THURSDAY, JULY 17, 1890.

THE INDIAN CIVIL SERVICE AND THE INDIAN FOREST SERVICE COMPETITIONS.

THOSE who devote attention to educational questions are looking with interest for the publication of the new schedule for the Indian Civil Service competitions. But past experience of the Civil Service Commissioners, who are largely responsible for these matters, and on whom the various departments must chiefly rely for the carrying out of their ideas, causes the interest of many of us to be not unmixed with a considerable degree of anxiety lest there should be in this case a repetition of the recent Woolwich and Sandhurst *fiascos*. Therefore, notwithstanding the favourable character of Sir John Gorst's recent reply to Sir Henry Roscoe, we hope that those at the Universities who are interested in the question, and the leaders in science, will not yet rest upon their oars, but that they will bring under the direct notice of the authorities at the India Office the present position of science studies at the Universities and the views that are held there on this important subject, in order that the latter, who we believe hold fair views upon the subject, may be in a position to judge of the fitness of any scheme that may be submitted to them and of its correspondence or the reverse with the present condition of higher education. We bring this subject again under the notice of our readers, partly because of its importance, and partly because in the new regulations for the India Forest Service we have recently been afforded a fresh example of the inability of those who are officially intrusted with these matters to properly estimate the requirements of the public services. These new regulations are, no doubt, better than those which they are intended to replace in several respects, notably so in that the absurd list of fourteen compulsory subjects by which this examination has hitherto been distinguished has now been abolished, and also in that the examinations will now run somewhat closely on the lines of the army competition—a change which will probably secure for them a wider field of candidates than they have hitherto had. But, considered as a method of selecting those who are most likely to do good work in a scientific profession, the scheme must be pronounced to be a failure, since it will neither insure the selection of the most promising men for the particular service required of them, nor, as many will think, encourage those who intend to compete to give themselves a really liberal education.

The subjects and their mark values are as set out below:

Class I.—Obligatory Subjects.

1. Mathematics, Elementary	2500	Not less than one-third of full marks in each of these subjects must be obtained to qualify.
2. English Composition	1000	
3. German	2000	

Class II.—Optional Subjects.

4. Mathematics, Higher	2000	Any two, but not more than two, of these subjects may be selected.
5. French	2000	
6. Latin	2000	
7. Greek	2000	
8. English History	2000	
9. Botany	2000	
10. Chemistry	2000	
11. Physics	2000	
12. Physical Geography and Geology	2000	

Class III.—Additional Subjects.

13. Freehand Drawing	500	Either or both of these may be taken up in addition to those in Classes I. and II.
14. Geometrical Drawing	300	

A close inspection of the scheme reveals at once certain serious objections to it. In the first place, whilst admitting that the authorities have done well to provide fairer opportunities for students whose education has been largely in literature, we must claim, both in the interests of the service and of the candidates, that those who seek admission as probationers for so essentially scientific a service ought in every case to be required to show some moderate degree of capacity for scientific work before they are admitted to their professional studies. The Professors at Cooper's Hill are men of the highest standing, and they will undoubtedly give an excellent training to all who are fitted to undergo it. But they cannot create scientific aptitude in those who are intrusted to them. Hence, if men who are deficient in the proper qualities are selected as probationers, either the service or the probationers must suffer; for it must happen, either that the scientific standard of some of those who are finally selected will be unduly low, or else that some probationers who ought never to have been selected will be finally rejected after much loss of time and much expenditure in money. To show how real this objection to the scheme is, it is only necessary to point out that under the new scheme a candidate may offer himself for examination in the following subjects with every reason to hope for success:—Elementary mathematics, English composition, German, Latin, Greek, drawing. We do not think that the staunchest upholders of the study of literature will support this selection of subjects as one by means of which a satisfactory judgment of the fitness of the candidates for a scientific profession can be made. It is plain that a young man who shows ability in these subjects may or may not have a reasonable degree of scientific aptitude also.

On the other hand, many will think that the new scheme permits even too great a neglect of literary studies on the part of those candidates whose bent is for science, since several combinations such as the following would also be possible:—Elementary mathematics, English composition, German, chemistry, physics. We are sure that many advocates of science teaching will feel that in this group of subjects literature is too much neglected; and we believe that a youth of nineteen or thereabouts might add to it another modern language, or some knowledge of Latin, with advantage to his studies in mathematics and science, as well as to his general education.

In connection with this question, too, it must be remembered that candidates are practically compelled by the severity of these competitions to stick to those subjects in which they are most likely to compete successfully, for a long period, often for several years, beforehand. So that, for example, a young man who is only moderately good at science and rather better at languages will be most likely to win a place in this scientific service by neglecting all scientific reading up to the age of nineteen or twenty years! Surely this is an example of how not to do it!

It seems to us, therefore, that the proposed scheme for the India Forests Department imperatively requires such amendment as shall secure evidence of a reasonable degree of capacity for science on the part of every probationer, whilst it would be well also if it could be made to encourage a rather wider range of literary study in the earlier education of those whose main interests lie in the direction of science. What is desirable could be attained in several ways. But it could, perhaps, be best effected by permitting candidates to offer themselves for examination in three subjects instead of two from Class II.; with the limiting condition that one at least of these three must be taken from numbers 5, 6, 7, 8, and one of them from 9, 10, 11, 12.

We trust that this subject may also be brought under the notice of the authorities at the India Office. It seems evident from the changes already made that they are in no way prejudiced against either scientific or literary studies, and we feel sure that if they will institute inquiries they will find that similar opinions to those we have expressed are widely held on this subject.

THE VOLCANOES OF HAWAII.

Characteristics of Volcanoes, with Contributions of Facts and Principles from the Hawaiian Islands: including a Historical Review of Hawaiian Volcanic Action for the past Sixty-seven Years, a Discussion of the Relations of Volcanic Islands to Deep-sea Topography, and a Chapter on Volcanic-Island Denudation. By James D. Dana. Illustrated by Maps of the Islands; a Bathymetric Map of the Atlantic and Pacific Oceans; and Views of Cones, Craters, a Lava-Cascade, a Lava-Fountain, &c. (London: Sampson Low, Marston, Searle, and Rivington, 1890.)

THE veteran geologist of the United States has rendered an inestimable service to science by the publication of this splendid monograph, which has just made its appearance simultaneously in this country and in the United States. To find any work on a similar subject comparable with it either in importance, or in the influence it is likely to exert upon geological thought, we must go back to the publication of Fouqué's "Santorin," of Von Waltershausen's "Etna," or Scrope's "Volcanoes of Central France."

The Hawaiian volcanoes are unquestionably the grandest on the face of the globe. Their vast dome-shaped masses, with slopes averaging from 6° to 8°, rise to heights of only 14,000 feet above the sea-level; but deep-sea soundings have shown that they stand on a floor 12,000 to 18,000 feet below that level, so that, as Prof. Dana points out, the higher volcanic mountains of the Sandwich Islands must have an elevation of not far from 31,000 feet above their bases! Beside these lofty and bulky domes, the graceful volcanic cones of the North and South American continents, of Japan and Java, sink into insignificance. The Hawaiian Archipelago contains no less than fifteen volcanoes of the first class, all but three of which appear to be now extinct. The active volcanoes of Hawaii give rise to lava-floods, which, in their bulk and in the distances they flow from their point of emission, are only surpassed by those of Iceland. In their remarkably non-explosive action, in the characters of their great pit-craters, in the wonderful liquidity of their lavas—giving rise to veritable fountains of molten rock—and in the beauty and singularity of some of their igneous products, the Hawaiian volcanoes are without a parallel anywhere else in the world.

The Hawaiian volcanoes appear to form two nearly parallel bands, which doubtless indicate great lines of fissure in the earth's crust, the extreme length of these being about 400 miles. The recent topographical surveys of the islands made by Prof. W. D. Alexander, Surveyor-General to the Hawaiian Government, and a number of recent soundings in the adjoining seas, enable us to realize, in a way that was not previously possible, the dimensions and forms of these vast volcanic piles.

Prof. J. D. Dana has enjoyed exceptional facilities for studying these unique centres of igneous activity. As naturalist of the U.S. Exploring Expedition, he visited the islands in November 1840, after the great eruption of Kilauea that had taken place in May of the same year. The work of the actual survey and description of the craters was unfortunately not committed to Prof. Dana; for the energetic, though scientifically untrained, head of the Expedition, Captain Wilkes, determined to undertake this task himself; and the naturalist was sent away to another station while the survey was in progress. Had Prof. Dana been present to advise and assist the surveying officers, it is clear that many unfortunate errors would have been avoided, and that the accounts of the volcanoes contained in the "Narrative of the United States Exploring Expedition" would have had far greater scientific value.

After his return to the States and his settlement at Yale College, Prof. Dana showed his continued interest in the Hawaiian volcanoes, by keeping up a constant correspondence with missionaries and other residents in the islands; and every great eruption was carefully chronicled in the pages of the *American Journal of Science*, which he has so long edited. The memoirs of Brigham and Captain Dutton, and the enlargement and correction of our topographical knowledge of the islands, resulting from the Government survey, seem once more to have aroused the author to a sense of the importance of the subject, and in 1887 he commenced a series of papers on the history of the changes in the Mount Loa craters. He had not proceeded far with this work, however, before he felt the need of a second personal examination of the district. With characteristic energy, he undertook, in spite of his advancing years, a ten-weeks' journey, involving over ten thousand miles of travel, in which he visited all the chief points of interest; and the book before us is the outcome and monument of his labours.

The work of criticizing and reconciling the accounts given by numerous travellers, beginning with notices written as long ago as the year 1823, has been admirably performed by Prof. Dana. Without his personal knowledge of the localities, and the aid afforded by the new and accurate maps of the islands, the task would, indeed, have been a hopeless one; for many of the descriptions were penned by unscientific and careless writers, and inaccuracies and exaggerations are encountered at every step. By sifting and correlating this confusing mass of evidence, however, the author is able to give a clear and connected narrative of the changes in the Kilauea crater, and to illustrate the position of its floor after each of the great eruptions, which took place in 1823, 1832, 1840, 1868, and 1886. The result is that we are furnished for the first time with the means of judging of the real nature of the processes going on in the pit-craters of non-explosive volcanoes. A similar discussion of the records concerning Mokuaweoweo, the summit-crater of Mount Loa, enables the author to furnish an interesting, but necessarily less complete, narrative of the operations going on there during the same period. The want of anything like synchronous action between these two great craters in the same mountain-mass, *one of which is at an elevation of* 10,000 *feet above the other*, has often been remarked upon; and the truth of the conclusion—one which must always be taken account of in attempts to explain volcanic phenomena—is fully established in the work before us.

Prof. Dana forcibly illustrates the remarkable contrast between the effusive eruptions of the Hawaiian volcanoes with their extremely liquid and perfectly fused basaltic lavas, and the explosive outbursts of Vesuvius, Krakatão, and Tarawera. He describes the characters and limited distribution of the curious glassy lavas, and their derivatives—the curious Pele's hair and the beautiful "thread-lace scoriæ"; and he points out the inaccuracy of the early chemical analyses of the Hawaiian lavas, which have misled so many subsequent writers. His remarks on the characteristics and origin of the chief varieties of the lava, and especially of the pseudo-bombs—vast pillow-like masses of lava covered with a thin vitreous crust—are remarkably interesting and suggestive.

One of the most valuable chapters in the book is that on the petrographical characters of the Hawaiian lavas, supplied by the author's son, Prof. E. S. Dana. The singular fissile basalts of the higher cone, which resemble phonolite, and several other remarkable types are here described for the first time. Very noteworthy are the curious feathery forms of augite which occur in some lavas, and the strangely-elongated crystals of olivine which are found in others. But the part of the chapter which will unquestionably awaken the greatest amount of interest in the minds both of mineralogists and geologists is that which deals with the curious stalactites found in certain caverns in the lavas. That these stalactites are formed by aqueous action there cannot, as Prof. E. S. Dana shows, be any reasonable doubt. Yet the stalactites are built up of crystals of felspar, augite, and magnetite, all the constituents formed by igneous action in the lavas themselves, being present, with the exception of olivine! All students of mineral synthesis are acquainted with the fact that the same species can often be formed by several, and sometimes by very diverse, methods. Mr. Sorby has even shown how fragments of quartz-crystals, originally formed in a granite or other igneous rock, may after enormous intervals renew their growth and become complete crystals again under purely aqueous conditions; so that the same crystal may in different parts be the result of totally different kinds of action. In spite of these facts, however, few petrographers would be prepared to find that, from aqueous solutions, rocks made up of felspar, augite, and magnetite could be formed in the way described in this interesting essay. Prof. E. S. Dana not unnaturally announces these remarkable conclusions with some diffidence and reserve; yet it is impossible to find any flaw whatever in the line of argument by which he seeks to establish their truth.

Prof. J. D. Dana has prefaced his description of the Hawaiian Islands by a sketch to which the title of "Characteristics of Volcanoes" is more directly applicable. In this introduction, which only extends to some 27 pages, many of the great problems of vulcanology are discussed with singular clearness and freedom from bias.

The work concludes with two interesting appendices, the first on "Volcanoes and Deep-sea Topography," and the second on "Denudation of Volcanic Islands; its Amount a Mark of Age." The book is well illustrated with maps and sketches, and some plates reproducing photographs will serve to give a just idea of the peculiar lava cascades and fountains of Hawaii—phenomena which have not unnaturally excited the imagination of untrained observers, and given rise to startling drawings and florid descriptions in popular works of travel. But the sober truth is, that the wonders of Hawaii stand in no need either of exaggeration or embellishment from the writer or the artist.

We heartily welcome the volume as the crowning labour of the greatest of America's men of science—the latest, and not by any means the least important, of a long series of contributions to science on very diverse subjects, but of unvarying excellence. J. W. J.

A POLYGLOT MEDICAL VOCABULARY.

Terminologia Medica Polyglotta: a Concise International Dictionary of Medical Terms. Compiled by Theodore Maxwell, M.D. Cantab. (London: Churchill, 1890.)

THE current literature of medical subjects is extensive and polyglot, and those who endeavour to keep themselves abreast of the most recent research in any branch require to dip into works in many languages, and need to have at hand some such aid as the present vocabulary, wherein they can seek for the several vernacular synonyms of those newer technicalities which modern developments of science have produced, and which are not to be found in the ordinary dictionaries. Moreover, it is often necessary that the special senses in which some of the older and more general words are used by medical writers should be defined. One may be very well acquainted with the anatomy of the brain as described in the English standard works, and yet have much difficulty in following the descriptions in German or French books on cerebral pathology, when vernacular names are used for the several parts; and one longs for some international agreement as to a uniform system of scientific terminology like the Latin generic and specific names of the Linnæan nomenclature.

The compilation of a new dictionary is a task involving an enormous amount of labour, and when, as in the work under notice, the synonyms of each term in seven languages have to be sought and tabulated, the difficulties of the undertaking are seriously increased. The compiler has evidently expended great care, and exercised much judgment in his toilsome task, and doubtless this vocabulary will prove of much help and be highly appreciated by students of foreign medical literature. The typography is excellent and clear, and the work is singularly free from errors of the press.

It is questionable whether the selection of French as the fundamental language was a wise choice. There are fewer original terms in the French scientific vocabulary than in either English or German; fewer modern writings of value in the medical literature of France than in either of the other great European literatures; and fewer students of medical literature in French-speaking countries than in either English or German-speaking countries. The Russian element might also without very much loss be eliminated; for as there are no Russian-French references it can be of little assistance to anyone reading Russian literature, and will only be of value to the limited class of Russian students of other literatures, or the still more limited class of foreigners writing medical works in Russian.

The special function of a work like the present is to supplement, not to supplant, the ordinary dictionary; therefore such a work should be reduced to as small a bulk as possible. To this end there should be as little overlap as possible, as there is no advantage in including such words as are to be found in the ordinary dictionaries, unless there is something specific in their use to be explained. In the work before us, this principle has not been adopted, and its size has consequently been unduly enlarged by the introduction of many common words which have no such peculiarities. Thus, taking at random the pages 184–5, there are the words Heirath, Heavy, Heat, Heating, Hebung, Hebel, Heften, Heifer, Height, Heiss, Heizung, Helfen, Hell, Helm, Hembra, Hemd, Heaviness, Heiter, Helios. In these pages alone, one-fourth of the entries are common dictionary-words. (By inadvertence, Helios, the sun, is said to be Latin.) Turning at random to another page (445) there are seventeen words in a row—Wave, Wax, Weak, Weaken, &c.—of the same description. One might, perhaps, defend the introduction of the word "Stays" in the sense of corset, but one does not see why "Star" (étoile) or "Stamp" (timbre) should have space devoted to them.

By rigid adherence to a definite order of languages in the enumeration of synonyms, the bulk of the work has also been largely increased. Much space would have been saved, and the utility of the book by no means impaired, if, when the same word was used for the same idea in two languages, instead of the repetition of the word the initials indicating the languages had been prefixed to one entry of the word. Thus, instead of wasting three lines with "E. Opodeldoc, soap-liniment; G. Opodeldoc; I. Opodeldoc," or two lines with "I. Organo; S. Organo," E. G. and I. might have been prefixed to the one, and I. and S. to the other. There is no gain of clearness in the tabulation, and a distinct loss of handiness; for it is especially true in the case of a dictionary that the greater the book the greater the evil.

While in most cases the author has confined himself to the enumeration of synonyms, there are some words to which he has appended definitions. The principle upon which words have been selected for this distinction does not seem apparent; for instance, it was surely unnecessary to define "Faux (f. fausse), adj., qui n'est pas vrai." Some of the definitions are curious; thus, "delusion" is defined as "a belief in something incredible to sane people, resulting from diseased working of the brain convolutions." It is doubtless right that "Daft" should appear in a polyglot dictionary, but it can scarcely be reckoned as English, and is as much deserving of having its nationality indicated as "Knocked up," which is given as "(en Amérique) enceinte."

In a few cases inconsistencies of spelling have eluded the corrector's eye: thus, the adjective "Lacrymalis" is accurately given, but a few lines below the neuter form appears as "Os lachrymale"; Aneurysm is spelled with an *i*; but oversights of this kind are very few.

The author has adopted in some cases the useful plan of marking with an asterisk those words under which the full synonymy is given. In a few instances this has become misplaced; thus for the Latin "Caduca" the equivalent "membrana decidua*" is given, but there is no such entry under "membrana," and opposite "Decidua" there is simply the French synonym "caduque," which is the heading to which the star should have been appended.

There are some words which one would have expected to have place in such a work that are not to be found. Ache, Aching, Acromegaly, Caul, Limbus, Limbic, Lobe, Monoplegia, Laparotomy, are a few of these. Black alder is given, but neither black wash nor black draught. Red precipitate, Citrine ointment, Daffy's elixir, are surely as deserving of place as Dover's or James's or Gregory's powder.

Fault-finding is at all times an ungrateful task, but it becomes especially unpleasant when the subject is a work of real merit, and we have indicated these weaknesses so that in subsequent editions the usefulness of the work may be increased. If its size were diminished by the exclusion of ordinary dictionary-words, by the better grouping of those that are identical, and by the judicious excision of unnecessary definitions, a portable, useful work would be produced, which we doubt not would find its place on the desk of the majority of students of foreign medical literature.

Alex. Macalister.

MASKS FROM NEW GUINEA AND THE BISMARCK ARCHIPELAGO.

Masken von Neu Guinea und dem Bismarck Archipel. By A. B. Meyer. *Königliches Ethnographisches Museum zu Dresden.* Band VII. Folio, pp. 15, Plates 15. (Dresden: Stengel and Markert, 1889.)

Dr. A. B. Meyer has written the seventh of the series of fine publications of the Royal Ethnographical Museum of Dresden which are brought out under his direction. He has selected for description and illustration the masks from New Guinea and the Bismarck Archipelago which are to be found in the collection under his care. The descriptions are as a rule very brief, but they are to the point, and indicate the zoological training of the author. The latter is shown not only by the precision of the descriptions, but also by the addition of the generic name to the animals represented by the masks or used in their adornment. Of the 83 specimens in the Dresden Museum, 61 have been illustrated in this memoir in a most admirable manner by a photographic process the excellence of which leaves little to be desired. On comparing these photographs with woodcuts of similar objects, the advantage of the former is at once apparent, as the texture of the various substances used in the manufacture of the masks is faithfully rendered, and the faultiness of the original design or pattern is not glossed over by an engraver. It is a great pity that the magnificent collections in the British Museum cannot be rendered available for home study by the publication of similar photographs.

It is to be regretted that Dr. Meyer confined his account of the masks to those contained in the Dresden Museum, and has not compared these with the specimens which are to be found in other museums.

A good opportunity for a thorough treatment of the subject has thus been lost. For example, allusion is made to the occurrence of masks in the Elema district of the Papuan Gulf, but no description or figure is given of them, although numerous specimens of these have found their way into museums. Of the eight masks which are figured from Torres Straits, one of the most characteristic varieties is unrepresented—that one which represents a crocodile's head surmounted by a human face. A fret pattern occurs on a mask from Jervis Island. This is alluded to by Dr. Meyer, and is compared with somewhat similar patterns, of which woodcuts are given, on two masks from German New Guinea, and with two patterns on arrows from Dutch New Guinea. The Torres Straits pattern, unlike the others, is precisely similar to the common form of the pattern, and as it does not occur on other objects from that district we can only conclude, contrary to Dr. Meyer, that it was directly copied from some introduced object; the same mask is further ornamented with some imported red woven material. Dr. Meyer suggests that the helmet masks from New Ireland, and the feather helmets and masks from the Sandwich Islands, are reminiscences of the helmets of the Spanish voyagers of the sixteenth or seventeenth century. He also considers it probable that the use of masks in this part of the world originated in New Ireland, and extended through New Hebrides to the northern portion of the German territory of New Guinea, and thence by an overland route to the head of the Papuan Gulf and Torres Straits. Other routes were northward to the Caroline Island, Mortlock, and south-east to New Caledonia. Dr. Meyer has been able to discover very little concerning the uses of masks; all that he can say is that they are used in "masquerades, festivals, general feasts, secular, religious, and war dances." It is, however, very probable that particular kinds of masks are used for definite occasions, and that the masks which are worn say during initiation ceremonies could not be put on at a seasonal festival. There is no evidence, so far as British New Guinea is concerned, that masks are ever worn at the festive or secular dance, or at the war dance; they appear to have a definite sacred or religious significance.

This valuable memoir concludes with an interesting quotation from Weisser's paper on masks from New Ireland. Early in May the men of one village repair to another village with which they have a feud. Each man then puts on the mask which he has been secretly preparing during the previous year, and the men of the one village dance opposite to those from the other. After this they have a feast, and exchange sago cakes, which they eat with caution, fearing poison; criticism of the masks of the opposite faction affords ample opportunity for the continuance of the animosity.

A. C. H.

OUR BOOK SHELF.

Larva Collecting and Breeding. By the Rev. J. Seymour St. John, B.A. (London: William Wesley and Son, 1890.)

THE alternative title of this little volume, which is of convenient size for the pocket, is "a hand-book to the larvæ of the British Macro-Lepidoptera and their food plants; both in nature and in confinement, with authorities," and is sufficiently explanatory of its scope and objects. The arrangement of the first portion of the book is entomological, of the second and concluding portion botanical. In the former the larvæ are arranged and named according to "The Entomologist Synonymic List of British Lepidoptera," and the food plants are enumerated as subsidiary to these. In the second half the food plants are specified in the order of the "London Catalogue of British Plants" (eighth edition). The book is therefore susceptible of a twofold use; it will induce the entomologist to become a field botanist, and conversely it will greatly aid the student who has some knowledge of the native flora in his efforts to become practically acquainted with the lepidopterous larvæ. So much energy is misdirected, particularly by young people, in making collections of butterflies and moths for the mere sake of collecting, that the intelligent use of this little book is calculated to effect a salutary change. It will, at least, direct greater attention to the life-histories of the Lepidoptera, and if it should be instrumental in inducing the collector to preserve and mount the larva alongside the male and female specimens of the mature butterfly or moth, so much the better. It is too common a practice to ignore the "grub" as unlovely and despicable; though from an economic point of view it possesses a higher interest than the winged insect, and is certainly not inferior to it in importance from a scientific standpoint. Nearly all the Lepidoptera which are familiar in this country as crop-pests are actively injurious only in the larval stage.

As the author intimates, such a work as this is necessarily a compilation, and, from its very nature, it is hardly possible to make it exhaustive. All who use it in the field will find opportunities to annotate and amplify it, and possibly to suggest emendations. The common names as well as the systematic names of the plants are given, and it might be useful if in a future edition the common names of the insects were, as far as possible, also enumerated. A few misprints have escaped notice, as *Galium sexatile* (p. 103), and *Rynchospora alba* (p. 137).

Mr. St. John's book represents a good idea well carried out, and it should have the effect of stimulating the study of natural history in the field.

Practical Chemistry for Medical Students. By Samuel Rideal, D.Sc. (Lond.), F.I.C., F.C.S., F.G.S. (London: H. K. Lewis, 1890.)

THIS book is intended by the author to embody the tests for those substances which a medical student is required to identify at the first examination of the Conjoint Examining Board in England. The attempt to compress this information into 53 small pages has resulted, as might have been expected under the circumstances, in a cram-book. Indeed, the only justification, if such it can be called, for the addition of another to the many works on qualitative analysis is that the book contains in the minimum space the knowledge required for a special examination. This knowledge is, however, frequently of a questionable nature. Thus, "calcium sulphate, $CaSO_4$ (*gypsum*)," is described as a "white *amorphous powder*;" "sodium carbonate, Na_2CO_3," as a "white solid, crystalline *or* amorphous;" "ferric chloride, Fe_2Cl_6, a yellow amorphous *powder*," and so forth: statements of a kind which, although they constitute a large portion of the book, are both fragmentary and inaccurate. The endeavour to attach valency values to the metals is carried out in all cases with the exception of iron, to which no value is affixed. The reason for this omission is not obvious, as the author does not hesitate to call lead a dyad, antimony a triad, &c. Amongst minor points the use of potassium antimony tartrate for potassium antimonyl tartrate, of arsenic acid for arsenic pentoxide, may be noticed.

The book may go some way to fulfil the author's expectation that it will give the student "some acquaintance with the art of test-tubing," but that it will materially increase his knowledge of the principles of practical chemistry, or sharpen his appreciation of the *raison d'être* of a chemical process, is another matter.

Manual of Pharmaceutical Testing. By Barnard S. Proctor, F.I.C. Pp. vii., 176. (London: *The Chemist and Druggist*, 1890.)

This book is a collection of tests suitable for ascertaining the purity of the chemicals of the British Pharmacopœia, &c. The tests described are the simplest possible, and can be carried out with the apparatus and chemicals in use at the dispensing counter. They apply more especially to the impurities of manufacture than to adulterations. In many cases they are simply those recommended by the British Pharmacopœia for determining if the purity of a material falls short of the required standard. As a rule they are qualitative, and sufficiently accurate for the purpose in view, although quantitative methods, more especially in determinations of solubility, or fixed residues of volatile liquids, are employed. The book contains a chapter on manipulation, which includes the method of weighing precipitates, and an index, and will be found a handy volume to the pharmacist.

The Encyclopædia of Photography. By Walter E. Woodbury. (London: Iliffe and Son, 1890.)

This work, which will be concluded in about twelve parts, is written on the same lines as other photographic encyclopædias, but treats especially of the sciences of optics and chemistry. The art of photography being so largely practised nowadays, it is curious what a small percentage of those who have taken it up know anything about optics or chemistry, which form the basis of the whole subject.

Throughout the book the author has borne this well in mind, and has spared no pains to place before the reader, in a simple and clear manner, the principles underlying the formation of images, the construction of lenses, chromatic and spherical aberration, the theory of atoms and molecules, and many other very important points relating to optics and chemistry.

The illustrations, which will be about 200 in number, consisting of explanatory sketches and diagrams, will be found, if up to the standard maintained in this first part, to serve their purpose well.

For amateurs this encyclopædia should be very useful, as it is written especially for beginners, and some of the most complicated terms likely to lead to confusion are avoided as much as possible.

Dynamics for Beginners. By the Rev. J. B. Lock, M.A. Third Edition, stereotyped. (London: Macmillan and Co., 1890.)

The author has fully succeeded in supplying the want that has been long felt, of a book which should explain the elementary principles of dynamics, illustrating them by easy examples in a manner suitable for use in schools with boys of ordinary mathematical attainments.

Section I. deals with rectilinear dynamics, in which the fundamental principles are explained. The words "velo" and "celo," abbreviations for unit velocity and unit acceleration respectively, are here used, and the author says in the preface, "Of their value for the purposes of teaching and explanation I have received the very strongest testimony from those best qualified to judge."

Sections II. and III. treat of "Direction" and "Illustrations," the former dealing with the parallelograms of distances, velocities, and accelerations, chords of quickest descent, &c., the latter with projectiles, oblique impact, relative motion, hodograph, &c.

Work, energy, power, are discussed in Section IV., and there is a chapter on the indestructibility of matter.

An excellent set of examples is collected at the end, and a series of examination papers is added, taken from the various examinations held from time to time at Oxford and Cambridge.

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 Climates of Past Ages."

I feel somewhat disappointed not to see a flood of correspondence in your pages arising out of Dr. Neumayr's very interesting lecture on the climates of past ages. The subject is difficult and complex, and the factors of the problem are no doubt various and of different kinds. I wish to make a few remarks on some of these.

It seems impossible to doubt that the sun is losing heat; and, consequently, that the quantity of heat annually received by the earth from the sun is less than it once was. Now, one of the most remarkable of the facts before us is the evidence, from fossil vegetation, of comparatively warm climates in the polar regions. There is no similar evidence respecting the equatorial regions; but it is probably impossible that such evidence should be preserved, so that its absence proves nothing as to the equatorial climate of the same period; but it is worth noticing that, if we suppose the force of solar radiation increased, the increase of terrestrial temperatures will be greater in high than in low latitudes, because, with the increased quantity of heat received into the atmosphere, an increased quantity will become latent by evaporation in the lower latitudes, and will be carried to the higher latitudes by vapour-bearing winds.

If our planet had neither atmosphere nor ocean, the temperature of the higher latitudes could be raised only as a direct result of increased solar radiation. If it had an atmosphere but no ocean, the increase of the temperature of the higher latitudes would be assisted by heat-bearing winds; and this would be at the expense of the temperature of the lower latitudes, which would be lowered by the heat so carried away. In the actual case of our earth, with both atmosphere and ocean, this action will be greatly increased by the power of vapour-bearing winds to carry heat in the latent form, which again becomes sensible heat on the condensation of the vapour. This appears to show that a considerable increase of temperature might be produced in the higher latitudes by a comparatively small increase in the force of solar radiation.

Dr. Neumayr says that the cause of the glacial climate is quite unknown; and at the same time he asserts that both hemispheres—the northern and the southern—were glaciated at the same time. I dispute both of these opinions. I think Mr. Croll has shown the direction in which the explanation of the glacial climate is to be sought; and if this is so, the two hemispheres were not glaciated at the same time, but alternately.

If, during a glacial period, the northern and the southern hemispheres were each alternately glaciated for geologically short periods, this would account for the fact mentioned by Dr. Neumayr, that the glacial period appears not to have been continuous, but interrupted by periods of milder climate. Croll's theory accounts for this. His theory is, that glacial periods occur at those astronomical epochs when the eccentricity of the earth's orbit is at its greatest; and a glacial climate is produced in the two hemispheres alternately, according as the summer of each hemisphere is in perihelion or in aphelion. This, in consequence of the precession of the equinoxes, will occur at intervals of about 25,000 years. That is to say, if in either hemisphere the summer is now in perihelion, at the end of 12,500 years its summer will be in aphelion, and in 12,500 years more it will be in perihelion again. Mr. Croll maintains that glaciation occurs in the hemisphere where there is perihelion summer and aphelion winter, because of the intense cold of such a winter. I think, on the contrary, that the facts of climate which come under our observation show that winter cold has little or no effect in producing glaciation; and that a cold summer, which leaves the winter snow unmelted, is the most favourable condition for glaciation. Such is the climate of the Antarctic continent now. It is obvious that a summer in aphelion, when the eccentricity of the earth's orbit was many times greater than now, must have been a very cold summer.

This theory of the glacial climate appears perfectly satisfactory. The astronomical cause is known to exist, the geological effects are known to exist, and the effect is that which the cause must necessarily produce.

Even if it were true that a glacial climate prevailed in both hemispheres at the same time, no geological evidence could prove such a fact. No geological evidence could tell whether glacial mounds in Norway and in Patagonia, for instance, were strictly contemporary, or separated in date by an interval of 12,000 years.

Dr. Neumayr appears to retain the old notion that changes of climate may be to some extent due to changes in the position of the earth's poles. I am no mathematician, and cannot speak on such a subject with any authority, but Sir William Thomson believes he has proved that the earth is for all dynamical purposes perfectly solid and rigid; and I should think that the axis of rotation of a perfectly rigid oblate spheroid is unchangeable.

Belfast, July 10. JOSEPH JOHN MURPHY.

The American Meteor.

I RECEIVED the following observations from my son, G. S. Henslow, who witnessed the fall of the meteor referred to lately in NATURE. I forward it, as it may perhaps interest some of the readers of this journal.

"The meteor fell about 5 p.m., and divided in mid-air, part of it falling in Minnesota near a town called Kasota; this portion was not found. The other and larger piece fell near Butt City, Iowa. The two places are about a hundred miles distant. It exploded on reaching the ground into myriads of fragments, a number of which have been picked up and sold at fabulous prices. The State University of Minnesota bought the largest piece. It fell on the open prairie, but broke into such small fragments that the surrounding soil was scarcely disturbed at all. We all saw it fall here at Windom. It illuminated the southern sky, and left a cloud resembling the smoke from the funnel of an engine. On bursting, there was a sound like a sharp peal of thunder." G. HENSLOW.

SPONTANEOUS IGNITION AND EXPLOSIONS IN COAL BUNKERS.

AT the Royal United Service Institution, on Friday, July 4, a paper on this subject was read by Prof. Vivian B. Lewes, Royal Naval College. Rear-Admiral N. Bowden-Smith was in the chair.

The lecturer, after premising that in the fast ocean steamers it is now becoming an event of frequent occurrence for the contents of the bunkers to spontaneously ignite, whilst in the Service such a thing as fire in the bunkers is practically unknown, and an occasional, although fortunately very rare, explosion of gas is the worst trouble which the coal stores of our naval monsters have given rise to, directed attention to the causes which give rise to the so-called "spontaneous ignition of coals," and traced the particular circumstances which tend to increase the tendency to it.

The pyrites or coal brasses present in the coal when exposed to dry air undergo little or no change, but when moisture as well as air is present they absorb oxygen and combine with it, forming sulphates of iron, and the ordinary explanation of the spontaneous ignition of coal is that this process of oxidation causes a rise of temperature in the coal which determines its ignition; this, however, has of late years been much doubted, and it can now be proved that the pyrites when present in ordinary quantities are perfectly incapable of doing more than adding slightly to the general rise of temperature, although when present in very large masses they may increase the tendency of the coal to spontaneous combustion by swelling during oxidation, and causing the coal to crumble, and also by setting free sulphur, which, having a lower melting-point of ignition than coal (482° F., or 250° C.) would lower the temperature at which the mass would catch fire.

The real causes which give rise to heating and ignition in any large accumulation of coal are twofold. First, the absorption of oxygen from the air by the carbon; and secondly, the chemical action set up by the absorbed oxygen with the hydrocarbons of the coal.

The most important point to be noticed is the extraordinary effect which initial temperature has on the rapidity of chemical actions of this kind. At a low temperature, and indeed up to about 100° F. = 38° C., the absorption of oxygen, and consequent chemical action, will go on slowly with practically little or no chance of undue heating taking place, but directly the temperature exceeds 100° F., then, with some classes of coal, ignition is only a question of time and mass.

Although the ignition point of various coals lies above 700° F., yet if many of these coals are powdered, and are placed in perforated zinc cases in masses of 2 lbs. or upwards, and these are kept at a steady temperature of about 250° F. in an oven, ignition will generally follow in a few hours; whilst between this and 150° F. it will take days instead of hours for the same result to follow, and at ordinary English temperatures several thousand tons of coal would have to be stored in a very broken condition before any risk of heating or ignition would ensue. In considering this question with regard to coal bunkers, it must be remembered that, although the considerations which had to be taken note of in the case of coal-laden ships still exist, yet they are considerably modified by the smallness of the amount of coal carried, and by the methods of loading and storage employed.

Liability to spontaneous ignition increases with:—

1. *The increase in the bulk of the cargoes.*—Evidence given before the Royal Commission of 1875 showed that in cargoes for shipments to places beyond Europe the cases reported amount to ¼ per cent. in cargoes under 500 tons; in cargoes from 500 to 1000, 1 per cent.; 1000 to 1500, to 3·5 per cent.; 1500 to 2000, to 4·5 per cent.; and over 2000 tons, to no less than 9 per cent. Mass influences this action in two ways:—

(*a*) The larger the cargo, the more non-conducting material will there be between the spot at which heating is taking place and the cooling influence of the outer air.

(*b*) The larger the cargo the greater will be the breaking-down action of the impact of coal coming down the shoot upon the portions first loaded into the ship, and the larger thereby the fresh surface exposed to the action of the air.

2. *The ports to which shipments are made* (26,631 shipments to European ports in 1873, resulting in only ten casualties, whilst 4485 shipments to Asia, Africa, and America gave no less than sixty).—This startling result is due to the length of time the cargo is in the vessel, the absorption and oxidation being a comparatively long action, but a far more active cause is the increase of temperature in the tropics, which converts slow action into a rapid one.

3. *The kind of coal of which the cargo consists* (some coals being especially liable to spontaneous heating and ignition).—There is great diversity of opinion on this

point, but it is pretty generally admitted that cases of heating and ignition are more frequent in coals shipped from east coast ports than in South Wales shipments. So much, however, depends upon the quantity of small coal present, that a well-loaded cargo of any coal would be safer than a cargo of Welsh steam coal in which a quantity of dust had been produced during loading.

4. *The size of the coal* (small coal being much more liable to spontaneous ignition than large.)—This is due to the increase of active absorbent surface exposed to the air, a fact which is verified by the experience of large consumers of coal on land; gas managers recognizing the fact that coal which has been stamped down or shaken down during storage is more liable to heat than if it has been more tenderly handled, the extra breakage causing the extra risk.

5. *Shipping coal rich in pyrites (or brasses) whilst wet.*—The effect of external wetting on coal is to retard at first the absorption of oxygen, and so to check the action; but it also increases the rate of oxidation of the pyrites, and they, when oxidized, swell and split the coal into pieces, and this increases heating due to the exposure of fresh dry surfaces.

6. *Ventilation of the cargo.*—For ventilation to do any good, cool air would have to sweep continuously and freely through every part of the cargo—a condition impossible to attain in coal cargoes—whilst anything short of that only increases the danger—the ordinary methods of ventilation supplying just about the right amount of air to create the maximum amount of heating. The reason of this is clear. A steam coal absorbs about twice its own volume of oxygen, and takes about ten days to do it under favourable conditions, and it is this oxygen which, in the next phase of the action, enters into chemical combination, and causes the serious heating. Ventilation, such as used to be sometimes arranged for by a box shaft along the keelson with Venetian lattice up-shafts, supplies about as much air as is necessary to produce the results which end in spontaneous ignition.

7. *Rise in temperature in steam colliers, due to the introduction of triple-expansion engines and high-pressure boilers.*—The increase in stokehold temperature, due to this, is from 5° to 10° F., and this affects the temperature of the adjacent parts of the vessel.

In the coal bunker, the question of mass, which plays so important a part in a hold laden with coal, is almost entirely eliminated, as 50 to 400 tons would be about the capacity of any ordinary bunker, and the cases of spontaneous ignition in masses of coal less than 500 tons do not amount to more than ¼ per cent. The question of initial temperature, therefore, becomes the one important factor. Bunker fires are almost entirely confined to vessels in which the bunker bulkheads are only separated from the funnel by a narrow air-space, or are in close proximity to the boilers themselves; but where the bunkers are stepped back from the funnel casing and boilers, spontaneous ignition is a great rarity. If coal is kept at a high temperature, even though it be far below its igniting point, ignition is only a question of time, and if the bunker coal next the bulkhead is kept at 120° F., any coal with a tendency to absorb oxygen will run a great chance of igniting within a few days. In order to prevent spontaneous combustion of the coal under these circumstances, all that is necessary is to reduce the temperature of the bulkhead in contact with the coal, as if this is kept at a temperature not exceeding 80° to 90° F., there is little or no fear of the oxidation of the hydrocarbons of the coal proceeding with such rapidity as to cause ignition in such a quantity of coal as can be carried in the bunkers, the iron decks, by subdividing the mass, also helping to reduce any risk. In order to reduce the temperature to the required extent, it would be necessary to make the bulkheads close to any heating surface, such as the funnel casing, double, and the side spaces six inches apart, the inner wall being provided at intervals with water-tight openings, through which the interior space can be coated with protective compositions from time to time. Through this double casing sea-water would be allowed to circulate very slowly, and would effectually prevent any undue rise of temperature, whilst to make the arrangements complete a thermostat should be fixed on the inner plate of each bulkhead, which, if the temperature rose to 100° F., would ring a bell in the captain's room, when the rate of flow of water could be increased until the required fall in temperature took place. Should this arrangement prove impossible from any structural cause, then a rapid current of air forced through the bunkers by means of a fan, or even an up-current formed by a good air-pump ventilator in the crown of the bunker, would go far to keep the temperature within safe limits. If such an arrangement were adopted in the fast liners, bunker fires would become a thing of the past, whilst such an arrangement of double bulkhead and water circulation would also solve the still more important problem of how to keep the magazines on board Her Majesty's ships at a sufficiently low temperature to fit them for the storage of E.X.E. and S.B.C. prism powders, and the still more delicately constituted smokeless powders, none of which could otherwise be kept in the auxiliary magazines of the new programme ships; as for safety they are placed between the boilers, and must, of necessity, reach a temperature far above that which any powder could stand without losing moisture, and in consequence developing far higher strains than the guns should properly be subjected to.

The question of explosions in coal bunkers and in the holds of coal-laden ships is a subject totally distinct from that of spontaneous ignition. During the conversion of woody fibre derived from various forms of vegetation into coal, considerable quantities of a gaseous compound of carbon and hydrogen, called methane, marsh-gas, or light carburetted hydrogen, is evolved, and as the action has been spread over long ages most of this gas has found its way to the surface of the coal seam and has diffused itself through the superincumbent soil and has escaped; but a portion has been occluded (absorbed) in the pores of the coal itself, and some also imprisoned in small cavities and fissures in the coal. Marsh-gas, when pure, is perfectly non-explosive, and burns quietly with a faint luminous flame, producing, as the products of its combustion, carbon dioxide and water vapour, but when mixed with ten times its own volume of air, and a light applied, it explodes with a force equal to about 210 lbs. on the square inch. Another cause which tends to increase the danger of explosion is that if the air is charged with fine coal-dust, less than one per cent. of marsh-gas mixed with it gives an explosive mixture, and also extends the area of explosion. In both colliers and coal bunkers the risk of explosion is greatest during the first ten days after shipment.

Marsh-gas is a non-supporter of combustion, so that the presence of the gas, or a mixture of it with air, if present, is a safeguard against spontaneous ignition; and if the precautions pointed out to prevent ignition were carried out in conjunction with simple precautions against explosion, explosions and fires in coal cargoes and bunkers would soon be a thing of the past.

The lecturer strongly advocated the adoption in the bunkers of all new vessels of the double bulkhead, and water circulation to such portion of the bunkers as impinge upon any unduly heated portion of the hold, and that all bulkheads should be made gas-tight; whilst in bunkers containing not more than 300 to 400 tons of coal, as thorough ventilation as possible should be obtained by fitting water-tight air-pump ventilators in the deck above the surface of the coal, while inlets for as cool air as possible should be provided at the bottom of the bunkers, and, where necessary, air driven in from the fan. Under no conditions should any but safety-lamps be used in coal holds or bunkers.

A discussion followed, and the proceedings closed with a vote of thanks to the lecturer.

A WINTER EXPEDITION TO THE SONNBLICK.[1]

IT is not often that an Alpinist finds leisure to spend a month in winter at an altitude of 10,154 feet above the level of the sea. It may, therefore, interest the members of the Alpine Club, to have the experiences of one who, though not a member of their Society, yet was fortunate enough to make the unusual ascent, which was chiefly undertaken in the interests of science.

It is well known that since 1886, thanks to the united efforts of the Alpine Club, and of the Imperial Austrian Meteorological Society, and in a special manner to the energy and public spirit of Herr Ignaz Rojacher, there is now a thoroughly equipped Observatory on the highest peak of the Sonnblick. This Observatory has been established with the view of affording to students of natural science, physics, astronomy, and meteorology, the means of making such observations as are only practicable at great heights; and of providing them with accommodation in a part of the building which has been named by the owner "The Study."

In carrying on certain inquiries which are only to be solved on high mountains, I had for this purpose spent a month in the summer of 1881 on the Hoch Obir (6716 feet) in Carinthia, and I determined the first winter after the erection of the Observatory on the Sonnblick still further to resume the investigations in a situation which afforded a clear, cold, winter atmosphere, which was absolutely necessary. I was unfortunately unable to realize my intention the first winter (1887), which was the more to be regretted inasmuch as the winter of 1887, and especially the month of February, was unusually fine, whereas that of 1888 was the severest ever known. The "oldest inhabitant" of those parts had no remembrance of such heavy falls of snow and such dark and stormy weather as we experienced in the February of 1888—the month for which I had made all my arrangements for an expedition to the Sonnblick.

My expedition was undertaken with the following objects:—(1) To investigate the radiation of the earth into space, and the irradiation of the atmosphere upon the earth's surface, in order to ascertain, more accurately than had hitherto been done, the temperature of the aërial envelope of the earth. (2) To investigate the question of the blueness of the sky. (3) To discover whether the sparkle of the stars was altogether due to the lower strata of air. Having had a grant from the Imperial Academy of Sciences in Vienna for the purpose, I succeeded in enlisting the services of Dr. Trabert, a young indefatigable man of science, as assistant, to make simultaneous observations on the Rauris, whilst I observed on the Sonnblick.

We reached Lend on the morning of February 3, where we handed over our seven cases of scientific instruments, and my provisions for a month's sojourn on the Sonnblick, to Herr Rojacher's men, who conveyed the whole on a couple of sledges through Embach to Rauris; we driving to Kitzloch Rauris, where we found Herr Rojacher awaiting us, and, after a tough climb of an hour and a quarter up the mountain pass of Kitzloch, we proceeded by sledge to Rauris.

This first day was perhaps the finest during our stay in the Rauris Mountains; on the next, it began to snow; and it was in a heavy snow-storm that I had to set out for Kolm; and so heavy was it, that it was with the greatest difficulty that Rojacher and I, in our sledge, followed by the *Rossknecht* with my baggage, were enabled to reach the Bodenhaus. From thence, through the woods, to Kreuzbichl, the snow fell thicker and thicker, and it seemed as if we should never get to our destination. Beyond Kreuzbichl there was no path of any sort, and we had simply to wade through the deep snow for fully an hour, before we reached Kolm, Herr Rojacher's residence (5249 feet). On my arrival, I was just in time to telephone to Rauris that I had reached so far in safety, the telephone communication being immediately thereafter interrupted. That journey from Rauris to Kolm had given me some idea of what a snow-storm in those regions meant. The avalanches caused by the weight of snow, had broken down the telephone wires, completely burying them, and, in one place, carrying them away for a distance of over two kilometres.

The *Rossknecht* had just reached Bodenhaus, but was utterly unable to push on further. It was four days before all my cases could be brought on to Kolm; and then the men had to carry them on their backs. Here was I, cut off from the world, snowed up at Kolm, and with little apparent prospect of getting to the Sonnblick; the snow falling faster and faster for four whole days, without intermission. But I was thankful enough to have reached there, for the valley beneath was laid waste with avalanches, making the roads impassable. However, the five days in which I was blockaded at Kolm were anything but wearisome. I could well have undergone a longer imprisonment with a companion so ingenious and intelligent as Rojacher. He had always some interesting subject to discuss, or new problem to set concerning the Tauern range. What perhaps interested me the most were his descriptions of winter life in this inhospitable altitude—its pleasures and difficulties, and particularly his explanation of the *Lahnen*, the local word for avalanches.

There are two kinds of *Lahnen*, he explained, *Windlahnen* or *Windsbretter* (wind avalanches), and *Jauk* or *Grundlahnen* (ground avalanches). The first belong exclusively to winter; the second to spring. These last are the avalanches of which people who live far out of the reach of avalanches have formed the one and sole idea of their nature and composition, thus confounding the two. They are, however, totally different.

The action of the ground, or *Jauk, lahn*, as its name denotes, is to break away from its base on the ground; and, as its second name denotes, mostly in consequence of warmer temperature, *i.e. Jauk*, south wind. It is composed of a huge mass of melting snow saturated with thaw water, that, restrained by the enormous friction of the earth, carries slowly along with it everything that impedes its course. It is set in motion when the moisture of the thawing ground has sufficiently diminished the earth's friction which has hitherto held it back. It needs no propelling medium; its own weight causes it to slide. The prevailing idea that any small particles of snow set primarily rolling by a bird, or any such unimportant agency, can gradually increase to the dimensions of an avalanche, is a pure fallacy. The rolling is a secondary matter; the primary agent in an avalanche is its sliding. They travel slowly, Rojacher said—that is, there is mostly time for escape on first hearing the roar of the heavy falling mass; with the *Windlahn* is no such hope, as both Rojacher, and all others whom I questioned, assured me.

The *Windlahn* he explained in the following manner. The first falls of winter snow fill up all inequalities of the surface. If it lies for a time, it consolidates and forms an even, slippery surface. More snow falling upon this smooth surface has a tendency, by its own weight, to slide off. This is certain to occur if after a heavy fall of snow the new layer has acquired such weight that its pressure overcomes the slight resistance of the underlying stratum, and any chance obstacles that hold it back. As soon as the top pressure is great enough to start a fissure, the

[1] By Dr. J. M. Pernter, of the Imperial Academy of Sciences in Vienna.

whole mass of the fresh-fallen snow sweeps with the velocity of the wind from off the slippery surface beneath. That is a *Windsbrett*, or *Windlahn*; so called, not that it is caused by the wind, but that in its headlong passage its velocity creates a storm wind which in its turn commits ravages and devastation far beyond the range of the falling avalanche.

I had many opportunities, while at Kolm and on the Sonnblick, of witnessing those terrible avalanches. During the night of February 4-5, a *Windsbrett* fell from Bucheben, filling the whole valley beneath for a distance of two kilometres with 13 feet of snow. The avalanche itself could not force its way up the side of the opposite mountain, but the wind caused by it unroofed a farmhouse, 650 feet above the valley, and blew in the windows.

The day I started for the Sonnblick, a *Windsbrett* parted from the Hoch Narr Glacier, causing such a terrific gale of wind in Kolm that the people were in terror of their lives. The next day we looked down from the Sonnblick on the snow-field whence the avalanche had parted, and Rojacher and his assistant, Peter Lechner, estimated its length and breadth at 650 feet, and depth 13 feet, representing a fallen mass of at least 160,000 cubic metres.

One peculiarity of wind avalanches, that makes them such a special danger to tourists, is that it is so easy to start one unawares. On an inclined, slippery surface of hardened snow, there lies a thick superstratum of fresh-fallen snow, ready, so to speak, to slip away at any moment. It often requires but the weight of one man, and there are generally at least two, to produce the slight pressure that sets loose the avalanche. In such a case there is heard a dull thundering crack, immediately after which, either the mass of snow starts, in which case the men are borne down on it with the swiftness of the wind, seldom to be seen again; or, after the first crack, the mass remains stationary, the *Windsbrett* has "settled," and the travellers proceed scatheless on their way.

I underwent such an experience during my ascent of the Sonnblick, not without considerable alarm, I must confess. Not far from the miner's lodge, at about 7550 feet of altitude, we had to cross a snow-field on a considerable incline. There were fifteen of us, with Rojacher and myself. Arrived at the middle of the incline, we heard a terrific muffled crack. We had started the *Windsbrett*. For a moment we knew not whether to go on or go back, the next we found that we had escaped with the fright—the avalanche had "settled."

It is not easy to say what are the causes that hold back an avalanche once started. It seems as if the "settling" of a *Windsbrett* only occurs when passed along at its topmost end; at any rate, prudence suggests that it is the only safe path to cross one; for, in the event of its giving way, the best hope of safety is to be on the highest point of the falling mass; there is, at least, the possibility of being able to obtain a foothold above, and thus of not being crushed by the on-coming snow. Should the *Windsbrett*, after being started, remain stationary, it is in all probability due to the fact that the lower part of the snow-field is too massive to be set in motion by the unsettlement of the upper portion, and therefore does not partake in the movement. Thus the former "settles."

The account above given of *Windsbretter* will explain why the inhabitants of the regions where they are to be met with maintain that it is next to impossible to escape with life from them. Once hear the fatal crash, the avalanche is upon them, and there is no escaping from it. Their advice is, to throw oneself prostrate, with hands outstretched, if possible behind some rock or boulder; there is the chance that the *Windsbrett* may pass over him, and if buried in the snow, one would be in the most favourable position to breathe, and therefore stand the best chance of being dug out alive; while to stand upright would be, to a certainty, to be carried with it. There were many such cases among Rojacher's people during my stay on the Sonnblick. This and similar talk made the time pass agreeably enough while I was waiting at Kolm.

While thus employing ourselves, Rojacher spoke through the telephone from time to time to his men in the station (Berghaus), 7870 feet above, asking if some thirteen or fourteen of them could venture down to take up my cases. For the first four days, the invariable answer was that there was too great danger of avalanches to undertake the descent; on the fifth day at noon they decided to venture down upon their *Knappenrossen*.[1] Barely an hour after we saw them come tearing down the declivity behind the Kolm house, or rather saw but a thick cloud of snow coming towards us, amid which an occasional hat, or alpenstock, was discernible. After the men had well warmed themselves, and had invigorated themselves with draughts of hot wine, my traps were distributed among them, and at 3 o'clock we started for the station. Our ascent was effected by means of snow-shoes, we keeping carefully to the rut made by the men on their passage down. There were no deviations, the snow had so completely filled up all uneven places, covering rocks and stones with its thick mantle, that it was one straight path. Our ascent was comparatively easy, and in three hours we had reached the Miner's House (*Knappenhaus*), after having, as already related, had a considerable panic from a *Windsbrett* some twenty minutes before.

The weather, which had, so far, been tolerably favourable, had changed for the worse during the night, and I expressed my fears to Rojacher in the morning, that we should be snowed up there for some days. But his calm reply was, "Once so far, we must reach the Sonnblick before dusk, cost what it may." To my objection that we might run the danger of avalanches, he laughingly said, experience had shown him that they had no love for him. It would be an unheard-of thing for one to travel his road. His confidence reassured me, and I made no further demur to continuing our route.

Rojacher, however, added other ten men to our escort, whose duty was to go first and tread down the snow on the way to the plateau, where he expected to find the fall had been much less heavy, and where the extra men could then load themselves with the store of wood, already stacked, for the use of the house on the Sonnblick. Our party now assumed a somewhat droll appearance, marching along in Indian file, across the vast snow-fields. During the whole way to the top we were enveloped in a dense mist; and our ascent through the stupendous masses of fresh-fallen snow, was a very slow one. The first man, the pioneer, sank up to his hips at every step, despite the snow-shoes; in five minutes his strength was exhausted and he fell out, taking his place as the last but one; I always remaining the twenty-fifth man, which made the ascent comparatively easy to me. As each man placed his left foot exactly in the left foot-print of the one who preceded him, and his right foot in the right foot-print, I, as last man, had firm ground to tread, my one care being to plant my feet well into those spaces, and thus I reached the summit but little fatigued. We had taken four hours to make the ascent; and it had enabled me to form some idea of the incredible bulk of snow that can collect on the Hochgebirge. Even on the upper plateau, the snow of the last four to six days had reached a depth of ten feet. This was proved to us, on coming up to the wood-stack. It had been carried up before the last snow-fall, and stacked to a height of about ten feet. Fortunately the men had had the foresight to mark the spot by an upright pole; without this landmark we should never have found

[1] Miner's sledges, formed of stout boards on runners.

it, for the wood was completely buried, and only a short length of the pole visible. Even Rojacher had not foreseen this, he being convinced that falls of snow were considerably less on the heights. So far he was right. The fall had been lighter above than below; but then below it had been almost unparalleled. To have formed an estimate of the quantity of snow that fell that winter on the Tauern, I should have needed a previous knowledge of the locality in summer; as, unfortunately, I had not that, I was obliged to content myself with Rojacher's computation at various points. The deepest level we could see, was on the lower plateau, some 8200 feet above the level of the sea, where the telephone wire stretches over a little glacier valley. Rojacher knew that this wire was carried 66 feet above ground in the deepest part of the valley. On passing by it, we found that the snow not only reached to the wire, but that the valley had become one even snow-field; thus proving a depth of 66 feet in that part. It is unnecessary to give further instances; no description could afford a true idea of the stupendous masses of snow. They must have been seen to be believed. Rojacher repeatedly said how glad he was that a Vienna Professor should have had the experience; and even went so far, in his good-natured raillery, as to wish that—without prejudice to my scientific researches—I might taste to the full the meaning of a severe winter on those heights.

His wish was granted, even beyond his desires, for I spent a February such as had never been known before, not only as regards snow and avalanches, but of destructive storm and variations of temperature. However, although I could have desired finer weather for my investigations, my stay on the Sonnblick was most enjoyable. The mountain sickness, from which I had hitherto always suffered severely, was very slight, and of not above three days' duration. My provisions were good, and lasted out excellently. In fact, I came to the conclusion, as far as health was concerned, that my winter expedition on the Sonnblick suited me infinitely better than a month in the Riviera would have done.

Shortly before I had started on my expedition there had been such accounts in the Vienna papers of the suffering from cold experienced by the man in charge on the Sonnblick, that I expressed some fears whether I should be able to stand the extreme cold in the house. Experience soon set those fears at rest. Our rooms were most comfortably warmed; the heating apparatus is perfect; indeed we had more than once to open a window to let out the hot air. It is quite a fallacy to suppose that one cannot keep warm on the Sonnblick.

These few remarks may serve to show those to whom their *café*, daily paper, *tarok*, or whist club are not matters of vital importance, that a winter sojourn on the Sonnblick has no great difficulties—when once they get there. As for occupation, there need be no lack; at any rate, so I found. On fine days, of which I counted but nine in the four weeks, I could barely give myself time to eat or sleep; they being entirely devoted to the specific objects of my investigations. On wet ones, I had enough to do examining and verifying the meteorological instruments belonging to the Observatory; and in initiating its solitary occupant, Peter Lechner, still further into their uses. The results of my observations have been since reported to the Imperial Academy of Sciences in Vienna.

It was no light work to get my apparatus suitably adjusted, all my observations having had to be made in the open air; and it is thanks to the skill and indefatigable energy of "the Hermit of the Sonnblick"[1] that I succeeded so well. Lechner is a most devoted servant of science, and carries out all his duties on that solitary peak in the most conscientious manner. He assisted me too in my observations on the radiation of the earth, and the sparkle of the stars. As these required to be made at night, the cold rendered it necessary to be well protected with fur-lined boots, fur travelling coat, fur gaiters and fur cap, well down over the ears; otherwise I could not have withstood those nights, standing and sitting, as we often required to do for hours, in a temperature of -4° F.

The simultaneity of my observations with those of Dr. Trabert were certified by the telephone, which acted admirably. The day after I arrived on the Sonnblick, the interruption between Kolm and Rauris had been repaired, and from that time there was only one day when connection was broken again—that time, unfortunately, between the Sonnblick and Berghaus, so that we were quite cut off. The next day, however, the point of breakage was found, and connection made again. It is no little difficulty to find out the point of breakage on such a height, and when the whole wire is buried under the snow.

Herr Rojácher has found a method, I do not know if in use elsewhere—anyway he found it out for himself. It is, of course, known to electricians that two near telephone stations can speak with each other if instead of one of the earth plates, connection is effected by means of any large mass of metal, as a stove, for instance, with which one of the telephones is connected. By analogy it ought also to be known (I do not know if it is) that in the case of three stations, as Kolm, Bodenhaus, and Rauris, should there be an interruption between Bodenhaus and Rauris, if that interruption has occurred near Rauris, Kolm and Bodenhaus would still be able to speak together, although, through the want of the ground conductor, there is no closed circuit. I have made that experiment myself. Now the above-mentioned larger mass of metal can be made to replace the wire from Bodenhaus to the point of interruption, supposing the wire to be long enough. It was on this last hypothesis that Rojacher founded his method—that of seeking the point where communication ceases up in the snow-fields. Taking a hand telephone with him, he starts from one of the stations between which communication is interrupted, and connects the hand telephone with the wire at one of the *Untersuchungstangen* (test poles) that are placed at intervals, and through which the wire passes, thus raised in triangular form out of the snow. As long as he can still speak with the station whence he has come he knows that the breakage has occurred farther on. When he can no longer speak he fixes a trumpet on to the telephone; if the answer, also spoken through a trumpet, be audible, the point of breakage is not far off. If the trumpet tone reaches his ear no longer, the spot is close, and a little examination enables connection to be re-established. Only by this method could connection be as quickly restored under difficulties so immense; and it is by this means that Rojacher is enabled to send out regular meteorological observations, with scarce a break, through an electrical aparatus perhaps the most perilously placed in the world.

During my stay on the Sonnblick I had opportunity to witness many rare atmospheric effects; and to become more closely acquainted with meteorological phenomena at that altitude. The second day I was there I saw a splendid sight. A white mist enveloped the whole base of the mountain up to within 500 feet of the summit; the shadow of the house on the Sonnblick being clearly projected on it. Suddenly the shadow was surrounded by a

[1] Alone for the most part throughout the year, cut off from all intercourse during the worst of the winter months, his occupation is to speak through the telephone three times daily, to record his readings on the maximum and minimum thermometers, on the sunshine recorder, the psychrometer, the hygrometer, and the hygrograph, on the anemometer, the barometer, and several other instruments; he hears, besides his own voice, generally that of one of his former comrades at the Miner's House where he used to work, inquiring, "Is all well on the Sonnblick?" And then the former silence is resumed.—Translator's note, from *Standard* of December 18, 1889.

triple rainbow of dazzling brightness. Had I not known that my eye was the centre of the exquisite sight, I must have judged the house, or rather its shadow, to be its central point. This I disproved by moving from east to west of the house, when the whole "glory" seemed displaced. I did not succeed in projecting my own shadow upon the mist, and in producing the effect myself; the "glory" remained attached to the shadow thrown by the house. I observed the same atmospheric effect several times afterwards while there, but never with such brilliancy. Another time I was struck on observing a magnificent ring round the sun, accompanied by other lesser rings. The sun was then in the east, about 14° above the horizon, and exactly over the peak of the Kleinen Sonnblick, at no great distance. The solar ring was 23½° radius, and of indescribably brilliant prismatic colours. At both extremities of the horizontal diameter was a lesser coloured sun of radiant brightness; but the strangest part of it was that I could see the lower portion of the vertical diameter of the solar ring, although it was more than 7° below the horizon. And now there appeared a lesser sun of dazzlingly white appearance, seeming as though rising behind the mountain peak; its dazzling whiteness rayed out high up into the heavens, forming, as it were, a column of light resting upon the Kleinen Sonnblick. On passing a horizontal line through this white secondary sun below the horizon, I found at a distance of 23½° to right and left of it, two coloured lesser suns, which, being also below the horizon, were projected on to the snow-fields of the Kleinen Sonnblick, and of the Goldberg-Spitze, forming a magical effect—indeed, the whole spectacle was one of entrancing beauty.

One lovely moonlight night, I was standing in front of the house, making observations with the scintillometer. After a time I was conscious of a series of rapid obscurations flitting over the field of my telescope. Looking up irritably, I perceived that small portions of the mist, which reached almost to the summit of the mountain, were being detached and borne swiftly over my head. My irritation, however, was quickly dispelled on looking at the moon through these icy veils of mist. Whenever a fleecy cloudlet passed between the moon and me, there was a gleam and lustre of rainbow hue with such intense brilliancy of the lunar surface that I had never seen the like before. I leave my readers to imagine the effect of this ever-changing moon, now of silver lustre, now iridescent with many-coloured rings, and they will understand that I quite forgot my interrupted observations in the absorbing sight.

The zodiacal light I saw there also, and more brilliantly than ever before. I cannot do better than recommend any one who is a lover of aërial effects to pass a winter on the Sonnblick. And perhaps the finest sight of all is the magnificent view—the grand panorama to be seen from such a height. The view from the Sonnblick, even on a fine summer's day, must be a sufficient reward for the toil of the ascent; on a fine day in winter it surpasses all description. The clearly marked horizon, on which there is no trace of mist or haze, the mountain ridges, even to the most remote, standing out in lines of perfect distinctness from the sky—the grandeur of the whole snow-clad scene is so overwhelming, that I could but express my surprise to Rojacher and his assistant, that no members of the Alpine Club had availed themselves of the hospitality of the house on the Sonnblick, to know and enjoy the delights of a fine winter's day on the Hochgebirge. Formerly the difficulty would have been that without shelter one could only have stayed a few minutes on the summit, and had the weather been unfavourable in those few minutes, the whole ascent would have been fruitless. But now that there is shelter on the summit, and a house so comfortably arranged, the whole difficulty is done away with. I have a strong conviction, moreover, that the ascent in winter is easier than in summer—given a normal winter with average snow-fall. It is far less fatigue to ascend steep places and cross glaciers on a moderate layer of new-fallen snow; one does not become so heated, and consequently breathing is not so difficult as in summer. And then, the infinitely finer view.

I am convinced that it can only be the inconvenience of leaving their business or professional callings at that busy season that has hitherto kept men back. So fascinated was I with the view, that I determined to advise all whose duties would permit them to pass a few winter days on the Sonnblick—the more surely that I can vouch for Herr Rojacher's hospitality removing all doubts on that score.

If phenomena of light most pleased the eye, other meteorological conditions gave me fuller scope for observation. In the first place, the height of the clouds. For the most part, unluckily, we were in them. Often we were above them, and had then the grand sight of the vast sea of cloud surging and swaying beneath us, now rising, now falling, called *Nebel boden* or *Boden nebel*. Several times, for days together, only those mountains whose peaks were higher than 8200 feet rose above the clouds; and we would be walking about in bright sunshine, while the valleys beneath were filled with cloud. At other times the northern valleys would be quite clear, and the southern ones full of cloud, or *vice versâ*. One evening we had the southern valleys a mass of cloud, the next morning, on looking out, they were perfectly clear, and the northern ones were thickly enveloped. It was as if the clouds had travelled over the Alps in the night from south to north.

With the exception of the cirri, I never saw clouds above us. These are easily traced to their source from the Sonnblick. They were more unwelcome to me even than the mist; they disturbed my observations to such an extent.

It is known that the cirri take their rise from the depression centres. Thus they were serviceable to me in determining the situation of the minimum pressure of the air. Nearly the whole of my stay on the Sonnblick depressions formed with curious persistency over the Tyrrhenian sea, passing over southwards. This was distinguishable to us by a heavy bank of cloud in the extreme south-west, whence the cirrus bands stretched out in our direction. With a change of depression to south-east, or east, the radiating point of the cirri shifted accordingly. We had nothing to fear from the southern depression; in fact, it in no way affected the weather on the Sonnblick. But if the cirri rose from the north-west, although from the extreme distance the heavy cloud bank was not visible to us, none the less were we certain within six to twelve hours that storm and mist would be the invariable consequences.

In the many violent storms I witnessed there, I directed my attention chiefly to two questions: Do the winds blow in gusts here on the summits of mountains, standing free as they do in the atmosphere? and What is the relation of the gusty winds to the "pumping" of the barometer? I had formerly been somewhat of opinion that on these free heights there was no sufficient cause for storms to blow in gusts; and in fact in storms from the south-west the gusts appeared to me to be considerably less than in Vienna, although fully perceptible. But with a gale from the north they far exceeded in violence anything on a lower level. I have no time to go more closely into this question, and will only briefly describe those of my observations which bear upon the "pumping" ("oscillations") of barometers during a storm. It is a subject that has been much under discussion of late; I will confine myself to my observations. I made use of four instruments—a mercurial barometer, a very fine Naudet's aneroid, a Richard's barograph, and a Redier's barograph. My observations, made alternately with these four, came to the same result. If the wind appeared to have lulled for a short time, there would be a sudden fall in the barometer of often more than two millimetres. A violent gust would then follow on the fall in the barometer, its strength varying in proportion to the fall of the barometer. During the gust the barometer would rise nearly as much as it had previously fallen.

From these observations, carried on through whole days, and often far into the night, it seemed to me that the cause of the gusts must be that slight, quickly passing depressions were over us.

If these observations are correct, and I can hardly doubt them, the suction of the wind is of secondary importance in considering the causes of the "pumping" ("oscillations").

I cannot allow myself to enter into all the interesting meteorological subjects that there presented themselves, and my views upon them, without trespassing too largely on the space assigned to me in these pages. I would only refer briefly to what I observed of the marked electrical activity in the telephone. It may seem strange to speak of a strong electrical development in winter, and I must confess to have been surprised on many days to hear a loud crackling at the telephone, so loud that it was almost impossible to speak through it. Still more astonished was I to see electric sparks going off from the electric plate ("*Blitz Platte*"). Unfortunately I had not time to examine this increased electric activity in its relation to the weather; but I fancied that a fall of snow with a south wind had most influence upon it. I requested Lechner to make daily observations of the crackling in the telephone, at a given hour, and to register the four stages—weak = 1, moderate = 2, strong = 3, electric sparks = 4. I have heard from him that he has been recording his observations five times a day, and, he thinks, with good result. A prolonged series of observations will easily determine its cause.

From these hastily collected extracts of my experiments and investigations on the Sonnblick, all must be satisfied of what great importance to science is the Observatory on its summit, and not less to Alpinists. It matters little how highly I prize it; my aim is to make its value known in wider circles.

But it behoves us, scientific men and tourists, not merely to wax enthusiastic over the Sonnblick Observatory, but to take measures to ensure its permanency. I am aware that the Alpine Club has already done its part,[1] and do not doubt but that in future it will shrink from no sacrifice to uphold and support this, its foster child, which, in conjunction with the Meteorological Society, it has brought into life. But I am inclined to think that there are nearer supporters of this our most important mountain Observatory, on whom there exists a prior claim. I am under the impression that certain influential members of the Alpine Club had been called upon to form a special Sonnblick Verein, part scientific, part tourist, who by a small yearly subscription should ensure the keeping up of this invaluable station.

My descent from the Sonnblick began on March 4, amid a storm of north wind, mist, and temperature at −22° F. We rode down on miners' sledges (*Knappenrossen*), but even then had great difficulty in forcing a passage, snow having fallen knee-deep overnight. We often had to call a halt, and wade through the snow, thereby causing great delay; it took us two hours to reach Kolm, a distance usually accomplished in one.

On March 5 I reached Rauris; leaving on the 6th with Dr. Trabert for Lend. Even on these two last days, the weather followed us with unremitting severity. The way from Kolm to Rauris had been made under a heavy snowfall; and in the night of the 5th–6th there were such deep snow-drifts, that we were two hours making our way from Rauris to Landsteg.

On March 7 we reached Vienna.

[1] The corporation of the Alpine Club has just signed an agreement with Herr Rojacher, by which it guarantees him a grant of 5000 fl. towards the enlargement of the Sonnblick Observatory.

BEDFORD COLLEGE.

SOME time ago we drew attention to the fact that Bedford College, which has done so much for the education of women, was in need of funds. The new laboratories are now in use, but they are not yet paid for, and the stock of apparatus is not all that could be desired. Our readers will remember that Mr. Henry Tate had promised a donation of £1000 provided the Council could raise a like amount from other sources. We believe that the College authorities are nearly in a position to claim his generous gift; but though this will free the building itself from debt, at least £500 more is wanted to pay for equipment on a very moderate scale.

The last twelve months have been, in matters educational, a ladies' year; but the true meaning of the successes which have been won at Cambridge and elsewhere will be missed, if they are regarded only as a nine days' wonder, or as proving *ambulando* that the higher levels of undergraduate attainment can be reached by girls. The lesson which has been so strikingly enforced is that no branch of learning is the exclusive property of either sex, and that girls are wronged if we do not afford them the same opportunities for acquiring knowledge which are provided for their brothers.

The founders of Bedford College acted on this principle when it was not so widely accepted and not so self-evident as in 1890, and we can only urge on the friends of the education of women not to forget, in the hour of their triumph, the toilers who have paved the way to their success.

In an unpretending building in an uninteresting London street an effort has for long been made to supply education of the highest class for London girls. Faith in the future and effort in the present have never been wanting, even when the story of the past seemed most discouraging. The College is now undeniably a success, but it is still sadly hampered by want of means. The adequate equipment of its laboratories is surely an object for which an appeal will not be made in vain to those who believe that the benefits which science can confer will never be fully attained till a knowledge of its main principles and methods forms part of the training of all educated men and all educated women alike.

NOTES.

WE regret to have to record the death of Mr. William Kitchen Parker, F.R.S., formerly Hunterian Professor of Comparative Anatomy at the Royal College of Surgeons. Next week we hope to give some account of his services to science.

A REUTER's telegram from New York states that the remains of the Swedish inventor, John Ericsson, will be conveyed to Sweden by one of the two new American war-vessels, *Baltimore* and *Philadelphia*.

THE Dutch Academy of Sciences in Haarlem has offered a gold medal of the value of 150 gulden for the best work in each of the following subjects:—(1) Researches on the part played by bacteria in the decomposition and formation of nitrogenous compounds in various kinds of soil; (2) Microscopic investigation of the mode in which different parts of plants can unite with one another, and especially the phenomena which accompany healing after the operation of grafting. The papers must be written in German, Dutch, or Latin (not in the handwriting of the author), and must be forwarded to Dr. J. Bosseka, Haarlem, by January 1, 1891.

The list of Civil List pensions granted during the year ended June 20, 1890, includes the name of Dr. William Huggins, to whom has been awarded a pension of £150. As we have already noted, a pension of £50 has been granted to Mrs. Jane Eleanor Wood, widow of the Rev. J. G. Wood, and a pension of £20 each to the four unmarried daughters of the late Rev. M. J. Berkeley, F.R.S.

Mr. David S. Capper, Assoc.M.Inst.C.E., has been elected to the Professorship of Mechanical Engineering at King's College, London.

The annual meeting of the Botanical Society of Italy will take place in Verona during the month of September.

French papers announce the death of M. Paul Loye, at the early age of 29. He was the author of a memoir on the physiology of death by decapitation, and had published many short notes on physiological questions. He had for some time been engaged in an elaborate study of the excretory functions of birds, concerning which he had collected many facts. M. Loye was assistant to Prof. Brouardel, and *Maître de Conférences* in the Faculty of Sciences of Paris, and had been Paul Bert's last assistant.

The death of M. Alphonse Favre, at the age of 77, is announced. He was formerly Professor of Geology at Geneva, and was recognized as an authority on the geology of the Alps.

The half-yearly general meeting of the Scottish Meteorological Society was held in Edinburgh on Monday, July 14. Lord McLaren presided. The following was the programme of business:—(1) Report from the Council of the Society; (2) address by the Chairman on the high and low level observatories of Ben Nevis; (3) on the meteorological conditions of desert regions, with special reference to the Sahara, by Dr. John Murray. In their report the Council express sincere regret at the death of Dr. James Stark, who long held the office of Superintendent of the Statistical Department in the Register House, Edinburgh, and gave very effective aid in founding the Society. The self-recording instruments, furnished by the Meteorological Council for the low level observatory at Fort William, arrived at the end of June, and it is contemplated that the regular work of recording the continuous observations will begin in August. The observations which have been carried on in Fort William by Mr. Livingstone in connection with those made at the top of Ben Nevis will be continued at least till the New Year, in order that a comparison may be made with them and the similar eye-observations made by Mr. Omond at the Observatory adjoining. It is arranged that Dr. Buchan's time will be wholly given, during next year, to the examination and discussion of the observations of the Ben Nevis observatories. In connection with this difficult and laborious undertaking, Mr. Omond will receive from the Meteorological Council three copies of their daily and weekly weather maps, on which he will enter certain of the meteorological data from the high and low level observatories, together with occasional remarks that may from time to time strike him as bearing more particularly on forecasting weather. The weather maps give two daily representations, with remarks, of the weather of Europe at 8 a.m. and 6 p.m. Thereafter, one of the three sets of maps will be sent to the Society's Office, the second to the Meteorological Office of London, and the third will be retained by Mr. Omond.

The Council also refer to the observations of Mr. Rankin on the number of dust particles in the atmosphere, carried on with the two sets of apparatus invented by Mr. Aitken. Though it would be premature to offer a statement of positive results, the Council think that some interesting conclusions appear to be indicated by the observations. The maximum number of dust particles in a cubic centimetre hitherto observed is 12,862, on March 31, and the minimum 50, on June 15. On March 31, at 4.30 p.m., the summit was clear, and the number of particles was 2785, but shortly thereafter a thickness was seen approaching from south-west, which by 6 p.m. reached the Observatory, and the number of particles rose to 12,862. On June 15 many observations were made during the day, when the number of particles fell from 937 at midnight to 50 at 10.30 and 11.42 a.m. The observations point to a daily maximum during the afternoon minimum barometer, and a minimum during the morning minimum barometer—these being probably intimately connected with the diurnal ascending and descending currents of the atmosphere. Interesting intimate relations are also indicated between the numbers of dust particles and the cyclones and anticyclones over North-Western Europe at the time. The observations also indicate that the dust particles may vary enormously during the presence of mist or fog, without being accompanied by any difference in the apparent density of the fog. The Council consider that the inquiry is an extremely hopeful one; and in view of the relations with cyclones and anticyclones, its bearings as regards the forecasts of the weather will be very specially investigated.

For several years past it has been the practice of the Indian Meteorological Department to issue in the month of June a forecast of the prospects of the monsoon rains, based partly on the reported extent and thickness of the Himalayan snows, partly on the distribution of the atmospheric pressure, the small variations of which are found by experience to be remarkably persistent in India, and to serve as an indication of the probable strength of the monsoon, and alternatively of the prevalence of dry land winds. The forecast for the forthcoming season announces that owing to the very slight snowfall of Afghanistan, Baluchistan, and almost the whole of the Himalayan region, the conditions are eminently favourable for a good strong monsoon. The only unfavourable indication is that the past winter has been very severe in Yarkand, and perhaps in other distant parts of Central Asia. The pressure is unusually low this year in Bengal, and above the average in Central India and the northern half of Bombay, and the local pressure conditions considerably resemble those of 1876. It is therefore considered probable that while the eastern half of the Ganges valley, Assam, and Burma will receive early and abundant rain, the rains may be late and scanty over a considerable area of North-Western India.

The Rev. E. Colin, S.J., Director of the newly-established Royal Observatory of Madagascar, at Tananarivo, has published the monthly results of meteorological observations at that place during 1889. As observations for Madagascar are scanty, we are glad to learn that observations are now taken at four stations in various parts of the island, and that others will shortly be established. The maximum temperature at Tananarivo, 87°·4, occurred on November 14, and the minimum, 41°·0, on July 31. Rain fell on 89 days; by far the greatest quantity falls between November and March. None fell in May 1889. The prevalent wind direction is between south-east and north-east. The Report contains summaries for the three other stations referred to, during 1889, and for Tananarivo from 1872–88. Some of the latter have never been published before, and form an important addition to our knowledge, but, having been made by various persons, may not be so trustworthy as those made at the Observatory.

The meeting lately held at the Mansion House, under the presidency of the Lord Mayor, for the furtherance of the International Congress of Hygiene, which will assemble in London in 1891, was attended by many influential medical men and students of sanitary science. Sir Douglas Galton explained the object and organization of the Congress, to which delegates had been already appointed by all the leading Societies of Great Britain and of the Continent. He mentioned that in any case the cost of the Congress would be considerable—probably not less than £5000—and that an appeal would be made to raise the required funds and to make the gathering worthy of Great Britain. Among the subsequent speakers were Lord Wantage, Prof. Humphry, Mr. Ernest Hart, Sir Spencer Wells, Sir Henry Thompson, and Dr. Thorne Thorne. The organizing committee is now taking steps to raise a sum of at least £5000, and no doubt its appeal will receive a liberal response from some of the great Societies and Corporations as well as from private individuals.

IN order to make the Parkes Museum, which is supported by the Sanitary Institute, available to all classes for the purpose of obtaining information on matters relating to hygiene and sanitary appliances, the Council have resolved to throw the Museum open free at all times except when meetings are being held.

THE Medical Academy for Women at St. Petersburg is to be reopened. At its sitting of June 9, the municipality of that city voted a yearly grant of £3000 for the support of the Academy, and decided to give it the use of a house belonging to the municipality, and to open the city hospitals to the students. Private subscriptions fully guarantee the further existence of the Academy. It is hoped, therefore, that the Government will not oppose the reopening of the institution, which has already given to Russia no fewer than 698 lady doctors. The decision of the municipality was based upon a report by Dr. Archangelsky, who speaks very favourably of the work done by the eleven lady doctors who are in the employment of the municipality for the inspection of city schools and the poorer districts of St. Petersburg.

THE joint meeting of the Essex Field Club and the Gilbert Club, held at Colchester on July 5, proved a great success in spite of the continuous downpour of rain which lasted throughout the day. Over fifty members of the two Societies assembled at 11.30 in the Castle Museum, where the Hon. Curator, the Rev. C. L. Acland, and Mr. H. Laver pointed out the objects of interest to the visitors. The party then visited Holy Trinity Church, wherein lie the remains of Gilbert, and which contains a mural tablet erected to his memory by his brothers. After inspecting the house in which Gilbert was born, and other places of local interest, the visitors adjourned to luncheon at the Red Lion Hotel, the chair being taken by Lord Rayleigh, who was supported by the Mayor of Colchester, the President of the Essex Field Club, and many well-known men of science and local residents. Among those present were Profs. D. E. Hughes, F.R.S., G. D. Liveing, F.R.S., J. Perry, F.R.S., R. Meldola, F.R.S., and S. P. Thompson, Messrs. G. Kapp, J. Paxman, Conrad Cooke, and F. H. Varley. The Chairman made a short speech, in the course of which he alluded to the importance of Gilbert's work, and pointed out that, although it is to Gilbert that we are indebted for the theory that the earth is a great magnet, we are not much in advance of this position at the present time, as nobody has yet explained the origin of terrestrial magnetism. The Mayor of Colchester then took the opportunity of welcoming the two Societies to the town on the part of the inhabitants. After luncheon some of the party drove to the Vale of Dedham, rendered famous in art by the paintings of Constable, who was born at Flatford Mill in this district. In the evening a reception was given at the Town Hall by the Mayor and Mayoress. Many electrical novelties were exhibited, and an incandescent light installation was supplied from premises on the other side of the road, where plant had been erected by Messrs. Christy, Son, and Norris, of Chelmsford. An interesting piece of apparatus, constructed on the pattern of Crookes's radiometer, but working in air instead of in a vacuum, was exhibited by its inventor, Mr. C. E. Benham, who attributed its rotation to the action of convection currents. There were also on view exhibits by Messrs. Crompton, of Chelmsford, lathes and sewing-machines worked by an electric motor, and other objects of interest. Prof. S. P. Thompson delivered an interesting lecture on the early magnetic experiments of Gilbert, illustrating his subject by experiments shown with the projecting lantern. A vote of thanks was proposed by the Mayor, and seconded by Mr. J. Paxman, who remarked that he should like to see Gilbert honoured not only by a statue in his native town but also in a more useful way, such as by the foundation of a Gilbert Scholarship in connection with one of the Universities. A vote of thanks was proposed by Prof. Meldola on behalf of both the Clubs to the Mayor and Mayoress, to Dr. Laver, and Mr. J. C. Shenstone, all of whom had by their exertions contributed to the success of the day's proceedings.

IN a paper on ornithophilous flowers,[1] contributed to the *Annals of Botany*, Mr. G. F. Scott-Elliot records the very interesting observation that the Cinnyridæ or sun-birds, which play an important part in the fertilization of flowers in South Africa, have the same habit as the Apidæ in other countries—that is, of not "mixing their honey," but, on the same journey, confining their visits pretty much to the same species of flower. The species of sun-birds which are especially good fertilizers in South Africa are *Nectarinia chalybea*, *N. bicollaris*, and *Promerops caper*. In accordance with the view of Darwin, but opposed to that of Wallace, Mr. Scott-Elliot believes that the identity of colour (an unusual shade of red) in the majority of ornithophilous flowers and on the breasts of species of *Cinnyris* is an important element in pollination by birds.

A NEW little magazine, which ought to be of service to those who devote attention to questions relating to manual training, has just been started. It is called *Sloyd or Hand-Craft*. Its primary object is to acquaint the members of the Home Sloyd Union, and all those who are interested in the development of a distinctively English form of manual instruction, with the progress of the Sloyd system as practised in this country. But it is by no means intended to exclude what is being done in other directions for the purpose of making education more practical by means of hand and eye training, more especially as regards children from eleven to fifteen years of age.

BY an Order in Council, dated June 30, 1890, which has been issued as a Parliamentary paper, it is prescribed that the following monuments in Ireland shall be deemed to be ancient monuments to which the Ancient Monuments Protection Act, 1882, applies:—

Monument.	County.	Parish.
(1) Cahernamactierech and Bee Hive Structures on the Promontory of Dingle ...	Kerry ...	Drumquim and Ballinroher.
(2) Round Tower, Lusk	Dublin ...	Swords.
(3) Round Tower, Kells	Meath ...	Kells.
(4) Stone Cashel with Galleries ...	Sligo ...	Cashelmore.
(5) Stone Circles and Pillar Stones ...	Fermanagh	Enniskillen.
(6) Round Tower of Tulloheran ...	Kilkenny ...	Tulloheran.
(7) Round Tower of Rathmichael, Church and Stone Cross	Dublin ...	Rathmichael.

A CALIFORNIAN salmon (*Oncorhynchus quinnat*, Günther) has recently been caught in the Mediterranean, near Banyuls. Probably it found its way thither from the River Aude, into which many young fish of this species have been introduced, in the hope that they may be acclimatized in France.

A PORTRAIT of the African explorer Captain Gaetano Casati forms the frontispiece of the May number of the *Bulletin of the Italian Geographical Society*. Casati reached Cairo early in May, and letters in the *Bulletin* deal with his journey to the coast with Emin and Stanley. An itinerary of his nine years of travel shows that he left Suakin for Berber and Khartoum in January 1880. In July of the same year he started in a sailing-boat down the White Nile to Mishra-el-Rek, and thence on foot to Wau, where he met with Gessi at the end of September. He then threaded his way southwards among the feeders of the Bahr-el-Ghazal to the Congo basin, and for some time made Tangasi, on the Welle or Makua branch, a centre for exploration. Close by, at Mboro, in June 1881, he met with Dr. Junker. Finally, he made his way to Ladò, on the main stream of the White Nile; and there, at the end of March 1883, he met Emin Pasha for the first time. Thence he walked up the left bank to Wadelai, and continued the voyage up the Albert Nyanza by steam-boat. It was not until April 28, 1888, that the meeting between Emin Bey, Casati, and Stanley took place on the plateau above Kavalli to the south-west of the lake. The journey down the Semliki valley, the exploration of Lake Albert-Edward, and the return to Zanzibar, are recent history. The remaining papers of the number deal mainly with South America. The most interesting of these is that of Count Orsi di Broglia di Mombello on the sculpture of the primitive inhabitants of the Upper Orinoco. Many carvings on the stones of tombs have been discovered among the villages of this district; the sculpture is rough and fantastic, but evidently aims at reproducing certain natural objects. Thus, at the Grotto of Caicara, near the right bank of the Orinoco, many rocks carved in the primitive manner of the slate sketches of school-days, evidently exhibit an attempt to figure a tiger that is very common in this district. In neighbouring caves were found mummies closely resembling Egyptian ones; this the author regards as further evidence of the common origin of the two races, previously suggested by the striking similarity in shape of the skulls of the South American Indians and those found in the tombs of Egypt.

A SWEDISH Expedition to Cameroon is being arranged by the Academy of Sciences in Stockholm. The object of those who are to take part in it will be to study the fauna of the Western Cameroon Mountains, and to make scientific collections for the Academy. Herr Yngve Sjöstedt is to be in command of the Expedition, which is expected to be absent for about fifteen months.

WE have received the following details of the researches in which Prof. Bastian is engaged on behalf of the Anthropological Museum of Berlin. In December last he forwarded to Berlin the results of excavations made at Tashkent; the terra-cotta vases and utensils all bearing strong evidence of Greek influence in their workmanship. During January he spent some time in Zanzibar and Mauritius, and at the latter place he was enabled to make a collection of Mascarene curiosities. From Tinivelly, in Southern India, he forwarded some bronze idols in February. March was spent at Malabar, April at Mysore, the beginning of May in Beloochistan, and the latter end of the month in Peshawar. Prof. Bastian has sent interesting ethnographical collections from all these districts.

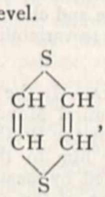
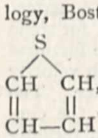
SIR ARTHUR GORDON lately received from the Pandits and Buddhists of Ceylon addresses in which, among other things, he was praised for the encouragement he had given, during his term of office, to science and learning. "Your Excellency," said the Buddhists, "with the laudable wish of preserving the philosophy and sciences contained in that most noble language the Pali, which is regarded by Eastern nations as the original language and the depository of the teachings of the blessed Buddha, as well as those found in the Sanskrit—the language of the gods—has caused many works, such as the 'Mahawansa' and others, to be translated into English, and given an incentive to the publication of Pali and Sanskrit works by allowing them to be printed at the Government Press." The Pandits took occasion to express a hope that Sir Arthur might still continue to exercise his influence on their behalf:—"It is with great pleasure that, whilst we gratefully express our thanks for the benefits already received at your Excellency's hands, we at the same time seize this opportunity of begging your Excellency not to relax your efforts on behalf of our literature and archæology, but to impress upon your Excellency's successor, as well as on Her Majesty's Government, the need not only to continue, but to increase, the exertions that are now being made to preserve the recollection of our glorious past, as an incentive to our countrymen of the present day to noble aims and heroic efforts in the future."

SOME discussion has been going on in Ceylon of late over the question of the language spoken by the Veddahs, the aborigines of that country. The subject (says the *Colonies and India*) would seem to be one well worthy the attention of philologists, and the brothers Sarasin, who have been pursuing their anthropological researches in Ceylon lately, express the opinion that if a philologist were to take the matter up great service would be rendered to all those engaged in the work of scientific research in the island. Tennant says of the Veddahs, "Their language, which is limited to a very few words, is a dialect of Singhalese without any admixture from the Sanskrit or Pali—a circumstance indicative of their repugnance to intercourse with strangers." Prof. Schmidt, of the Leipzig University, who visited the Veddahs last year, says, "Their language is similar in construction to the Dravidian languages—that is, similar in grammatical construction; but they have adopted a great number of Singhalese words," which enabled him to hold converse with them by means of a Singhalese interpreter. The Drs. Sarasin also managed to make themselves understood by means of Singhalese.

IN the last issue of the *Records* of the Geological Survey of India, Mr. Griesbach's mission to Afghanistan is thus referred to:—"Mr. Griesbach returned to India last July. His work with the Ameer was, as is now so very largely the case in the Survey, geologico-industrial, though this was greatly retarded by unforeseen political complications in the State. During his journey in 1888, up the Logar Valley to the Khurd Kabul Valley, Upper Wardak, Cherkh, Kharwar, Zanakhan, Ghazni, &c., the most interesting geological work was the recognition of at least three horizons: the Rhætic with *Lithodendron* (in Kharwar), the Upper Jurassic (or possibly Neocomian) plan-beds near the Shutargardan; and, finally, well-developed nummulitics (in Kharwar and Shilghar). He examined the copper lodes of the Logar and Khurd Kabul areas, the magnesite of the Logar and the entrance to the Taugi Wardak, the graphite of Cherkh, the iron and lead ores of Kharwar, and the argentiferous lead ore of Zanakhan near Ghazni. It turns out, also, that the entire Upper Surkh-ab Valley from near Doab-i-Mekzari to near Dahana Iskar is practically one big coal-field with numerous thick seams of good coal of Triassic and Rhætic age."

THE official Report of the survey work done towards the close of the Chin-Lushai Expedition shows, according to the Allahabad *Pioneer*, that the Boinu River, which flows only six miles west of Haka, is undoubtedly the main stream of the Koladyne, which was so familiar in connection with General Tregear's movements. Captain Bythell, R.E., who was at first on the Chittagong side, accompanied General Symons on his tour southwards from Haka, and traced the stream to within twelve miles of where he had last seen it from the Blue Mountain side. It is satisfactory to have this confirmation of the statements sent by correspondents with the Field Force, particularly as the upper course of the Koladyne was unknown to our geographers. The total area of topography, by the way, covered by the operations of Captain Bythell's party, is put down at about five thousand square miles, while the surveyors on the Burmah side must also be credited with work on a similar scale. The new maps of the Chin-Lushai hills, when they come to be published, will no longer show those great blank spaces which have hitherto been so noticeable in the old issues.

IN Grinnell Land, at sea-level (81° 44′ N. lat.), the mean day temperature is above freezing-point from about June 13 to August 23, *i.e.* 72 days. It has been recently pointed out by Dr. Hann, that on the top of the Sonnblick, at a height of about 10,000 feet, and in 47° N. lat., the temperature returns above freezing-point about the same time (viz. June 8); but it is not till the end of September or beginning of October that it goes below that point again. On the other hand, the mean summer temperature on the Sonnblick is considerably lower than in Grinnell Land at sea-level.

BIOPHENE,  , an entirely new compound intermediate between the fatty and aromatic series, and somewhat resembling thiophene in properties, has been prepared by Dr. Louis E. Levi, of the Massachusetts Institute of Technology, Boston (*Technology Quarterly*, May 1890). Thiophene,  was discovered by Prof. Victor Meyer in 1883; the discovery attracted considerable attention at the time, and has since led to the preparation of a whole series of derivatives analogous in many cases to those of benzene. Dr. Levi worked for some time in the laboratory of Prof. Meyer, and has subsequently followed up ideas then discussed, which have now resulted in the preparation of biophene. Just as thiophene is obtained by the action of phosphorus trisulphide upon succinic acid, so biophene is found to be produced by the action of trisulphide of phosphorus upon thio-diglycollic acid, $COOH—CH_2—S—CH_2—COOH$. A mixture of five grams of thio-diglycollic acid with ten grams of phosphorus trisulphide is heated, together with 15–20 c.c. of ether, in a sealed tube for two hours at a temperature of 170° C. After cooling, the end is opened at the blowpipe, when a great rush of accumulated sulphuretted hydrogen gas occurs. The contents of the tube are separated in the usual manner by means of a tap funnel, and washed with caustic potash solution. After withdrawing the alkali, the remaining oil is dissolved in ether and dried by means of fused calcium chloride. The ether is finally evaporated, and the residual oil fractionally distilled. As the result of this latter process, a liquid is eventually obtained boiling between 165° and 170°, which on analysis yields numbers agreeing with the formula of biophene, $C_4H_4S_2$. When biophene is mixed with sulphuric acid and a crystal of isatine added, a beautiful violet coloration is produced, a reaction which appears to be analogous to that of thiophene, which produces with sulphuric acid and isatine a dark blue coloration. Biophene also reacts with acid chlorides in presence of aluminium chloride like thiophene, thus with acetyl chloride aceto-bienone or bienyl acetyl ketone, $C_4H_3S_2 . CO . CH_3$, is produced, hydrochloric acid being eliminated. This ketone is a thick, heavy liquid which may be distilled in steam and possesses an aromatic odour somewhat resembling that of aceto-thienone. Heated alone aceto-bienone boils, but with decomposition, at 300°. Sunlight rapidly turns it dark brown. Aceto-bienone also reacts with phenylhydrazine with formation of a compound of the composition $C_4H_3S_2 . C\left\langle\begin{matrix}CH_3\\N_2HC_6H_5\end{matrix}\right.$. The similarity of biophene in all the above reactions to thiophene and benzene is very striking, the replacement of two of the CH groups of benzene by sulphur not being accompanied by any very great change in chemical behaviour. The formation of biophene from thio-diglycollic acid, also affords another instance of the passage from the fatty series to bodies of aromatic properties, and biophene itself will stand as an additional link between the two series.

THE additions to the Zoological Society's Gardens during the past week include a Great Anteater (*Myrmecophaga jubata* ♀) from British Guiana, presented by the Directors of the Botanical Gardens, Demerara; an Egyptian Gazelle (*Gazella dorcas*) from Suakim, presented by Commander W. Crofton, R.N.; a Cape Ratel (*Mellivora capensis* ♀) from Suakim, presented by Captain J. F. M. Prinsep; a Jackal Buzzard (*Buteo jacal*), a —— Hawk Eagle (*Nisaetus spilogaster*) from Cape Colony, presented by Mr. W. H. Wormald; a Guillemot (*Lomvia troile*), British, presented by Mr. T. H. Nelson; a Greater Spotted Woodpecker (*Dendrocopus major*), British, presented by Mr. W. H. B. Pain; an Australian Crow (*Corvus australis*) from Australia, deposited; two Chinchillas (*Chinchilla lanigera*) from Chili, an Indian Chevrotain (*Tragulus meminna* ♂) from Ceylon, an Elate Hornbill (*Ceratogymna elata*), a White-necked Crow (*Corvus scapulatus*) from West Africa, a Large Grieved Tortoise (*Podocnemis expansa*) from the Amazon River, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on July 17 = 17h. 42m. 41s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 4361	—	—	17 57 9	−24 21
(2) G.C. 4415	—	—	18 22 44	+74 31
(3) 74 Herculis	5·5	Yellowish-red.	17 17 15	+46 21
(4) σ Ophiuchi	4	Yellow.	17 21 6	+ 4 14
(5) α Ophiuchi	2	White.	17 29 48	+12 38
(6) D.M. + 36° 3168 ...	8	Red.	18 28 31	+36 55
(7) R Lyræ	Var.	Reddish-yellow.	18 51 59	+43 48

Remarks.

(1) The spectrum of this remarkable nebula has not yet been completely examined. In 1868, Captain Herschel observed two lines in the spectrum, and, in addition, a decided continuous spectrum from the brightest point, which is "*not* stellar." These lines are stated to be ill-defined; and now that it is asserted by some observers that the nebula lines are always sharp, they should be re-examined with special reference to this point. Seeing that the brightest point is not a star, it will be well also to look for maxima of brightness in the con-

tinuous spectrum, the usual flame comparisons being employed if necessary. In the General Catalogue the following description is added :—" A very remarkable object ; very bright ; exceedingly large ; extremely irregular figure ; with large cluster." Webb refers to it as "a splendid galaxy object, visible to the naked eye."

(2) This nebula was discovered by Tuttle in 1859, and, according to D'Arrest's observations in 1863, it would appear to be variable. It is oval in shape, 2′ long and 80″ broad, and is said to be "pretty bright." No attempt has been made, as far as I know, to determine its spectrum, to say nothing of any variations of spectrum which may accompany the supposed changes in brilliancy.

(3) This star is one of Group II. at a very late stage. Dunér states that "the bands 2, 3, 7, 8 are visible, but they are difficult to recognize as bands, because of their little width. The spectrum is almost of the same type as that of α Tauri." A special study should be made of the lines which accompany the bands, with special reference to how they differ either in position or intensity from the darkest lines in the solar spectrum.

(4 and 5) According to Vogel, these stars have very well developed spectra of the solar type and of Group IV. respectively.

(6) The spectrum of this star is a well-marked one of Group VI., the principal bands being very wide and dark. There is possibly also a trace of band 4 (λ 589).

(7) This variable will reach a maximum about July 24. Its spectrum is of the Group II. type, and is stated by Dunér to be one of the finest in the heavens. The range of variation is small—4·3–4·6—in a period which is not yet completely determined (46? days, according to Gore). Observations similar to those suggested for other variables of the same type should be made. A. Fowler.

Photographs and Drawings of the Sun.—The Memoirs of the Royal Astronomical Society, vol. xlix. Part 2, 1887–89, have just been issued, and contain, with other papers, one presented by the late Father Perry in June 1889 on the above subject.

The areas of spots derived from the solar photographs of 1887, and published in the "Greenwich Observations," have been compared with similar values computed from the measures of the drawings made at Stonyhurst College Observatory. The area computed from the photographs, however, shows a decided general excess over those obtained from the drawings. An idea of the difference may be obtained from the values of the mean daily spotted area, that for 1887 taken from the photographs being 179, while the drawings give 171.

On 29 days penumbra are found in the drawings and not in the photographs, whilst such records occur on the photographs alone only 16 times ; hence the greater area obtained from the photographs cannot be explained by a failure in the drawings to record faint spots and penumbral markings.

An attempt was made to compare the faculæ recorded on the drawings and on the photographs, but unsuccessfully, owing to the enormous excess obtained from the former over that computed from the latter. To eliminate this difference Father Perry suggested that the conditions necessary to obtain good photographs of faculæ may differ from that which is best for spots, and that, therefore, a twofold series of photographs may be necessary, one for spots and the other for faculæ. Two plates, showing sun-spot drawings in 1887, from the Stonyhurst series, accompany the memoir.

Observations of the Zodiacal Light.—Prof. Arthur Searle, in *Astron. Nachr.*, No. 2976, contributes a note on zodiacal light observations made at Harvard College Observatory during the last fifty years. With respect to the permanence of the ordinary western zodiacal light, the observations support the results obtained by previous observers, viz. that it must be considered as a very permanent phenomenon, and one subject only to slight variations in its degree of visibility, apart from atmospheric causes. Another principal subject of investigation was the normal distribution of light in the zodiac and its vicinity, and it is noted that the zodiacal bands, apparently forming a prolongation of the ordinary zodiacal light, were never seen at Harvard College. A number of permanent bands or belts of faint light, however, not confined to the zodiac, although certain portions of them follow the course of the ecliptic, are described in the records. A comparatively large number of observations of the phenomena of a feeble maximum of light in opposition to the sun, commonly known as *Gegenschein*, have been obtained. Prof. Searle thinks that the photometric observations of Müller and Parkhurst, which show that as an average asteroid approaches opposition its brightness increases by about 0·03 of a magnitude for every degree by which its phase is increased, may afford an explanation of this slight maximum of light in opposition to the sun, the light being reflected in this case from the meteoritic matter dispersed through the solar system. Indeed, if the amount of light received from a meteoritic particle be supposed to increase even proportionally to its phase, a maximum appears at opposition, while the law of increase in light assumed for the asteroids, was approximately proportional to the fourth power of the phase.

Ring Nebula in Lyra.—The current number of *Comptes rendus* (July 7) contains a note by M. G. Rayet on a photograph of this nebula obtained at Bordeaux Observatory with an exposure of three hours. The photograph shows all the stars observed near the ring by Lord Rosse in 1844 ; the star with the signification 3, however, is double, whereas that astronomer, and later Prof. Hall, mapped it as triple. There is also a very definite indication of a nebulous star of the 14th or 15th magnitude, almost in the centre of the ring. Although this star has been observed by many astronomers (*e.g.* Hahn, Secchi, Lassell, Schultz, and Holden) and has been photographed by Gothard, other astronomers (viz. Herschel, D'Arrest, Lord Rosse, Hall, and Vogel) have observed the nebula when the star was not visible, and it does not appear on the photographs taken by the Brothers Henry previous to 1886. M. Rayet therefore concludes that the star is variable, and hopes to make such observations and obtain such photographs as will enable him to demonstrate the fact. Stars in or near nebulæ and clusters seem from recent investigations to be more subject to variability than those not so situated.

Photographs of Stellar Spectra.—In the same number of *Comptes rendus* a note occurs by Admiral Mouchez, on some photographs of stellar spectra taken by the Brothers Henry at Paris Observatory, and presented by him to the Academy. Some of the photographs were obtained by means of a prism having an angle of 45° placed in front of the object-glass of the photographic equatorial, others by means of a prism having an angle of 22° ; and Admiral Mouchez remarked that, although the results represented the first attempts in this direction, they compared very favourably with those obtained in America, where work of the same nature has been carried on for some time. It is noted that the Brothers Henry attribute the fuzziness of the lines in the spectra of stars like Altair to a high velocity of rotation and a great amount of agitation at the surface. The photographs are the beginning of an investigation into the chemical composition of stars and motion in line of sight, recently begun at this Observatory.

ON THE SUPERFICIAL VISCOSITY OF WATER.[1]

THE idea that liquids are endowed with a viscosity peculiar to the surface is to be found in the writings of Descartes and Rumford ; but it is to Plateau that its general acceptance is due. His observations related to the behaviour of a compass needle, turning freely upon a point, and mounted in the centre of a cylindrical glass vessel of diameter not much more than sufficient to allow freedom of movement. By means of an external magnet the needle was deflected 90° from the magnetic meridian. When all had come to rest the magnet was suddenly removed, and the time occupied by the needle in recovering its position of equilibrium, or rather in traversing an arc of 85°, was noted. The circumstances were varied in two ways : first, by a change of liquid, *e.g.*, from water to alcohol ; and, secondly, by an alteration in the level of the liquid relatively to the needle. With each liquid observations were made, both when the needle rested on the surface, so as to be wetted only on the under side, and also when wholly immersed to a moderate depth. A comparison of the times required in the two cases revealed a remarkable dependence upon the nature of the liquid. With water, and most aqueous solutions, the time required upon the surface was about *double* of that in the interior ; whereas, with

[1] Paper read before the Royal Society, by Lord Rayleigh, Sec.R.S., on June 5, 1890.

the liquids of Plateau's second category, alcohol, ether, oil of turpentine, &c., the time on the surface was about *half* of the time in the interior. Of liquids in the third category (from which bubbles may be blown), a solution of soap behaved in much the same manner as the distilled water of the first category. On the other hand, solutions of albumen, and notably of saponine, exercised at their surfaces an altogether abnormal resistance.

These experiments of Plateau undoubtedly establish a special property of the surface of liquids of the first and third categories; but the question remains open whether the peculiar action upon the needle is to be attributed to a viscosity in any way analogous to the ordinary internal viscosity which governs the flow through capillary tubes.

In two remarkable papers,[1] Marangoni attempts the solution of this problem, and arrives at the conclusion that Plateau's superficial viscosity may be explained as due to the operation of causes already recognized. In the case of water and other liquids of the first category, he regards the resistance experienced by the needle as mainly the result of the deformation of the meniscuses developed at the contacts on the two sides with the liquid surface. This view does not appear to me to be sound: for a deformation of a meniscus due to inertia would not involve any dissipation of energy, nor permanent resistance to the movement. But the second suggestion of Marangoni is of great importance.

On various grounds the Italian physicist concludes that "many liquids, and especially those of Plateau's third category, are covered with a superficial pellicle; and that it is to this pellicle that they owe their great superficial viscosity." After the observations of Dupré[2] and myself,[3] supported as they are by the theory of Prof. Willard Gibbs,[4] the existence of the superficial pellicle cannot be doubted; and its mode of action is thus explained by Marangoni[5]:—"The surface of a liquid, covered by a pellicle, possesses two superficial tensions; the first, which is the weaker and in constant action, is due to the pellicle; the second is in the latent state, and comes into operation only when the pellicle is ruptured. Since the latter tension exceeds the former, it follows that any force which tends to rupture the superficial pellicle upon a liquid encounters a resistance which increases with the difference of tensions between the liquid and the pellicle." In Plateau's experiment the advancing edge of the needle tends to concentrate the superficial contamination, and the retreating edge to attenuate it; the tension in front is thus inferior to the tension behind, and a force is called into operation tending to check the vibration. On a pure surface it is evident that nothing of this sort can occur, unless it be in a very subordinate degree, as the result of difference of temperature.

This is an important distinction, discussed by Willard Gibbs, according as the contamination, to which is due the lowering of tension, is merely accidentally present upon the surface, or is derived from the body of the liquid under the normal operation of chemical and capillary forces. In the latter case, that, for example, of solutions of soap and of camphor, the changes of tension which follow an extension or contraction of the surface may be of very brief duration. After a time, dependent largely upon the amount of contaminating substance present in the body of the liquid, equilibrium is restored, and the normal tension is recovered. On the other hand, in the case of a surface of water contaminated with a film of insoluble grease, the changes of tension which accompany changes of area are of a permanent character.

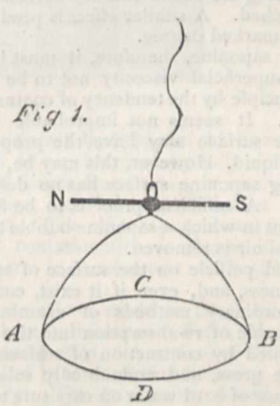
It is not perfectly clear how far Marangoni regarded his principle of surface elasticity as applicable to the explanation of Plateau's observations upon distilled water; but, at any rate, he applied it to the analogous problem of the effect of oil in calming ripples. It is unfortunate that this attempt at the solution of a long-standing riddle cannot be regarded as successful. He treats the surface of the sea in its normal condition as contaminated, and therefore elastic, and he supposes that, upon an elastic surface, the wind will operate efficiently. When oil is scattered upon the sea, a non-elastic surface of oil is substituted for the elastic surface of the sea, and upon this the wind acts too locally to generate waves. It is doubtless true that an excess of oil may render a water surface again inelastic; but I conceive that the real explanation of the phenomenon is to be found by a precisely opposite application of Marangoni's principle, as in the theories of Reynolds (Brit. Assoc. Rep., 1880) and Aitken (Edinburgh Roy. Soc. Proc., 1882-83, vol. xii. p. 56). Marangoni was, perhaps, insufficiently alive to the importance of *varying degrees* of contamination. An ordinary water surface is indeed more or less contaminated; and on that account is the less, and not the more, easily agitated by wind. The effect of a special oiling is, in general, to increase the contamination and the elasticity dependent thereupon, and stops short of the point at which, on account of saturation, elasticity would again disappear. The more elastic surface refuses to submit itself to the local variations of area required for the transmission of waves in a normal manner. It behaves rather as a flexible but inextensible membrane would do, and, by its drag upon the water underneath, hampers the free production and propagation of waves.

The question whether the effects observed by Plateau upon the surface of distilled water are, or are not, due to contamination must, I suppose, be regarded as still undecided. Oberbeck, who has experimented on the lines of Plateau, thus sums up his discussion:—"Wir müssen daher schliessen, entweder, dass der freien Wasseroberfläche ein recht bedeutender Oberflächenwiderstand zukommt, oder dass eine reine Wasseroberfläche in Berührung mit der Luft überhaupt nicht existirt" (*Wiedemann's Annalen*, vol. xi. 1880, p. 650).

Postponing for the moment the question of the origin of "superficial viscosity," let us consider its character. A liquid surface is capable of two kinds of deformation, dilatation (positive or negative) and shearing; and the question at once presents itself, Is it the former or the latter which evokes the special resistance? Towards the answer of this question Marangoni himself made an important contribution in the earlier of the memoirs cited. He found (p. 245) that the substitution for the elongated needle of Plateau of a circular disk of thin brass turning upon its centre almost obliterated the distinction between liquids of the two first categories. The ratio of the superficial to the internal viscosity was now even greater for ether than for water. From this we may infer that the special superficial viscosity of water is not called into play by the motions of the surface due to the rotation of the disk, which are obviously of the nature of shearing.

A varied form of this experiment is still more significant. I have reduced the metal in contact with the water surface to a simple (2") ring, ACBD, of thin brass wire (Fig. 1). This is

Fig. 1.



supported by a fine silk fibre, so that it may turn freely about its centre. To give a definite set, and to facilitate forced displacements, a magnetized sewing needle, NS, is attached with the aid of wax. In order to make an experiment, the ring is adjusted to the surface of water contained in a shallow vessel. When all is at rest, the surface is dusted over with a little fine sulphur,[1] and the suspended system is suddenly set into rotation by an external

[1] *Nuovo Cimento*, Ser. 2, vols. v.–vi., April 1872: Ser. 3, vol. iii., 1878.

[2] "Théorie Mécanique de la Chaleur," Paris, 1869, p. 377.

[3] "On the Tension of Recently Formed Liquid Surfaces," Roy. Soc. Proc., vol. xlvii., 1890, p. 281 (*supra*).

[4] Connecticut Acad. Trans., vol. iii., Part II., 1877–78. In my former communication I overlooked Prof. Gibbs's very valuable discussion on this subject.

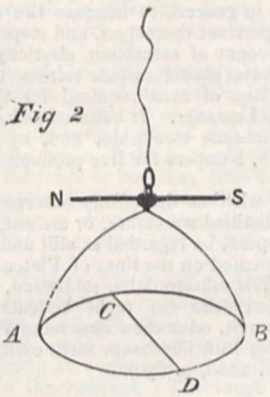
[5] *Nuovo Cimento*, vols. v.–vi., 1871–72, p. 260 (May 1872).

[1] Sulphur seems to be on the whole the best material, although it certainly communicates some impurity to the surface. Freshly heated pumice or wood-ashes sink immediately; and probably all powders really free from grease would behave in like manner.

magnet. The result is very distinct, and contrasts strongly with that observed by Plateau. Instead of the surface enclosed by the ring being carried round with it in its rotation, not the smallest movement can be perceived, except perhaps in the immediate neighbourhood of the wire itself. It is clear that an ordinary water surface does not appreciably resist shearing.

A very slight modification of the apparatus restores the similarity to that of Plateau. This consists merely in the addition to the ring of a material diameter of the same brass wire, CD (Fig. 2). If the experiment be repeated, the sulphur in-

Fig 2



dicates that the whole water surface included within the semicircles now shares in the motion. In general terms the surface may be said to be carried round with the ring, although the motion is not that of a rigid body.

Experiments of this kind prove that what a water surface resists is not shearing, but local expansions and contractions of area, even under the condition that the total area shall remain unchanged. And this is precisely what should be expected, if the cause of the viscosity were a surface contamination. A shearing movement does not introduce any variation in the density of the contamination, and therefore does not bring Marangoni's principle into play. Under these circumstances there is no resistance.

It remains to consider liquids of the third category in Plateau's nomenclature. The addition of a little oleate of soda does not alter the behaviour of water, at least if the surface be tolerably fresh. On the other hand, a very small quantity of saponine suffices to render the surface almost rigid. In the experiment with the simple ring the whole interior surface is carried round as if rigidly attached. A similar effect is produced by gelatine, though in a less marked degree.

In the case of saponine, therefore, it must be fully admitted that there is a superficial viscosity not to be accounted for on Marangoni's principle by the tendency of contamination to spread itself uniformly. It seems not improbable that the pellicle formed upon the surface may have the properties of a solid, rather than of a liquid. However, this may be, the fact is certain that a contracting saponine surface has no definite tension alike in all directions. A sufficient proof is to be found in the well-known experiment in which a saponine bubble becomes wrinkled when the internal air is removed.

The quasi-solid pellicle on the surface of saponine would be of extreme thinness, and, even if it exist, could hardly be recognizable by ordinary methods of examination. It would moreover be capable of re-absorption into the body of liquid if unduly concentrated by contraction of surface, differing in this respect from the gross, and undoubtedly solid, pellicles which form on the surface of hard water on exposure to the atmosphere.

Two further observations relative to saponine may here find a place. The wrinkling of a bubble when the contained gas is exhausted occurs also in an atmosphere (of coal gas) from which oxygen and carbonic acid are excluded.

In Plateau's experiment a needle which is held stiffly upon the surface of a saponine solution is to a great extent released when the surface is contaminated by grease from the finger or by a minute drop of petroleum.

To return to the case of water, it is a question of the utmost importance to decide whether the superficial viscosity of even distilled water is, or is not, due to contamination with a film of foreign matter capable of lowering the tension. The experiments of Oberbeck would appear to render the former alternative very improbable; but, on the other hand, if the existence of the film be once admitted, the observed facts can be very readily explained. The question is thus reduced to this: Can we believe that the water surface in Plateau's apparatus is almost of necessity contaminated with a greasy film? The argument which originally weighed most with me in favour of the affirmative answer is derived from the experiments of Quincke upon mercury. It is known that, contrary to all analogy, a drop of water does not ordinarily spread upon the surface of mercury. This is certainly due to contamination with a greasy film; for Prof. Quincke (*Poggendorff's Annalen*, vol. cxxxix., 1870, p. 66) found that it was possible so to prepare mercury that water would spread upon it. But the precautions required are so elaborate that probably no one outside Prof. Quincke's laboratory has ever witnessed what must nevertheless be regarded as the normal behaviour of these two bodies in presence of one another. The bearing of this upon the question under discussion is obvious. If it be so difficult to obtain a mercury surface which shall stand one test of purity, why may it not be equally difficult to prepare a water surface competent to pass another?

The method by which I have succeeded in proving that Plateau's superficial viscosity is really due to contamination consists in the preparation of a pure surface exhibiting quite different phenomena; and it was suggested to me by an experiment of Mr. Aitken (*loc. cit.*, p. 69). This observer found that, if a gentle stream of air be directed vertically downwards upon the surface of water dusted over with fine powder, a place is cleared round the point of impact. It may be added that on the cessation of the wind the dust returns, showing that the tension of the bared spot exceeds that of the surrounding surface.

The apparatus, shown in Figs. 3 and 4, is constructed of sheet brass. The circular part, which may be called the *well*, has the dimensions given by Plateau. The diameter is 11 cm., and the depth 6 cm. The needle is 10 cm. long, 7 mm. in breadth at the centre, and about 0·3 mm. thick. It is suspended at a height of $2\frac{1}{2}$ cm. above the bottom of the vessel. So far there is nothing special; but in connection with the well there is a rectangular trough, or tail-piece, about $2\frac{1}{2}$ cm. broad and 20 cm. long. Between the two parts a sliding door may be inserted, by which the connection is cut off, and the circular periphery of the well completed. The action of the apparatus depends upon a stream of wind, supplied from an acoustic bellows, and discharged from a glass nozzle, in a direction slightly downwards, so as to strike the water surface in the tail-piece at a point a little beyond the door. The effect of the wind is to carry any greasy film towards the far end, and thus to purify the near end of the tail-piece. When the door is up, this effect influences also the water surface in the well upon which the jet does not operate directly. For, if the tension there be sensibly less than that of the neighbouring surface in the tail-piece, an outward flow is generated, and persists as long as the difference of tensions is sensible. The movements of the surface are easily watched if a little sulphur be dusted over; when the water in the well has been so far cleansed that but little further movement is visible, the experiment may be repeated without changing the water by contaminating the surface with a little grease from the finger or otherwise. In this way the surface may be freed from an insoluble contamination any number of times, the accumulation of impurity at the far end of the tail-piece not interfering with the cleanness of the surface in the well.

Another device that I have usually employed facilitates, or at any rate hastens, the cleansing process. When the operation is nearly complete, the movement of the surface becomes sluggish on account of the approximate balance of tensions. At this stage the movement may be revived, and the purification accelerated, by the application of heat to the bottom of the well at the part furthest removed from the tail-piece. It may, perhaps, be thought that convection currents might be substituted altogether for wind; but in my experience it is not so. Until a high degree of purity is attained, the operation of convection currents does not extend to the surface, being resisted by the film according to Marangoni's principle.

When the apparatus was designed, it was hoped that the door could be made a sufficiently good fit to prevent the return of the greasy film into the well; but experience showed that this could not be relied upon. It was thus necessary to maintain the wind during the whole time of observation. The door was, however, useful in intercepting mechanical disturbance.

A very large number of consistent observations have been recorded. The return of the needle, after deflection to 90°, is timed over an arc of 60°, viz. from 90° to 30°, and is assisted by a fixed steel magnet acting in aid of the earth's magnetism. A metronome, beating three times per second, facilitates the time measurement. As an example, I may quote some observations made on April 11.

The apparatus was rinsed and carefully filled with distilled water. In this state the time was 12 (beats). After blowing for a while there was a reduction to 10, and after another operation to 8. The assistance of convection currents was then appealed to, and the time fell to $6\frac{3}{4}$, and after another operation to 6. This appeared to be the limit. The door was then opened, and the wind stopped, with the result that the time rose again to 12. More water was then poured in until the needle was drowned to the depth of about half an inch. Under these conditions the time was $6\frac{3}{4}$.

It will be seen that, while upon the unprepared surface the time was nearly twice as great as in the interior, upon the purified surface the time was somewhat less than in the interior.

For the sake of comparison, precisely similar observations were made upon the same day with substitution for water of methylated alcohol. Before the operation of wind the time was 5; after wind, 5; on repetition, still 5. Nor with the aid of convection currents could any reduction be effected. When the needle was drowned, the time rose to $7\frac{1}{4}$. The alcohol thus presents, as Plateau found, a great contrast with the unprepared water; but comparatively little with the water after treatment by wind and heat.

An even more delicate test than the time of vibration is afforded by the behaviour of the surface of the liquid towards the advancing edge of the needle. In order to observe this, it is necessary to have recourse to motes, but all superfluity should be avoided. In a good light it is often possible to see a few motes without any special dusting over. In my experience, an unprepared water surface always behaves in the manner described by Plateau; that is, it takes part in rotation of the needle, almost from the first moment. Under the action of wind a progressive change is observed. After a time the motes do not begin their movement until the needle has described a considerable arc. At the last stages of purification, a mote, situated upon a radius distant 30° or 40° from the initial direction of the needle, retains its position almost until struck; behaving, in fact, exactly as Plateau describes for the case of alcohol. I fancied, however, that I could detect a slight difference between alcohol and water even in the best condition, in favour of the former. With a little experience it was easy to predict the "time" from observations upon motes; and it appeared that the last degrees of purification told more upon the behaviour of the motes than upon the time of describing the arc of 60°. It is possible, however, that a different range from that adopted might have proved more favourable in this respect.

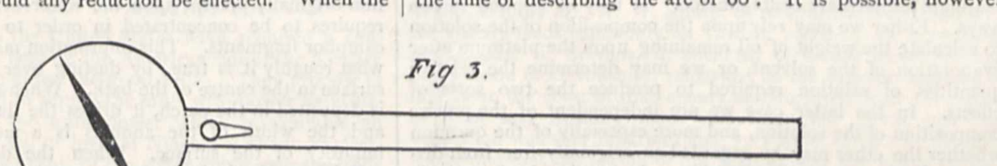


Fig 3.

PLAN

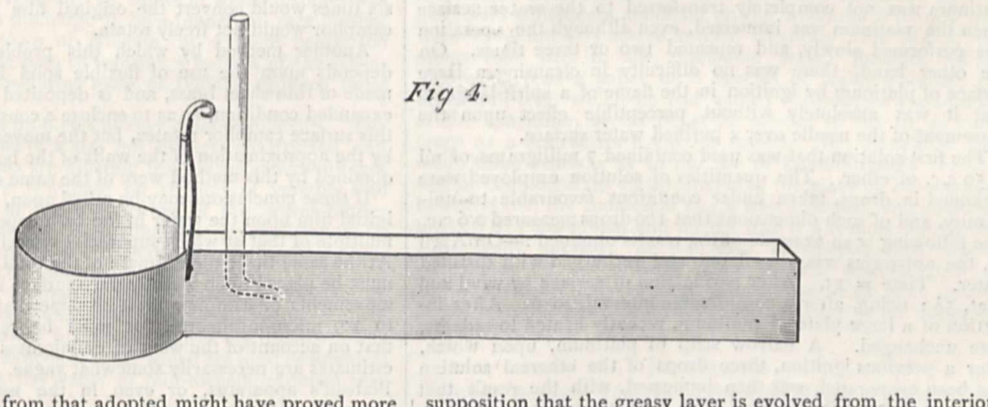


Fig 4.

The special difficulties under which Plateau experimented are well known, and appealed strongly to the sympathies of his fellow workers; but it is not necessary to refer to them in order to explain the fact that the water surfaces that he employed were invariably contaminated. Guided by a knowledge of the facts, I have several times endeavoured to obtain a clear surface without the aid of wind, but have never seen the time less than 10. More often it is 12, 13, or 14. It is difficult to decide upon the source of the contamination. If we suppose that the greasy matter is dissolved, or, at any rate, suspended in the body of the liquid in a fine state of subdivision, it is rather difficult to understand the comparative permanence of the cleansed surfaces. In the case of distilled water, the condition will usually remain without material change for several minutes. On the other hand, with tap water (from an open cistern), which I have often used, although there is no difficulty in getting a clean surface, there is usually a more rapid deterioration on standing. The progressive diminution of the tension of well-protected water surfaces observed by Quincke (*Poggendorff's Annalen*, vol. clx., 1877, p. 580) is most readily explained by the gradual formation of a greasy layer composed of matter supplied from the interior, and present only in minute quantity; although this view did not apparently commend itself to Quincke himself. If we reject the supposition that the greasy layer is evolved from the interior of the liquid, we must admit that the originally clean free surface, formed as the liquid issues from a tap, is practically certain to receive contamination from the solid bodies with which it comes into contact. The view, put forward hypothetically by Oberbeck, that contamination is almost instantly received from the atmosphere is inconsistent with the facts already mentioned.

Some further observations, made in the hope of elucidating this question, may here be recorded. First, as to the effect of soap, or rather oleate of soda. A surface of distilled water was prepared by wind and heat until the time was $5\frac{1}{2}$, indicating a high degree of purity. The door being closed, so as to isolate the two parts of the surface, and the wind being maintained all the while, a few drops of solution of oleate were added to the water in the tail-piece. With the aid of gentle stirring, the oleate found its way, in a few minutes, under the door, and reached the surface of the water in the well. The time gradually rose to 13, 14, 15; and no subsequent treatment with wind and heat would reduce it again below 12. In this case there can be no doubt that the contamination comes from the interior, and is quickly renewed if necessary; not, however, so quickly that the tension is constant in spite of extension, or the surface would be free from superficial viscosity.

In like manner, the time upon the surface of camphorated distilled water could not be reduced below 10, and the behaviour of motes before the advancing needle was quite different from that observed upon a clean surface. A nearly saturated solution of chloride of sodium could not be freed from superficial viscosity; while, on the other hand, an addition of $\frac{1}{2}$ per cent. of alcohol did not modify the behaviour of distilled water.

The films of grease that may be made evident in Plateau's apparatus are attenuated in the highest degree. In a recent paper (*supra*, p. 364) I have estimated the thickness of films of olive oil competent to check the movements of camphor fragments as from one to two micro-millimetres; but these films are comparatively coarse. For example, there was never any difficulty in obtaining from tap-water surfaces upon which camphor was fully active without the aid of wind or special arrangements. I was naturally desirous of instituting a comparison between the quantities necessary to check camphor movements and the more minute ones which could be rendered manifest by Plateau's needle; but the problem is of no ordinary difficulty. A direct weighing of the contamination is out of the question, seeing that the quantity of oil required in the well of the apparatus, even to stop camphor, would be only $\frac{1}{64}$ milligram.

The method that I have employed depends upon the preparation of an ethereal solution of olive oil, with which clean platinum surfaces are contaminated. It may be applied in two ways. Either we may rely upon the composition of the solution to calculate the weight of oil remaining upon the platinum after evaporation of the solvent, or we may determine the relative quantities of solution required to produce the two sorts of effects. In the latter case we are independent of the precise composition of the solution, and more especially of the question whether the ether may be regarded as originally free from dissolved oil of an involatile character. In practice, both methods have been used.

The results were not quite so regular as had been hoped, the difficulty appearing to be that the oil left by evaporation upon platinum was not completely transferred to the water surface when the platinum was immersed, even although the operation was performed slowly, and repeated two or three times. On the other hand, there was no difficulty in cleansing a large surface of platinum by ignition in the flame of a spirit-lamp, so that it was absolutely without perceptible effect upon the movement of the needle over a purified water surface.

The first solution that was used contained 7 milligrams of oil in 50 c.c. of ether. The quantities of solution employed were reckoned in drops, taken under conditions favourable to uniformity, and of such dimensions that 100 drops measured 0·6 c.c. The following is an example of the results obtained:—On April 25, the apparatus was rinsed out and recharged with distilled water. Time = 13. After purification of surface by wind and heat, $5\frac{1}{2}$; rising, after a considerable interval, to 6. After insertion of a large plate of platinum, recently heated to redness, time unchanged. A narrow strip of platinum, upon which, after a previous ignition, three drops of the ethereal solution had been evaporated, was then immersed, with the result that the time was at once increased to $8\frac{1}{2}$. In subsequent trials, two drops never failed to produce a distinct effect. Special experiments, in which the standard ether was tested after evaporation upon platinum, showed that nearly the whole of the effect was due to the oil purposely dissolved.

The determination of the number of drops necessary to check the movements of camphor upon the same surface seemed to be subject to a greater irregularity. In some trials 20 drops sufficed; while in others 40 or 50 drops were barely enough. There seems to be no doubt that the oil is left in a rather unfavourable condition,[1] very different from that of the compact drop upon the small platinum surface of former experiments; and the appearance of the platinum on withdrawal from the water often indicates that it is still greasy. Under these circumstances it is clearly the smaller number that should be adopted; but we are safe in saying that $\frac{1}{10}$ of the oil required to check camphor produces a perceptible effect upon the time in Plateau's experiment, and still more upon the behaviour of the surface before the advancing needle, as tested by observation of motes. At this rate the thickness at which superficial viscosity becomes sensible in Plateau's apparatus is about $\frac{1}{10}$ of a micro-millimetre, or about $\frac{1}{5000}$ of the wave-length of yellow light.

[1] It should be stated that the evaporation of the ether, and of the dew which was often visible, was facilitated by the application of a gentle warmth.

A tolerably concordant result is obtained from a direct estimate of the smaller quantity of oil, combined with the former results for camphor, which were arrived at under more favourable conditions. The amount of oil in two drops of the solution is about 0·0017 milligram. This is the quantity which suffices to produce a visible effect upon the needle. On the large surface of water of the former experiments the oil required to check camphor was about 1 milligram. In order to allow for the difference in area, this must be reduced 64 times, or to 0·016 milligram. According to this estimate the ratio of thicknesses for the two classes of effects is about as 10 : 1.

Very similar results were obtained from experiments with an ethereal solution of double strength, one drop of which, evaporated as before, upon platinum, produced a distinct effect upon the time occupied by the needle in traversing the arc from 90° to 30°.

I had expected to find a higher ratio than these observations bring out between the thicknesses required for the two effects. The ratio 15 : 1 does not give any too much room for the surfaces of ordinary tap water, such as were used in the bath observations upon camphor, between the purified surfaces on the one side and those oiled surfaces upon the other, which do not permit the camphor movements.

It thus became of interest to inquire in what proportion the film originally present upon the water in the bath experiments requires to be concentrated in order to check the motion of camphor fragments. This information may be obtained, somewhat roughly it is true, by dusting over a patch of the water surface in the centre of the bath. When a weighed drop of oil is deposited in the patch, it drives the dust nearly to the edge, and the width of the annulus is a measure of the original impurity of the surface. When the deposited oil is about sufficient to check the camphor movements, we may infer that the original film bears to the camphor standard a ratio equal to that of the area of the annulus to the whole area of the bath. Observations of this kind indicated that a concentration of about six times would convert the original film into one upon which camphor would not freely rotate.

Another method by which this problem may be attacked depends upon the use of flexible solid boundary. This was made of thin sheet brass, and is deposited upon the bath in its expanded condition, so as to enclose a considerable area. Upon this surface camphor rotates, but the movement may be stopped by the approximation of the walls of the boundary. The results obtained by this method were of the same order of magnitude.

If these conclusions may be relied upon, it will follow that the initial film upon the water in the bath experiments is not a large multiple of that at which superficial viscosity tends to disappear. At the same time, the estimate of the total quantity of oil which must be placed upon a really pure surface in order to check the movements of camphor must be somewhat raised, say from 1·6 to 1·9 micro-millimetre. It must be remembered, however, that on account of the want of definiteness in the effects, these estimates are necessarily somewhat vague. By a modification of Plateau's apparatus, or even in the manner of taking the observations, such as would increase the extent of surface from which the film might be accumulated before the advancing edge of the needle, it would doubtless be possible to render evident still more minute contaminations than that estimated above at one-tenth of a micro-millimetre.

[P.S. *June* 4.—In order to interpret with safety the results obtained by Plateau, I thought it necessary to follow closely his experimental arrangements; but the leading features of the phenomenon may be well illustrated without any special apparatus. For this purpose, the needle of the former experiments may be mounted upon the surface of water contained to a depth of 1 or 2 inches in a large flat bath. Ordinary cleanliness being observed, the motes lying in the area swept over by the needle are found to behave much as described by Plateau. Moreover, the motion of the needle under the action of the magnet used to displace it is decidedly sluggish. In order to purify the surface, a hoop of thin sheet brass is placed in the bath, so as to isolate a part including the needle. The width of the hoop must, of course, exceed the depth of the water, and that to an extent sufficient to allow of manipulation without contact of the fingers with the water. If the hoop be deposited in its contracted state, and be then opened out, the surface contamination is diminished in the ratio of the areas. By this simple device there is no difficulty in obtaining a highly purified surface, upon which motes lie quiescent, almost until struck by the oscillating needle. In agreement with what has been stated above, an expansion of three or four times usually sufficed to convert the ordinary water surface into one upon which superficial viscosity was tending to disappear.

I propose to make determinations of the actual tension of surfaces contaminated to various degrees; but in the meantime it is evident that the higher degrees of purity do not imply much change of tension. In the last experiment, upon a tolerably pure surface, if we cause the needle to oscillate rapidly backwards and forwards through a somewhat large angle, we can clear away the contamination from a certain area. This contamination will, of course, tend to return, but observation of motes shows that the process is a rather slow one.

The smallness of the forces at work must be the explanation of the failure to clean the surface in Plateau's apparatus by mere expansion. For this experiment the end wall was removed from the tail-piece (Fig. 3), and a large flexible hoop substituted. By this means, it was hoped that when the whole was placed in the bath it would be possible, by mere expansion of the hoop, to obtain a clean surface in the well. The event proved, however, that the purification did not proceed readily beyond the earlier stages, unless the passage of the contamination through the long channel of the tail-piece was facilitated by wind.]

UTILIZATION OF NIAGARA FALLS.

A SYNDICATE in the United States have acquired a considerable area of land on the American side of the Niagara River, at some distance above the great Falls. They propose to use it for mill sites, and to supply the mills with power by utilizing a small fraction of the water-power which is available on the Falls. The actual fall of level at Niagara is about 200 feet. Suppose that about 4 per cent. of the water going over the Falls is taken, and an effective fall of 140 feet, irrespective of losses in the tail race, obtained, there might be utilized 120,000 horsepower. It is proposed to take the water by a short lateral canal, to allow it to descend vertically in shafts in which turbines will be placed, and then to discharge it by a tunnel tail race passing beneath the present town of Niagara, at a point below the Falls. It is part of the plan to transmit a portion of the power to the important manufacturing town of Buffalo, eighteen miles distant.

The project involves problems of very great complexity. The hydraulic motors will be of a size not hitherto constructed, and the governing conditions are different from those commonly met with where water power is utilized on streams of variable and limited flow. Then in the distribution of the power further problems arise. Power can be distributed to great distances by Hirn's system of wire ropes, as at Schaffhausen; by water or air under pressure, as in the compressed air systems of Paris and Birmingham and the Hydraulic Power Company's system in London. In Switzerland and America progress has been made in distributing large power to great distances electrically. The choice amongst such methods of those which are most economical and most likely to suit the wants of mill-owners, requires very careful consideration.

Hence the Cataract Company have resolved to invite from certain selected engineers and engineering firms, plans for the utilization at Niagara of 120,000 horsepower, and to submit the plans for an authoritative opinion to the judgment of a Scientific International Commission. The Commission will consist of Sir William Thomson, F.R.S., as President; Prof. Mascart, Member of the Institut, and Director of the Bureau Central Météorologique, Paris; Colonel Theodore Turrettini, who was director of the works of the Saint Gothard Tunnel, and is director of the works for the utilization of the motive power of the Rhone at Geneva; and, lastly, Dr. Coleman Sellers, formerly of the firm of Messrs. Sellers and Co., of Philadelphia, and now Professor of Engineering at the Stevens Institute, Hoboken, and at the Franklin Institute of Pennsylvania. Prof. W. C. Unwin, F.R.S., is the Secretary to the Commission.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, June 19.—Prof. Charles Stewart, President, in the chair.—Mr. W. H. Beeby exhibited a specimen of *Rumex propinquus* new to Britain, and procured in Shetland.—Mr. Thomas Christy exhibited and made remarks upon a specimen of *Callistemon rigidum*.—Mr. E. M. Holmes exhibited some marine Algæ new to Britain, including *Ascocyclus reptans*, *Halothrix lumbricalis*, *Harveyella mirabilis*, *Sorocarpus uvæformis*, and *Vaucheria litorea*; also specimens of *Rhodymenia palmata* with antheridia, and *Punctaria tenuissima* in fructification, the last two not having been previously recorded to occur in this state in Great Britain.—The following papers were then read:—Observations on the protection of buds in the tropics, by M. C. Potter.—On the distribution of the South American Bell-birds belonging to the genus *Chasmorhynchus*, by J. E. Harting.—On the vertical distribution of plants in the Caucasus, by Dr. Gustav Radde.—Notes on the *Forficulidæ*, with descriptions of new genera and species, by W. F. Kirby.—This meeting terminated the Session of 1889–90.

Entomological Society, July 2.—Prof. J. O. Westwood, Hon. Life-President, in the chair.—Lord Walsingham exhibited some rare Micro-Lepidoptera collected by himself at Cannes, including *Eudemis helichrysana*, *Conchylis rubricana*, Millière; a new *Depressaria* from *Opoponax cheironium*, which is about to be described by M. A. Constant, and *Bucculatrix helichrysella*; and also a volume of drawings of larvæ of the genus *Eupithecia*, by Mr. Buckler, which formerly belonged to the late Rev. H. Harpur Crewe.—Mr. McLachlan exhibited larvæ and cocoons of *Mecyna deprivalis*, Walk., sent by Mr. W. W. Smith, of Ashburton, New Zealand; the species feeds commonly on *Genista capensis*, an introduced plant.—Mr. S. Stevens, in speaking of a tour which he had lately made in Devonshire, remarked on the extreme scarcity of insects on the coast of that county as compared with the coasts of Kent and Sussex; there were very few larvæ, and the vegetation was very luxuriant and very little eaten: he thought it possible that the reason of the scarcity was the heavy rainfall of South Devon, which washed off and destroyed the young larvæ. Mr. Barrett said that his experience had been the same, and that he put it down to the violence of the winds which beat the insects from the trees. Mr. Blandford remarked that he had found Coleoptera abundant on the Braunton Burrows, near Barnstaple, but very scarce in other localities. Mr. Mason and others took part in the discussion which followed.—Prof. Westwood read a paper on a species of Aphis affecting the bread-fruit tree, which he had named *Siphonophora artocarpi*: at the conclusion of his paper he alluded to the use of Paris-green as a destructive agent for insects. Mr. Blandford then made some remarks as to the use of London-purple (another arsenic compound) as an insecticide in the place of Paris-green; he stated that the compound was a waste product, and one-tenth the cost of Paris-green, and further that it was more soluble and more easily applied; he was also of opinion that arsenic compounds do not greatly affect sucking insects, such as Aphides, the ordinary kerosene preparations being more suitable for their destruction. Several Fellows took part in the discussion that followed.

EDINBURGH.

Royal Society, June 16.—The Hon. Lord M'Laren, V.P., in the chair.—A list of West Australian birds, showing their geographical distribution throughout Australia, by Mr. A. J. Campbell, Melbourne, was communicated.—Dr. Buchan discussed a difference between the diurnal barometric curves at Greenwich and at Kew.—Dr. Sang communicated a paper on the general formulæ for the passage of light through a spherically arranged atmosphere.—Dr. Buchan gave an account of a remarkable barometric reading at the Ben Nevis Observatory on April 8, 1890.—Prof. Crum Brown read the third part of a paper, written by himself in conjunction with Dr. James Walker, on synthesis by means of electrolysis.

July 7.—Sir William Thomson, President, in the chair.—The Victoria Jubilee Prize for 1887–90 was presented to Prof. Tait for his work in connection with the *Challenger* Expedition and his other researches in physical science. The Keith Prize for 1887–89 was presented to Prof. Letts for his researches into the organic compounds of phosphorus. The Neill Prize for 1886–89 was awarded to Mr. Robert Kidston for his researches in fossil botany.—Sir W. Thomson read a paper on the submarine cable problem, with electromagnetic induction. The solution of the problem with intermittent or alternating currents of period so long that the distribution of current over a given cross-section of the core is uniform, is already well known. Sir W. Thomson extends the solution, through all intermediate stages, to the case in which the period is so short that the current is confined to an exceedingly thin surface-layer of the core. He has worked out the conditions which obtain with a core and sheath of any forms. The thickness of the layer depends only, other things being equal, upon the period of alternation—the law being that given by Fourier for the penetration of the annual and diurnal heat-waves into the earth's crust. The distribution of density throughout the layer depends upon the form and relative position of the core and the sheath.—Prof. Crum Brown and Dr. James Walker, in continuation of their research on the formation of dibasic acids by electrolysis, communicated a paper on the synthesis of suberic acid and a new acid $(CH_2)_{12}(COOH)_2$.—Prof. Tait exhibited some graphic records of impact, obtained by the method described in a previous paper.—Dr. James Geikie read a paper by Mr. R. Kidston, on the fossil flora of the Potteries coal-field.—The Hon. Lord M'Laren read a paper on the reduction of certain algebraic equations.—Prof. Tait read an account, by Prof. A. C. Mitchell, of a preliminary experiment on the thermal conductivity of aluminium, which he makes out to be almost exactly equal to that of the best copper.—Dr. Ralph Stockman and Mr. D. B. Dott communicated a paper on the pharmacology of morphine and its derivatives.—Dr. W. Somerville made a communication on *Larix europæa* as a breeding-place for *Hylesinus piniperda*.

PARIS.

Academy of Sciences, July 7.—M. Hermite in the chair.—Photographic stellar spectra obtained by MM. Henry at Paris Observatory, by Admiral Mouchez. (See Our Astronomical Column.)—On the oxidation of the sulphur of organic compounds, by MM. Berthelot, André, and Matignon. The authors give a general method for the estimation of sulphur in all organic bodies containing that element, consisting in burning the body either alone or mixed with camphor in an atmosphere of compressed oxygen in the presence of about 10 c.c. of water, with subsequent precipitation of the sulphuric acid in the usual manner.—Heats of combustion of some sulphur compounds, by MM. Berthelot and Matignon.—Heats of combustion of erythrite, arabinose, xylose, raffinose, and inosite, by MM. Berthelot and Matignon.—New experiments on the silent discharge, by M. P. Schutzenberger.—The active elasticity of muscle, and the energy used in its creation in the case of static contraction, by M. A. Chauveau.—Note on the difficulty in recognizing the Cysticerca of *Tænia saginata* or *inermis* in the muscles of the calf and cow, by M. A. Laboulbène.—On the propagation of sound in cylindrical tubes, by M. V. Neyreneuf.—The theory of periodic comets, by M. O. Callandreau. The author finds that the "capture" theory of periodic comets is sufficient to explain the characteristic properties of their orbits and the objections that have been opposed to it.—On a photograph of the ring nebula in Lyra obtained at Bordeaux Observatory, by M. G. Rayet. (See Our Astronomical Column.)—Partial eclipse of the sun of June 17, by M. J. Léotard. The times of first and last contact are given.—Occultation of the double star β Scorpii by the moon on June 29, by the same author.—On the anomalous propagation of waves, by M. Gouy.—Action in the dry way of different arsenates of potassium and sodium on the sesquioxides of some metals, by M. C. Lefèvre.—On a new method of preparing basic nitrate of copper and some crystallized subnitrates, by M. G. Rousseau. The basic nitrates are obtained in large crystals from the hydrates of corresponding neutral salts.—On double bromides of phosphorus and iridium, by M. G. Geisenheimer.—On some chromoiodates, by M. A. Berg.—The artificial production of boracite in the wet way, by M. A. de Gramont.—On the nitroprussides, by M. Prud'homme.—On the cause of the alteration which certain compounds of the aromatic series undergo under the influence of air and light, by M. André Bidet.—Transformation of glucose into sorbite, by M. J. Meunier.—On the hydrogenation of sorbine and the oxidation of sorbite, by MM. Camille Vincent and Delachanal.—Syntheses by means of cyanacetic ether: dicyanacetic ethers, by M. A. Haller.—The preparation of certain ethers by means of fermentation, by M. Georges Jacquemin.—On the physiological action of thallium salts, by Mr. J. Blake.—On the pretended circulatory system and genital organs of Neomenidæ, by M. G. Pruvot.—On the *rôle* of the bud-shaped pedicles of sea-urchins, by M. Henri Prouho.—On the histological constitution of some Nematoids of the order Ascaris, by M. Léon Jammes.—On the comparative physiology of the sense of smell, by M. Raphael Dubois.—The basaltic eruptions of the valley of the Allier, by M. Marcellin Boule.—On the mineralogical composition of the volcanic rocks of the islands of Martinique and Saba, by M. A. Lacroix.—On the relation between joints and some surface wrinklings near Doullens, by M. Henri Lasne.

BERLIN.

Physical Society, June 27.—Prof. von Helmholtz, President, in the chair.—Dr. Dubois spoke on magnetic closed circuits, whose theory constitutes, in addition to hysteresis, the most important advance which magnetism has made in recent times. He gave a short historical review of the more important published works on the subject, pointing out that they were at first the result rather of an endeavour to make the requisite calculations connected with dynamos for technical purposes, and had only attracted the attention of physicists in a secondary and subordinate degree. The works of Faraday, Maxwell, Sir W. Thomson, Hopkinson, Lord Rayleigh, and the experimental researches of Rowland, were briefly mentioned; Hopkinson's formulæ and Lord Rayleigh's graphic representations were then more fully treated; and, finally, the formula for the magnetization of a closed circuit was developed.—Dr. Raps described an arrangement of Topler's mercurial air-pump, by means of which he had made it work automatically; he further described a compensated air-thermometer which he had constructed, and exhibited both instruments to the Society.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The International Annual of Authony's Photographic Bulletin, vol. 3, 1890–91 (Iliffe).—Reflections on the Motive Power of Heat: N. L. S. Carnot; edited by R. H. Thurston (Macmillan and Co.).—Hypnotism: A. Moll (W. Scott).—Light: E. W. Tarn (Lockwood).—Elementary Mechanics (Blackie).—Timbers, and How to Know Them: Dr. R. Hartig; translated by W. Somerville (Edinburgh, Douglas).—Introduction to Fresh-water Algæ: Dr. M. C Cooke (K. Paul).—Short Logarithmic and other Tables, 4th edition: W. C. Unwin (Spon).—Walks in the Ardennes, new edition: P. Lindley (London).—Tourist Guide to the Continent: P. Lindley (London).—Sectional Map of South Dakota (Chicago, Rand).—Pocket Map, &c., of Michigan (Chicago. Rand).—Confidential Chats with Mothers: Mrs. Bowdick (Baillière).—British Cage Birds, Part 3: R. L. Wallace (L. Gill).—Canary Book, Part 3: R. L. Wallace (L. Gill).—Mathematical and Physical Papers, vol. 3: Sir Wm. Thomson (Cambridge University Press).—Electric Light Fitting: J. W. Urquhart (Lockwood).—Catalogue of the Fossil Reptilia and Amphibia in the British Museum (Natural History), Part 4: R. Lydekker (London).—L'Esprit de Nos Bêtes: E. Alix (Paris, J. B. Baillière).—Journal of the Royal Agricultural Society, vol. 1 (third series), Part 2; General Index to ditto, second series, (Murray).—Transactions of the Royal Society of Victoria, vol. 1, Part 2 (Melbourne).—Proceedings of the Royal Society of Edinburgh, vol. 16, pp. 385 to 846; vol. 17, pp. 1 to 128 (Edinburgh).—Transactions of the Royal Society of Edinburgh, vol. 33, Part 3; vol. 35, Parts 1 to 4 (Edinburgh).

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