long by 2 broad. It occupies a narrow valley between the Olympus Range on the one hand and the Traveller Range on the other. A depth of 590 ft. is recorded. Its basin probably lies in sandstone (carboniferous?), the structure of the adjoining mountains being sandstone capped by greenstone (diabase).

Both Prof. Spencer and myself, being believers in the glacier theory of Alpine lakes, had half expected to find evidences of glaciation, especially as we had heard of well marked signs being found on the west coast, some 50 or 60 miles to the northwest. However, we could not find the slightest trace of glacial action. From the top of Mount Olympus, rising about 2350 ft. above the surface of the lake, we got a magnificent view of the country. The Traveller Range opposite is really the edge of a great greenstone plateau, stretching away with a roughly undulating surface for miles beyond. The surface of this plateau is studded all over with lakes and tarns of various sizes and at different levels. In other directions, too, lakes can be seen here and there nestling in the valleys. In all we counted between thirty and forty lakes and tarns from the top of Mount Olympus. Two small basins of water—the "Olympian Tarns"—rest on the flanks of the mountain itself. On the opposite side of Olympus from St. Clair lies Lake Petrarch, occupying an oval basin and apparently of shallow depth. This lake is about 560 ft. above St. Clair. On the right shore of St. Clair occurs another small lake (Lake Laura) 50 ft. above the former, and separated from it by a ridge about 3400 yards across.

A characteristic feature of this district are the "button-grass" flats. These are open, marshy expanses covered with "button-grass" (*Gymnoschœnus sphærocephalus*) and other plants. They are traversed by numerous little runlets of water, which usually unite into one or more main streams. Here and there in many of them masses of greenstone protrude. Between these "flats" are generally low ridges of greenstone covered with Eucalyptus and Banksias, &c. Many of these flats or marshes—as, for instance, those in the Cuvier Valley, at the head of which lies Lake Petrarch—reminded me very strongly of the moorland scenery in the Scottish Highlands, and the plateau, already referred to, with the lakes and tarns scattered over its surface, might be a scene in Sutherlandshire. But in all our wanderings we did not find the slightest sign of glaciation either in the form of moraines or of striated rock-surfaces. We were not able to examine the lakes on the plateau mentioned, but from its configuration I am confident that evidences of glaciation do not exist. On the west coast, notably about the Pieman River, signs of glaciation are, I believe, abundant, and numerous tarns and rock basins are associated with them. Here the neighbouring mountains are not so high as those further inland, and it was probably their proximity to the coast that was the cause, during the last glacial epoch, of glaciers being formed there and not further inland.

So then, though in Tasmania there are instances of rock-basin lakes being associated with undoubted evidences of glaciation, yet, as I have shown, the glacier theory will not account for by far the greater number of the Alpine lakes on the great central greenstone plateau. I do not propose to put forth any theory to account for these lake-basins, but have put down the above facts in the hope that they may prove of some interest in the question at issue, and to show that at least there are some exceptions to Mr. Wallace's statement that Alpine lakes only exist in glaciated regions.

I may add that Lake St. Clair has been accounted for by Gould, who explained it by supposing that a flow of basalt had dammed up the lower end of the valley in which the lake lies. I am, however, much inclined to doubt the existence of this basalt. Though we traversed the end of the lake where it is said to occur, we did not recognise any basalt.

It may also be remarked about the "button grass" flats or swamps, that they really occupy rock-basins, and may perhaps be regarded as the analogues of the peat-bogs of Scotland and Ireland. All those occurring in the same drainage area seem to be directly connected with each other, and I think there can be little doubt that many of them were formerly occupied by lakes.

Melbourne University, May 7. GRAHAM OFFICER.

THE Editor having given me the opportunity of reading Mr. Graham Officer's interesting letter, I will make a few remarks upon it.

It seems to me that, without further information as to the nature of the search for drift, erratics, or ice-worn surfaces, and judging from the statement that the plateau studded with lakes and tarns was only looked down upon from an adjacent mountain summit, we can hardly give much weight to the positive statements, "I am confident that evidences of glaciation do not exist," and—"as I have shown, the glacier theory will not account for by far the greater number of the Alpine lakes on the great central greenstone plateau." Some light may perhaps be thrown on the matter by the consideration that the undoubted marks of glaciation in many parts of Australia are believed to have been caused by, comparatively, very ancient glaciers, since some of the glaciated surfaces are overlain by pliocene deposits, while others are believed to be of palæozoic age. If the Tasmanian glaciation was also of pliocene age, most of the superficial indications may have been destroyed by denudation, or, if preserved, may be hidden by vegetation or by alluvial deposits. We must therefore wait for a much more thorough examination of the district and of other parts of Alpine Tasmania before it can be positively stated that no evidences of glaciation exist.

ALFRED R. WALLACE.

Vectors and Quaternions.

I WOULD like to ask Prof. Knott whether there would be any fatal objection to defining the scalar product of two vectors as equal to the product of their tensors into the cosine of the angle between them, so that, if the vectors are

$$ix_1 + jy_1 + kz_1,$$

and

$$ix_2 + jy_2 + kz_2,$$

the scalar product would be

$$x_1 x_2 + y_1 y_2 + z_1 z_2,$$

and not

$$ix_1 . ix_2 + jy_1 . jy_2 + kz_1 . kz_2.$$

If this is done, and, for the sake of associativeness of products, i^2 is made equal to -1, the distributive or quaternionic product of two (or more) vectors would be their vector product *minus* their scalar product. The change suggested would enable students to gradually accustom themselves to the notation of the calculus, which would in fact then form an abridged notation for the cartesian expressions and operations which enter into physical investigations.

I would ask Prof. Knott to give this suggestion his careful consideration, as I am sanguine enough to believe that in it, simple as it appears, lies the possible reconcilement of the new school of vector analysts with the quaternionists. Possibly some symbol other than S would have, at any rate at first, to be employed for this new scalar product. Perhaps, with Prof. Macfarlane, it might be called the cos-product, though that notation properly belongs to the scalar product of two vectors only, and loses its significance if applied to the scalar product of three or more vectors. No single letter symbol could be better than S, as it is distinctive and quick to write. However, the first question is whether there is any possibility of the modification being adopted.

The quaternionic product of a vector by itself would be minus its scalar square, but without any mystery attached to the fact. For the product of two vectors = vector product – scalar product, and therefore, if the vector product is zero, the quaternionic product = – the scalar product. Hence, instead of having

$$(\alpha + \beta)^2 = \alpha^2 + 2S\alpha\beta + \beta^2,$$

we should have

$$S(\alpha + \beta)^2 = S(\alpha^2 + 2\alpha\beta + \beta^2).$$

Reciprocal vectors satisfy the equation $\beta^{-1}\beta = 1$, so that $S\beta^{-1}\beta = -1$, *i.e.* β^{-1}, β are oppositely directed vectors.

The quaternion

$$\alpha\beta^{-1} = \frac{\alpha\beta}{\beta^2} = \frac{V\alpha\beta - S\alpha\beta}{-S\beta^2} = -\frac{V\alpha\beta}{S\beta^2} + \frac{S\alpha\beta}{S\beta^2},$$

showing clearly that both the vector and scalar products of $\alpha\beta^{-1}$ are opposite in sign to those of $\alpha\beta$, as must, of course, be the case since β^{-1} and β are oppositely directed vectors. This fact is obscured with the orthodox notation. In fact, so far as I have been able to test the proposed change, I have found no drawbacks, but rather an improvement. ALFRED LODGE.

Sagacity in Horses.

FROM the window opposite, as I write, I have just witnessed an interesting performance on the part of two horses. Bordering the park is a strip of land, doomed to be built upon, but meanwhile lying waste, and used for common pasturage, on which the horses under notice were leisurely grazing. A pony in a cart, having been unwisely left by the owner for a time unattended on the grass, suddenly started off, galloping over the uneven ground at the risk of overturning the cart. The two horses, upon seeing this, immediately joined in pursuit with evident zest. My first supposition, that they were merely joining in the escapade in a frolicsome spirit, was at once disproved by the methodical and business-like manner of their procedure. They soon reached the runaway, by this time on the road, one on one side of the cart, and one the other; then, by regulating their pace, they cleverly contrived to intercept his progress by gradually coming together in advance of him, thus stopping him immediately in the triangular corner they formed. Until the man came up to the pony's head they remained standing thus together quite still; when the two horses, evidently satisfied that all was now right, without any fuss trotted back again together to their grass.

The sagacious conduct of the horses, acting in such perfect cooperation, formed a pretty sight; and it was apparent that, instead of making the pony more excited, they really pacified and calmed him. Why should they not receive "honourable mention" as much as if they were proud human beings?

WILLIAM WHITE.

The Ruskin Museum, Sheffield, June 20.

TERCENTENARY OF THE ADMISSION OF WILLIAM HARVEY TO GONVILLE AND CAIUS COLLEGE, CAMBRIDGE.

BORN at Folkestone, and educated at the King's School, Canterbury, William Harvey was admitted to Gonville and Caius College as a minor scholar in his sixteenth year, on May 31, 1593. The tercentenary of this event was celebrated by Harvey's College on Wednesday, June 21, this being the earliest day after the date of his admission at which rooms were available for those coming from a distance. The guests were received and welcomed by the Master and Fellows, at five o'clock, in the large Combination Room, where tea was provided. In the smaller adjacent room were exhibited a number of objects of interest connected with Harvey, including his pestle and mortar, from the Museum at Folkestone, a rubbing from his mother's tomb, an autograph letter of Harvey, lent by the Master of Sidney Sussex College, and a coloured drawing of Harvey's coat-of-arms, recently discovered on the walls of the buildings of the University of Padua. The latter was presented to the College by the University of Padua, followed on the day of the festivity by a long congratulatory Latin telegram from the Rector, on behalf of the University, which ran as follows:—"Universitatis Patavinæ quæ cum aliis Britannis discipulis tum Harveio Caioque gloriatur, quorum alterius merita insigne Collegium vestrum nunc recolit nomenque ex altero invenit, festi in Harveii honorem indicti participem se profitetur et in renovanda cum celeberrima Universitate Cantabrigiensi vetere studiorum amicitiæque memoria summopere lætatur, pro Academico Senatu, Ferraris Rector." Also an autotype of the panel portrait of Harvey from Rolls Park, Chigwell, Essex, presented to the College by Sir Andrew Clark, as one of a series of eight, consisting of a central portrait of Harvey's father, surrounded by those of his seven sons. Some early editions of the works of Harvey and of some of his more immediate predecessors and followers were also displayed, together with the admission book of the College, containing the original record of his admission. At seven o'clock the guests assembled once more in the Combination Room, whence they proceeded to dinner in the College Hall, led by the butler, bearing the original "caduceus," as used by Dr. Caius when President of the College of Physicians. The dinner was presided over by the Master of Gonville and Caius College, the Rev. N. M. Ferrers, D.D., F.R.S., above whose chair were displayed a copy of the bust from the Harvey Memorial, crowned with a laurel wreath, and the much-prized portrait of Harvey from the Master's Lodge. After dinner the Grace Anthem of the College, composed by Mr. C. Wood, was sung. The Master then proposed the usual loyal toasts, after which Sir James Paget proposed the toast of the evening, "The Memory of William Harvey."

He remarked that the reason why he had the honour of being asked to propose that toast was his relationship to his brother, who, he believed, made the proposal that there should be that tercentenary of the admission of William Harvey. He desired to remember that, and to speak as he thought his brother would have spoken if he had had the opportunity. He was sure that if he had been present he would have referred to the honour which was due to the college which Caius founded. He would have done that out of the deep sense of gratitude which he had for the College. For it was the Fellowship founded by Caius that led his brother to the study of medicine, and, on the occasion of that Fellowship which he held becoming vacant, to give himself entirely to it. To that he owed a great part of the happiness of his life, and he hoped he (the speaker) would not be deemed wrong if he said that indirectly he himself was also deeply indebted to Caius College, for it was through the large income which was associated with that Fellowship that his brother was enabled, out of his abundant generosity, to help him greatly in the study of his profession at St. Bartholomew's Hospital, of which Harvey was so great an ornament and honour. He wished that they knew more of the time and the work he did in Caius College. Indirectly Harvey owed to Caius himself the opportunity of being a student of the College. It was not, he thought, known whether Harvey was originally destined to be a student of medicine or physic, or whether he was led to it mainly by that which he found in that College, from the help and advantages given to the study of medicine and sciences. Harvey found there—and there alone, he thought, certainly amongst the Colleges of the University—a license for dissection. A license was obtained from the King to dissect in that College the bodies of criminals, and Dr. Venn, in the register of St. Mary's parish, had found records of two who were executed here in Harvey's time. The register said distinctly "They have been buried here after being anatomised in Caius College. He might add that the bodies were to be interred with great reverence, and the Masters and Fellows had to attend the funerals. From that College Harvey went to Padua, where he had the best learning from the best biological teachers of the time. He took the degree of doctor of medicine with the highest honour, and then he returned to the practice of his profession and the teaching of it in the University. Alluding to Harvey's discovery of the circulation of the blood, Sir James said he thought he might venture to say that that was the greatest discovery in biological science ever made by one man. He thought there never had been any one man to whom biology was so indebted as to Harvey for that discovery, and that was in the early part of his life. He supposed they could not now think of what would have been the progress of biology but for that discovery, neither could they form any just estimate of the honour due to Harvey for that discovery, which was to them now so plain, so evident, that one might wonder how it could ever have been doubted, but was then surrounded by difficulties which it seemed impossible ever to overcome. It was marvellous, if one looked back at it, to think what must have been the power of observation, the ingenuity, the constant, resolute industry of the man who could find that out, not only in the face of actual difficulties of inquiry, but in the face of those who were perfectly satisfied with their own opinions. He worked on and on until he brought out the best result he could obtain. He had shown by his discoveries, which had had even a greater influence on the progress of biological knowledge, the right method of inquiry. He had to find his results in the face of that full and perfectly-satisfied belief that all truth in such a science as that of medicine could be deduced from general principles then prevalent, and from the physiological doctrines which few men then dared to doubt. Nothing could have proved more than Harvey's results that the way to knowledge in biological science was through continual observation and experiment and recording. That was what Harvey showed, and it had never been forgotten. Again and again Harvey said in his works, and more especially in that admirable introduction to his work "De Generatione," that the way to knowledge was by observing, experimenting, and recording, and not by thinking. The same thought was expressed by John Hunter, who said, "Don't think; try." Those were words he (Sir James) would venture to say every one pursuing biology might well bear in mind. Both of those men were most earnest and profound thinkers. This could be traced in all their works, but that in which they distinguished themselves from other men of the same calling and the same pursuits was that they tried their thoughts. They tested them by every possible observation and experiment. They thought, and thought, and thought, but they were never satisfied with thinking; every thought they had was tried by experiment. When they remembered that Harvey was not only the greatest physiologist of the time, but the greatest physician, it was well to look and see as far as they could how much he himself followed that out, and he thought it would be found, unhappily, they had scarcely any record of Harvey's observations in practice. Repeatedly in his works that were published he stated that he intended to publish his medical observations. Now the whole of those, he supposed, were lost, and yet his (the speaker's) brother pointed out there was no certain knowledge at all either of the time or of the manner in which they were lost. Those observations would be of inestimable value if they could be found. They might hope that some of the younger Caius men would find out where those manuscripts were. It would be well if the MSS. could be published in *facsimile*, or in the same manner as the one done by the College a few years ago, when they at last found Harvey's lectures on anatomy and surgery. He asked them to drink to the memory of Harvey, who had made discoveries surpassing those ever made by any one man, and had showed the true and only sure methods by which biological science could be increased.

After the toast had been duly honoured the College "Carmen Caianum" was sung, the words of which were written by the President, Rev. B. H. Drury, and set to stirring music by Mr. C. Wood. An extra verse commemorative of Harvey and Glisson, also once a member of the College, was introduced into the song for this occasion.

Dr. Clifford Allbutt (Regius Professor of Physic) proposed the toast of "The Guests," and in doing so read a letter which had been received from the rector of Padua University, in which he expressed the pride of that University at having been the place where William Harvey had pursued his studies. Dr. Allbutt also referred to the sense of loss which was felt by all present at the death of Sir George Paget, who would, had he lived, have been the man of all others upon whom it would have been fitting that the duty of taking an important part in that celebration should have fallen.

Sir Andrew Clark responded, and said he desired in the name of his more distinguished fellow guests to give them his grateful thanks for permitting them to be present that evening.

Prof. Gairdner next proposed "The University," and said that the toast needed nothing on his part to recommend it. He could not conceive a greater eulogium upon the University of Cambridge than that it contained such magnificent representatives of ancient learning as their Vice-Chancellor, Dr. Peile; and at the same time eminent representatives of what he would venture to call the modern scientific method as the Professor of Medicine, Dr. Clifford Allbutt and of Physiology, Prof. Michael Foster. The University was an institution founded upon all that was best in human learning and in human experiments, and it will go on and prosper to the end of time. He proposed the health of the University and coupled with it the name of their distinguished Vice-Chancellor, Dr. Peile.

The Vice-Chancellor, in returning thanks, alluded to the great development of studies and of buildings in the University during the past thirty years. He was sorry to say that their rapid development had almost caused alarm in certain most important quarters. He read the other day one of the leading newspapers of England, which called attention to their unsatisfied spirit of innovation. He thought that that must have reference to some of their most recent developments of the engineering tripos. Yet surely it might have occurred to any one that the sciences of engineering were most closely connected with the study of mathematics, which was their chief glory in Cambridge. Possibly, also, it had reference to the development of agricultural science. Well, agricultural science was a very excellent thing. It seemed to him that, after all, some of the greatest discoveries in science had been made, not merely by students or by lecturers, but by men who had been carrying on professional work and working purely with mercantile aims. The duty of the University, he took it, was to encourage those studies as well as others. But there would always be a problem before them. At present the problem would raise the very, very old story—the limited means of the University. The problem was how far could they encourage new studies, which could never lead those who were following them to any great pecuniary rewards, although they might lead them to the rewards of learning. He thought that was a great problem. This year they had an examination in Oriental languages. There was one candidate for it; they had four examiners, and the cost of that examination, he supposed, was £50 or £60 to the University, which was perfectly right. It was just those studies which could not pay their way, which could never be supported without the help of endowments. He did not think the problem was so serious as it might seem at first. Two years ago, when his predecessor, Dr. Butler, resigned his office, he pointed out some of the needs of the University. Dr. Butler's clear and lucid statement brought forth one magnificent gift and nothing more. He (the speaker) in his turn, in the first year of his office, sent forth "a bitter cry" of the needs of the University. Yet that bitter cry brought forth nothing. Did it seem possible that after all a general cry might not be specially efficacious, while a request for special help might serve for a cry? He was happy to say that this seemed at last to solve the problem of how they could develop the newer studies with the help of those outside who were willing to support them. The engineering school had, by the labours of Prof. Ewing and Mr. Horace Darwin, received money, which he hoped would carry it on sufficiently. He believed so ancient an institution as the Observatory of Cambridge was going to ask for a new telescope to carry on its work. That being so, it seemed that partially at least the problem was solved. The problem concerning agricultural science had been solved by the liberal aid of the County Councils. Those bodies had come to their aid in the most generous manner, and given them enough to carry on their work for at least some years. He hoped, as he said before, they would see their way, not merely to maintain and develop those old institutions which had been from all times the glory of Cambridge, but also to carry on those newer studies and newer developments which would keep them in touch with the nation, and make them remembered for all times, and which, whatever developments might arise elsewhere, would make Cambridge one of our greatest centres of educational life.

In proposing the "Health of the Master and Fellows," the Right Hon. T. H. Huxley, who was enthusiastically received, said he was charged with a very pleasant duty, and one which could be happily performed without either gifts of eloquence or even those of voice, in which unhappily he was at present sadly deficient, and he would not be withdrawn from the simple discharge of that duty by the invitation which had been addressed to him by a previous speaker to enter upon the field of controversy. In proper time and place he imagined that he could hardly be said to have shown any unwillingness for the discussion of controverted questions, but in his judgment they were extremely inappropriate and out of place in a meeting of that kind, and he desired absolutely to abstain from that, and to confine himself to the business in hand, which was of a far more pleasant, and, he ventured to think, more profitable nature: it was to propose to them the health of the Master and Fellows of that College. All those who were present would understand the gratitude which they all felt for the generous and gracious hospitality which they had shown to them on that occasion, but it was a traditional hospitality, and it went back to the time when that important corporation, of which they were the present representatives, extended their hospitality to William Harvey, whose name and fame they were met to celebrate. He did not know whether the Master and Fellows of that time were aware of what they were doing in training and disciplining that young man—boy, indeed, to them—to make the best use of the faculties with which he was endowed, but he thought it lay to their credit that from that time to this, the hospitality which they extended to science—to biological science especially, and to that branch of it which was called the science of medicine very particularly—that that had been continued with unbroken openness and readiness. It was for that reason, he thought, that the large proportion of persons present in that room who were devoted to scientific studies would with the greatest possible cordiality drink the toast which he had to propose. For in this matter Gonville and Caius College occupied a position as isolated as it was honourable. He was aware that the studies of biological sciences, and more especially those which had relation to medicine, could not be cleared of the accusation then made against them of utility to mankind. He admitted to the full the charge that was made against those studies, but the present showed, and the future would show more strongly, that quite apart from the bearing of direct utility, it must be regarded as a happy instinct, if not as a purpose of intelligence, which had led that College for these 300 years to cherish and to promote those studies. It was on that ground they who were so deeply interested in its pursuits felt that they owed a debt of gratitude to the College, and he knew of no reason, except the fact that he once took an active part in those biological matters, which had led to his selection as proposer of the health of the College to them on that occasion. Sir James Paget had fully and exhaustively told them, in that admirable language which he had always at command, the great claims of Harvey upon their respect and veneration. He had justly told them that Harvey regarded himself, not merely as a discoverer, but as a propounder and champion of a new method. Dr. Venn was good enough to tell him before the dinner of a fact of which he (the speaker) was entirely ignorant: that before Harvey's time that College possessed what was called an "anatomer," a gentleman whose duties appeared to have been to dissect bodies, which were given over to him and others, to give the students of the College a practical contact with the nature of things. It was in that respect that modern science differed from ancient science; it was in that respect that Harvey was essentially modern. It was therefore to the wise provision of the founders of that College that they owed the beginning of that movement commenced in this country by Gilbert, followed up in Italy by Galileo, followed up conscientiously here by Harvey himself, which had led to the great modern development of scientific culture. They trusted that the hospitality which had hitherto been extended by that College to purely scientific investigations would be continued upon the lines laid down by Harvey. It might be that Harveys existed among them now, and the only thing they had to hope for, and to wish for was, that those Harveys of the future might not be compelled, as the Harvey of the past, to obtain a higher scientific training by going to the University at Padua. They hoped that in this University men would have the opportunity of obtaining the highest scientific culture which was to be given. That he understood was the object and purpose and desire of the Master and Fellows of that College, in inviting persons like himself to take part in that great celebration; he presumed they wished them to understand that they recognised Science as a fundamental branch of human culture, and that they would do what in them lay to promote that happy commemoration to which he ventured to allude.

The toast was drunk with enthusiasm.

The Master briefly returned thanks, and stated that it had given them very sincere pleasure to entertain so illustrious an assembly, and expressed his deep regret that Sir George Paget was not with them.

The Rev. B. H. Drury proposed the health of the younger members of the College, saying that they were the life-blood of their College to-day, the source of their vitality, without whom they would have really little cause for existence.

Mr. Keeble, Natural Science Scholar of the College, made a short and graceful reply.

At the conclusion of dinner a move was made to the Combination Room, where friendly and animated intercourse was kept up for some time, and it was late before the last of those engaged in the celebration separated for the night.

Breakfast was provided the following morning from eight to ten for those resident in College overnight, and by midday the guests had departed, leaving the courts once more to solitude, and to their hosts a keen feeling of satisfaction at the honour done to the memory of William Harvey and to the College by the recent presence of so representative and distinguished a gathering of visitors.

SOME POINTS IN THE PHYSICS OF GOLF.[1]

III.

IN Part II of this paper (NATURE, Sept. 24, 1891) the following statements were made :—

"The only way . . . of reconciling the results of calculation with the observed data is to assume that, for some reason, the effects of gravity are at least partially counteracted. This, in still air, can only be a rotation due to undercutting."

"And, as a practical deduction from these principles, it would appear that, to secure the longest possible carry, the ball should be struck so as to take on considerable spin———."

As these statements, and some of their consequences, have been strenuously denied, I must once more show at least the nature of the evidence for them

It depends, in one of its most telling forms, upon the contrast between the length of time a well-driven ball remains in the air (as if in defiance of gravity) and the comparatively paltry distance traversed. Every one who thinks at all on the subject must see that, *without some species of support*, the ball could not pursue for six seconds and a half a course of a mere 180 yards, nowhere more than 100 feet above the ground.

In fact, if we assume the initial slope of the path to be 1 in 4, as determined for the average of fine drives by Mr. Hodge with his clinometer (NATURE, Aug. 28, 1890) the carry of a non-rotating ball will be approximately (in feet)

$$AgT^2,$$

where g is the acceleration due to gravity, T the time of flight in seconds, and A a numerical quantity depending on the resistance. The value of A varies continuously between the limits, 2 for no resistance, and 1 for infinitely great resistance. [It is assumed that the resistance is as the square of the speed.]

This formula gives, with the average observed value of T ($6^s{\cdot}5$, see Part II.) carries varying from about 900 down to 450 *yards!* The initial speed required varies from 416 foot-seconds upwards. The longest actually measured carry on record, when there was no wind, is only 250 yards. Unfortunately, in that case T was not observed, but analogy shows that it was probably much more than 7^s. Even if we take it as 7^s only, the "carry" ought to have been, by the formula (which is based on the absence of rotation), 522 yards at the very least!

I have purposely, in this example, kept to the case of an initial slope of 1 in 4; because those (and they are many, some of them excellent golfers) who altogether reject the notion that undercutting lengthens the carry, would of course in consistency refuse to believe that a long ball may sometimes start horizontally. But, to those who allow *this* statement, the fact that the action of gravity is occasionally largely interfered with, or even counteracted, is obvious without any numerical calculations. In fact, from my present point of view, initial slope is of little importance:—except, of course, in avoiding hazards. The want of it is easily made up for by a slightly increased rate of spin.

Another way of looking at the matter is to assume, from Mr. Hodge's data, 180 yards as a really fine carry, and thence to calculate by the formula the requisite time of flight. It varies from $4^s{\cdot}1$ to $2^s{\cdot}9$ according as the resistance, and therefore the necessary initial speed, are gradually increased; the former from *nil* to infinity, the latter from 132 foot-seconds upwards. Thus the observed time exceeds that which is really required when there is no spin, by 60 per cent. at the very least!

The necessity for underspin being thus demonstrated, we have next to consider how its effect is to be introduced in our equations. On this question I expressed a somewhat too despondent opinion in the previous part of this paper. A rather perilous mode of argument (which I have since been able to make much more conclusive) first suggested to me that the deflecting force, which is perpendicular at once to the line of flight and to the axis of rotation, must be at least approximately proportional to the speed and the angular velocity conjointly. But I tried (with some success) to verify this assumption by various experimental processes. These, as will be seen, led also to a numerical estimate of the magnitude of the deflecting force. [And I was greatly encouraged in this work by the opinion of Sir G. G. Stokes, who wrote:—"I think your suggestion of the law of resistance a reasonable one, and likely to be approximately true." This is quite as much as I could have hoped for.]

First: by the well-known phenomena called heeling, toeing, and slicing, which are due to the ball's rotation about a vertical axis. I have often seen a well-sliced ball, after steadily skewing to the right through a carry of 150 yards or even less, finally move at right angles to its initial direction, and retain very considerable spin when it reached the ground. Neglecting the effects of gravity, the equations of the path should be, in such a case,

$$\ddot{s} = -\frac{\dot{s}^2}{a},$$

$$\frac{\dot{s}^2}{\rho} = k\dot{s}\omega;$$

expressing the accelerations in the tangent, and along the radius of curvature, respectively. If we introduce the inclination, ϕ, of the tangent to a fixed line in the plane of the path, the second equation becomes

$$\dot{\phi} = k\omega,$$

showing that the time-rate of change of direction is proportional to the speed of rotation. The first equation gives, of course,

$$\dot{s} = V\epsilon^{-\frac{s}{a}},$$

where V is the initial speed.

The space-rate of change of direction, *i.e.* the curvature of the path, is thus

$$\frac{d\phi}{ds} = \frac{k\omega}{V}\epsilon^{\frac{s}{a}},$$

increasing in the same proportion as that in which the speed of translation diminishes; and, if we regard ω as practically unaltered during the short time of flight, the intrinsic equation of the path is

$$\phi = \frac{ka\omega}{V}(\epsilon^{\frac{s}{a}} - 1).$$

A rough tracing from this equation is easily seen to reproduce distinctly all the characteristics of the motion

[1] Part of the substance of a paper on the Path of a Rotating Spherical Projectile, read to the Royal Society of Edinburgh on June 5.

of a sliced, or heeled, ball. And, by introducing an acceleration in the plane of the path, constant in magnitude and direction, the path might be made to intersect itself repeatedly.

By the statement made above as to the whole change of direction in the course of a well-sliced ball, and with 5^s as the time of flight (for it, like the carry, is notably reduced by slicing) we have

$$\frac{\pi}{2} = 5k\omega.$$

Thus it is clear that we may easily produce rotation enough in a golf-ball to make the value of $k\omega$ as great as 0·3 or even 0·4. And this can, of course, be greatly increased when desired. This datum will be utilised later. The fact (noticed above) that the time of flight, and the carry, are both reduced by slicing, gives another illustration of the necessity for underspin when the time of flight is to be long, and the carry far.

Secondly: by a laboratory experiment which, I have only recently learned, is due in principle to Robins. (*An Account of Experiments relating to the Resistance of the Air.* R.S. 1747.) I suspended a wooden shell, turned very thin, by a fine iron wire rigidly fixed in it, the other end of the wire being similarly attached to the lower end of a vertical spindle which could be made to rotate at any desired rate by means of multiplying gear. Thin as was the wire, it was but slightly twisted in any of the experiments, so small was the moment of inertia of the wooden shell. The wire acted as a universal flexure joint; and, by lengthening or shortening it I could make the ball's mean speed, in small pendulum-oscillations, vary within considerably wide limits. I verified this result by substituting for the shell a leaden pellet of equal mass but of far smaller radius, as I feared that some part of the result might be due to stiffness of the wire, produced by torsion. But with the pellet the rotation of the orbit was exceedingly slow. Thus ω, and the average value of $\dot{s}$, could have any assigned values; and from the elliptic form and the rate of *rotation* of the orbit of the ball, the transverse force was found to be proportional to either of them while the other was kept constant. An exceedingly interesting class-illustration can be given by making the ball revolve as a *conical* pendulum, and while it is doing so giving it spin alternately with, and opposite to, the direction of revolution. The effects on the dimensions of the orbit and on the periodic time are beautifully shown. This form of experiment could be easily applied to considerable speeds, both of the translation and of rotation, if the use of a proper hall could be secured. But it cannot be made strictly comparable with the case of a golf-ball; as the speed of translation can never much exceed that for which the resistance is as its first power only. [Robins' suspension was bifilar, and the rotation he gave depended more on the twisting of the two strings together than on the torsion of either. In this mode of arrangement it is difficult to measure the rate of spinning of the bob, and almost impossible to vary it at pleasure.]

We must next say a few words as to the manner in which the spin, thus *proved* to have so much influence on the length of the carry, is usually given. I pointed out, in the earliest article I wrote on the subject, "The Unwritten Chapter on Golf" (*Scotsman*, Aug. 31, or NATURE, Sept. 22, 1887), that spin is necessarily produced when *the direction of motion of the club-head*, as it strikes the ball, *is not precisely perpendicular to the face.* Now, even when the head is not purposely laid a little back in addressing the ball, (many of the longest drivers do this without asking Why) it must always become so in the act of striking if the player stand ever so little behind the ball:—especially if, as Mr. Hutchinson so strongly urges upon him, he makes the path of the head at striking as nearly straight as possible. Mr. Hutchinson gives a highly specious, but altogether fanciful, reason for this advice. We now see why the suggestion is a really valuable one. A "grassed" club, and especially a spoon, gives this result more directly. As soon as I recognised this, I saw that it furnished an explanation of a fact which had long puzzled me:—viz. that one of my friends used invariably to call for his *short* spoon when he had to carry a bunker, so distant that it appeared impossible of negotiation by anything but a play-club. And, if the ball be hit ever so little under the level of its centre, with the upper edge of the face, very rapid underspin may be produced. This was probably at least one of the objects aimed at (however unwittingly) by the best club makers of last generation, for they made the faces of drivers exceptionally narrow. Some time ago I proposed, with the same object in view, to bevel the face by deeply rasping off both its upper and lower edges: —thus in addition saving the necessity for the "bone."

I have neither leisure nor inclination to attempt (for the present at least) more than a first approximation to the form of the path under the conditions just pointed out. Anything further would involve a laborious process of quadratures, mechanical or numerical, only to be justified by the command of really accurate data as to the values of a and V. I shall therefore at once assume that neither gravity nor the spin affects the translatory speed of the ball. (If the spin have such an effect, it will be taken account of sufficiently by a slight change in the constant of resistance; and the effect of gravity on a low trajectory is mainly to produce curvature which, in this case, is to a great extent counteracted by the spin. It is easy to see that the effects of this ignoration of gravity, in the tangential equation of motion, are to make the path rise a little too slowly at first, then too fast; to make it rise too high, and descend at too small a slope.) Hence we may keep the first equation of motion above, and write the second as

$$\dot{\phi} = k - \frac{g}{\dot{s}}$$

where ϕ is reckoned positive in the ascending part of the path; and k is written for $k\omega$, its dimensions being those of angular velocity. With the help of the value of $\dot{s}$, above, this becomes

$$\frac{d\phi}{ds} = \frac{k}{\text{V}}\epsilon^{\frac{s}{a}} - \frac{g}{\text{V}^2}\epsilon^{\frac{2s}{a}}.$$

In x, y cöordinates, x horizontal, this is nearly

$$\frac{d^2y}{dx^2} = \frac{k}{\text{V}}\epsilon^{\frac{x}{a}} - \frac{g}{\text{V}^2}\epsilon^{\frac{2x}{a}}.$$

Thus the x coördinate of the point of contrary flexure is found from

$$\epsilon^{\frac{x}{a}} = \frac{k\text{V}}{g},$$

so that there must be such a point, *i.e.* the path is concave upwards at starting, if kV be ever so little greater than g.

Again

$$\frac{dy}{dx} = e + \frac{ka}{\text{V}}(\epsilon^{\frac{x}{a}} - 1) - \frac{ga}{2\text{V}^2}(\epsilon^{\frac{2x}{a}} - 1)$$

where e is the initial slope. The x cöordinate of the vertex is found by putting

$$\frac{dy}{dx} = 0.$$

Finally, the approximate equation of the path is

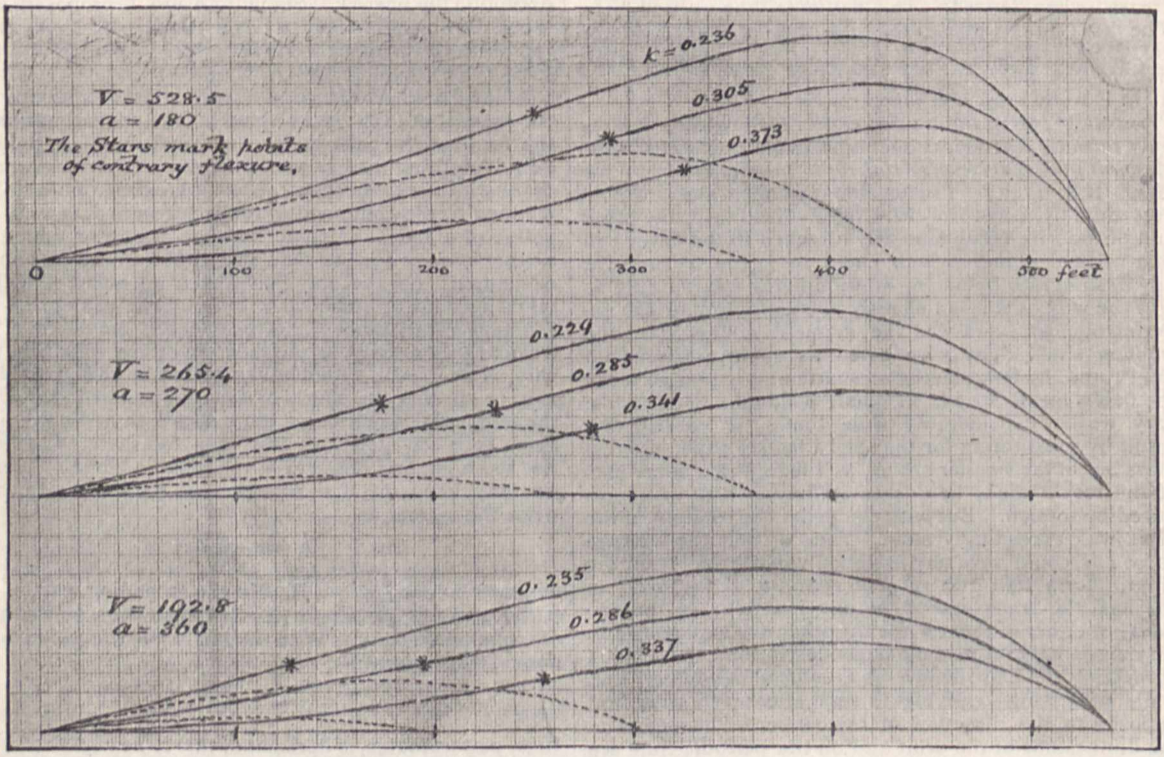
$$y = ex + \frac{ka^2}{\text{V}}\left(\epsilon^{\frac{x}{a}} - 1 - \frac{x}{a}\right) - \frac{ga^2}{4\text{V}^2}\left(\epsilon^{\frac{2x}{a}} - 1 - \frac{2x}{a}\right).$$

To deal expeditiously with these equations I formed a table of values of the various factors in brackets, by the help of Glaisher's data of natural antilogarithms (*Camb. Phil. Trans.* xiii, 243). Next, I utilized in the equation

$$\text{V}t = a(\epsilon^{\frac{x}{a}} - 1)$$

the well ascertained data $6^{s}\cdot 5$ for the time of flight, and 540 feet for the carry, thus obtaining a general expression for V in terms of a. Then, in consequence of the want of accurate data, I chose three values of a, one considerably less than, the second nearly equal to, and the third considerably greater than, that which results from Bashforth's experiments with iron spheres. Thus I found the following values:—

a	V
180	528
270	265·4
360	193.



Next, with each pair of these numbers, and with the successive values $\frac{1}{4}$, $\frac{1}{8}$, and 0 for e, I found k from the condition that $y = 0$ for $x = 540$. These values of k are of course greater as e is less, and also as a is less. But all are found to lie between the limits derived, above, from the data for a sliced ball. All the constants being thus found, the curves were easily traced by a few points:—and the position of the maximum ordinate was found as above. For contrast, I have put in (dotted) the paths of drives corresponding in all respects with the others, except the absence of rotation. Poorly as these show, they are probably unduly favoured at the expense of the others, as I have taken a the same for each of the group; though it is probably reduced by the spin, so that rotation increases the direct resistance. The comparison of these with those in which rotation has a share shows that, though strength and agility are undoubtedly of importance in long-driving, even a store of these qualities equalling in amount that of a full-sized tiger is comparatively inefficient as against the skill which imparts a sound undercut. For here, as elsewhere, the race is not to the swift, nor the battle to the strong. Craft beats Kraft all the world over! La Puissance! ce n'est pas frapper fort, mais frapper juste!

From the very nature of the process I used in approximating, none of these curves can be quite trustworthy, those giving the greater elevations being most at fault. I regret this for the additional reason that I should have liked to add an illustration of an extremely exaggerated path in which e is (say) zero, and k unity at the least. Under conditions of this kind there might be kinks in the path! For a similar reason I cannot attempt to work out the effect of wind with any attempt at precision, at least in the case when the drive is against the wind and the upward concavity of the path becomes in consequence much more prominent. It is easy in every case to *form* the more exact equations, but the labour of treating them even to a rough approximation would be considerable.

I am engaged at present in endeavours to find something like a proper value of a, or of V, above; so as to have reasonable confidence in my data before I engage in what promises to be a heavy task. Of course, if I can obtain a satisfactory value of one of them, that of the other would follow. But independent determinations of both would enable me to subject the theory to the most complete test imaginable. I am inclined to think that the value of a (280 feet), which I calculated from Bashforth's data, is too large (*i.e.* it makes the resistance too small) for a golf-ball:—and thus that the true path is intermediate in form between those of the first and of the second series in the cut. For the initial speeds required, even with $a = 270$, to give a carry of 540 feet *without spin*, are 462 and 653 foot-seconds for slopes of 1 in 4 and 1 in 8 respectively:—the corresponding times of flight being only $3^{s}\cdot 7$ and $2^{s}\cdot 6$.

P. G. TAIT.

NOTES.

WE are glad to record that the Council of the Imperial University of Kasan has elected Prof. J. J. Sylvester honorary member of the University.

THE Albert Medal of the Society of Arts for the present year has been awarded to Sir John Bennet Lawes and a like medal to Mr. John Henry Gilbert "for their joint services to scientific agriculture, and notably for the researches which throughout a period of fifty years have been carried on by them at the experimental farm, Rothamsted."

PROF. W. H. PICKERING, the Director of the Harvard College Mountain Observatory at Arequipa, is expected to be in London in the course of a few days.

THE distribution of prizes to the students of Charing Cross Hospital Medical School will take place at the School on July 4, at three o'clock precisely. The Right Hon. the Baron de Worms, M.P., F.R.S., will occupy the chair.

THE Dental Hospital of London will hold a *conversazione* at the Royal Institute Galleries, Piccadilly, on July 14. There will be a distribution of prizes at 8.30 p.m. by Prof. Sir W. H. Flower, K.C.B., F.R.S.

ACCORDING to Dalziel's agency, a cyclone passed over Williamstown and its immediate vicinity on June 21. Its path was six miles long by half a mile wide, and in this track not a house, barn, or tree was left standing. The wind-rush was followed by a terrific downpour of rain. About twenty persons lost their lives.

DR. NANSEN and the members of his expedition to the North Pole sailed from Christiania on Saturday, and arrived at Laurvig on the following day. After taking on board two covered boats, to be used in case the members of the expedition are compelled to leave the *Fram* in the ice, the vessel proceeded on her voyage. Reuter says that intelligence has been received from Siberia that twenty-six dogs, for service with the expedition, have been brought down to the mouth of the River Olensk. Parties have been sent out to leave stores of provisions for twelve men at two places on the islands of Kotelnoi and Liakow. These depots will be inspected in 1894 and 1895. Sealers report that the sea around these islands was quite open in 1888, 1889, and 1890, while in 1891 and 1892 there was little ice in the vicinity.

INFORMATION with regard to the social, physical, and mental condition of children is being accumulated by the committee appointed by the International Congress of Hygiene and Demography. Nearly 30,000 children, chiefly in London Board Schools, have been inspected, and important facts have been obtained as to the variation of educational requirements of boys and girls, and the causes of low mental development. It is desired to extend the inquiry among 100,000 children before submitting the statistics to a complete investigation. For this purpose Sir Douglas Galton, writing from the Parkes Museum, has made an appeal for financial help. The deep importance of the work is fully understood by educationalists, hence there should be no difficulty in obtaining sufficient funds to render the investigation as comprehensive as possible.

A SPECIAL general meeting of the Royal Geographical Society will be held on July 3 in the hall of the University of London, Burlington Gardens, to consider the proposal that ladies should be admitted as ordinary Fellows. On the evening of the same day the Earl of Dunmore will give a paper on his "Journeys in the Pamirs and Central Asia."

DR. M. MŒBIUS, of Heidelberg, has been appointed Director of the Botanic Gardens at Frankfort-a-M., and Dr. F. Pax, Director of the Botanic Garden at Breslau.

THE French Academy has awarded the Prix Desmazières to M. P. Viala, for his researches on viticulture; the Prix Montagne to M. l'Abbé Hue, for his work in lichenology; and the Prix de la Fons Mélicocq to M. Maseleff, for his work on the Botanical Geography of the north of France.

SOME interesting scientific documents changed hands at the sale this week of the library of the late Lord Brabourne. Among the lots was a quantity of the correspondence of Sir Joseph Banks, to whom Sir Edward Knatchbull, Lord Brabourne's father, was executor. An interesting autograph letter from John Hunter, dated 1792, appears to be one covering the despatch to the Royal Society of his paper on the natural history of the common bee. He hopes Sir Joseph and his worthy council will think the results of twenty years of observation and experiment suitable for publication in the transactions, and details some of the obstacles which had prevented an earlier forwarding of the paper. About 1830 the Royal Society claimed and received from Sir Edward Knatchbull the letters and papers of Sir Joseph Banks, referring to the society over which he so long presided, but evidently this particular letter was overlooked. A document, apparently in the handwriting of Duhamel du Monceau, is an appeal to Sir Joseph on behalf of Dolomieu, the French mineralogist, imprisoned at Messina, by order of the Neapolitan Court, as he was returning to Europe from serving on the scientific staff which accompanied Bonaparte's expedition to Egypt. It is signed by thirty-nine famous men of science of the time, including Cuvier, Lamarck, Laplace and Lalande.

THE weather during the past week has been of a decidedly unsettled type. On Thursday, the 22nd inst., a depression advanced over Scotland and moved slowly to the North Sea and Norway, causing some rain over most parts of these islands; in the north-east of Scotland the fall amounted to 2·2 inches in forty-eight hours, and a moderate gale blew from the north-west on our northern coasts. During the early part of the present week important depressions advanced over the western parts of the country from off the Atlantic, causing exceptionally heavy rain in the south of Ireland, the fall at Roche's Point on Tuesday morning registering 1·1 inch, while the amount was very considerable in many other parts of the kingdom. During the first part of the period the temperature was from 25° to 30° lower than in the previous week; the maxima rarely reached 70° in any part of the country, while in the north the highest daily readings were frequently below 60°, but on Tuesday the temperature rose considerably in most parts, and reached 80° at Cambridge. The *Weekly Weather Report* of the 24th inst. showed that the rainfall exceeded the mean in the east of Scotland only. Bright sunshine was above the average in Ireland and the greater part of England; the percentage of possible duration amounted to 77 in the Channel Islands.

MR. W. H. PREECE, F.R.S., in giving evidence before the joint committee of Lords and Commons on Electric Powers Protective Clauses, is reported to have shown a series of diagrams illustrating the effect upon the earth of the City and South London Electric Railway. That railway did not designedly use the earth, but the return circuit was made by means of the rails, and also by the tubes or tunnels. Currents were produced which had disturbed the observing instruments at Greenwich, and which had been traced as far as North Walsham, in Norfolk. Last year the disturbances began to increase, and his attention was called to the fact that in Clapham Road there was a chemist who had in his shop window an instrument for recording the passing movement of every train on the electric railway, the instrument being connected on one side with gas-pipes, and on the other with water-pipes in the house. He had caused the currents to be measured, and they were found to be sufficient to light a lamp or, as he had proved, to ring one of the division bells of the House of Commons. Another difficulty had occurred in connection with the railway block system. Some years ago the London and North-Western Railway lighted Holyhead Harbour by electricity. The effect of the five arc-lamps employed was to break down the block signals in the district within a mile. But the difficulty was removed by supplying metallic circuits to the signals. At Blackpool the disturbing currents from the electric tramway had lowered a block-signal on the railway and fired a time gun at the same moment, a minute or so before the time when the gun ought to have been discharged.

IN the recently published number of the Proceedings of the Société Française de Physique there is an account of a standard condenser formed by two plates of silvered glass separated by three blocks of quartz accurately worked to the same thickness. The instrument almost exactly realises a theoretical condenser, as the central part is only separated from the guard-ring by a narrow line along which the silver has been removed. The only disadvantage is that the insulation is rather bad, and when the air is not perfectly dry there is a small current between the central disc and the guard-ring. To get over this difficulty the author (M. P. Curie) joins the electrometer to the continuous plate of the condenser, charges the central disc of the other plate with the battery, and connects the guard-ring with the earth. Under these conditions the field of force between the plates is no longer uniform, but the charge of the condenser is the same as in the ordinary arrangement. With this arrangement the insulation is all that can be desired, as the quartz blocks are very good insulators, and little affected by moisture in the air.

A SIMILAR condenser to that described above has been employed by M. Abraham in his determination of the ratio between the electromagnetic and electrostatic units (see Proceedings of Société de Physique, p. 332, 1893). The method employed for measuring the distance between the plates is as follows. In front of the space between the plates a finely-divided glass scale is placed with its plane perpendicular, and the lines of the graduations parallel to the plates. The silvered plates constitute excellent mirrors, and give a series of images of the divisions of the scale, the distances between which were measured by means of a microscope. This method gives the mean distance between the plates, which was found to vary each time the instrument was set up, and to differ slightly from the length of the quartz blocks employed to separate the plates.

THE photographic study of sources of light by means of a carefully graduated series of exposures was first applied with great success by M. Janssen to the investigation of the minute structure of the solar surface. M. Crova has applied a similar method to the study of the carcel standard and the electric arc. A contrast between the various parts of the magnified photographic image of the carcel flame does not appear until the exposure is reduced to the minimum necessary to secure an impression, and to bring out this contrast the negative must be developed slowly and subsequently intensified. Four photographs thus obtained were exhibited at a recent meeting of the French Academy. The axis of the flame appears dark, and the zone of combustion exhibits two bright lines representing the external and internal surfaces of combustion of the hydrocarbons, with a dark line between them corresponding to the space where combustion is incomplete. Photographs of the flames of a candle, an amyl-acetate burner, and a bat's-wing gas jet were also exhibited, showing analogous phenomena. The same method applied to the arc light yielded some interesting results. As the time of exposure was reduced the arc gradually vanished, the negative carbon was reduced to a very small surface, and the positive carbon exhibited a surface riddled with dark spots, and granulated like the surface of the sun in M. Janssen's photographs. These granulations could be seen in violent motion on the ground glass screen of a camera with the lens sufficiently stopped down. It follows that it is not admissible to screen off all but a very small portion of the luminous source in order to reduce the amount of light in the same proportion as the area of luminous surface. With very small surface elements both the amount of light and the temperature, and hence also the tint of the light, may be constantly changing.

HERR VON LUPIN, of Munich, has recently called attention to two thermometer liquids as free from certain drawbacks of the spirit thermometer. One of these is sulphuric acid diluted with water. According to experiments by Sohncke, the quantity of water removed by distillation in the thermometer-tube was a minimum even when the free end was surrounded with ice; and (what is still more important) in a short time this very small quantity was reabsorbed. The expansion of the liquid is approximately constant. In a recent expedition by Herr Vogel to Central Brazil these minimum thermometers were used, and found to act very well. The other liquid referred to is chloride of calcium in spirit (10 to 15 per cent. of the anhydrous salt is best). This is specially recommended for medical use, because its pronounced colour enables it to be more easily read at night than the mercury thermometer. Here, too, there is no distillation-error. A further advantage is that the thermometer takes the body-temperature very quickly (in about three minutes). The regularity of expansion between 0° and 50° C. is good, though not in the same degree as with sulphuric acid; and the proportion of calcium chloride is here influential. The solution, like that of sulphuric acid, does not solidify even in the artificial cold of evaporating carbonic acid snow; and with the proportion of salt given, no salt is separated out in the bulb.

To the current number of the *Zeitschrift für physikalische Chemie* Herr Altschul communicates from Prof. Ostwald's laboratory a series of observations on the critical constants of some fatty and aromatic hydrocarbons. Unlike most observers in this field the author thus deals with chemically related substances which have a comparatively simple structure. With the ascent of a homologous series it appears that the critical temperatures increase and the critical pressures decrease at rates which gradually diminish. Chemical constitution also affects the magnitudes of the critical values, the three metameric xylenes, for example, have different constants. From his observations the author deduces the values of (*a*) and (*b*) in Van de Waals's equation, Guye's critical coefficient, &c., and traces relationships between their magnitudes.

Is colour-blindness a product of civilisation? An investigation described in *Science* by Messrs. Blake and Franklin, Physical Laboratory, Kansas University, favours an affirmative answer to the question. Of 159,732 persons tested in Europe and America, nearly four per cent. were found to be colour-blind. But when the ordinary Berlin worsteds were used to test the colour perception of a number of Indians, representing many tribes, only 3 in 418, or 0·7 per cent., were found to be deficient. These were full-blooded Indians, and all males. It appears, therefore, that, as with civilised peoples, the percentage of colour-blind males is greater than that of females.

THE peculiar phenomenon sometimes observed near the Wetter Lake in Sweden, and called by the natives *Motalaströms stadnande*, the standing still of the Motala river, has been the subject of speculation ever since the times when it used to be regarded as a miracle and a portent. The Motala river emerges from the Wetter Lake, and the phenomenon in question consists in the cessation of the flow and the drying up of the bed, accompanied by a retention of water within the lake.

According to Block, this is due to a sudden sharp frost, which freezes the river to the bottom at a shallow place without allowing time for the formation of mere surface ice. It is probable that a strong east wind is a necessary condition, and that the detention of the water is aided by the reeds growing near the outflow of the lake. A collection of records of the occurrence has been made by Herr Robert Sieger in a paper on the oscillations of lake and ocean levels in Scandinavia, which appears in the *Zeitschrift der Gesellschaft für Erdkunde*. He finds six observations during the sixteenth, twelve during the seventeenth, and eighteen in the eighteenth century. He does not, however, think that the general level of the lake is perceptibly influenced by the phenomenon.

TWO organisms resembling the cholera bacillus have recently been obtained by Bujwid from water during an outbreak of cholera ("Ueber zwei neue Arten von Spirillen im Wasser," *Centralblatt für Bakteriologie*, vol. xiii. 1893, p. 120). These are designated as *Bacillus choleroides* α and β in consequence of their striking resemblance to Koch's cholera organism. It is quite possible, however, that these forms may really be identical with the original cholera spirillum, and that the differences noted in cultures and microscopic specimens may be simply due to the modifications undergone by the latter after long residence in artificial culture media. Finkelnburg ("Zur Frage der Variabilität der Cholera bacillen," *ibid.* p. 113) has made careful comparative studies of cholera bacilli obtained from different centres during the recent cholera epidemic. He found that whereas those obtained from Paris and Hamburg respectively were practically identical, they presented slight but distinct deviations from the laboratory specimen of Koch's spirillum originally brought from India. Finkelnburg points out as the result of his investigations that in the course of the many years during which this organism has been cultivated outside the human body and in foreign surroundings, it has apparently undergone a gradual attenuation, and that in this process of degeneration it has lost some of its vital energy. Whether it has also suffered a diminution in its toxic properties Finkelnburg has not yet determined, but concludes by emphasising the importance of such an inquiry as calculated to throw some light on the possible future attenuation of the virus during its residence in Europe.

MR. F. C. SELOUS, who has spent twenty years in South Central Africa, has now completed the book in which he describes his experiences in the country. Messrs. Rowland Ward and Co. will publish the work in the autumn.

A BOOK by Capt. Hayes on "The Points of the Horse," and dealing chiefly with equine conformation, will be published next month by Messrs. Thacker and Co.

THE trustees of the South African Museum have issued their report for the year 1892. Mr. Roland Trimen, F.R.S., the curator of the museum, reports favourably of the condition of the collection generally. The donations amount to 4857 specimens, presented by 90 donors, as against 4677 specimens presented by 105 donors in the year 1891. For a long time extended accommodation has been needed, and we are glad to note that the Parliament granted the application for a sum of £20,000 to satisfy the want. Designs for the new museum building have been invited, and the work will be proceeded with as soon as possible.

THE proceedings of the Bath Natural History and Antiquarian Field Club, No. 4, contains an article by the Rev. H. H. Winwood, on some deep-well borings made in Somerset and one or two other counties. A description is given of the thickness and nature of the beds pierced in each case.

THE second part of "Phycological Memoirs," being researches made in the botanical department of the British Museum, contains, among other papers, several notes on the morphology of the Fucaceæ. Mr. George Murray contributes a comparison of the marine floras of the warm Atlantic, Indian Ocean, and the Cape of Good Hope.

A SERIES of monographs dealing with the principal gold fields of Victoria are being prepared under the direction of Mr. A. W. Howitt, the Secretary for Mines. A report by Mr. E. J. Dunn, on the Bendigo gold-fields, forms one of the number. According to Mr. Dunn, the primary features of this gold-field are that the mass of silurian strata which he investigated is made up of bands auriferous to varying degrees or barren of gold, and that the whole of the strata are bent along certain lines into anticlinal folds with intervening synclinals. The report is illustrated by numerous plans, sections, and diagrams.

THE Scientific Society of the University College of Wales has issued its first report. Natural history specimens collected during the excursions have been identified, and the results recorded in the report furnish some useful information with regard to Welsh fauna and flora.

AT the meeting of the Russian Chemical Society on March 16 K. D. Khroushchoff, who is well known for his remarkable synthesis of hornblende and other minerals, made a communication to the effect that he also has obtained artificial diamonds in a way similar to that of Moissan. He prepared a carbonide of silver, Ag_2C, obtained by the heating of cuminate of silver. At the temperature of boiling, silver absorbs about six per cent. of carbon, which is given out on cooling. Cooling was effected rapidly, as by Moissan, so that a crust was formed which prevented the increase of volume of the metal, and produced a considerable interior pressure. It appeared that part of the dissociated carbon had the properties of diamond—the dust consisting of minute broken crystals and laminæ, colourless and transparent, strongly refracting light, quite isotropic, and scratching corundum; on combustion they give carbon dioxide, with an insignificant amount of ash. Diamond dust obtained in this way was shown to Prof. Beketoff the day after Moissan's communication had been received at St. Petersburg.

FURTHER interesting experiments with the electric furnace are described by M. Moissan in the current number of the *Comptes Rendus*. By attaching to the furnace a condensing tube of copper shaped like the letter U, and so constructed as to be surrounded by an outer jacket of cold water constantly changing under high pressure, M. Moissan has been enabled to distil and condense most of the elements which have hitherto been found so refractory. When a piece of metallic copper weighing over a hundred grams was placed in the inner crucible of the furnace and subjected to the arc furnished by a current of 350 ampères, brilliant flames shot forth from the apertures through which the carbon terminals were inserted. The flames were accompanied by copious yellow fumes, due to the combustion of the issuing vapour of copper in contact with the oxygen of the air. After the expiration of five minutes nearly thirty grams of copper had been volatilised. Under the cover of the furnace an annular deposit of globules of metallic copper was found, and upon examination of the condensing tube a large proportion of the volatilised copper was discovered condensed in almost a pure state. It has long been known that silver is volatile; it is now found that at the temperature of an arc of the above description silver may be brought to full ebullition in a few moments, and it distils with ease, condensing in the copper condenser in the form of small globules, whose size varies from that of small shot to spherules of microscopic dimensions, and a certain proportion is usually deposited in the form of arborescent fragments. Platinum fuses in a few minutes, and very soon after commences to volatilise, and condenses in the U-tube in brilliant little spheres and fine dust. Aluminium distils very readily, and condenses in the form of a grey powder, containing admixed spherules exhibiting brilliant metallic lustre. Tin likewise distils with facility, and the condensed product usually contains a considerable proportion of a curious fibrous variety of the metal. The distillation of gold in the electric furnace is particularly interesting. Abundant fumes of a light yellowish green colour are emitted at the electrode apertures, and the metal is deposited in the condenser in the form of a powder, exhibiting a beautiful purple sheen. The powder consists of minute regular spheres which, when examined under the microscope, appear to reflect the usual yellow colour of gold. Upon the under side of the cover of the furnace three distinct annular deposits are observed, the inner one consisting of yellow globules of considerable size, round which is a metallic deposit of smaller spheres of such a size as to reflect a bright red tint, and outside this is an annular sublimate of a deep purple colour. Manganese is remarkably volatile; a quantity of the metal weighing four hundred grams entirely volatilised in ten minutes. Iron is likewise readily distilled, and is deposited in the form of a grey powder, among which are interspersed numerous small particles exhibiting brilliant surfaces.

Not only are the metals capable of distillation at the temperature of the electric arc. Silicon rapidly volatilises and condenses in the copper condensing tube in minute spheres and dust. Carbon becomes almost immediately converted to graphite, which distils over into the condenser and deposits in the form of light semi-transparent plates, which by transmitted light exhibit a beautiful chestnut colour. Distilled carbon would thus appear to consist of the fourth variety of the element recently described by M. Berthelot. The refractory alkaline earths appear also to be capable of distillation in the electric furnace. The experiment succeeds best, however, with a more powerful arc. Employing an arc furnished by a current of a thousand ampères, M. Moissan has distilled one hundred grams of lime in five minutes, the vapour condensing in the copper tube like fine flour. Magnesia passes over somewhat more slowly than lime, but its distillation is one of the prettiest of these remarkable experiments, the tints assumed by the escaping fumes and the brilliance of the incandescent vapour being particularly striking.

Notes from the Marine Biological Station, Plymouth.—The following list completes the summary begun last week of the records given during the last six months of the breeding seasons of marine animals at Plymouth. Among Mollusca, the Prosobranchs *Littorina littoralis*, *Nassa reticulata*, *Buccinum* (*undatum*), *Purpura* (*lapillus*), *Murex erinaceus* and *Capulus hungaricus*, the Opisthobranchs *Lamellaria perspicua*, *Aplysia punctata*, *Philine aperta*, many Nudibranchs, and the Cephalopod *Loligo media*; among Crustacea, the Cladocera *Podon* and *Evadne*, various Cirripedia, the Leptostracan *Nebalia bipes*, several Amphipoda, the Schizopoda *Siriella jaltensis*, *Leptomysis mediterranea*, *Macromysis flexuosa* and *inermis*, *Schistomysis arenosa*, the Cumacean *Pseudocuma cercaria*, the Macrura *Crangon* (*vulgaris*), *fasciatus* and *sculptus*, *Palæmon* (*serratus*), *Palæmonetes varians*, *Pandalus annulicornis* and *brevirostris*, *Hippolyte Cranchii*, *Virbius varians*, *Pagurus lævis* and *Bernhardus*, *Galathea squamifera*, the Brachyura *Porcellana longicornis* and *platycheles*, *Carcinus* (*mænas*), *Portunus depurator*, *holsatus*, *arcuatus*, *marmoreus* and *pusillus*, *Cancer* (*pagurus*), *Pilumnus hirtellus*, *Xantho floridus* and *rivulosus*, *Eurynome* (*aspera*), *Stenorhynchus phalangium* and *tenuirostris*; among Echinodermata, *Echinus miliaris*, *Asterina gibbosa*, and *Amphiura elegans*; among Tunicata, *Botryllus violaceus* and *Styclopsis grossularia*; and among Cephalochordata, *Amphioxus lanceolatus*, have been recorded. It should also be mentioned that the following larvæ have been townetted in large numbers at certain periods:—Veligers, *Cyphonantes*, *Nauplii* and *Zoeæ*, and the various larvæ of Echinoderms.

The additions to the Zoological Society's Gardens during the past week include two Mozambique Monkeys (*Cercopithecus pygerythrus*, ♂ ♂) from East Africa, presented respectively by Mr. J. B. Tomkins and Mr. B. J. Travers; two Llamas (*Lama peruana*, ♂ ♀) from Peru, presented by Lady Meux, F.Z.S.; a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, presented by Mrs. Bason; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mr. Lewis Baily; a Cinereous Waxbill (*Estrelda cærulescens*), two Hooded Finches (*Spermestes cucullata*), a Grenadier Weaver Bird (*Euplectes oryx*) from West Africa, an Amaduvade Finch (*Estrelda amandava*), two Nutmeg Finches (*Munia punctularia*), a Black-headed Finch (*Munia malacca*) from India, presented by Mr. W. L. Jeffrey; two Greater Spotted Woodpeckers (*Dendrocapus major*) British, presented by Miss Miriam A. Birch Reynardson; two Alexandrine Parrakeets (*Palæornis alexandri*) from India, presented by Mr. Wyndham Gibbs; two Brazilian Tortoises (*Testudo tabulata*) from Trinidad, W.I., presented by Mr. J. S. Toppin; an Ocellated Skink (*Seps ocellatus*) from Malta, presented by Col. C. H. Rooke; two Infernal Snakes (*Boodon infernalis* jv.) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two Emus (*Dromæus novæ-hollandiæ*) from Australia, deposited; two Collared Fruit Bats (*Cynonycteris collaris*), a Burrhel Wild Sheep (*Ovis burrhel*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Comet Finlay (1886 VII.).—The ephemeris for this comet for this week is as follows:—

12h. *Paris. M.T.*

1893.		R.A. (app.) h. m. s.		Decl. (app.) ° ′
June 29	...	2 52 20	...	+14 8·7
30	...	2 57 5	...	14 31·9
July 1	...	3 1 49	...	14 54·6
2	...	6 33	...	15 16·9
3	...	11 16	...	15 38·6
4	...	16 0	...	15 59·9
5	...	20 42	...	16 20·6
6	...	3 25 24	...	+16 40·9

Stars having Peculiar Spectra.—On an examination of the stellar spectra photographs taken at Cambridge (U.S.A.) and Arequipa it has been found (*Astronomy and Astrophysics* for June) that several of them have spectra which qualify their designation as "interesting objects."

The following list we take from the note referred to:—

Designation	R.A. 1890. h. m.	Decl. 1890. ° ′	Mag.	Description.
B.D. +49°·41	0 12·2	+49 44	9·4	Type IV.
B.D. −13·893	4 24·5	−13 17	5·8	F line bright.
A.G.C. 5429	4 43·8	−36 23	7½	Type IV.
A.G.C. 11890	8 42·4	−29 21	7½	Type IV.
—	15 27·0	−71 32	—	Type III. (H lines bright).
A.G.C. 22838	16 47·9	−44 50	8·2	Type V. (bright lines).
Z.C. XVIII.h 56	18 3·3	−63 38	9½	Type III. (H lines bright).
A.G.C. 26129	18 59·7	−38 17	8½	Type IV.
B.D. −21°·6376	23 6·3	−21 32	9·0	Type IV.

All these stars, it will be noticed, with the exception of the first two, have a large southerly declination.

Photographic charts of the region about Z.C. XVIII.h 56 have confirmed the variability of this star. Among other results, photographs of U Virginis, V Boötis, S Geminorum, T Cassiopeiæ, R Piscis Australis et Geminorum, show that these stars give bright hydrogen lines.

The Sun's Motion through Space.—The methods that have generally been adopted for determining the direction of the sun's motion have been based on the same general principle and the position of the apex of the direction of motion has been deduced by analysing the proper motions of the stars. A new determination on different lines (a spectroscopic method) appears in the *Astronomical Journal* (No. 298), and in this Mr. A. D. Risteen, the writer, bases his method on the three assumptions (1) that the stars used in the computation have no tendency to drift in any particular direction; (2) that their absolute velocities do not depend upon their apparent positions in the heavens; (3) that their absolute velocities are not functions of their own directions. Another minor assumption is that the absolute velocity of a star is not a function of the star's brightness. The values he gets for the right ascension and declination are given in the following table, in which we include those of Bischoff, Ubaghs, L. Struve, and Stumpe.

	R.A.	Decl.
Bischoff ...	285·2 ...	+48·5
Ubaghs ...	262·4 ...	26·6
L. Struve ...	273·3 ...	27·3
Stumpe ...	285·1 ...	36·2
Risteen ...	218·0 ...	45·0

The value Risteen obtains shows that the method may prove a very valuable one in future when more stars can be included (here about 42 observation-equations are used), and the result he obtains shows that at any rate the *reality* of the sun's motion (the value he gets is 10·9 English statute miles per second), and that our present knowledge of the direction of this motion is at any rate approximate.

AN ASCENDING METEOR.—Prof. von Niessl has been investigating the path of the meteor that appeared on July 7, 1892, and was seen both in Austria and Italy. The result of this computation has shown that, undoubtedly, the path of the meteor at the *latter* end of its course (*Naturwissenschaftliche Wochenschrift*, No. 26) was directed upwards. The length of its path measured 1100 kilometres from its nearest approach to the earth surface (68 kilometres above the surface) to the point where it disappeared, which was at a height of 158 kilometres. This is about the first time that the path of a rising meteor has been so accurately investigated.

THE SATELLITES OF JUPITER.—In this column for March 30 of this year we referred briefly to the very important results that were being reaped by Prof. W. H. Pickering, with the help of Mr. Douglas, at Arequipa, with reference to the peculiar forms which the satellites were found to assume at different periods of their rotation. In the June number of *Astronomy and Astrophysics* we have before us a much more detailed account of these and later observations, which seem to have confirmed those made previously in nearly all respects. In this article, which is of some length, the author deals first with the third satellite, the largest and most easily observed of the group. The results of twelve series of observations, taken on seven different nights, each series consisting of six independent observations, gave the value of $-10°·5$ for the position angle of the major axis, the satellite being on the eastern side of its orbit, and presenting an elliptical disc. The observations for the elliptical phase at the western side were not very satisfactory, owing to bad meteorological conditions, but the results suggested that "they would imply a revolution of the axis about the line perpendicular to the orbital plane, in about the same period as the satellites' rotation upon the axis itself." With regard to the surface features, there seems to be a marking having the appearance of a fork, the angle of the prongs varying from 30° to 60°. Sometimes this forked-shaped feature is turned to the left and sometimes to the right, and occasionally a double fork is seen. The position angle of the axis of the belt gave a value of $+15°·5$, and when the values obtained on January 1 and 16 are compared with those attained for the major axis on the same dates, they indicate that the two axis are inclined at between 46° to 35° to one another. The attempt to determine the direction and period of rotation indicated that perhaps the period of rotation coincided with that of the revolution of the satellite in its orbit. The surface features on the first satellite consisted of the bands lying in an approximately north and south direction, that on the second of a small patch detected only upon one occasion, and that on the fourth of a broad band (sometimes a narrow line), and also a bright spot recorded several times at the North Pole and once near the south. Later determinations of the period of rotation of the second satellite confirmed the earlier value (41h. 24m.), but sometimes discrepancies in the time of the flattening of the disc still occurred. The direction and period of rotation of satellite 4 has not been determined, but its disc has been recorded upon fourteen different dates as being shortened in the direction of the plane of its orbit, and upon eleven other days as being circular in form.

After summing up the main facts with regard to these satellites respecting their small density, directions of rotation, changes of shape, &c., Prof. Pickering shows how Laplace's "ring theory" with the following premises, suits the facts:—

(1) Jupiter was formerly surrounded by a series of rings similar to those now surrounding Saturn.

(2) The direction of rotation of these rings was direct, like that of the planet.

(3) By some force, whose cause is not explained, they were shattered, their components uniting, but still retaining the same orbit.

(4) Like the original rings, each satellite still consists of a swarm of meteorites, their consolidation having been prevented by the enormous tides produced in them by their primary.

At the conclusion of this discussion, in which Prof. Pickering takes each point individually, he has drawn up a syllabus regarding the points to which an observer can be most profitably directed in the case of each satellite, subdividing them into grades according to the difficulty of the observations.

TURACIN: A REMARKABLE ANIMAL PIGMENT CONTAINING COPPER.[1]

THE study of natural colouring matters is at once peculiarly fascinating and peculiarly difficult. The nature of the colouring matters in animals and plants, and even in some minerals (ruby, sapphire, emerald, and amethyst, for example) is still, in the majority of cases, not completely fathomed.

Animal pigments are generally less easily extracted and are more complex than those of plants. They appear invariably to contain nitrogen—an observation in accord with the comparative richness in that element of animal cells and their contents. Then, too, much of the colouration of animals, being due to microscopic structure, and therefore having a mechanical and not a pigmentary origin, differs essentially from the colouration of plants. Those animal colours which are primarily due to structure do, however, involve the presence of a dark pigment —brown or black—which acts at once as a foil and as an absorbent of those incident rays which are not reflected.

Many spectroscopic examinations of animal pigments have been made. Except in the case of blood and bile pigments very few have been submitted to exhaustive chemical study. Spectral analysis, when uncontrolled by chemical, and when the influence of the solvent employed is not taken into account, is very likely to mislead the investigator. And, unfortunately, the non-crystalline character of many animal pigments, and the difficulty of purifying them by means of the formation of salts and of separations by the use of appropriate solvents, oppose serious obstacles to their elucidation. Of blood-red or hæmoglobin it cannot be said that we know the centesimal composition, much less its molecular weight. Even of hæmatin the empirical formula has not yet been firmly established. The group of black and brown pigments to which the various melanins belong still await adequate investigation. We know they contain nitrogen (8½ to 13 per cent.), and sometimes iron, but the analytical results do not warrant the suggestion of empirical formulæ for them. The more nearly they appear to approach purity, the freer the majority of them seem from any fixed constituent such as iron or other metal. It is to be regretted that Dr. Krukenberg, to whom we are indebted for much valuable work on several pigments extracted from feathers, has not submitted the interesting substances he has described to quantitative chemical analysis.

I must not, however, dwell further upon these preliminary matters. I have introduced them mainly in order to indicate how little precise information has yet been gathered as to the constitution of the greater number of animal pigments, and how difficult is their study.

And now let me draw your attention to a pigment which I had the good fortune to discover, and to the investigation of which I have devoted I am afraid to say how many years.

It was so long ago as the year 1866 that the solubility in water of the red colouring matter in the wing-feathers of a plantain-eater was pointed out to me. [One of these feathers,

[1] A discourse delivered at the Royal Institution by Prof. A. H. Church, F.R.S.

freed from grease, was shown to yield its pigment to pure water.] I soon found that alkaline liquids were more effective solvents than pure water, and that the pigment could be precipitated from its solution by the addition of an acid. [The pigment was extracted from a feather by very dilute ammonia, and then precipitated by adding excess of hydrochloric acid.] The next step was to filter off the separated colouring matter, and to wash and dry it. The processes of washing and drying are tedious, and cannot be shown in a lecture. But the product obtained was a solid of a dark crimson hue, non-crystalline, and having a purple semi-metallic lustre. I named it "turacin" (in a paper published in a now long defunct periodical, *The Student and Intellectual Observer*, of April, 1868). The name was taken from "Turaco," the appellation by which the plantain-eaters are known—the most extensive genus of this family of birds being *Turacus*.

From the striking resemblance between the colour of arterial blood and that of the red touraco feathers I was led to compare their spectra. Two similar absorption bands were present in both cases, but their positions and intensities differed somewhat. Naturally I sought for iron in my new pigment. I burnt a portion, dissolved the ash in hydrochloric acid, and then added sodium acetate and potassium ferrocyanide. To my astonishment I got a precipitate, not of Prussian blue, but of Prussian brown. This indication of the presence of copper in turacin was confirmed by many tests, the metal itself being also obtained by electrolysis. It was obvious that the proportion of copper present in the pigment was very considerable—greatly in excess of that of the iron (less than ½ per cent.) in the pigment of blood.

Thus far two striking peculiarities of the pigment had been revealed, namely, its easy removal from the web of the feather, and the presence in it of a notable quantity of copper. Both facts remain unique in the history of animal pigments. The solubility was readily admitted on all hands, not so the presence of copper. It was suggested that it was derived from the Bunsen burner used in the incineration, or from some preservative solution applied to the bird-skins. And it was asked, "How did the copper get into the feathers?" The doubters might have satisfied themselves as to copper being normally and invariably present by applying a few easy tests, and by the expenditure of half-a-crown in acquiring a touraco wing. My results were, however, confirmed (in 1872) by several independent observers, including Mr. W. Crookes, Dr. Gladstone, and Mr. Greville Williams. And in 1873 Mr. Henry Bassett, at the request of the late Mr. J. J. Monteiro, pushed the inquiry somewhat further. I quote from Monteiro's "Angola and the River Congo," published in 1875 (vol. ii. pp. 75-77):—"I purchased a large bunch of the red wing-feathers in the market at Sierra Leone, with which Mr. H. Bassett has verified Prof. Church's results conclusively," &c., &c. Mr. Bassett's results were published in the *Chemical News* in 1873, three years after the appearance of my research in the Phil. Trans. As concentrated hydrochloric acid removes no copper from turacin, even on boiling, the metal present could not have been a mere casual impurity; as the proportion is constant in the turacin obtained from different species of touraco, the existence of a single definite compound is indicated. The presence of traces of copper in a very large number of plants, as well as of animals, has been incontestably established. And, as I pointed out in 1868, copper can be readily detected in the ash of banana fruits, the favourite food of several species of the "turacin-bearers." The feathers of a single bird contain on the average two grains of turacin, corresponding to ·14 of a grain of metallic copper; or, putting the amount of pigment present at its highest, just one-fifth of a grain. This is not a large amount to be furnished by its food to one of these birds once annually during the season of renewal of its feathers. I am bound, however, to say that in the blood and tissues of one of these birds, which I analysed immediately after death, I could not detect more than faint traces of copper. The particular specimen examined was in full plumage; I conclude that the copper in its food, not being then wanted, was not assimilated.

Let us now look a little more closely at these curious birds themselves. Their nearest allies are the cuckoos, with which they were formerly united by systematists. It has, however, been long conceded that they constitute a family of equal rank with the Cuculidæ. According to the classification adopted in the Natural History Museum, the order Picariæ contains eight suborders, the last of which, the Coccyges, consist of two families, the Cuculidæ and the Musophagidæ. To the same order belong the Hoopoes, the Trogons, the Woodpeckers. The plantain-eaters, or Musophagidæ, are arranged in six genera and comprise twenty-five species. In three genera—Turacus, Gallirex, and Musophaga—comprising eighteen species, and following one another in zoological sequence, turacin occurs; from three genera (seven species)—Corythæola, Schizorhis, and Gymnoschizorhis—the pigment is absent. [The coloured illustrations to H. Schlegel's Monograph (Amsterdam, 1860) on the Musophagidæ were exhibited.] The family is confined to Africa—eight of the turacin-bearers are found in the west sub-region, one in the south-west, two in the south, two in the south-east, four in the east, two in the central, and two in the north-east. It is noteworthy that, in all these sub-regions save the south-east, turacin-bearers are found along with those plantain-eaters which do not contain the pigment. Oddly enough two of the latter species, *Schizorhis africana* and *S. zonura*, possess white patches destitute of pigment in those parts of the feathers which in the turacin-bearers are crimson. These birds do not—I will not say cannot—decorate these bare patches with this curiously complex pigment. [Some extracts were here given from the late Mr. Monteiro's book on Angola, vol. ii. pp. 74-79, and from letters by Dr. B. Hinde. These extracts contained references to curious traits of the Touracos.]

Usually from twelve to eighteen of the primaries or metacarpo-digitals and of the secondaries or cubitals amongst the wing-feathers of the turacin-bearers have the crimson patches in their web. Occasionally the crimson patches are limited to six or seven of the eleven primaries. I have observed this particularly with the violet plantain-eater (*Musophaga violacæa*). In these cases the crimson head-feathers, which also owe their colour to turacin, are few in number, as if the bird, otherwise healthy, had been unable to manufacture a sufficiency of the pigment. I may here add that the red tips of the crest-feathers of *Turacus meriani* also contain turacin.

In all the birds in which turacin occurs this pigment is strictly confined to the red parts of the web, and is there unaccompanied by any other colouring matter. It is therefore found that if a single barb from a feather be analysed, its black base and its black termination possess no copper, while the intermediate portion gives the blue-green flash of copper when incinerated in the Bunsen flame. [A parti-coloured feather was burnt in the Bunsen flame with the result indicated.]

Where it occurs turacin is homogeneously distributed in the barbs, barbicels, and crochets of the web, and is not found in granules or corpuscles.

To the natural question, "Does turacin occur in any other birds besides the touracos?" a negative answer must at present be given. At least my search for this pigment in scores of birds more or less nearly related to the Musophagidæ has met with no success. In some of the plantain-eaters (species of Turacus and Gallirex) there is, however a second pigment closely related to turacin. It is of a dull grass-green colour, and was named turacoverdin by Dr. Krukenberg in 1881. I had obtained this pigment in 1868 by boiling turacin with a solution of caustic soda, and had figured its characteristic absorption band in my first paper (Phil. Trans., vol. clix., 1870, p. 630, fig. 4). My product was, however, mixed with unaltered turacin. But Dr. Krukenberg obtained what certainly seems to be the same pigment from the green feathers of *Turacus corythaix* by treating them with a two per cent. solution of caustic soda. I find, however, that a solution of this strength dissolves, even in the cold, not only a brown pigment associated with turacoverdin, but ultimately the whole substance of the web. By using a much weaker solution of alkali (1 part to 1000 of water) a far better result is obtained. [The characteristic absorption band of turacoverdin, which lies on the less refrangible side of D, was shown; also the absorption bands of various preparations of turacin.] I have refrained from the further investigation of turacoverdin, hoping that Dr. Krukenberg would complete his study of it. At present I can only express my opinion that it is identical with the green pigment into which turacin when moist is converted by long exposure to the air, or by ebullition with soda, and which seems to be present in traces in all preparations of *isolated* turacin, however carefully prepared.

A few observations may now be introduced on the physical and chemical characters of turacin. It is a colloid of colloids. And it enjoys in a high degree one of the peculiar properties of colloids—that of retaining when freshly precipitated, an immense proportion of water. Consequently when its solution in ammonia is precipitated by an acid, the coagulum formed is very voluminous. [The experiment was shown.] One gram of turacin is capable of forming a semi-solid mass with 600 grams of water. Another character which turacin shares with many other colloids is its solubility in pure water and its insolubility in the presence of mere traces of saline matter. It would be tedious to enumerate all the observed properties of turacin, but its deportment on being heated, and the action of sulphuric acid upon it, demand particular attention.

At 100° C., and at considerably higher temperatures, turacin suffers no change. When, however, it is heated to the boiling-point of mercury it is wholly altered. No vapours are evolved, but the substance becomes black and is no longer soluble in alkaline liquids, nor, when still more strongly heated afterwards can it be made to yield the purple vapours which unchanged turacin gives off under the same circumstances. This peculiarity of turacin caused great difficulty in its analysis. For these purple vapours contain an organic crystalline compound in which both nitrogen and copper are present, and which resists further decomposition by heat. [Turacin was so heated as to show its purple vapours, and also the green flame with which they burn.] This production of a volatile organic compound of copper is perhaps comparable with the formation of nickel and ferro-carbonyl.

The action of concentrated sulphuric acid upon turacin presents some remarkable features. The pigment dissolves with a fine crimson colour and yields a new compound, the spectrum of which presents a very close resemblance to that of hæmatoporphyrin [turacin was dissolved in oil of vitriol; the spectrum of an ammoniacal solution of the turacoporphyrin thus produced was also shown], the product obtained by the same treatment from hæmatin; in other respects also this new derivative of turacin, which I call turacoporphyrin, reminds one of hæmatoporphyrin. But, unlike this derivative of hæmatin, it seems to retain some of its metallic constituent. The analogy between the two bodies cannot be very close, for if they were so nearly related as might be argued from the spectral observations, hæmatin ought to contain not more, but less metal than is found to be present therein.

The percentage composition of turacin is probably carbon 53·69, hydrogen 4·6, copper 7·01, nitrogen 6·96, and oxygen 27·74. These numbers correspond pretty nearly to the empirical formula $C_{82}H_{81}Cu_2N_9O_{32}$; but I lay no stress upon this expression.

I have before said that copper is very widely distributed in the animal kingdom. Dr. Giunti, of Naples, largely extended (1881) our knowledge on this point. I can hardly doubt that this metal will be found in traces in all animals. But, besides turacin, only one organic copper-compound has been as yet recognised in animals. This is a respiratory, and not a mere decorative pigment like turacin. Léon Fredericq discovered this substance, called hæmocyanin. It has been observed in several genera of Crustacea, Arachnida, Gastropoda, and Cephalopoda. I do not think it has ever been obtained in a state of purity, and I cannot accept for it the fantastic formula—$C_{867}H_{1369}CuS_4O_{258}$—which has recently been assigned to it. On the other hand, I do not sympathise with the doubts as to its nature which F. Heim has recently formulated in the *Comptes Rendus*.

It is noteworthy in connection with the periodic law that all the essential elements of animal and vegetable organic compounds have rather low atomic weights, iron, manganese, and copper representing the superior limit. Perhaps natural organic compounds containing manganese will some day be isolated, but at present such bodies are limited to a few containing iron, and to two—hæmocyanin and turacin—of which copper forms an essential part.

If I have not yet unravelled the whole mystery of the occurrence and properties of this strange pigment, it must be remembered that it is very rare and costly, and withal difficult to prepare in a state of assured purity. It belongs, moreover, to a class of bodies which my late master, Dr. A. W. von Hofmann, quaintly designated as "dirts" (a magnificent dirt, truly!)—substances which refuse to crystallise, and cannot be distilled. I have experienced likewise during the course of this investigation, frequent reminders of another definition propounded by the same great chemist when he described organic research as "a more or less circuitous route to the sink"!

I am very glad to have had the opportunity of sharing with an audience in this institution the few glimpses I have caught from time to time during the progress of a tedious and still incomplete research into the nature of a pigment which presents physiological and chemical problems of high, if not of unique, interest.

Let my last word be a word of thanks. I am indebted to several friends for aid in this investigation, and particularly to Dr. MacMunn, of Wolverhampton, the recognised expert in the spectroscopy of animal pigments.

ARTIFICIAL IMMUNITY AND TYPHOID FEVER.

THE announcement by Metchnikoff of his beautiful theory of the "mechanism," as it were, of immunity, which he conceives as dependent upon the activity of the phagocytes or migratory cells of the body in the presence of disease germs, has called forth an immense number of researches in this direction from all parts of the world. But whilst some bacteriologists are engaged upon studying critically the experimental evidence which can be adduced in support of this theory, others are busy with the practical side of the subject and are devoting themselves to the investigation of what substances are capable of conferring immunity upon animals towards any particular disease, and hardly a month passes without some contribution being made to this important inquiry. The great discovery made by Behring that the blood serum of animals rendered artificially immune against a particular disease will, on being introduced into other animals, protect them from an attack of that particular disease, has been confirmed in the case of tetanus or lockjaw by Behring and Kitasato, and as regards diphtheria by Behring. In a more recent contribution Brieger, Kitasato, and Wassermann ("Ueber Immunität und Giftfestigung," *Zeitschrift für Hygiene*, vol. xii. 1892) have, amongst other investigations, succeeded in protecting and healing mice from the evil effects of inoculations with the typhoid bacillus by the introduction of serum obtained from a guinea-pig immune against typhoid. The further study of immunity with reference to this disease is the subject of two elaborate memoirs in the *Annales de l'Institut Pasteur*, November, 1892, by Sanarelli in Siena, and Chantemesse and Widal in Paris, and the ground covered by these two investigations is to a great extent identical. Sanarelli selected guinea-pigs as the subjects for his experiments, these animals being, as is well known, more difficult to protect from the fatal results of typhoid inoculations than mice. He states that if 0·5 c.c. of therapeutic serum be simultaneously introduced with an otherwise fatal dose of a typhoid culture, these animals *without exception* develop no typhoid symptoms, whilst guinea-pigs inoculated with an equally fatal dose of typhoid, but without the curative serum, invariably die. Chantemesse and Widal have pursued the inquiry still further, and have investigated the properties of serum taken from normal animals—that is to say, from animals which have not been infected with or rendered artificially immune from typhoid. Investigations similar to those made previously by Stern have also been conducted with human serum obtained from patients who have recovered from typhoid fever and also from those who have never been attacked by this malady.

Chantemesse and Widal state that whereas the serum derived from typhoid patients and from immune animals invariably confers protection upon infected animals, that obtained from normal animals and from people who have never had typhoid, only exceptionally exercises any curative power. These authors have also compared the degree of immunity induced in animals by the inoculation of curative serum and sterilised cultures of the typhoid bacillus respectively. This latter process is another method of protecting animals against infection, and was resorted to before the experiments with serum were made. It was found that whilst the serum acts rapidly, and confers immunity when administered in small quantities, its protective power only extends over a short period of time, apparently disappearing in less than a month. The sterilised typhoid cultures on the other hand, although working more slowly and requiring to be introduced in larger doses than the serum, endow the animal with immunity over a longer space of time, animals having been found immune even after the lapse of two months. Finally, attempts were made to arrest the progress of typhoid fever in people by the inoculation of therapeutic serum obtained from guinea pigs. So far, however, these investigations have not been successful, and if it be remembered that one point of cardinal importance in the production of immunity or in healing the disease is the time which elapses between the infection and the protective inoculation, that the action of the latter is the more rapid and the more successful the sooner it follows upon the former, it is at once apparent where, at any rate, some of the difficulties lie in its successful application to human beings. Whereas the exact moment is known when the experimental infection in the animal takes place, in the human subject days or weeks may pass between the infection and the declaration of the disease.

THE CENTENARY OF GILBERT WHITE.

THE wonted tranquillity of the little Hampshire village of Selborne was disturbed on Saturday by the invasion of a band of pilgrims who came to look upon the shrine of Gilbert White, and by the sight obtained a renewed love of nature. Drawn by a feeling of regard, members of the Selborne Society, and other disciples of White, congregated from all parts of the country, and paid homage to their master. Never within the memory of the oldest inhabitant had so many people been gathered together at Selborne, and we doubt not that the villagers failed to realise what attraction there could be in a man whose characteristics, according to an old woman who remembered him, were that "he would walk about the lanes tap-tapping at the trees, and stooping every now and then to wipe the dust off his shoes." But one thing marred the enjoyment of Saturday's meeting. A band of gipsies, with a terrible barrel-organ, and all the paraphernalia of a country fair, had installed themselves not a stone's-throw from the house in which Gilbert White lived his peaceful life. And, worst of all, they possessed a steam-syren, the shriek and screech of which penetrated everywhere, even to the high Hangers, in which the Selborne naturalist supposed that swallows hibernated.

The Earl of Selborne presided at luncheon, and, in proposing "The Memory of Gilbert White," dwelt upon the sterling qualities of the man, and the remarkable character of his books dealing with the natural history and antiquities of Selborne. White's life was devoted to observing and recording natural productions and phenomena. He was gifted with shrewdness of discernment, and that one essential qualification of a true man of science—the power of faithfully chronicling all and every observation. It was thought by some that the naturalist whose centenary they were commemorating had nothing else to do but wander about, and observe the habits of birds, beasts, fishes, and insects; but that was a great mistake. He had to perform "the daily round, the common task" that falls to the lot of all, and diligently did he fulfil his duties.

Mr. Darwin proposed "Prosperity to the Selborne Society and its branches." In responding, Mr. Otter, one of the founders of the society, dwelt upon the fact that their object was to inculcate and foster a love of nature, and to wage war in defence of her beauties. To them the ruthless field-naturalist and the sporting collector of specimens were enemies.

Mr. Wakefield followed with a description of the good work done by the Thames Valley branch in preserving "beauty-spots" from jerry-builders and their kindred.

The Earl of Stamford, in proposing "Prosperity to the Hampshire Field Club," the members of which joined the London party at Selborne, remarked that he had found reason to believe that one of the figures shown in the quarto edition of White's book is a likeness of the author himself, hence it could no longer be said that no portrait of him was in existence. Mr. R. H. White, however, was of the opinion that the evidence was not of a positive character.

The question of a memorial to White was touched by the Earl of Selborne, but he thought that the best plan would be to "Look not on the picture, but the book," and leave that to be handed down to the end of time, for nothing more was needed to perpetuate the memory of the man. With this sentiment we by no means agree. A monument is not erected merely to prevent a man's name and deeds from sinking to oblivion. It should show to the people that he was one whom men delight to honour. We are apt to be far too prosaic in these matters, and to consider the raising of images and other memorials as more or less unnecessary conventionalities. This conviction has grown upon us because we have seen statues erected to comparatively obscure individuals time without number, while the works of men of science are unrecognised. It does not say much for the naturalists of this country if the centenary of Gilbert White is allowed to pass without some tangible illustration being given of their regard for the father of them all.

INTERFERENCE BANDS AND THEIR APPLICATIONS.[1]

THE formation of the interference bands, known as Newton's rings, when two slightly curved glass plates are pressed into contact, was illustrated by an acoustical analogue. A high-pressure flame B (Fig. 1) is sensitive to sounds which reach it in the direction EB, but is insensitive to similar sounds which reach it in the nearly perpendicular direction AB. A is a "bird-call," giving a pure sound (inaudible) of wave-length (λ) equal to about 1 cm.; C and D are reflectors of perforated zinc. If C acts alone, the flame is visibly excited by the waves reflected from it, though by far the greater part of the energy is transmitted. If D, held parallel to C, be then brought into action, the result depends upon the interval between the two partial reflectors. The reflected sounds may co-operate, in which case the flame flares vigorously; or they may interfere, so that the flame recovers, and behaves as if no sonnd at all were falling upon it. The first effect occurs when the reflectors are close together, or are separated by any multiple of $\frac{1}{2}\sqrt{2}.\lambda$; the

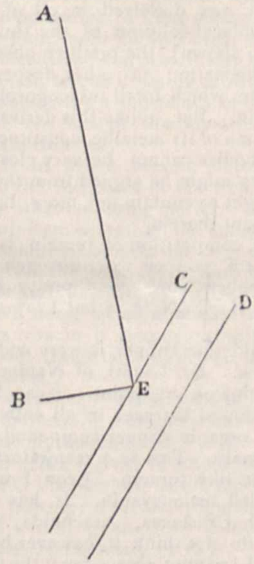


FIG. 1.

second when the interval is midway between those of the above-mentioned series, that is, when it coincides with an odd multiple of $\frac{1}{4}\sqrt{2}.\lambda$. The factor $\sqrt{2}$ depends upon the obliquity of the reflection.

The coloured rings, as usually formed between glass plates, lose a good deal of their richness by contamination with white light reflected from the exterior surfaces. The reflection from the hindermost surface is easily got rid of by employing an opaque glass, but the reflection from the first surface is less easy to deal with. One plan, used in the lecture, depends upon the use of slightly wedge-shaped glasses (2°) so combined that the exterior surfaces are parallel to one another, but inclined to the interior operative surfaces. In this arrangement the false light is thrown somewhat to one side, and can be stopped by a screen suitably held at the place where the image of the electric arc is formed.

The formation of colour and the ultimate disappearance of the bands as the interval between the surfaces increases, depends upon the mixed character of white light. For each colour the bands are upon a scale proportional to the wave-length for that colour. If we wish to observe the bands when the interval is

[1] Abstract of a lecture delivered at the Royal Institution, on Friday, March 24, 1893, by Lord Rayleigh.

considerable—bands of high interference as they are called—the most natural course is to employ approximately homogeneous light, such as that afforded by a soda flame. Unfortunately, this light is hardly bright enough for projection upon a large scale.

A partial escape from this difficulty is afforded by Newton's observations as to what occurs when a ring system is regarded through a prism. In this case the bands upon one side may become approximately achromatic, and are thus visible to a tolerably high order, in spite of the whiteness of the light. Under these circumstances there is, of course, no difficulty in obtaining sufficient illumination; and bands formed in this way were projected upon the screen.[1]

The bands seen when light from a soda flame falls upon nearly parallel surfaces have often been employed as a test of flatness. Two flat surfaces can be made to fit, and then the bands are few and broad, if not entirely absent; and, however the surfaces may be presented to one another, the bands should be straight, parallel, and equi-distant. If this condition be violated, one or other of the surfaces deviates from flatness. In Fig. 2, A and B represent the glasses to be tested, and C is a lens of two or three feet focal length. Rays diverging from a soda flame at E are rendered parallel by the lens, and after reflection from the surfaces are re-combined by the lens at E. To make an observation, the coincidence of the radiant point and its image must be somewhat disturbed, the one being displaced to a position a little beyond, and the other to a position a little in front of, the diagram.

The eye, protected from the flame by a suitable screen, is placed at the image, and being focused upon AB, sees the field

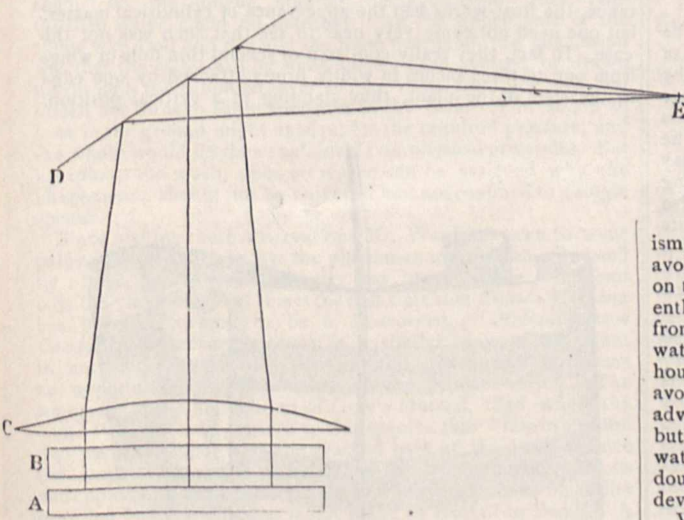


FIG. 2.

traversed by bands. The reflector D is introduced as a matter of convenience to make the line of vision horizontal.

These bands may be photographed. The lens of the camera takes the place of the eye, and should be as close to the flame as possible. With suitable plates, sensitised by cyanin, the exposure required may vary from ten minutes to an hour. To get the best results, the hinder surface of A should be blackened, and the front surface of B should be thrown out of action by the superposition of a wedge-shaped plate of glass, the intervening space being filled with oil of turpentine or other fluid having nearly the same refraction as glass. Moreover, the light should be purified from blue rays by a trough containing solution of bichromate of potash. With these precautions the dark parts of the bands are very black, and the exposure may be prolonged much beyond what would otherwise be admissible.

The lantern slides exhibited showed the elliptical rings indicative of a curvature of the same sign in both directions, the hyperbolic bands corresponding to a saddle-shaped surface, and the approximately parallel system due to the juxtaposition of two telescopic "flats," kindly lent by Mr. Common. On other plates were seen grooves due to rubbing with rouge along defined track, and depressions, some of considerable regularity, obtained by the action of diluted hydrofluoric acid, which was allowed to stand for some minutes as a drop upon the surface of the glass.

By this method it is easy to compare one flat with another, and thus, if the first be known to be free from error, to determine the errors of the second. But how are we to obtain and verify a standard? The plan usually followed is to bring *three* surfaces into comparison. The fact that two surfaces can be made to fit another in all azimuths proves that they are spherical and of equal curvatures, but one convex and the other concave, the case of perfect flatness not being excluded. If A and B fit another, and also A and C, it follows that B and C must be similar. Hence, if B and C also fit one another, all three surfaces must be flat. By an extension of this process the errors of three surfaces which are not flat can be found from a consideration of the interference bands which they present when combined in three pairs.

But although the method just referred to is theoretically complete, its application in practice is extremely tedious, especially when the surfaces are not of revolution. A very simple solution of the difficulty has been found in the use of a free surface of water, which, when protected from tremors and motes, is as flat as can be desired.[1] In order to avoid all trace of capillary curvature it is desirable to allow a margin of about $1\frac{1}{2}$ inch. The surface to be tested is supported horizontally at a short distance ($\frac{1}{10}$ or $\frac{1}{20}$ inch) below that of the water, and the whole is carried upon a large and massive levelling stand. By the aid of screws the glass surface is brought into approximate parallelism with the water. In practice the principal trouble is in the avoidance o. tremors and motes. When the apparatus is set up on the floor of a cellar in the country, the tremors are sufficiently excluded, but care must be taken to protect the surface from the slightest draught. To this end the space over the water must be enclosed almost air-tight. In towns, during the hours of traffic, it would probably require great precaution to avoid the disturbing effects of tremors. In this respect it is advantageous to diminish the thickness of the layer of water; but if the thinning be carried too far, the subsidence of the water surface to equilibrium becomes surprisingly slow, and a doubt may be felt whether after all there may not remain some deviation from flatness due to irregularities of temperature.

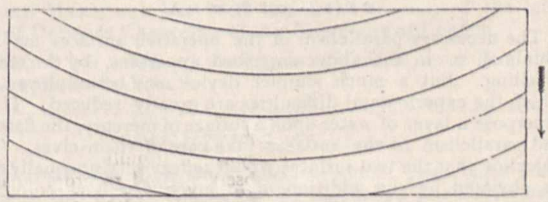


FIG. 3.

With the aid of the levelling screws the bands may be made as broad as the nature of the surface admits; but it is usually better so to adjust the level that the field is traversed by five or six approximately parallel bands. Fig. 3 represents bands actually observed from the face of a prism. That these are not straight, parallel, and equi-distant is a proof that the surface deviates from flatness. The question next arising is to determine the direction of the deviation. This may be effected by observing the displacement of the bands due to a known motion of the levelling screws; but a simpler process is open to us. It is evident that if the surface under test were to be moved downwards parallel to itself, so as to increase the thickness of the layer of water, every band would move in a certain direction, viz. *towards* the side where the layer is thinnest. What amounts to the same, the retardation may be increased, without touching the apparatus, by so moving the eye as to *diminish* the obliquity of the reflection. Suppose, for example, in Fig. 3 that the movement in question causes the bands to travel downwards, as indicated by the arrow. The inference is that the surface is concave. More glass must be removed at the ends of the bands than in the middle in order to straighten them. If the object be to correct the errors by local polishing operations

[1] The theory is given in a paper upon "Achromatic Interference Bands," *Phil. Mag.*, August 1889.

[1] The diameter would need to be 4 feet in order that the depression at the circumference, due to the general curvature of the earth, should amount to $\frac{1}{20}\lambda$.

upon the surface, the rule is that *the bands, or any parts of them, may be rubbed in the direction of the arrow.*

A good many surfaces have thus been operated upon; and although a fair amount of success has been attained, further experiment is required in order to determine the best procedure. There is a tendency to leave the marginal parts behind; so that the bands, though straight over the greater part of their length, remain curved at their extremities. In some cases hydrofluoric acid has been resorted to, but it appears to be rather difficult to control.

The delicacy of the test is sufficient for every optical purpose. A deviation from straightness amounting to $\frac{1}{10}$ of a band interval could hardly escape the eye, even on simple inspection. This corresponds to a departure from flatness of $\frac{1}{20}$ of a wave-length in water, or about $\frac{1}{30}$ of the wave-length in air. Probably a deviation of $\frac{1}{100}\lambda$ could be made apparent.

For practical purposes a layer of moderate thickness, adjusted so that the two systems of bands corresponding to the duplicity of the soda line do not interfere, is the most suitable. But if we wish to observe bands of high interference, not only must the thickness be increased, but certain precautions become necessary. For instance, the influence of obliquity must be considered. If this element were absolutely constant, it would entail no ill effect. But in consequence of the finite diameter of the pupil of the eye, various obliquities are mixed up together, even if attention be confined to one part of the field. When the thickness of the layer is increased, it becomes necessary to reduce the obliquity to a minimum, and further to diminish the aperture of the eye by the interposition of a suitable slit. The effect of obliquity is shown by the formula

$$2t(1 - \cos\theta) = n\lambda.$$

The necessary parallelism of the operative surfaces may be obtained, as in the above-described apparatus, by the aid of levelling. But a much simpler device may be employed, by which the experimental difficulties are greatly reduced. If we superpose a layer of water upon a surface of mercury, the flatness and parallelism of the surfaces take care of themselves. The objection that the two surfaces would reflect very unequally may be obviated by the addition of so much dissolved colouring matter, *e.g.* soluble aniline blue, to the water as shall equalise the intensities of the two reflected lights. If the adjustments are properly made, the whole field, with the exception of a margin near the sides of the containing vessel, may be brought to one degree of brightness, being, in fact, all included within a fraction of a band. The width of the margin, within which rings appear, is about one inch, in agreement with calculation founded upon the known values of the capillary constants. During the establishment of equilibrium after a disturbance, bands are seen due to variable thickness, and when the layer is thin, persist for a considerable time.

When the thickness of the layer is increased beyond a certain point, the difficulty above discussed, depending upon obliquity, becomes excessive, and it is advisable to change the manner of observation to that adopted by Michelson. In this case the eye is focused, not, as before, upon the operative surfaces, but upon the flame, or rather upon its image at E (Fig. 2). For this purpose it is only necessary to introduce an eye-piece of low power, which with the lens C (in its second operation) may be regarded as a telescope. The bands now seen depend entirely upon obliquity according to the formula above written, and therefore take the form of circular arcs. Since the thickness of the layer is absolutely constant, there is nothing to interfere with the perfection of the bands except want of homogeneity in the light.

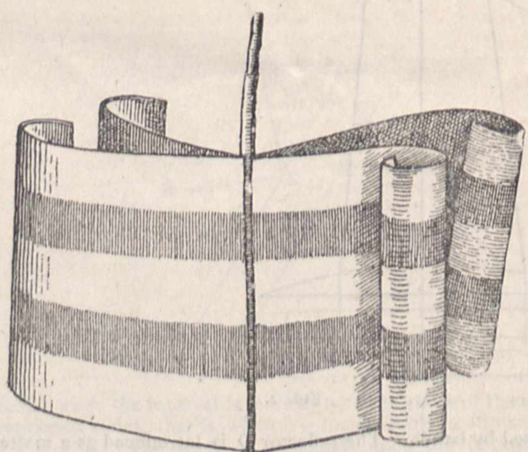
But, as Fizeau found many years ago, the latter difficulty soon becomes serious. At a very moderate thickness it becomes necessary to reduce the supply of soda, and even with a very feeble flame a limit is soon reached. When the thickness was pushed as far as possible, the retardation, calculated from the volume of liquid and the diameter of the vessel, was found to be 50,000 wave lengths, almost exactly the limit fixed by Fizeau.

To carry the experiment further requires still more homogeneous sources of light. It is well known that Michelson has recently observed interference with retardations previously unheard of, and with the aid of an instrument of ingenious construction has obtained most interesting information with respect to the structure of various spectral lines.

A curious observation respecting the action of hydrofluoric acid upon polished glass surfaces was mentioned in conclusion. After the operation of the acid the surfaces appear to be covered with fine scratches, in a manner which at first suggested the idea that the glass had been left in a specially tender condition, and had become scratched during the subsequent wiping. But it soon appeared that the effect was a *development* of scratches previously existent in a latent state. Thus parallel lines ruled with a knife edge, at first invisible even in a favourable light, became conspicuous after treatment with acid. Perhaps the simplest way of regarding the matter is to consider the case of a furrow with perpendicular sides and a flat bottom. If the acid may be supposed to eat in equally in all directions, the effect will be to *broaden* the furrow, while the depth remains unaltered. It is possible that this method might be employed with advantage to *intensify* (if a photographic term may be permitted) gratings ruled upon glass for the formation of spectra.

FROST FREAKS.

MR. LESTER F. WARD describes some remarkable frost figures in the current number of *The Botanical Gazette.* He says that on a bright frosty morning in December, 1892, Mr. Victor Mason and himself observed some white objects looking like icicles close to the ground, along the border of a pine wood. A closer examination showed that they were in truth nothing but ice, but that instead of icicles they were veritable freaks of frost. Every one was firmly attached to the stem of a small herbaceous plant which had succumbed to the season but still stood erect. The attachment was always close to the base, often at the very ground, sometimes an inch above. At a distance, the frost-works had the appearance of cylindrical masses, but one need not come very near to see that such was not the case. In fact, they really consisted of several thin foils or wings from one to three inches in width, firmly attached by one edge to the stem of the plant, thus standing in a vertical position.



From this attachment each of these little ice sheets projected out horizontally or with a slight upward tendency, not straight and stiff, but gently and gracefully curving or coiling into a beautiful conch-like roll at the distal margin. There were always several of these, usually three, four, or five, all attached to the same vertical portion of the stem but at regular intervals around it like the paddles of a flutter-wheel, but all curving in the same direction after the manner of a turbine-wheel. Thus, where there were four they stood with each pair opposite, as in the figure, which represents a side view. The amount of curving varied considerably, and the coil filled up most of the interval between the plates giving the object a compact appearance. The ice was white, opaque, and singularly light, as if consisting of congealed froth, but in all cases the scrolls bore horizontal stripes like those of a flag, resulting from degrees in the whiteness, varying from alabaster to nearly transparent. These stripes added greatly to the beauty of these singular objects. In some cases the inner margin, instead of being straight, was sinuous, giving a fluted character to the base of the wing. Many other peculiarities were noted in these evanescent toys, but they soon vanished.

But here is the chief wonder. There grew in the same situation some dozen or twenty small herbaceous plants of about the same general character which would all seem equally liable to exhibit such a phenomenon. There were species of Aster, Solidago, Chrysopsis, Pycnanthemum, Polygonum, Ludwigia, Sericocarpus, &c., and with these in considerable but not specially marked abundance, *Cunila Mariana*. The first frost-works seen were attached to this plant, which was supposed for a while to be an accident; but soon it was perceived that such was not the case, and an examination of hundreds of cases revealed the fact that they were exclusively confined to this species. No sign or semblance of them could be found on any other plant. They were, therefore, so far as observation went, a specific character, and it is this alone which prompted Mr. Ward to give the above account in the hope that others might be able to confirm or invalidate this induction by a wider one.

This plant persists after frost with all its branches, sere leaves, and empty seed vessels intact, so that its identity was as complete as in midsummer. The bark, which remained firm everywhere else, was seen to be longitudinally split into strips at the zone occupied by the frost-work, but as it could be seen between the several ice sheets, these rifts must have been covered by their bases. In other words, it cannot be doubted that the liquid matter out of which they were formed had passed through these longitudinal openings and been deposited by molecular accretions in the symmetrical forms observed. It was inferred from this that they might consist entirely of the juices of the plant, but on placing them on the tongue nothing distinguishable from pure distilled water could be detected. As the upper part of the stems was dead and dry and the roots perennial, the conclusion was that the water had by some agency been pressed or drawn up through the cambium layer of the roots from the soil and forced out through these apertures in the bark. The action of frost in the ground might account for the required pressure, and the whole would be thus explainable on physical principles. But it explains too much, since no reason can be assigned why the phenomenon should not be universal and not confined to a single species.

Since making these observations Mr. Ward has been to some pains to ascertain whether the phenomenon has been witnessed by others, but so far the inquiry has proved futile. It seems possible, therefore, that this is the first time that *Cunila Mariana* has been discovered to be a frost-weed. *Helianthemum Canadense*, however, behaves in a similar way. That plant is not common in the dittany and there has not been an opportunity to observe it at the proper season. The statement in the first edition of Gray's Manual, 1848, where the name "frost weed" is given to this species, that "late in autumn crystals of ice shoot from the cracked bark at the root, whence the popular name," repeated in all subsequent editions and copied into many other books, is doubtless founded on earlier recorded observations, but is not found in Nuttall or Pursh. A frost-figure also appears in Mr. Wm. Hamilton Gibson's recent book entitled "Sharp Eyes."[1] This figure is somewhat fanciful, being a vignette constituting the first letter of this chapter of his book and aiming to show all the parts of the plant in addition to the frost work. Although it is, according to this representation, a much less definite and less beautiful object than the dittany "frost-flowers," there can be no doubt that the principle on which it was formed is the same. The author's description of it as "fashioned into all sorts of whimsical feathery curls and flanges and ridges" indicates at once the inadequacy of his figure to do it justice, and the close analogy between it and the "frost flower" of Cunila.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

PROF. JEBB, M.P., in presenting the prizes and certificates on Tuesday to the students who successfully passed the last Cambridge local examination at Eastbourne centre, observed that thirty years ago examinations were believed to be a panacea for every educational defect. Now a reaction had set in, and some went so far as to hold that success in examinations afforded no trustworthy criterion of merit. The truth, of course, lay between these two extremes. An examination was not an infallible test, and was more favourable to some temperaments than to others; but, when well managed, was a sound test. An examiner must have at least three qualifications: he must know a great deal more than the subject in which he examined, or he would not have a proper sense of intellectual proportion and perspective; he must have a certain measure of acuteness to enable him to penetrate disguise or simulated knowledge; and, above all, he must have common sense in order to take proper account of particular circumstances of each case. The two older Universities, in the early part of the century, were said to be no longer in touch with the nation, and were regarded rather as great schools reserved for the education and, equally perhaps, the amusement of a select few; but now they had spread a network of examination, and were diffusing their influence over the country, becoming what they were in the Middle Ages, really national, but national in the higher sense, in the desire that every one who sought it should have the means of a liberal education, and that the best things which literature or science had to show should be placed within reach of all.

MR. ROBERT HOLT, late Assistant Lecturer in Engineering at University College, Liverpool, has been appointed Professor of Engineering at the People's Palace, London. Mr. Holt has held both Whitworth and National Scholarships, as well as one of the research scholarships founded by the Commissioners of the Exhibition of 1851.

AT a council meeting of the University College of Wales, Bangor, on June 21, a scheme for the supervision and residence of women students of the college next session was carried by a large majority.

LORD HERSCHEL has been appointed to succeed the late Earl of Derby as Chancellor of the University of London.

OXFORD has conferred the degree of D.C.L. upon Sir John B. Lawes, Bart., F.R.S.

SCIENTIFIC SERIAL.

Meteorologische Zeitschrift, May.—Rainfall probability and cloud in the United States, by W. Köppen. The author has submitted the rainfall charts published by the United States Government to a thorough investigation. The following are the generalised results as regards the distribution of rainfall:—(1) There is a district of continental summer rains, enclosed on both sides by littoral winter rains, which, corresponding to the contrast of the yearly oscillation of temperature, are much more marked in the west than in the east. (2) A district of isobaric summer rains, in the south-east, with equatorial sea-winds in summer, and with anticyclonic weather in winter. (3) Transition districts, in which both rainfall maxima occur near each other, while the minima occur in spring and autumn. Maxima after the equinoxes are nowhere very well marked, but the April and May rains of Colorado and Kansas and the autumn rains on Lake Superior are indications of them. With regard to the seasonal distribution in the tropical zone, the differences of temperature play only a small part compared to that of extra-tropical regions; this result naturally follows from the small variation of temperature in the tropics.—On the dynamics of the atmosphere, by M. Möller. This first part deals chiefly with the causes of the inversion of temperature with height, and with the cold experienced in the centres of areas of high barometric pressure. He deals especially with three causes of inversion:—The cooling of the lower strata by radiation, the effects on the higher strata by dynamic heating or cooling analogous to those caused by the action of Föhn winds, and the transference of warm air to the higher regions by horizontal winds coming from warmer parts. Various cases are separately considered from data afforded by mountain stations, such as Ben Nevis, and from discussions by Dr. Hann and others. Particular attention is also given to the formation and motions of clouds, as furnishing visible evidence of the processes in action in the higher strata of the atmosphere.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 8.—"Preliminary Report of the Joint Solar Eclipse Committee of the Royal Society, the Royal Astronomical Society, and the Solar Physics Committee on the

[1] New York, 1892. Article "The Frost Flower," pp. 210, 211.

Observations of the Solar Eclipse of April 16, 1893." Communicated by Dr. Common, F.R.S.

This report merely states the work undertaken by the British observers during the recent total solar eclipse, and the number and kind of photographs that were obtained. This information has appeared, from time to time, in these columns. A more detailed report, giving the results of the discussion of the pictures will shortly be published.

PARIS.

Academy of Sciences, June 12.—M. Lœwy in the chair.—Experimental verifications of the theory of weirs without lateral contraction, the sheet being free below, by M. J. Boussinesq.—On a simplification introduced into certain formulæ depending upon the resisting power of solids by introducing the greatest linear extension Δ which can be supported by the material, in the place of the corresponding elastic force R_0, by M. J. Boussinesq. In formulæ relating to the strength of elastic solids in motion, mechanicians as a rule introduce a quantity R_0 denoting the greatest tension which a fibre can sustain upon unit sectional area without breaking, instead of the maximum elongation Δ which does not endanger the texture. M. Boussinesq shows that many formulæ may be considerably simplified by introducing Δ. Thus the maximum velocity V which can be safely impressed upon an element of a solid under concussion is related to the velocity of sound in the solid and to Δ in a manner given by the formula $V = k\omega\Delta$, where k is a constant depending on the figure and mass of the solid, and ω is the velocity of sound in it. If V be the peripheral velocity of a flywheel in the form of a narrow ring with a large radius, the maximum safe velocity is given by the formula $V = \omega\sqrt{\Delta}$.—On various methods of observing the so-called anomalous focal properties of diffraction gratings, by M. A. Cornu.—On the extraction of zirconia and thorina, by M. L. Troost.—Study of some new phenomena of fusion and volatilisation produced by means of the heat of the electric arc, by M. Henri Moissan.—On Liouville's linear element surfaces, and surfaces with constant curvature, by M. Émile Waelsch.—On a general property of electric and magnetic fields, by M. Vaschy.—Study of the filtration of liquids, by M. R. Lezé. A porous vessel containing the liquid to be studied was placed in a test-tube and subjected to very rapid rotation. By a comparison of the weights of the porous vessel and its contents before and after rotation, the velocity of outflow through the porous walls due to centrifugal force was ascertained. Taking that of distilled water as unity, the figure for a five per cent. solution of sodium chloride was 1·023, for the nitrate 1·051, for ammonium sulphate 0·993. The velocity of efflux for alcohol solution showed a minimum at 40°, where it was 0·50. The numbers are those for a pressure of eight or ten atmospheres applied during ten minutes, during which the tubes travelled from 40 to 50 km.—On the combinations of molybdates and sulphurous acid, by M. E. Péchard.—On bromine-boracites; bromine compounds of iron and zinc, by MM. G. Rousseau and H. Allaire.—On fluorides of copper, by M. Poulenc.—Action of electricity upon the carburisation of iron by cementation, by M. Jules Garnier.—On the rotatory power of bodies belonging to an homologous series, by M. Ph. A. Guye. It is shown theoretically that if the schematic tetrahedron is slightly deformed, the rotatory powers of a homologous series of bodies must pass through a maximum.—On the rotatory powers of the ethers of valeric and glyceric acids, by MM. Ph. A. Guye and L. Chavanne. This paper contains experimental evidence supporting the conclusions of the previous paper.—Heat of formation of some derivatives of indigo, by M. R. d'Aladern.—On right-handed licareol, by M. Ph. Barbier.—A new apparatus for measuring the intensity of perfumes, by M. Eugène Mesnard. The instrument is based upon the property of essence of terebenthine of extinguishing the phosphorescence of phosphorus when mixed with the surrounding air in a certain minimum proportion. The phosphorescent body is a small piece of starch dipped into a concentrated solution of phosphorus in carbon bisulphide. After once determining the quantity of essence necessary to extinguish phosphorescence, the quantity of essence contained in air may be ascertained by passing sufficient of the air through the apparatus to produce extinction. This air is mixed with other air containing a known quantity of the essential oil or other perfume to be examined, and the odoriferous power of the latter is given by the quantity required to produce a "neutral" scent.—On the fertilisation of the Puccinicei, by M. Paul Vuillemin.—Magnesian chalk of the environs of Guise (Aisne), by M. H. Boursault.—On the cavern of Boundoulaou (Aveyron), by MM. E. A. Martel and Émile Rivière.—On the utilisation of the waste products of the vineyard, by M. A. Muntz.—Mode of action of the substances produced by microbes upon the circulatory apparatus, by MM. Charrin and Gley.—On a soluble derivative of β-naphthol, by MM. Dujardin-Beaumetz and Stackler.—On morbid intercurrencies in sulphate of quinine fevers, by M. Alcide Treille.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Primitive Music: R. Wallaschek (Longmans).—Life with Trans-Siberian Savages: B. D. Howard (Longmans).—Nineteen Charts of the Isle of Wight and Solent Tides from Portland Bill to the Owers: T. B. C. West and F. H. Collins (Potter).—Photography Annual, 1893 (Iliffe).—Lehrbuch der Zoologie, new edition: Dr. R. Hertwig (Jena, Fischer).—Das Kleine Botanische Practicum für Anfänger, new edition: Dr. E. Strasburger (Jena, Fischer).—Die Pilzgärten einiger Sudamerikanischer Ameisen: A. Möller (Jena, Fischer).—Smithsonian Meteorological Tables (Washington).—On the Chemistry of the Blood: L. C. Wooldridge (K. Paul).—Walks in the Ardennes, new edition: edited by P. Lindley (London).—On English Lagoons: P. H. Emerson (Nutt).

PAMPHLETS.—The Condition of the Western Farmer: A. F. Bentley (Baltimore).—Report of the Trustees of the South African Museum, 1892 (Cape Town).—Il Terremoto a Roma del 22 Gennaio, 1892: Dr. G. Agamennone (Roma).—The Brighton Life Table: Dr. A. Newsholme (Brighton).—Die Medicinische Electrotechnik und ihre Physikalischen Grundlagen: Dr. J. L. Hoorweg (Leipzig, Engelmann).—Ueber das Norian oder Ober-Laurentian von Canada: F. D. Adams (Stuttgart, Koch).—Geometrical Constructions for Cutting from a Cone of Revolution: E. A. Engler (St. Louis).

SERIALS.—Proceedings of the Bath Natural History and Antiquarian Field Club, Vol. vii. No. 4 (Bath).—Journal of the Polynesian Society, Vol. 2, No. 1 (Wellington).—Bulletin of the New York Mathematical Society, Vol. 2, No. 9 (New York, Macmillan).—Journal de Physique, June (Paris). Séances de la Société Française de Physique, November-December, 1892 (Paris).—Proceedings of the American Philosophical Society, Vol. xxxi. No. 140 (Philadelphia).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1893, Part 1, January-March (Philadelphia).—Bulletins de la Société d'Anthropologie, No. 5, June 15 (Paris, Masson).—Reale Istituto Lombardo di Scienze e Letture, Rendiconti, Serie 2, Vol. 26, fasc. xi.-xii. (Milano, Hoepli).—Phycological Memoirs: edited by G. Murray, Part 2 (Dulau).—Zeitschrift für Physikalische Chemie, xi. Band, 6 Heft (Leipzig, Engelmann).—The American Naturalist, June (Philadelphia).—Bulletin de la Société d'Encouragement pour l'Industrie Nationale, Avril (Paris).

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