THURSDAY, APRIL 30, 1891.

THE SCIENCE MUSEUM AND GALLERY OF BRITISH ART AT SOUTH KENSINGTON.

THE memorial which we print below, and which is still in course of signature, shows with no uncertain sound what is the general opinion of men of science with regard to the suggested allocation of the plot of ground opposite the Royal College of Science for an Art Gallery.

The memorialists, of course, are as appreciative as others of Mr. Tate's munificent offer, nor do they in the least object to the erection of art galleries; their only point is that the plot of ground in question finds its natural use in the additional buildings for the College, for which, indeed, it was understood plans have been for some time in course of preparation.

The reason for this is not far to seek. Not only is it convenient that the two halves of the College should be as near together as possible, but the professors in both use daily in their lectures the apparatus in the Science Museum; there is therefore necessarily the most intimate connection with the buildings in which the teaching is carried on, and those in which the materials for teaching are stored.

The suggested arrangement sunders them as far as possible from each other.

The treatment of the needs and claims of science by Ministers of all shades of party politics may probably account for the present dilemma. It may turn out that those who are responsible for having made the offer to Mr. Tate had not an idea that there was the slightest objection to the proposed action; and not only so, but it is quite possible, and we believe it is true, that the scientific authorities of the Science and Art Department were never consulted at all in the matter.

It is from this point of view that the Memorial, representing the opinion of Oxford, Cambridge, Edinburgh, and the other teaching centres in London, not to mention the President and Officers of the Royal and other scientific Societies, is of so great importance.

Men of science have waited patiently for nearly 20 years for the realization of the recommendation of the Duke of Devonshire's Commission that the pure and applied sciences, on which our national industries depend, should be treated like the other branches of human knowledge. Natural history, antiquities, literature, and art, find places in our Museum system. Why, then, not physical science?

During these years Committee after Committee has been appointed. They have all endorsed the recommendation to which we have referred; but the Government cares so little for these things, that some years ago, in 1877, they refused the offer of the ground on which the Imperial Institute is now being erected and £100,000 for a building, from the Royal Commissioners of the 1851 Exhibition.

Penultimately a Committee appointed by the Treasury reported that the claims of science were just and must be met. The Treasury then found £100,000 to buy the land they might have got for nothing more than ten years ago.

And now finally, as it would appear, lest they should be tempted to carry out their own intentions of a year ago, they hand over to an Art Gallery a large slice of the land bought for Science, and that slice the one which, in the opinion of everybody whose opinion is worth having, if thus occupied, will make the whole transaction ridiculous.

The Memorial, which is still in course of signature, runs as follows:—

Memorial to the Most Honourable the Marquis of Salisbury, K.G., F.R.S., Premier and Secretary of State for Foreign Affairs.

I. Whereas in 1890 Parliament voted £100,000 for the purchase of a site at South Kensington upon which to erect suitable buildings for the Science Museum of the Department of Science and Art and for the extension of its Science Schools; in accordance with the recommendations of the Royal Commission over which the Duke of Devonshire presided in 1874, as well as of various Committees and other high scientific authorities and of a Treasury Committee appointed in 1889,

II. And whereas it has recently been proposed to appropriate a considerable portion of this site nearest to those institutions fo the erection of a new Gallery of British Art,

III. And whereas it has been decided that this Gallery shall have no connection with the Science and Art Department; a building devoted to Art, under independent administration, being thus interpolated between the two parts of a Scientific Institution under that Department,

IV. And whereas it is obviously expedient that, whatever arrangements may be made, the various portions of the Science Museum and of the Science School should be as close as possible to, and in direct contiguity with, one another—for the reason that the Instruments and Museum specimens exhibited in the one have to be used by the Professors and students in the other—and that this will not be the case if the Science Collections are housed in the East and West Picture Galleries as proposed,

We desire most respectfully to express to your Lordship our strong opinion that the development and efficiency of the Science Museum and of the Science School and Laboratories of the Department of Science and Art will be seriously jeopardized by any arrangements that may be made for erecting a Gallery of British Art upon that portion of the land which was recently purchased by Parliament; and that this site is none too large, seeing the progress that is being made in the matter of Scientific and Technical Instruction, to provide for the future requirements of the Collections and Schools controlled by the Department of Science and Art; and that we would therefore earnestly entreat that another site should be found for the proposed Gallery of British Art.

Sir WILLIAM THOMSON, President of the Royal Society, Professor of Natural Philosophy, University of Glasgow.

MICHAEL FOSTER, F.R.S., Secretary Royal Society, Professor of Physiology, Cambridge.

Lord RAYLEIGH, F.R.S., Secretary Royal Society, Professor of Physics, Royal Institution.

Sir DOUGLAS GALTON, K.C.B., F.R.S., General Secretary British Association.

Sir JAMES PAGET, Bart., F.R.S., late President Royal College of Surgeons.

General STRACHEY, C.S.I., F.R.S., Chairman Meteorological Council.

ROBERT H. SCOTT, F.R.S., Secretary Meteorological Council.

W. BURDON SANDERSON, F.R.S., Professor of Physiology, Oxford.

Sir WILLIAM TURNER, F.R.S., Professor of Anatomy, University of Edinburgh.

T. McKENNY HUGHES, F.R.S., Woodwardian Professor, Cambridge.

W. H. M. CHRISTIE, F.R.S., Astronomer-Royal, Past President Royal Astronomical Society.

ETTRICK W. CREAK, Commander R.N., F.R.S.

W. T. BLANFORD, F.R.S., late Director Meteorological Department, India.

JOHN RAE, F.R.S.

FRANCIS GALTON, F.R.S., President Kew Observatory.

Prof. W. H. FLOWER, C.B., F.R.S., Director Natural History Museum.

P. L. Sclater, F.R.S., Secretary Zoological Society.
Sir William Roberts, M.D., F.R.S.
Hugo Müller, F.R.S., Past President Chemical Society.
Dr. E. Frankland, F.R.S., Past President Chemical Society.
Dr. J. H. Gilbert, F.R.S., Past President Chemical Society.
Dr. W J. Russell, F.R.S., Past President Chemical Society.
Prof. Raphael Meldola, F.R.S., Foreign Secretary Chemical Society.
Shelford Bidwell, F.R.S.
Dr. J. H. Gladstone, F.R.S., Vice-Chairman School Board for London.
Sir Richard Quain, Bart., F.R.S., President Royal College of Physicians.
Prof. W. E. Ayrton, F.R.S., President Physical Society.
Prof. John Perry, F.R.S., Secretary Physical Society.
Prof. W. Grylls Adams, F.R.S., Past President Physical Society.
William Crookes, F.R.S., President Society of Telegraphic and Electrical Engineers.
T. G. Bonney, F.R.S., Professor of Geology, University College, London.
Sir William Bowman, Bart., F.R.S.
Lazarus Fletcher, M.A., F.R.S., Past President Mineralogical Society.
Sir Henry E. Roscoe, F.R.S., M.P., Past President British Association.
Sir John Lubbock, Bart., F.R.S., M.P., Chairman London County Council.
G. H. Darwin, F.R.S., Plumian Professor, Cambridge.
Sir G. G. Stokes, Bart., M.P., Past President of the Royal Society.
Sir Frederick Bramwell, F.R.S., Past President British Association.
Sir Bernhard Samuelson, Bart., F.R.S.
T. Archer Hirst, F.R.S., Past President Mathematical Society.
Bartholomew Price, F.R.S., Professor of Natural Philosophy, Oxford.
E. J. Stone, F.R.S., Radcliffe Observer, Oxford, Past President Royal Astronomical Society.
W. Odling, F.R.S., Professor of Chemistry, Oxford, Past President Chemical Society.
R. B. Clifton, F.R.S., Professor of Experimental Philosophy, Oxford, Past President Physical Society.
G. Carey Foster, F.R.S., Professor of Physics, University College, London, Past President of Physical Society and of Institute of Electrical Engineers.

COUNTY COUNCILS AND TECHNICAL EDUCATION.

THERE was one announcement in Mr. Goschen's Budget speech which came as welcome news to the friends of technical education. It was that the new grant out of the beer and spirit duties, which is at the disposal of County Councils with the power to use it for educational purposes, will not be diverted to other objects, but will be permanently renewed. In an announcement made in December last in reply to a question from Lord Hartington, the Chancellor of the Exchequer had held out the prospect of the permanence of the grant provided it were well used for the purpose for which it was granted. But none the less the definite language used on Thursday last is reassuring, and is likely to act as a useful stimulus to those few County Councils which have not yet definitely decided on the appropriation of the fund. It takes away the last shred of excuse from the obstructives who oppose entertaining any large scheme of educational organization on the ground that the grant should be treated as a windfall and not as income.

We have before us two small volumes recently issued by the National Association for the Promotion of Technical and Secondary Education, containing two series of selected reports of County Councils, and other typical schemes and proposals which have been drawn up in various districts for the utilization of the new fund. These publications, issued with the view of giving the various County Councils full and timely information as to what is being done and proposed elsewhere, bear welcome testimony to the energy and ability with which, as a whole, the County Councils are grappling with the unaccustomed task which the possession of the new grant entails upon them. It is highly creditable that only two County Councils in England (London and Middlesex) have as yet failed entirely to rise to the level of their new opportunities, and have yielded to the sordid temptation to sink the whole of the first year's revenue in the rates. There is already evidence that Middlesex means next year to reconsider its hastily-adopted policy, and a scheme is already being drafted for that county. Of the case of London we shall have more to say presently.

Of the remaining English Councils, 29 have devoted the whole, and 9 part of the fund to educational purposes. In 8 more counties the special Committees appointed to consider the matter have not yet finally reported, but in nearly every case their report is likely to be in favour of the application of the money to education. In Wales, which has a separate and more comprehensive Act of its own, which it is to be hoped will shortly be extended to England, almost the whole of the fund is being divided between the purposes of technical and intermediate education. In Scotland, unfortunately, very little has as yet been done, owing largely to the cumbrous and ineffective machinery of the Technical Schools Act which is in force in that country. The amendment of that Act is urgently required in order to prevent the whole or major part of the Scotch grant from falling into the rates.

Taking the country as a whole, there cannot be a doubt of the immense stride that has been taken during the last few months in the matter of national education, and the stirring up of interest in the subject all over the country is quite as valuable as the actual assistance given out of the fund. It is indeed almost necessary to warn County Councils against the temptation to move too fast, and to adopt schemes which will at once absorb the whole grant, and create vested interests hard to deal with in the future. The whole matter is in an experimental stage, the problem of the organization of technical education by counties being completely new. In attacking such a problem blunders are certain to be made, and money is certain to be wasted at first. Fresh legislation will probably be needed before County Councils possess the requisite powers to deal comprehensively with the whole question, and it will be well for them at least to be alive to the danger of creating future difficulties by tying up the whole grant at once. Up to a few months ago there was a danger lest the grant should cease to be applicable to education if unappropriated within the financial year. All doubt, however, on this point has now been removed by Sir Henry Roscoe's Act of the present session, under which full powers are possessed by County Councils of banking and accumulating any unappropriated balance.

It is fair to say that the schemes before us recognize for the most part the danger to which we have alluded, and proceed to work cautiously and experimentally. Variety and not uniformity is the leading note of the collection of schemes before us, and within a few months a series of most valuable educational experiments will have been worked out in various parts of the country—none the less valuable because a certain proportion of them may end in failure. Some of the most active counties are those in the west. Devonshire has a plan already in full swing for the delivery of courses of lectures throughout the rural districts on the sciences bearing on agriculture, under the auspices of the University Extension scheme. Gloucestershire has a system of cookery schools in the villages. Somersetshire has a carefully thought out scheme of distribution, under which the fund will be divided into three parts, one of which will be distributed to the various districts, in proportion to population; the second reserved in the hands of the County Council, to be administered for purposes common to the whole county; and the third granted to such Districts as are willing to levy a rate under the Technical Instruction Acts. Thus local activity is stimulated and assisted, without running the risk of frittering away the fund in small and comparatively useless doles. Here, as elsewhere, in the absence of regularly elected District Councils, it has been found necessary to divide the county into districts for administrative purposes, with specially constituted Committees for the disposition of the grant.

The county boroughs have not, on the whole, been backward to avail themselves of their new powers, though the delay that has taken place in the adjustment of the claims of counties and county boroughs has kept them in doubt for some time as to the exact amount which they might expect to receive. The energy, however, of the county boroughs as well as the counties has received lately a great impetus from the passage of the new Act empowering local authorities to contribute to institutions outside their district. This new power, which they did not before possess, makes it possible for a great institution like the Yorkshire College or the Durham College of Science to be made a centre for the instruction of the inhabitants of the surrounding counties, and to be supported by contributions from the whole of the area benefited.

To the general progress of the country London has hitherto presented a melancholy exception. The reasons are not far to seek. The London County Council is overworked, and being *in articulo mortis* is in a special degree afraid of its ratepaying constituents. The rates in the metropolis are high, and the requirements in all directions exceptionally large. All this forms no valid excuse for "grabbing" a fund ear-marked for educational purposes, but it explains the strength of the temptation, to which the Council have unfortunately yielded, to divert the fund to the relief of local taxation. It must, moreover, be confessed that the temptation was increased by the fact that the special Committee, appointed to inquire into the application of the fund, presented to the Council a very one-sided report, based on somewhat partial evidence, recommending the distribution of the balance for the first year almost exclusively among one class of institutions—the new polytechnics now being constituted out of the funds of the City parochial charities. It was felt that, useful as these polytechnics might be, they did not cover the whole ground, and representatives of older institutions were loud in their complaints. It is true that the special recommendations of the Committee were confined to the distribution of a balance of £23,000 which remained over for the first year, the bulk of the money received having been already thrown into the rates; and that the report contemplated the framing of a much more comprehensive scheme for the disposition of the funds to be received in future years. But a report like this is hastily read, and probably the one-sided appearance of the proposals had much to do with their rejection, and, unless a great effort is made, may yet prejudice the case for technical education in London for some time to come.

One thing, however, is certain. No decision of the present moribund Council as to the permanent appropriation of the fund to the relief of the rates can possibly bind its successor, and we may hope that before long the London Council will take steps to remove the stigma of being the only important Council in England which deliberately shirks its duty with respect to the education of the inhabitants. But if it still persists, some means of compulsion must be found, or the fund may have to be intrusted to the administration of some other Board, where it will not be subject to the depredations of the "ratepayers' friends."

PHILIP HENRY GOSSE.

The Life of Philip Henry Gosse, F.R.S. By his son, Edmund Gosse, Hon. M.A. of Trinity College, Cambridge. (London: Kegan Paul, Trench, Trübner, and Co., Ltd., 1890.)

THE second son of Thomas and Hannah Gosse, Philip Henry, was born in Worcester on April 6, 1810. A couple of years afterwards his parents went to reside at Poole, where Philip Henry's love for natural history appears to have been very early awakened. From his Aunt Bell he learned of the metamorphosis of insects, the name of our common red sea anemone, and she even suggested to the boy that he should try to keep sea anemones alive in vessels of fresh sea-water; this Aunt Bell was the mother of the well-known Prof. Thomas Bell, the the latter of whom was some eighteen years Gosse's senior. Thomas and Hannah Gosse belonged to two different orders of being. From a physical standpoint, we read of the father that he was a grey and withered man, who never smiled, who had no push in him, no ambition, and no energy; while the mother was a very handsome and powerfully built woman, with a pastoral richness of nature, like a Sicilian shepherdess of the olden times. Both parents did their duty, each after their light, to each other and to their offspring; but the artist father, the at one time pupil of Sir Joshua Reynolds, the man who had stored up in his brain masses of artistic and literary information, the man of pure conduct and pious habits, met with but little sympathy and but little appreciation from his splendid wife, and yet we read that he died in his eightieth year "entirely tranquil." This mother, too, was a good mother; she looked well after the education of her children, and the aptitude for learning which her second son, Philip Henry, showed, induced her to make peculiar sacrifices for his advantage, so that in 1823 she got him admitted into the Blandford School, where he acquired a fair knowledge of the rudiments of both Latin and Greek.

After a short sojourn in a large mercantile house, as a junior clerk, at Poole, Philip's mother accepted for him an appointment in the counting-house of the Messrs. Harrison and Co., at Carbonear, Newfoundland, where his elder brother, William, had for some time been. Here he met William St. John, whose portraiture is exceedingly well drawn for us in Philip's notes; here, too, he had large opportunities, of which he availed himself, for miscellaneous reading, some for falling in love, which he did not avail himself of; and here, too, he seems to have made up his mind to devote himself to the study of natural history. After five years in Newfoundland he returned on leave to Poole, and once more, after a brief six weeks' visit to his home, we find him back at Carbonear, which was finally left in June 1835 for Canada. Here farming was attempted, with rather miserable results. After a three years' trial, he found himself at twenty-eight years of age, not possessed, when all his property was told, of so many pounds. Through all this dreary time, the one thing that enabled him to overcome fatigue, and made him even forget his poverty, was his enthusiastic pursuit of the natural sciences, and amidst his ploughing, planting, and reaping, he made the observations and kept the notes which enabled him afterwards to publish his "Canadian Naturalist."

The next year of Gosse's life was spent in Alabama. On his way south he spent a few weeks in Philadelphia, where he made the acquaintance of Titian R. Peale, then preparing to start on the Wilkes Expedition to the South Seas, and of Prof. Nuttall, the botanist; and he occupied himself with visiting all those spots which were associated with the memory of America's greatest ornithologist, Alexander Wilson—"here was his residence; in yonder house he kept school; here were the very scenes described in his truly delightful volumes." Proceeding in the *White Oak*, a small schooner bound to the port of Mobile, he reached King's Landing, Alabama, after a somewhat disagreeable voyage of nearly a month's duration. He now became master of a school composed of the sons of a Mr. Justice Saffold and of other landed proprietors of the district. The school-house was situated in a very romantic spot, amidst a clearing of a great oak-forest hard by the village of Mount Pleasant. The teaching did not occupy the whole of Gosse's time. Birds and insects abounded; one little prairie knoll, about a mile from his own house, was a mass of blue larkspurs and orange milkweed, and was a marvellous haunt of butterflies.

"An eye," he writes, "accustomed to the small and generally inconspicuous butterflies of our own country, can hardly picture to itself the gaiety of the air here, where it swarms with large and brilliant-hued swallow-tails and other patrician tribes, some of which, in the extent and volumes of their wings, may be compared to large bats. These occur, too, not by straggling solitary individuals: in glancing over a blossomed field on my prairie knoll you may see hundreds, including, I think, more than a dozen species; besides other butterflies, moths, and flies."

Nor was larger game wanting—squirrels, opossums, and bears were often too numerous. The natural history surroundings were delightful; but, naturalist though he was, Gosse could not concentrate his thoughts on nature, and in dwelling on the conditions of the slaves, in witnessing the lawlessness of some of the slave-masters, his mind sickened, and he left the country and fled.

Five weeks—the first five of 1839—were occupied in the voyage to Liverpool. After eleven years of wandering, the traveller was back again. He had essayed many things, and none of them seemed to have succeeded; and yet all the while he had been "unconsciously serving an apprenticeship for the occupation for which he was fitted," and in which he was to spend the rest of his life. During the voyage home he commenced to write out his notes on Canadian natural history, and the rough manuscript of the "Canadian Naturalist" was finished ere the vessel entered the Mersey. His fortunes at this time seemed to have reached about their lowest ebb, when, on the recommendation of Prof. Bell, the amiable and now venerable publisher, Van Voorst, offered him one hundred guineas for the manuscript of the "Canadian Naturalist." Gosse's troubles were then, in great measure, over, and his career as an author had begun. There were, no doubt, still periods of poverty, borne with an admirable patience; but from 1840 to 1844 he published an "Introduction to Zoology," "The Ocean," contributed to some of the scientific journals; became a familiar visitor to the British Museum, and, towards the close of 1844, went on an exploring expedition to Jamaica. Nearly two years were spent most profitably in this lovely island; and in that most delightful of Gosse's books, the "Naturalist's Sojourn in Jamaica," the reader will find abundant proof of how well the time was spent in investigating the treasures of the place. As this was the last experience which Gosse had in studying the fauna or flora of other regions save those of the British Isles, we may remark, in passing, that he seems, in the tropics, to have passed over without much notice the forms of marine life: thus, while the mammals, birds, reptiles, and fish of Jamaica are carefully noted, and many new forms described, only a casual glance is bestowed on its corals and sea anemones. With his after-experience, had Jamaica been revisited, the glories of the "many small corals, apparently alive, of different species, some of which were very pretty," and of "the magnificent living corals forming great bushes at the bottom of the sea," would not have been left unsung.

Between 1846 and 1851 his volume on the "Birds of Jamaica" was published, and was well received; it was in octavo form, uniform with the afterwards published "Naturalist's Sojourn in Jamaica," but without illustrations; these were afterwards issued in a small folio volume, one of the most difficult nowadays to procure of all Gosse's works. Through an error in calculation there was a slight pecuniary loss on every copy subscribed for, and means were not resorted to to publish extra copies at an advanced price. Several popular works were written about this time for the Society for Promoting Christian Knowledge. On November 22, 1848, he married Emily, daughter of William Bowes, of Boston. In 1849 he commenced to pay particular attention to the lower forms of animal life, particularly the Rotifers; and he made the acquaintance of Quekett and Bowerbank. In 1849 his son Edmund was born. In 1850 the manuscript of the "Naturalist's Sojourn in Jamaica" was returned to him by Mr. John Murray, only to be accepted to their ultimate advantage by the Messrs. Longmans.

Towards the close of 1851 a complicated series of circumstances combined to drive Gosse away from his constant writing and drawing in London, where the monotony of his life was even more deleterious to his health than the severity of his labours, and his active work at the southern sea-shore of Great Britain now began. St. Marychurch, on the coast of Devonshire, was first of all selected.

"It lies open to the east, on a level with the tops of the cliffs, and enjoyed on clear days a refreshing view of the purple tors of Dartmoor away in the west. It was little in Philip Gosse's mind, when he first stepped up the reddish-white street of St. Marychurch, that in this village he would eventually spend more than thirty years of his life, and would close it there."

For the present his stay was transitory, but his investigations and discoveries supplied the material which enabled him to bring out "A Naturalist's Rambles on the Devonshire Coast." He also was busily working on the subject of marine vivaria, which he and Mr. Warington succeeded in bringing into notice and use, and which gradually from a toy have become a means of scientific research. "The Devonshire Coast" was published in 1853, and resulted in a fairly remunerative sale. At this time Gosse was asked to lecture: he had never attempted such a thing, but willingly promised to give a lecture on Sponges, accompanying his remarks with some life-like sketches on the black-board. One is not astonished to learn that the experiment was most successful, and that he for the future adopted lecturing as a branch of his labours. In 1854 "The Aquarium" was published. This little work had an immense success; the subject and the half-dozen coloured plates were attractive, but from a scientific standpoint the work will not bear comparison with the "Naturalist's Rambles on the Devonshire Coast"; still the profit on its sale reached the large sum of over £900. This volume was reviewed by Kingsley in the *North British Review* for November 1854, and Kingsley afterwards expanded this notice into a little volume called "Glaucus." In 1854, "Tenby," the last of this series of illustrated books treating of British marine zoology, was published, and also the "Manual of Marine Zoology." In 1856 he was elected a Fellow of the Royal Society; and his memoir on the "Manducatory Organs in the Class Rotifera" was published in this Society's Transactions.

In February 1857, Mr. Gosse had the sorrow of losing his first wife, and her death seems to have marked a crisis in his career; he became more than ever reserved; London became inexpressibly disagreeable to him, and towards the close of 1857 we find him settled at Sandhurst, hard by St. Marychurch, which now became his home.

Gosse was at this time forty-seven years of age; his life from his seventeenth year had been one succession of wanderings. The struggle for existence had never been very severe; his scientific work had merited and secured the esteem of many; his writings had been, all things considered, a success; and, in despite of some feebleness of bodily health, he had been enabled to work hard with pencil, pen, and tongue. His religious views were peculiar, and he gave them a quite peculiar prominence in many of his writings; still he never was subjected to any extreme or very unkindly criticism therefor. As a completely self-taught naturalist he had succeeded in training himself up to a comparatively high standard of knowledge, and at this period all his friends hoped that once time had worn away some of the sorrow from his heart, he would have returned to his studies with renewed zest; and so in time he did, but not before entering into a vague and unsatisfactory series of speculations on the origin and creation of life. Perhaps no work since the "Vestiges of Creation" was received with a greater tempest of adverse criticism than "Omphalos," published by him in 1857. The work of a serious biologist, its at-once-felt unreality, though charming in its way, was clearly not the object aimed at by its author: as a play of metaphysical subtlety, with its postulates true and its laws fairly deduced, it stands complete: Bishop Berkeley would have appreciated this volume, though even he would not have believed in its conclusions. Neither Gosse's friends nor foes seemed to have had any appreciation for it. Here, though we feel bound to make but a passing allusion to this work, we cannot refrain from an expression of regret that Kingsley's letter should have been published without the passage alluding to Newman having been first omitted. The book was a failure from a pecuniary point of view, and yet, though the fact is not alluded to in this memoir, its author contemplated "a sort of supplement, or rather complement, to it, examining the evidences of Scripture; not merely the six-day statements, but the whole tenor of revelation." This never appeared.

The "History of British Sea-Anemones" was published in twelve parts, the first of which appeared on March 1, 1858; it is an excellent monograph of our native forms, which will always be a work to refer to. The complete work appeared in January 1860, after which Gosse published his "Romance of Natural History," and married Miss Eliza Brightwen, who still survives him. Although we read that "the year 1861 was the last in which my father retained his old intellectual habits and interests unimpaired," yet the series of memoirs on the Rotifers published between 1861 and 1862, which culminated in his contributions to Dr. Hudson's well-known memoir, his papers on other natural history subjects, not to speak of his large correspondence, showed no impairment of intellect, and we should have thought that the latter years of his long life were among the most enjoyable of them all.

The end came on the morning of August 23, 1888, the immediate cause being an attack of senile bronchitis, brought on by exposure to cold while investigating some double stars. His remains lie buried in the Torquay Cemetery.

To all the numerous readers of Gosse's works and to his many correspondents, this biography by his only son will be most welcome. From a literary point of view, it leaves nothing to be desired; his chief object has been "to present his father as he was," for in so doing he felt sure of that father's approval. The task was, no doubt, a difficult one, for to write such a life from a perfect standpoint would have required, over and above literary skill, not only a most tolerant sympathy with the religious views and feelings of the man, but also a large acquaintance with the studies of his life, and neither of these qualifications does Mr. Edmund Gosse lay claim to. Still in the chapter of "General Characteristics" there is a fair summing up, though we cannot agree that "his letters were usually disappointing." In these columns we have not felt at liberty to allude, except in the most passing way, to the subject of the religious features of Gosse's life, and yet to the student of the man this aspect is one of great interest: for it we must refer the reader to his "Life."

The aim of this volume was to present the reader with a sketch of a man who has left his mark on the popular natural history of this century, and we believe that this object has been fully accomplished.

Here and there we have noticed, as we read, a few oversights, the most of which the biological reader will be able to correct as he reads. The words on p. 194, "the prints of Musignano," might not at first seem to indicate "the Prince of Musignano," afterwards known as the Prince of Canino; and in reference to the statement on p. 241, about Gosse's discovery of *Balanophyllia regia* on the coast of Devonshire, it is nowhere stated in the "Rambles on the Devonshire Coast" that the genus Balanophyllia was only known till then as containing fossil forms. Philip Gosse certainly knew otherwise.

E. P. W.

A CLASS-BOOK ON LIGHT.

A Class-book on Light. By R. E. Steel, M.A., Bradford Grammar School. (London: Methuen and Co., 1891.)

MR. STEEL'S book contains a curious mixture of good and bad. There is a great deal in it which may be most warmly commended. Many of the explanations are clear and concise; the figures are, on the whole, good, and the proofs sound and complete; the style is generally interesting, and the important points duly emphasized; but, with all this, there are some truly astounding statements, which detract immensely from the merits of the whole.

In the first chapter, Mr. Steel endeavours to explain how it is that we look upon light as a form of energy. He tells us (p. 4): "Thus, we can measure energy by the mass moved into the distance moved through against a known force, or by the mass moved multiplied by the increase of velocity produced"—two false statements which have not even the merit of being consistent. Or, again: "Light is a form of energy. . . ." "It is a vibration of the particles of ether. . . ." A form of energy cannot also be a vibration; the vibrating particles of ether possess energy, and if the vibrations be of a certain kind, this energy affects the eye as light. On p. 5 we have the following statement:—"We can pass electricity along substances such as carbon which offer great resistance, and thereby the electricity is converted into light. . . ."

It is, of course, difficult to explain, at once simply and in precise language, the relations between the various forms of energy; but this difficulty hardly affords sufficient excuse for the vague or erroneous expressions found in this part of the book; and it is to be hoped that, whenever another edition is called for, there may be some modifications introduced here. For, as has already been mentioned, the optical part is good, though some points are open to criticism. Thus, the proof given in § 12 does not really deduce the law of reflexion from the wave theory. This cannot be done without the principle of interference. It is said, p. 25, line 11, that, under certain circumstances, "DC must be the reflected wave"; but why? The point requires proof, and none is given. A similar criticism applies to the explanation of the law of refraction (§ 29). These points might with advantage have been further considered after § 80, which gives the explanation of the rectilinear propagation.

The first eight chapters deal mainly with geometrical optics, and contain nearly all the propositions which are ordinarily required. The formulæ for refraction through a lens proved in § 41 might also have been proved in a manner similar to that employed in § 22, when treating of reflexion at a mirror.

The earlier part of chapter vi., dealing with dispersion and the spectroscope, is rather meagre. A figure might with advantage have been introduced showing the path of a pencil of rays in a spectroscope.

Throughout the book a little more care might have been bestowed on the position of some of the figures. Thus, Fig. 72 is referred to on pp. 92 and 93, and not on 94. Had it been placed on p. 93, it could have been seen without turning over the leaf; it is, however, on p. 94, where it is not wanted. The same sort of thing is noticeable in other places; thus § 39 is headed "Concave Lenses"; after four lines of letterpress we have Fig. 61, but this is a figure of a convex lens referred to in the previous section. The figures for § 39 are over the page; of course the letterpress makes it clear which figures are meant, but still the arrangement is confusing to the student.

A good deal of matter—perhaps a little too much—is condensed into the last three chapters, dealing with interference, double refraction, and the interference of polarized light, but all that these chapters contain may be read with advantage. In the hands of a teacher who will correct the errors of the earlier part, the book may be used with profit.

THE PHILOSOPHY OF SURGERY.

Studies of Old Case-books. By Sir James Paget, Bart. Pp. ix. and 168. (London: Longmans, Green, and Co., 1891.)

IN this little book, Sir James Paget has digested some of the stores of knowledge which he has acquired in the course of a long and active professional life. Since he first became a medical student, Sir James tells us, he made it a practice to take notes of all the interesting cases which came under his notice, so that the quantity of material which he possesses in the way of unedited manuscript must be enormous, and he is thereby brought into the same class as John Hunter, though we may sincerely hope that, for the benefit of posterity, his records of cases will not share the same fate as befel Hunter's. Sir James is, however, rather sceptical as to the value of the observations which he has recorded at the cost of so much labour, for he says: "While looking through, and in some degree studying, the many thousand cases thus recorded, it has not been difficult to find why they would be now so nearly useless and uninteresting to anyone but myself." The changes which a progressive science like surgery necessarily undergoes in the course of sixty-one years enable the notes to be read by the light of our present knowledge, often with disheartening results when we reflect how difficult a thing it is to interpret a fact, even when it has been recorded. The use, however, of keeping up such a series of case-books is thus forcibly put forward by the author:—"The habit of recording facts is nearly essential to the habit of accurately observing and remembering them. That which we intend to record we are bound, and may be induced, to observe carefully, and the very act of carefully recording is nearly equivalent to a renewed observation." These studies, however, are not mere transcriptions from the old case-books, but they are a series of short essays upon rare or little-considered surgical cases which from time to time have come under the notice of the author. Each essay is full of that philosophy which can only be acquired by the most highly cultivated minds, and even by them only after a long and observant career. Each essay, too, is full of suggestion for further work—work which will not be carried out by Sir James Paget himself, but work which he may well live to see carried through by the younger generation of scientific surgeons, who look to him as a friend and a guide in those numerous difficulties which beset all earnest inquirers after truth.

The essays are seventeen in number, and, with a few exceptions, deal with subjects which are of interest only to the medical man. Two of the chapters are of interest to the general reader, however—that on errors in the chronometry of life is the most interesting, in which an attempt is made to show how many of the phenomena in health and disease may be explained by assuming that they are due to a disturbance of the time-rate at which the processes of life are discharged. For example, certain turtles lay their eggs in hollows made in the sand, leave them there to be hatched, and, at the time of hatching, return to them for the sake of their young. It might be asked, How can these creatures, and many others in similar cases, reckon the passage of time? Most probably they do not reckon it at all: but just as the timely-attained fitness of their organization for preparing and filling their nests impelled them to those acts, so some time-regulated organic processes, taking place in them after the laying of their eggs, bring about at length a new condition, of which a dim consciousness becomes an impulse to them to return to their nests. In the other essay of general interest, Sir James Paget discusses the use of the will for health.

OUR BOOK SHELF.

Euclid's Elements of Geometry. Books III. and IV. Edited by H. M. Taylor, M.A. (Cambridge: University Press, 1891.)

IT is only of late years that the University of Cambridge has taken the wise step of giving greater scope to the teaching of geometry by not insisting on the actual proofs of propositions as presented in Euclid's text. All the definitions, axioms, and postulates, together with the sequence of propositions which he adopted, are retained, but in solving them "no proof will be accepted that assumes anything not proved in preceding propositions."

The work under consideration is published in the "Pitt Press Mathematical Series," and it will be found to contain some important changes, both with regard to the proofs and method of arrangement. In the first few propositions of Book III. the author has introduced several proofs which seem preferable to those generally adopted, while their order of sequence has been extensively changed. The alterations in the remaining propositions of this book have not been carried to any extent, but several outlines of alternative constructions have been inserted in cases where they may be most instructive.

Of the additional propositions introduced, one involves the principle of the rotation of a plane figure about a point in its plane, while following Proposition 37 are two others, the latter of which is commonly known as Ptolemy's theorem. Ten other additional propositions are at the end of this book, and they will serve as a good introduction to the many theorems on poles and polars, radical axes of circles, orthogonal circles, and the nine-point circle of a triangle. In the fourth book the propositions are as usual, no material alteration having been made. Throughout the work each proposition is accompanied by numerous exercises, while a capital set of miscellaneous problems terminates each book. We may safely say that the work of which this is an instalment will take the first place among the many books on the elements of geometry.

Zoological Articles contributed to the "Encyclopædia Britannica." By E. Ray Lankester, M.A., &c., and others. (London: Adam and Charles Black, 1891.)

UNDER the above title, Prof. E. Ray Lankester has republished a series of articles communicated by himself and others to the recently published edition of the "Encyclopædia Britannica," the whole forming a very admirable and well-illustrated treatise on some of the more important groups of the animal kingdom.

The memoirs on the Protozoa, Hydrozoa, Mollusca, Polyzoa, and Vertebrata are by Prof. Lankester; that on the Sponges is by Prof. Sollas; that on the Planarians is by Prof. von Graff; that on the Nemertines is by Prof. Hubrecht; that on the Rotifers is by Prof. Bourne; and that on the Tunicates is by Prof. Herdman.

While not professing to be a complete hand-book to zoology, this volume will be a very useful one to the student, as it will give him an excellent summary of the most recent views of the above specialists on the various groups treated of.

A few new figures have been added to the original text, and a few errors—due to previous oversight—have been corrected. Some quite recent discoveries are alluded to in footnotes, while in the preface Prof. Lankester points out one or two important alterations in classification and nomenclature which it was not possible to incorporate in the text.

The illustrations are refreshingly new, and they are as attractive as instructive. This volume is one we can very strongly recommend to all working students.

The British Empire: the Colonies and Dependencies. By W. G. Baker. (London: Blackie and Son.)

THIS volume is one of the series known as "Blackie's Geographical Manuals," and forms the second part of a book on the British Empire, the first part having dealt with "the home countries." The author sets to work systematically, beginning with the British possessions in Europe, then advancing to those in Asia, Africa, America, and Australasia. The method he adopts in describing the various countries included in the British Empire has the great merit of being natural, simple, and clear. The position and extent of each country are first noted; next he explains the form of surface, describes the climate, and traces the coast-line. Details as to population, industries, and commerce come afterwards, and these are followed by "such facts concerning the social and political condition of the people as are usually included in a course of geography for children: the form of government, and descriptions of the political divisions and chief towns." Mr. Baker has selected his facts with much discretion, and presents them in a way which is likely to interest young readers and to exercise the intelligence as well as the memory. There are many very good illustrations and several coloured maps.

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 Rôle of Quaternions in the Algebra of Vectors.

PROF. GIBBS seems to forget that his pamphlet was specially announced as *Arranged for the use of Students in Physics.* When I wrote the remark which has called forth his recent letter, I was discussing the reasons for the comparatively slow progress of Quaternion Analysis in recent times. And, as it is precisely to students of Physics that I think we must look for such progress, anything which is calculated to divert their attention from Quaternions, or to confuse them in their use of Quaternion symbols, must be regarded as tending to retard the progress of the method. The Vector-Analysis supported by Prof. Gibbs involves a serious departure from the usage of ordinary Algebra, inasmuch as $\alpha\beta$ is not regarded as a product, but merely as a "kind of product." This is specially likely to confuse an ordinary student, and is undoubtedly artificial in the highest degree:—while one of the chief recommendations of Quaternions is their naturalness, *i.e.* the utter absence of artifice in their fundamental rules.

Some of Prof. Gibbs' suggested notations are very ingenious, and well calculated to furnish short cuts to various classes of results *already obtained.* But they do not seem to have led to any important extensions of such results; and must therefore be regarded as having claims to consideration very much inferior to those of the novelties of notation suggested by Mr. M'Aulay; for these have undoubtedly shown their value as instruments of research, while they do not depart from algebraic usage in a matter so fundamental as is the notation for ordinary multiplication.

It is singular that one of Prof. Gibbs' objections to Quaternions should be precisely what I have always considered (after perfect inartificiality) their chief merit:—viz. that they are "*uniquely adapted to Euclidian space,* and therefore specially useful in some of the most important branches of physical science." What have students of physics, as such, to do with space of more than three dimensions?

I do not agree with Prof. Gibbs in his statements as to the superiority of his notation over that of Quaternions, so far at least as can be judged from the examples he cites. Surely the simple equality

$$S\sigma\phi\rho = S\rho\phi'\sigma = \Sigma(S\alpha\sigma S\lambda\rho)$$

shows, at least as plainly as does

$$\sigma.(\alpha\lambda + \beta\mu + \gamma\nu).\rho = \rho.(\lambda\alpha + \mu\beta + \nu\gamma).\sigma,$$

that the common value is "exactly the same kind of function of σ that it is of ρ." As to $S\sigma\phi$, it is simply meaningless, and I cannot imagine anyone writing such an expression.

But I have a positive objection of another kind to Prof. Gibbs' proposed notation. To save complexity a great number of *indispensable* brackets, or their equivalents, are omitted. If all the really necessary signs of this kind were inserted, the translations of moderately complex quaternion formulæ into his language would be frightful even to look at. To take a very simple case:—so far as I understand him Prof. Gibbs would write

$$(\alpha \times \beta) \times (\gamma \times \delta) \times (\epsilon \times \zeta)$$

as the equivalent of

$$VV(V\alpha\beta V\gamma\delta)V\epsilon\zeta.$$

But, if so, how would he write, without additional parentheses,

$$VV\alpha\beta VV\gamma\delta V\epsilon\zeta, \quad V.\alpha\beta VV\gamma\delta V\epsilon\zeta, \quad V.V\alpha\beta V\gamma\delta V\epsilon\zeta, \text{ \&c.?}$$

Even the first of the four just written has but *one* pair of brackets, besides the five Vs; while Prof. Gibbs' equivalent has *three* pairs of brackets, besides the five $\times$s. And, so far as I can see, Prof. Gibbs must invent a new notation entirely for the last two formulæ written above, since the product of three vectors is nowhere provided for in his system, important as it is and of constant recurrence in physical problems.

St. Andrews, April 25. P. G. TAIT.

P.S.—As I see that the correspondence in the *Athenæum* regarding the notice of Hamilton in the *Dictionary of National Biography*, has just been summarily closed, I would remark that the patent error of that notice is the confusion of Hamilton's *Varying Action* with his *Quaternions.* The consequence is that Hamilton gets no credit whatever for his absolutely invaluable contribution to Theoretical Dynamics!

The Influenza.

THE influenza—or the peculiar fever we know by that name—seems to be again making its appearance amongst us; at present in scattered cases in the towns, villages, and country districts of the west of England, and with some severity in Yorkshire. How is it that having had the disease once does not give immunity against future attacks, as is more or less generally the case with diseases of this kind, *i.e.* those caused by microbes, as this is now held to be? It would be interesting if some statistics could be arrived at with regard to the present outbreak, showing the proportion of cases of those who had already had influenza, and of those, who, having escaped before, have now taken the disease. E. ARMITAGE.

April 22.

Antipathy [?] of Birds for Colour.

YOUR correspondent "M. H. M." asks the question in your issue of April 16 (p. 558), "Has the sparrow an aversion to yellow?" because of its habit of destroying, as if in wantonness, the petals of crocuses of that particular colour. From some observations I made on the subject, when it was broached about four years ago, an account of which appeared in your columns (vol. xxxvi. p. 174), it is evident that the birds show a decided *fondness* for the colour, instead of an aversion to it. In the case I described, the birds used laburnum sprays only for their nests. It may interest your correspondent to know of such an incident, and to refer to the other correspondence on the subject alluded to on the occasion. WILLIAM WHITE.

The Ruskin Museum, Sheffield, April 22.

A Meteor.

SEEING the note in your issue of the 16th inst. (p. 566) as to a meteor observed by a lady correspondent at Ruanlanihorn, Cornwall, on March 26 last, I send you the following extract from my note-book in reference to a meteor observed by me at Martock, Somerset, on the same evening, and which there would seem to be little doubt was the same as that referred to in your note:—

"March 26.—At Martock, Somerset, saw a very fine meteor in the southern sky; time 7.16 p.m. It was travelling from west to east at a comparatively slow speed; the meteor was of a white hue, and left a visible train, in length about equal to the apparent diameter of the moon; the train was a deeper yellow tint and was broken up towards its end. The meteor was visible fully 5 seconds."

Martock is about 110 miles from Ruanlanihorn.

Addiscombe Road, Croydon, April 20. W. BUDGEN.

ON TIDAL PREDICTION.[1]

THE moon and the earth revolve in an orbit about their centre of inertia, and the centrifugal force due to the revolution exercises an unequal repulsion on the various parts of the earth; the moon also attracts the nearer parts of the earth more strongly than the remote parts. The differential action arising from these two forces is the cause of the tides.

This is explained in many elementary books, and although the exposition is often obscure rather than lucid, I will not stop to go over the demonstration, but will merely refer to Fig. 1, which shows the direction and relative intensity of the tide-generating force at various parts of the surface of a planet.

It is obvious, from the figure, that if the system were at rest the ocean would be elongated towards and away from the satellite, and equally depressed all round the sides. This result is expressed in technical language by saying that the figure of equilibrium is a spheroid with its axis of symmetry pointed at the satellite.

Newton, the founder of the equilibrium theory of the tides, was well aware that this theory would give no approximation to the truth when the system is endowed with motion, and when the ocean is interrupted by land. But whilst he recognized that the theory was useless as directly furnishing a method of tidal prediction, he was

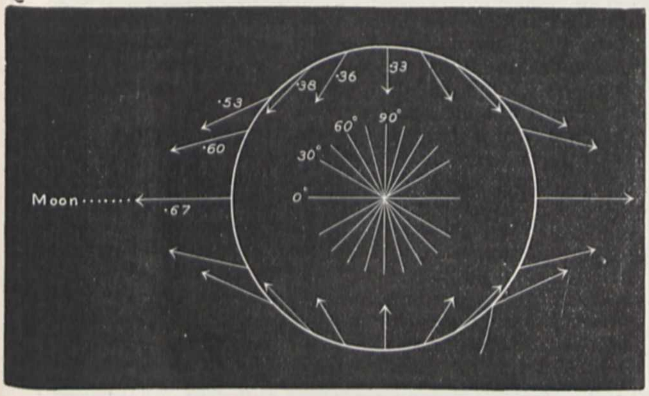


FIG. 1.—Diagram of tide-generating forces.]

clearly aware that it was convenient to retain the conception of the figure which an ocean would assume if the whole system were at rest. In this view he was right, and all that can be attained in tidal science rests on the equilibrium theory. It is true that the word theory is not a good one to express what is meant; but if it had been called "the equilibrium specification of tidal forces," the objections which have been raised against it would have been obviated.

The meaning of this is that, if the ocean covered the whole earth, and if all the motions were very slow, the tides would be such as are determined by the equilibrium theory. Since the intensity and direction of tide-generating forces are independent of the velocities of the earth's rotation and of the moon's motion, and also of the shape and size of the oceans, the so-called equilibrium tide serves to specify the tidal forces in the actual case. It is accordingly convenient to speak of the equilibrium tide as of a real phenomenon, and to make the transition to the actual oscillations of the sea at a later stage of the discussion.

We have only as yet spoken of the lunar equilibrium tidal spheroid, but for similar reasons there must also be a solar spheroid, of a smaller degree of elongation or ellipticity. The superposition of these two spheroids specifies the total resultant tidal force.

Since the major axes of the lunar and solar spheroids point to their respective luminaries, and since the earth rotates, the motion of the resultant spheroid relatively to the earth's surface, and the law of variation of its ellipticity, are very complex; further analysis of the two spheroids becomes, therefore, necessary. It is, accordingly, usual to regard the lunar spheroid as being built up out of three others, as follows: first, a prolate spheroid with its major axis in the equator in the same meridian as the moon; secondly, a prolate spheroid with its major axis in latitude 45°, also in the same meridian as the moon, the latitude being north or south, according as the moon is north or south of the equator; thirdly, an oblate spheroid with its axis coincident with the polar axis of the earth.

The axes of the first and second of these spheroids always follow the moon's meridian, and the ellipticities of all three vary slowly as the moon changes her linear distance from the earth and her angular distance from the equator. The ellipticity of the first or equatorial spheroid oscillates about a mean value; but the ellipticity of the second kind of spheroid is greatest when the moon is furthest north, diminishes to nothing (so that the spheroid becomes a sphere) when the moon is on the equator, and its axis leaps from 45° N. latitude to 45° S. latitude at the moment that the moon crosses from north to south, when the ellipticity begins to increase again.

The third kind of spheroid is of comparatively little importance in tidal theory, and I shall not make further reference to it.

Each of the two lunar spheroids moves slowly in space as it follows the moon's meridian; but to an observer, who is carried rapidly round by the earth's diurnal motion, the oscillations of the ocean due to the two spheroids would have different characters. In the case of the first sort of spheroid, he would pass through *both* protuberances in the course of one rotation of the earth, whilst he would only meet one protuberance of the second sort of spheroid, on account of its unsymmetrical position with regard to the earth's axis. Thus the first sort would by itself give him two high waters *per diem*, but the second sort would only give him one high water. The first may therefore be called the lunar semi-diurnal spheroid, and the second the lunar diurnal spheroid.

A similar analysis of the total solar spheroid gives a solar semi-diurnal and a solar diurnal spheroid; and the third sort of solar spheroid may again be omitted from consideration.

The axes of the two lunar and of the two solar spheroids follow the moon's and sun's meridians respectively, and their ellipticities vary with the linear distances of the two bodies from the earth, and with their angular distances from the equator. Taking these spheroids as specifications of tidal force, we see that the lunar and solar tidal influences are not yet analyzed into forces which are regularly periodic, either in intensity or in period. Accordingly, the next step is to break up each of the four variable spheroids into a number of new ones, which have invariable ellipticity and which move with uniform velocity relatively to the earth's surface. It is theoretically necessary to take an infinite number of these new spheroids to build up each of the former ones, but about five new spheroids suffice to represent any one of the old ones so closely that the error is negligible.

The result of this analysis of tidal forces is, then, to get some twenty equilibrium spheroids, which do not any longer follow the moon's or sun's meridian, but which do move with uniform velocity over the earth, and which are invariable in shape. These spheroids are, like the former ones, divisible into two groups—a semi-diurnal group,

[1] A paper or lecture delivered to the Cambridge Philosophical Society on February 23, 1891. A few passages in it have already appeared in NATURE, in the account of the Bakerian Lecture delivered to the Royal Society on January 29, 1891. (See Phil. Trans. Roy. Soc., 1891.)

each of which would, by itself, make high water twice in a period not much different from a day, and a diurnal group, giving high water about once a day.

These tidal spheroids are merely specifications of the tidal forces, and may be used in the actual case with the same confidence as if the sea covered the whole earth, and as if the system were at rest. The spheroids, in fact, indicate that if the globe were covered with water, and rotated very slowly, there would be regularly periodic rises and falls of water equal to those given by each of the ideal equilibrium spheroids. But the determination of the oscillations of a sea of variable depth and irregular outline on a rotating planet quite surpasses the power of mathematical analysis, even when the generating forces are regularly periodic. It can only be asserted that at each place there will actually occur a regular rise and fall of the sea with unknown amplitude, exactly co-periodic with the passage of the equilibrium spheroid, and that high water will follow the crest of the ideal equilibrium spheroid by some unknown but constant interval. It may, however, be certainly concluded that if two spheroids of the same class—both semi-diurnal, or both diurnal—move with nearly equal speed over the earth, then the heights of the corresponding high waters will be nearly proportional to the ellipticities of the respective spheroids, and the intervals of retardation will be nearly equal to one another. It follows from this law that an equilibrium spheroid, which varies slowly in ellipticity, and which moves nearly uniformly over the earth, will correspond at any place with a real oscillation of the sea, of amplitude nearly proportional to the varying ellipticity, and with nearly constant retardation of high water behind the passage of the crest of the equilibrium tide. The more rapidly the ellipticity varies, and the more irregular the motion of the spheroid, the less accurate does this law become.

Now this brings us to consider two methods of treating tidal observation and prediction—namely, the synthetic and the analytic methods.

In the synthetic method all the semi-diurnal spheroids are added together to form a resultant luni-solar semi-diurnal spheroid, and a similar recomposition of the other group gives a resultant luni-solar diurnal spheroid. These two resultant spheroids are found to move over the earth with some approach to uniformity, and their ellipticities vary with moderate slowness. It is, then, assumed that at any port the heights of the semi-diurnal and diurnal tide-waves will vary *pari passu* with the ellipticities of the two spheroids, and that the retardations in time will be nearly constant.

In the analytic method as many tidal spheroids are considered as are wanted to give a sufficiently accurate representation of the tidal forces. Each one of these constituent spheroids is then known to give rise to a mode of oscillation of the sea, which at any port is constant in amplitude and in retardation.

In both plans astronomical considerations are essential: in the synthetic method the laws of the variation of the ellipticity and of the speed of motion are dependent on the positions of the moon and sun; and in the analytic method we have to determine how many different spheroids of constant ellipticity are wanted to build up the resultant tidal spheroid with sufficient accuracy, and what are their various uniform velocities over the earth's surface.

In the synthetic method there is a single semi-diurnal and a single diurnal spheroid, and the singleness of the spheroids is the important consideration.

In the analytic method the number of spheroids is immaterial, whilst the constancy of their ellipticities and the uniformity of their motions are the objects aimed at.

On account of this difference of aim, it is necessary to fix the positions of the moon and sun in different ways in the two methods. In the synthetic method the positions are determined by angles which bear a simple relationship to the earth's surface; in the analytic method by angles which increase at a uniform rate.

A point on the earth is fixed by its longitude and latitude, but when this system is applied to a celestial object, its longitude, measured from the meridian of the place under consideration, is called the local hour angle, and its latitude is called the declination. Accordingly, in the synthetic method the semi-diurnal and diurnal spheroids have their ellipticities and velocities specified by means of the hour angles, declinations, and linear distances (or horizontal parallaxes) of the moon and sun. There are apparently six variables, but as both the sun's declination and its parallax depend on the time of year, they are equivalent to only five variables.

The principal variations of the total semi-diurnal spheroid depend on the two hour angles, but there are also subordinate changes due to variations in the two declinations and the two parallaxes.

In the case of the diurnal spheroid we must regard not only the two hour angles but also the moon's declination as principal variables, whilst the changes due to the sun's declination and to the two parallaxes remain subordinate.

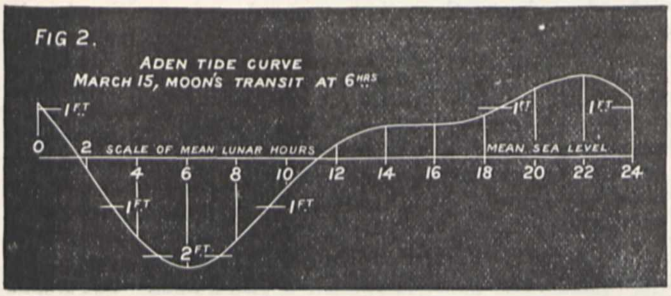
It happens that on the eastern coasts of the North Atlantic the diurnal spheroid gives rise to very little oscillation of sea-level, and therefore in Europe the two hour angles may be regarded as principal variables, and the two declinations and parallaxes as subordinate ones. The apparent time of the moon's transit being only another name for the excess of the sun's hour angle over the moon's, it is easy to regard the time of moon's transit and the interval thereafter as the two principal variables, instead of the hour angles themselves. The passage of the luni-solar equilibrium semi-diurnal spheroid over the meridian of any place bears an intimate relationship to the time of moon's transit at that place; accordingly, in the synthetic method, the interval after moon's transit and the height of rise are given as specifying the tide. The interval and height are afterwards corrected according to the declinations and parallaxes of the two bodies.

At places where the diurnal tide is small, the synthetic method possesses a great advantage—namely, that the inequalities in the height and interval are so regular in each half lunation, that a single table arranged according to the hour of moon's transit, is sufficient; there are, of course, also tables of corrections arranged according to the declinations and parallaxes of the moon and sun.

We have seen, however, that as concerns the diurnal spheroid, the moon's declination should rank as a principal variable, and the application of the synthetic method to non-European ports has been an attempt to keep the moon's declination down to its supposed proper position as a subordinate variable. In this attempt the corrections have shown an ever-increasing tendency to complication, and there have been constructed correctional tables depending not only on the two declinations and two parallaxes, but also on the rates of change of the moon's right ascension, declination, and parallax. Even with all this care the results are not easily made satisfactory for places with a large diurnal tide. As late as 1840, Whewell had not realized that European tides are abnormal in their simplicity. In a later memoir he was evidently surprised at the importance of the diurnal tide in other parts of the world, but he seems always to have regarded it as a matter to be treated by means of corrections.

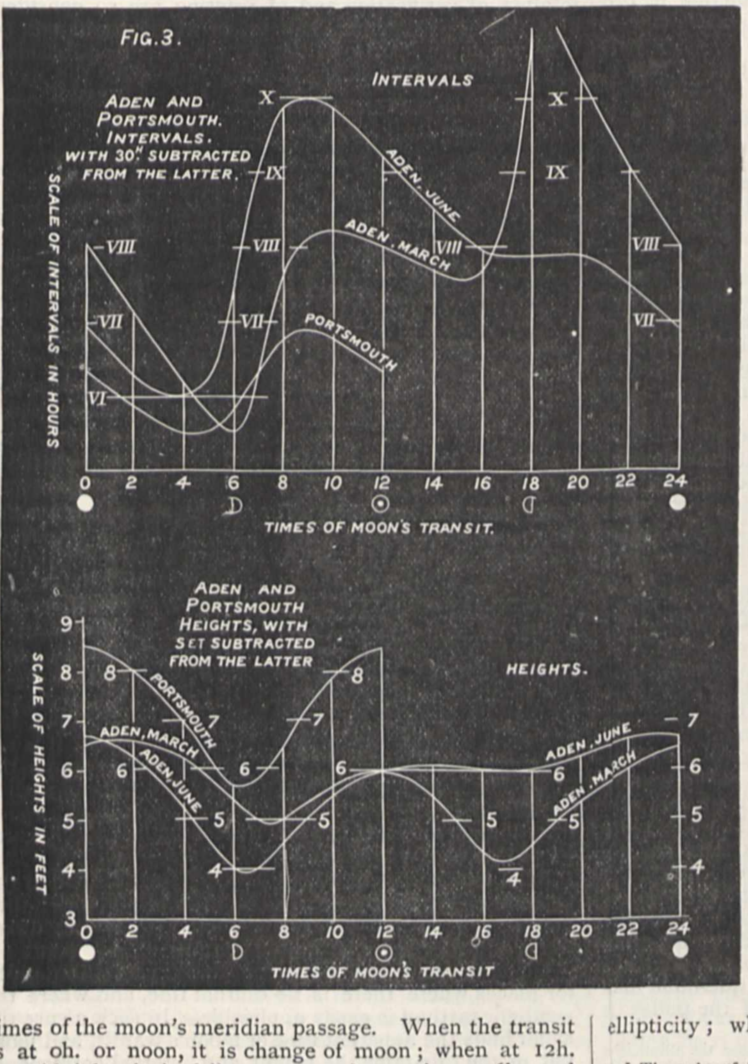
In order to prove how futile corrections must be in such cases, I refer to Fig. 2, which exhibits the rise and fall of water during one day at Aden when the moon crosses the meridian at 6 p.m. in the middle of March. In the synthetic method we should have to suppose as a first approximation that there were two equal high waters and two equal low waters in the day. In this tide curve traces of the second high water and of the second low water are seen in the lingering of the water a little above mean water-level during about four hours—namely, from 12½h. to 16½h.

The same thing is shown in even a more striking way



by Fig. 3, which gives the fortnightly inequality in the height and interval during any half lunation at Portsmouth, and also shows the same inequalities during two different whole lunations at Aden.

In these figures the horizontal line is a scale of the



times of the moon's meridian passage. When the transit is at 0h. or noon, it is change of moon; when at 12h. or midnight, it is full moon; and transits at 6h. and 18h. are the half moons, waxing and waning. In the curves of intervals the ordinates are the number of hours after moon's transit to high water; and in the curves of heights the ordinates give the number of feet of rise at high water.

The Aden curves only show the height and interval for the high water which follows the moon's upper or visible transit, and the same curves would have to be repeated with their second halves in the first place, and with their first halves in the last place, in order to give the height and interval for the high water following the moon's lower or invisible transit.

If the synthetic method were applied to Aden, we must as a first approximation regard the March curve and the June curve as identical, and the second halves of each as a repetition of the first halves. It is obvious that it is impossible to work with any pretence to accuracy on such an hypothesis.

It will be noticed that the March curve of intervals is interrupted about the time when the moon's transit is at 18h.; this means that the corresponding high water is missing. Fig. 2, in fact, has exhibited that one of the two high waters is missing. The missing or evanescent high water is the one which ought to have followed moon's transit at 18h., and the existent one follows moon's transit at 6h.[1] Great as is the difference between the two halves of the March curve of intervals, it is less conspicuous than the contrast between the two halves of the June curve.

Finally, whilst the greatest interval exceeds the least by about an hour and a half at Portsmouth, it exceeds by nearly six hours at Aden.

The contrast between the March and June curves of heights for Aden is not so striking as in the case of the intervals, but is still very great.

Enough has now been said to show the necessity for a new departure in the really normal case of a considerable diurnal tide, and the impulse was given by Sir William Thomson when he proposed the harmonic analysis of tidal observations. This is, in fact, the analytic method the principles of which have been already sketched.

In the harmonic method, about twenty equilibrium spheroids are taken as giving a sufficiently accurate representation of the tidal forces. It has already been shown that constancy of ellipticity and uniformity of velocity are the leading considerations, and that the speeds of the spheroids are to be expressed by means of angles or quantities which increase at a uniform rate. Accordingly, the speeds are now expressed in terms of the mean solar time, and of the mean longitudes of the moon, of the sun, of the lunar perigee, of the lunar node, and of the solar perigee—all of which increase at a uniform rate. Since the longitude of the solar perigee is to all intents a constant, there are five variables instead of the six of the synthetic method.

Only three of these variables—namely, the time and the mean longitudes of the moon and sun—are required to express the speeds of the spheroids of the largest ellipticity; whilst the speeds of the less important

[1] The order of these two high waters is apparently inverted in the figure, because the first ordinate corresponds to a time 7h. 54m. after moon's transit, being the moment of "mean lunar semi-diurnal high water." The high water which follows the transit at 6h., has just passed when the figure begins.

spheroids require also the longitudes of the moon's perigee and node.[1] There are, therefore, three principal variables and two subordinate ones, but no advantage is taken of this subordination, each spheroid being treated by itself.

Each spheroid, in its passage over the earth corresponds with regularly alternating tidal forces, which generate regular oscillations of the sea of unknown amplitudes and retardations. Observation of the tides at any port affords the means of determining the amplitude and retardation of each mode of motion, and, accordingly, there are two tidal constants corresponding to each equilibrium spheroid.

The separation of tidal oscillations into some fifteen or twenty distinct simple oscillations is admirable as a method of analysis and registry, but its advantages for the purpose of prediction are less conspicuous. Since high and low water are single events arising from a number of separate causes, synthesis is essential to prediction, and the computation of a tide-table involves the determination of nearly four maxima and minima per diem of an algebraic expression of fifteen or twenty terms. Dr. Börgen, of Wilhelmshaven, has attacked the problem with courage and success, but the work remains, and always must remain, very laborious. In fact, prediction in the harmonic system labours under one great disadvantage—it is very expensive.

Sir William Thomson was so conscious of this disadvantage, that, shortly after his initiation of the harmonic method, he suggested that the obstacle might be turned by the mechanical synthesis of the separate oscillations. It was in 1872, I believe, that he laid down the principles upon which a synthetic machine might be constructed. Mr. Edward Roberts, of the *Nautical Almanac* Office, bore a very important part in the realization of the idea, and the tide-predicting instrument, now in the Indian Store Department at Lambeth, was constructed by Légé, under his direction. A paper by Sir William Thomson, in the sixty-fifth volume of the Transactions of the Institution of Civil Engineers, and the subsequent discussion, contain details of the respective parts played by Sir William Thomson, Mr. Roberts, and Messrs. Légé in the realization of the idea.

The machine cost several thousands of pounds,[2] and it is the only one of its kind. It requires skill and care in manipulation, and it has been ably worked by Mr. Roberts for the production of tide-tables for the Indian Government, ever since its completion. Tables for thirty-one ports in the Indian Ocean are now being mechanically computed and published annually.

A whole lecture would be required for a full description of the instrument, and I will confine myself to a diagrammatic illustration of the principles involved.

A cord passes over and under a succession of pulleys, being fixed at one end and having at the other end a pen which touches a revolving drum. If all the pulleys but one be fixed, and if that one executes a simple harmonic motion up and down, the pen will execute the same motion with half amplitude. If a second pulley be now given an harmonic motion, the pen takes it up also with half amplitude. The same is true if all the pulleys are in harmonic motion. Thus the pen sums them all up, and leaves a trace on the revolving drum. When the drum and pulleys are so geared that the angular motion of the drum is proportional to mean solar time whilst the harmonic motions of the pulleys correspond in range and phase to all the important lunar and solar tides, the trace on the drum is a tide curve, from which a tide-table may be constructed. The harmonic motion of the pulley is given by an arrangement indicated only in the case of the lower pulley in Fig. 4. The pulley frame has attached to its vertical portion a horizontal slot, in which slides a pin fixed to a wheel. If, whilst the drum turns through one mean solar hour, the wheel turns through two mean lunar hours, the pulley executes lunar semi-diurnal oscillations. When the throw of the pin and its angular position are adjusted so as to correspond with the range and phase of the observed lunar semi-diurnal tide, the oscillation of the pulley remains rigorously accurate for that tide for all future time, if the gearing be rigorously accurate, and with all needful accuracy for some ten years of tide with gearing as practically constructed. The upper pulleys have to be carefully counterpoised, as indicated. It has not been found that any appreciable disturbance is caused by the inertia of the moving parts, even when the speed of working is high. It requires about four hours to run off a year's tides. The Indian instrument combines twenty different harmonic motions.

But, notwithstanding its admirable construction, the tide-predicting instrument mitigates rather than annihilates the disadvantages of the harmonic method. For, besides the large initial capital expenditure, the expenses of running off the curve, of the measurement of maxima and minima, of verification and of printing, are so considerable that the instrument cannot, or at least will not, be used for minor ports in remote places. It is, besides, not impossible that national pride may deter the naval authorities of other nations from sending to London for their predictions.

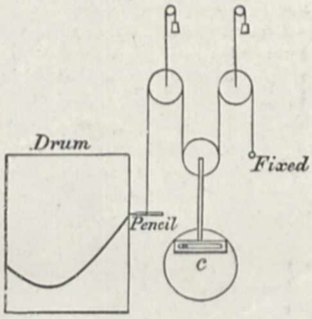


Fig. 4.—Diagram of tide-predicting machine.

There is, then, a want of other methods of forming tide-tables, if they can be devised. Now, a tide-table for any port is not necessarily of the kind produced by the machine, for such tables may be divided into two classes, which may be called the general and the special. In a special table the times and heights of high and low water are specially predicted for each day of each future year. This is the kind of table furnished by the machine, but although it is the table which a sailor likes best, it is too expensive for universal use. A general tide-table, on the other hand, merely states the law of the tides in such a form that, by reference to the *Nautical Almanac* and a little simple arithmetic, a few tides may be quickly computed for the days required.

Such tables may be computed once for all, so that the expense of producing them is of little importance; they may be printed and published along with sailing directions, and they serve for all future time.

General tide-tables have for years past been given for places where there is no diurnal tide, and where the synthetic method is easily applicable. In such a case the sailor finds the apparent time of moon's transit, and looks out in the tide-table the height and interval corresponding to that time. He then adds the interval to the mean time of moon's transit, and he has thus obtained the approximate time and height of high water. Tables of

[1] In actual use the method is partially synthetic, because the ellipticities and speeds of all the spheroids of lunar origin are taken as varying to a very small extent with the longitude of the moon's node, which completes its circuit in about nineteen years.

[2] Sir W. Thomson tells me that with a different sort of gearing the expense of construction may be much diminished.

corrections according to the moon's declination, moon's parallax, and the time of year furnish him with the means of obtaining the correct time and height.

I do not think, however, that any general tide-tables, worthy of the name, have hitherto been made for ports with a large diurnal tide, and these are the majority of all ports. It is the object, then, of this paper to show how such tables may be formed. The tables are, unfortunately, not nearly so short as when the tides are simply semi-diurnal, and the expense of computing them will be not inconsiderable, but it will be an expense incurred once for all for each port.[1] The system proposed will not render the tide-predicting instrument less useful, but it will fulfil other requirements, and will, I think, obviate the disadvantage which seemed inherent to the analytic or harmonic method.

A sketch of the manner in which it is proposed to compute these tables, and of their form when completed, will now be given.

In the complete harmonic analysis of the tides there are three principal variables—namely, the mean solar time and the mean longitudes of the moon and sun; and two subordinate ones—namely, the mean longitudes of the lunar perigee and node. In order to effect a partial synthesis of the harmonic spheroids, I introduce the conception of a fictitious satellite which moves along the equator in exactly the same way as the moon moves along the ecliptic, and which coincides with the moon at the moment that she is in the equator.[2] The time is counted from the moment at which the fictitious satellite crosses the meridian of the place of observation, so that, for each high water, time is counted from a different epoch; thus, mean solar time is replaced by the interval since fictitious transit: this is the first of the principal variables. The second variable is the moon's phase counted in degrees from 0° to 360°, or, what amounts to the same thing, the excess of the moon's true longitude above the sun's. The third variable is the time of year, or, speaking exactly, the sun's true longitude. For the subordinate variables, the longitude of the moon's node is retained as one, whilst the excess of the moon's parallax above or its defect below its mean value is taken as the other.

By means of this change of variables, the fifteen or twenty true harmonic spheroids may be replaced by three semi-diurnal and three diurnal spheroids, whose ellipticities vary within very narrow limits, and whose speeds over the earth's surface are nearly uniform. Four of these six spheroids are subject to small corrections for the variability of the moon's parallax and for the longitude of the moon's node; and the two remaining spheroids should, in strictness, be subject to corrections for the sun's parallax, which are, however, so small that they may perhaps be neglected.

The transition from the equilibrium spheroids to the actual oscillations of the sea is made by means of the harmonic tidal constants, which are supposed to have been found by the reduction of tidal observations.

Then, for any given time of year I compute the interval from fictitious transit to high water, and the height, for all phases of the moon; certain factors are also computed, by means of which the nodal and parallactic corrections to the time and height may be found.

If these computations were made at frequent intervals of the year and of the moon's phase, we should have the desired general tide-table, but it would not be given in a very convenient form, because the time of fictitious transit is not given explicitly in the *Nautical Almanac.*

The next step is, therefore, to find formulæ for the interval of time between a fictitious transit and a moon's real transit, and for the moon's phase in terms of the time of moon's transit. By means of these formulæ the tables are transformed, so that we get a table of intervals after moon's transit arranged according to the hour of transit. The table resembles that in use in the synthetic method, save that we have not to consider the moon's or sun's declinations, and that there are only two correctional tables; it is necessary, however, to compute for all times of year.

I had hoped that short tables computed at intervals of a month would have sufficed for practical purposes, but when the diurnal tide is large, the changes in the height and interval are so abrupt and so large that a skeleton table would give very inaccurate results, unless used with elaborate interpolation. We can clearly not expect sailors to use tables of double entry, in which interpolation to second and even third differences is required, and indeed all interpolation is objectionable. I therefore propose that the tables shall be made so full as to obviate interpolation; tables for about every ten days, and for every 20m. of moon's transit, seem to satisfy the requirements of the case.

The computation would run to a great length if the calculation had to be made in full, but it is fortunately possible to reduce the work to a great extent.

It appears that the height, interval, and corrections for any tide occurring after a moon's transit at any hour and at any time of year, are nearly identical with the same quantities for the tide occurring after a moon's transit twelve hours later and six months later. Thus the tables need only be formed for the half instead of for the whole year. Further, it is found to give an adequate insight into the law of tide, if the computations are actually made for each month and for each hour of transit. Since there are six months and twenty-four hours of transit, the computations are made for 144 heights, intervals, and correctional factors. There are other artifices by which the work is abridged, but into which I will not enter.

The interpolations necessary for the completion of the tables for every 20m. of moon's transit, and for every ten days, may be made with sufficient accuracy by means of curves. Although this work is tedious, it is better to throw it on the professional computer, who does it once for all, rather than on the sailor, who would do it piecemeal and many times over on various occasions. It has occurred to me that the most succinct form in which the final results could be published would be by means of these curves without tabulation, but ten or twenty pages of a book convey the information in a much more portable form than an atlas of curves, so that I am doubtful of the advisability of the graphical form of tabulation. I may here remark that although the general tide-table is primarily intended for use in the general form, it may also be used for the computation of a special tide-table without heavy expense.

Such a piece of work as this can only be deemed complete when an example has been worked out to test the accuracy of the tidal prediction, and when rules have been drawn up for the arithmetical processes, forming a complete code of instructions to the computer.

I chose the port of Aden for the example, because its tides are more complex and apparently irregular than those of any other place which, as far as I know, has been thoroughly treated. The tidal constants for Aden are well determined, and the annual tide-tables of the Indian Government afford the means of comparison between my predictions and those of the tide-predicting instrument. The arithmetic of the example was long, and the plan of marshalling the work was rearranged many times. An ordinary computer is said to work best when he is ignorant of the meaning of his work, but in this kind of tentative work a satisfactory arrangement cannot be attained without a full comprehension of the reason of the methods. I was, therefore, very fortunate in securing the enthusiastic assistance of my friend Mr.

[1] I believe that it may cost about £50 to compute my table, but it is impossible as yet to give any precise estimate.

[2] The rigorous definition of the fictitious satellite is that its right ascension is equal to the moon's true longitude measured in her orbit.

Allnutt, and I owe him my warm thanks for the laborious computations he carried out. After computing fully half of the original tables, he made a comparison for the whole of 1889 between our predictions and those of the Indian Government.

The theory of the several methods of tidal prediction has hitherto taken up all our attention, and the reader will naturally be interested to know what degree of accuracy is attained by all this labour.

Captain Wharton has kindly supplied me with a comparison between the predictions, made by the synthetic method, for Portsmouth, and the curves registered by the tide gauge for the month of August 1886.

I find that the errors are as follows:—

Time		Height	
Magnitude of error. m. m.	No. of cases.	Magnitude of error in inches.	No. of cases.
0 to 5	25	0, 1, 2, 3	16
5 to 10	21	4, 5, 6	16
10 to 15	10	7, 8, 9	13
17	1	10, 11	8
32	1	13, 14	3
		16...	1
		18...	1
	58		58

The table of errors in the height, however, gives much too favourable an impression of the success of the prediction, for the prediction is so generally higher than the actuality, that the mean of all the predictions is 5½ inches higher than the truth.

I find that the probable error in the time is 4m.; that is to say, half the actualities fall within 4m. on each side of the prediction. Again, allowing 6 inches for the systematic error in the heights, I find that the probable error in the heights is 3½ inches. The average amplitude of oscillation of sea-level at spring tide (or the spring rise) is 13 feet 6 inches, and at neap tide (or the neap range) is 7 feet 9 inches. The errors in height are therefore not considerable compared with the total changes.

Let us next consider a case where the predictions were made by the tide-predicting machine. Colonel Baird sent me some time ago a comparison, made in the office of the Survey of India, between the Aden predictions and actualities for the year 1884.[1]

Taking the month of January as probably a fair sample, I find that the errors of the 57 high water predictions of that month were as follows:—

Time		Height	
Magnitude of error. m. m.	No. of Cases.	Magnitude of error. in.	No. of cases.
0 to 5	24	0	8
5 to 10	24	1	17
10 to 15	7	2	9
19	1	3	11
21	1	4	7
		5	5
	57		57

Mr. Allnutt determined the probable errors in time and height of the 698 high water and 698 low water predictions for the whole year, and found ± 4m. in time, and ± 2 inches in height, for both high and low waters.

On account of the magnitude of the diurnal tide at Aden, it is useless to state the spring rise of water, but about spring tide the range from high water to low water often amounts to 6 feet 6 inches or 7 feet. Hence, we may consider that an error of 1 inch at Aden has about the same relative importance as 2 inches at Portsmouth. Although the tidal constants of Aden were not so well determined in 1884 as they are now, I think the Aden predictions are better than those at Portsmouth—at least if the month of August 1886 is a fair sample of the latter port. It is fair, however, to state that the disturbance due to the irregular winds of our latitudes is very considerable at Portsmouth;[1] whereas the effects of the regular day and night breezes which occur at such a place as Aden are included in the tide-table.

It is a remarkable fact, at once creditable to the Indian Government and discreditable to all others, that the tides round the enormous range of coast of the Indian Ocean are better known than over any other area in the world.

Finally, let us consider our general tide-table, which has as yet only been worked out for high water at Aden. For the purpose of comparison, I computed a tide-table from March 10 to April 9, and from November 12 to December 12, 1884. In these periods there are 117 actual high waters, and one evanescent high water. Where a tide is evanescent—that is to say, when there is no rise of water—the general tide-table necessarily assigns a definite time for high water, although the rise of water may be *nil*. The following table classifies the errors of the (retrospective) predictions, without reference to signs:—

Time		Height	
Magnitude of error. m. m.	No. of cases.	Magnitude of error. in.	No. of cases.
0 to 5	35	0	22
5 to 10	34	1	42
10 to 15	23	2	26
15 to 20	10	3	17
20 to 25	7	4	3
25 to 30	3	5	5
39 to 46	4	6	1
108	1	7	1
Evanescent ...	1	Evanescent ...	1
	118		118

Omitting the cases of evanescence and of 108m. (which will be shown to be justifiable), the probable errors are 8m in time,[2] and 1·4 inches in height.

When the rise from the higher low water to the lower high water is *nil*, there is evanescence, and when that rise is small there is approximate evanescence.

I have accordingly examined the 24 cases in which the time-error is greater than 15m. (there being two cases of 15m. error exactly), and the following table gives the results:—

Rise from L.W. to H.W.	Time errors.
Nil	(Evanescence)
2 in.	108m.
6 in. to 11 in. ...	22, 22, 39, 44, 46m.
14 in. to 18 in. ...	16, 17, 22, 28, 29, 40m.
19 in. to 29 in. ...	16, 18, 18, 18, 19, 19, 20, 21, 21, 22, 22m.
3 ft. 6 in.	25m.

When we consider that the rise often amounts to 7 feet, all these tides, excepting the last, must be considered as approximating to evanescence. I suspect that the last of these errors must have been due to wind.

We have seen in Fig. 2 the sort of hang there is in the water in the case of an evanescent tide, and, accordingly,

[1] I have recently learnt that the method of comparison in use at that time was such that there was a bias in favour of the prediction. It is not easy to read the time of high water on a tide-curve with accuracy, and previous knowledge of the predicted time gives the observer a bias in favour of a particular reading. This faulty method of testing the predictions has long since been superseded.

[1] I have no space to discuss another aspect of tidal prediction—namely, the influences of wind and weather on the tides; this is in itself a subject of considerable magnitude, and one which is not yet completely understood.

[2] There are circumstances connected with the alleged time-actuals for November 1884, which appeared to demand further inquiry, and I wrote to India accordingly. Colonel Hill, R.E., has now sent me a re-measurement of the tide-curves, but it is not possible to complete the examination of the results in time for this paper. I may state, however, that the comparison in the text leads to a substantially correct estimate.—*March* 20, 1891.

in approximate evanescence an error of half an hour, or even an hour, is quite insignificant.

If these 24 cases be subtracted, the probable error in the time falls to 7m.

These retrospective calculations are distinctly better as regards height than those of the Indian Government, but are worse in the times. It is proper, however, to remark that better values of the tidal constants were used than were accessible in 1884.

The comparison between our predictions and the Indian ones made for the whole of 1889 by Mr. Allnutt, led to conclusions very similar to those just explained.

It may be concluded from these comparisons that highly satisfactory tide-tables may be made from our general tide-tables, when the tidal constants are accurately known.

If the general tide-table were computed in the full form, both for high water and for low water, it would cover 36 pages of rather small type. But at many places it may not be thought worth while to proceed to the full degree of accuracy. If the computation were made for every half-hour of moon's transit, and for every fortnight, the table would be reduced to 16 pages; and if high waters only were given, to 8 pages.

I believe, however, that this is not the direction in which abridgments may be best made, but that it would be more advantageous to drop some of the corrections. Thus the correction to the height, which depends on the longitude of the moon's node, and a certain small correction to the time, which depends on a portion of the equation of time (of which no mention has been made), might be dropped without much loss of accuracy. If the tables are still supposed to be half-hourly and fortnightly, these omissions might reduce them to 6 pages. At some ports a rough high water tide-table might even be contained in 2 or 3 pages, but in this case it might be necessary to add a warning of the possibility of sensible error in the times and heights of the lower high water.

The question of how far to go in each case will depend on a variety of circumstances. The most important consideration is, I fear, likely to be the amount of money which can be spent on computation and printing; after this will come the degree of desirability of a trustworthy tide-table, and the accuracy with which the tidal constants are known—for it is obviously useless to form a nominally accurate tide-table from inaccurate values.

My aim has been to reduce the tables to a simple form, and if, as I imagine, the mathematical capacity of an ordinary ship's captain will suffice for the use of the tables, whether in full or abridged, the principal object in view has been attained. G. H. DARWIN.

THE PHOTOGRAPHIC CHART OF THE HEAVENS.

SUFFICIENT time has elapsed since the dismissal of the International Congress to permit a deliberate view to be taken of the results of the meeting, and to determine how far the prospects of the photographic chart have been improved by the resolutions which the Committee has entered on its minutes. But there is the preliminary consideration, that it is now four years since the scheme was definitely conceived and resolved upon. Have those four years been profitably employed? Doubtless the initiatory steps, such as procuring the necessary instruments, and acquiring facility in their use, as well as a knowledge of their capacities, have occupied a longer time than was originally anticipated, but seen in the light of a fuller experience the completion of so many details in a shorter time was not warranted. It is a matter of legitimate congratulation that all the participating Observatories are adequately and similarly equipped, and that a work undertaken in hope, rather than founded on experience, is moving surely and certainly to a successful issue, and strictly on the lines originally contemplated. The early resolutions of the Committee were not guided by trial and practice, and it is fortunate that so little has had to be altered, and that the scheme in its integrity remains practically as it was conceived. The words with which Admiral Mouchez closed the Conference on the fourth day of debate happily described the situation, and the attitude of the various members of the Committee. "You have had to treat difficult questions," said he; "you have treated them without reference to any other thought than that of truth, and the progress of science. Divergences of views have exhibited themselves as to the means to be employed, never as to the end to be attained. These divergences were inevitable; they were even necessary; they have only made us see the questions in a clearer light, and have never disturbed the cordiality of your relations. The unanimity which has marked your final decisions is a certain pledge of ultimate success. It is in this conviction that I declare the Conference of 1891 closed, and the work of the photographic chart of the heavens commenced."

The keynote of one of these "divergences of views" was struck by the Admiral himself in his opening address, and the subsequent debate led to the exhibition of considerable excitement on the part of individual members of the Committee, which has been commented on elsewhere. It is well known that under the auspices of the International Committee it is proposed to effect two objects, each very desirable in itself. One of these, the one which really called the Committee into existence, is the photographic representation of the present condition of the heavens, in which shall be delineated stars which, under a certain definition, are called the fourteenth magnitude. The other, a more recent thought, is the preparation of a catalogue in which shall be exhibited the co-ordinates of stars down to the eleventh magnitude, on the scale of Argelander's zones. The question arose, as Admiral Mouchez pointed out, whether these two distinct and separate objects should be proceeded with *pari passu*, or whether, since a shorter time would be sufficient to furnish the plates from which the catalogue would be deduced, it was desirable that the data for the catalogue should be supplied before the more arduous undertaking of the chart plates should be commenced. Admiral Mouchez, in his opening address, clearly indicated the advisability of the latter method of proceeding, and adduced very excellent arguments for such a course. A catalogue, as he remarked, is immediately useful, and is a want among astronomers of the present day, whereas the chart is really intended for the benefit of future astronomers, who will value it the more in proportion to its completeness. Further, there are reasons to believe that the duration of exposure necessary to secure, on the negative, a black measurable image of a star of the fourteenth magnitude, will be much longer than has hitherto been anticipated, and that consequently a much greater expenditure of time will be necessary for the production of the plates than has generally been imagined. If, then, the two series of plates are obtained simultaneously, the shorter exposure plates will be injudiciously delayed, and the manufacture of the catalogue cannot be proceeded with till the chart plates of long exposure are completed—a course manifestly to the detriment of the present condition of astronomical science. On the other hand, if by co-operation the series of negatives of short exposure be secured in about two years, which is quite possible, the hands of astronomers will be greatly strengthened by the welcome which will be accorded to the catalogue, and they will be encouraged to undertake the more onerous part of the work. Additional experiments and information could be collected, and it is not too much to hope that some improvement in the preparation of photographic films might secure additional sensitiveness, and so facilitate and abridge the operations.

It is evident that here the lines of cleavage are saliently marked. It was inevitable that some members would feel themselves definitely pledged to the chart, and nothing but the chart; while others, looking to the applicability of photography to the rapid registration of stars, and the accuracy with which their places can be determined, would see greater immediate advantages to be drawn from the compilation of a catalogue, and would endeavour to divert the energies of the Committee into that particular channel.

The debate which followed was interesting, but turned mainly on the points indicated above. The Astronomer-Royal, who had not heard, but was probably acquainted with, the views of Admiral Mouchez, maintained the principle of the chart in its integrity, and this he could do the more boldly since he advocated but a very moderate exposure as sufficient to secure the impression of stars of the fourteenth magnitude. He admitted that, on nights when the sky was not wholly fine, it might be advantageous to take plates of short exposure, suitable for the catalogue, but that the general principle to be observed was, to take the short and long exposed plates, the one directly after the other, in order to avoid the frequent adjustment of the equatorial to the particular part of the sky to be photographed. Moreover, the Astronomer-Royal urged, and the force of the argument cannot be denied, that the Government funds had been granted with the special view of procuring the chart, properly so called, and that a breach of faith would be involved in urging forward any other work with instrumental means so provided, till that for which the grant was allowed was completed.

The proposal in favour of immediately proceeding with the catalogue plates found an able advocate in Dr. Gill, who, however, urged his entire devotion and loyalty to the completion of the chart as originally conceived. He reiterated the points that Admiral Mouchez had so clearly put in his opening address; and in addition, that it was undesirable to embark upon a work of unknown magnitude and extent, seeing that one ready to the hand, which equally involved the construction of a chart, called imperatively for their attention. Captain Abney had previously pointed out a method by which he believed the sensitiveness of the emulsion and the measurability of the images could both be improved, and Dr. Gill seized this point to urge on the Committee, with the weight due to his experience in this department of astronomical photography, the necessity of longer delay and further experiments. Other members of the Committee, with more or less originality, repeated these arguments, and in the hopelessness of arriving at a decision the President (M. Bakhuyzen) adjourned the debate, in the hope that reflection and informal interchange of opinions might disclose a *modus vivendi*.

The method pursued in this discussion has been dealt with at length because it is typical of the procedure that was adopted whenever it seemed impossible to arrive at a unanimous vote. In this case the expectation of the President was not disappointed. An informal meeting of several members of the Committee was held before the next *séance*, and the following resolution was drafted, which was carried without discussion and without dissent:—

"The work undertaken by the Congress of 1887 comprises two series of negatives made with different exposure: the permanent Committee, whilst recommending to observers to urge forward, with the greatest possible diligence, the execution of the negatives of the second category (negatives destined for the construction of a catalogue) is of opinion that they ought also to profit by the greatest possible number of fine nights to take negatives of long exposure for the first series (those of the chart)."

The resolution thus carried happily combines all sections of the Committee in a common work, and though the execution of either the chart or the catalogue was never really jeopardised, it is certain that both will go forward with greater alacrity and harmony on the part of all concerned.

In the early days of the scheme, members talked very glibly of the fourteenth magnitude, though not recklessly, for they were cautious to couple it with a definition, the accuracy of which may be now impugned, but which at the time sufficiently limited the acceptation. It was assumed, though possibly on insufficient evidence, that the exposure of a plate two and a half times that of a given exposure would insure the impression of one additional magnitude. This point has been submitted to further experiment, and it was shown at the Congress, principally by Dr. Scheiner, of Potsdam, that this law did not hold. The time required for the impression of stars brighter than the ninth magnitude is so short that there is a very great difficulty in determining it accurately. Dr. Scheiner had therefore, with a view of settling this question, carried out a series of experiments under various conditions—

(1) By employing plates of low sensibility.

(2) By covering the object-glass with a gauze screen having a very fine mesh.

(3) By reducing the aperture of the object-glass to nine centimetres, and employing both rapid and slow plates.

The result at which he has arrived after these experiments, the most complete that have been made, is that by multiplying the time of exposure by two and a half, a whole magnitude is never gained, but only a half or seven-tenths of a magnitude, and that consequently to obtain the fourteenth magnitude an exposure of *seven hours* would be necessary. Some objections may be raised to the method in which the magnitudes were determined by Dr. Scheiner, particularly to the result deduced from counting the stars. On a plate which he had exposed for eight hours he found impressed only 5689 stars, whereas it might have been anticipated that there would have been 10,566 stars. The argument drawn from the numbers counted might be valid if applied to a large portion of the sky, but on the small district covered by one plate various causes might operate to invalidate the law. Taking Dr. Scheiner's results, however, as they stand, the chart of the fourteenth magnitude is rendered impossible, and the Committee felt it necessary to adopt a method of proceeding which would render the chart not only possible, but should secure uniformity, and also permit of the scheme being carried out within a reasonable time.

By the kindness of the Paris authorities in forwarding early proof-sheets of the *procès-verbaux*, we are enabled to put before our readers a rigorous translation of the resolutions which the Congress has adopted, with unanimity, to this end.

"With the view of enabling observers to pass in a uniform and certain manner from the ninth magnitude of Argelander to the eleventh, which it is sought to obtain on the negatives destined for the catalogue, a Commission (consisting of MM. Christie, Henry *frères*, Pritchard, and Vogel) will distribute among the participating Observatories, screens, having a metallic webbing, absolutely identical for all the Observatories. These screens, placed in front of the objective of the photographic telescope, will diminish the brilliancy of a star by two magnitudes, and in the determination of the diminution of magnitude, the Commission will adopt the coefficient 2·512 for the ratio between two consecutive magnitudes."

Since there is no difficulty by various photometrical methods in determining the magnitude of a number of stars of the ninth magnitude, every Observatory will, with strict uniformity, be able to insure on its plates the impression of stars of the eleventh magnitude. And, in order to insure a reasonable limit of exposure for the chart plates, and to avoid any ambiguity which the employment of such a term as the fourteenth magnitude might produce, the Committee has further passed the following resolution :—

"The Committee indicates forty minutes as the duration of exposure for the negatives destined to form the chart, in the mean atmospheric conditions of Paris, and with plates prepared by the firm of Lumière, which are at present in use at Paris.

"The Commission mentioned above will forward to MM. Henry a screen, by means of which they will determine the time (t), expressed in minutes, required to form impressions of stars of the eleventh magnitude, starting from the ninth magnitude of Argelander. Then, for all observers who will be furnished with an identical screen, the ratio $\frac{40}{t}$ will be the factor by which the time of exposure necessary to give the eleventh magnitude is to be multiplied, in order to obtain the stars of the feeblest magnitude for the chart."

If the Congress had done nothing more than adopt these two resolutions, the meeting would have been justified. They not only render possible, but really insure, the execution of both the chart and the catalogue. In addition, however, much good work was done, both as concerns the method of measurement of the plates and the arrangements for effecting the meridian observations of the "*étoiles de repère*." There is, too, left on their minutes this further important resolution :—

"Concerning the method of reproduction of the stars for the chart, only purely photographical processes shall be employed, to the exclusion of all other processes which demand the intervention of manual labour."

The names of Captain Abney and M. Cornu are added to those of a previously existing Committee to settle the details of the process.

It is anticipated that the metallic screens will be distributed among the various Observatories in less than two months. In the meanwhile each Observatory is at liberty to proceed at once with the catalogue plates, care being taken that stars of the eleventh magnitude are impressed. In this sense, Admiral Mouchez's words are to be understood when he declared the work of the photographic chart of the sky commenced.

MAGNETIC ANOMALIES IN FRANCE.[1]

BY M. MASCART.

[*Translation.*]

THE first magnetic observations made in France by M. Moureaux, in 1884 and 1885, at not more than 80 stations, only enabled us to sketch the magnetic characteristics of the country in their broad features. Nevertheless, these first results were sufficient to detect certain anomalies, which are independent of the well-known irregularities which occur near rocks of volcanic origin, and which show no trace of the uniformity which is generally supposed to exist elsewhere. It was therefore necessary to multiply the stations in order to obtain a representation of the facts, which, if not absolutely correct, should be at least substantially accurate.

We therefore propose to cover France with a much closer network, which shall include about 600 stations properly distributed. Up to the present the north and the north-west, or, rather, the districts penetrated by the Chemins de Fer du Nord et de l'Ouest, are nearly completed, together with the region which extends southwards from Paris to the Loire. The number of stations is now over 200. The following is a description of the first results of these new investigations.

The map of the isogonal lines, in the district under consideration, discloses two principal anomalies, the one in Brittany, the other in the immediate neighbourhood of Paris. The first of these, discovered by M. Moureaux in the year 1886, has not as yet been fully studied. It is still necessary to observe at some points between Pontivy and Morlaix, and along the coast line between the mouth of the Loire and the bay of Douarnenez. Nevertheless, it is beyond question that a centre of disturbance exists near Pontivy, where the declination, which has been determined twice, with the interval of a year between the two observations, is less than at Ploërmel, which is 90 kilometres to the east. To the west of Pontivy, the isogonals are bent towards the north-west, and are drawn together near Morlaix. They next run south, or even south-south-east, thus roughly imitating the form of the coast-line of Brittany. It is possible that when the survey of this district is completed, the primitive rocks which occur in it will serve to explain the peculiarities which observation has brought to light. It is therefore unnecessary to discuss it fully at present.

The second anomalous region, however, is of special interest, on account of the nature of the soil. The attention of M. Moureaux was called to it by the results which he obtained at Chartres on June 22, 1889. Other observations, made both in this town itself and at various other places in the district, have confirmed the accuracy of the first measurements, and have led the observer to study the phenomena in detail in order to discover the magnitude and extent of the anomaly.

According to the provisional map of the true isogonals, the direction of the lines of equal declination only becomes approximately uniform towards the northern boundaries of France. Irregularities can be detected even in the latitude of Amiens, which extend as far as the Ardennes.

The isogonal of 15° 20′, which passed through Paris on January 1, 1890, instead of extending towards Orleans, bends to the south-south-east as far as Gien, then doubles suddenly on itself, running north-west to Houdan, which is west of Paris, and finally resumes a southerly course on the geographical meridian of Chartres. The isogonals, drawn for every 10′ of declination, have all the same shape, from the English Channel as far south as the network of stations at present extends—that is, to near Cosne ; and this conclusion is justified by a large number of concordant observations.

If the map of the true isogonals is compared with that in which the isogonals are assumed to be regular in form, we can deduce for every station the difference between observation and theory ; and if the points where these quantities have the same value are connected, we obtain a map of the isanomalous curves of the declination.

A zone, corresponding to an excess of declination, extends on this map from the shores of the Channel (Dieppe) as far as the Loire (Cosne), and increases in intensity towards the south. The excess is 14′ at Neufchâtel-en-Bray, 19′ at Nantes, 24′ at Chevreuse, 30′ at Gien, and 36′ at Cosne. The northern extremity of this zone extends towards the east to the neighbourhood of Laon, where the excess is still 7′. Immediately to the west of this zone there is another, almost symmetrical with it, which corresponds, however, to declinations of less than normal value. Like the first, it increases in intensity towards the south. The discordance is −6′ near the mouth of the Seine, −8′ at Evreux, −10′ at Dreux, −13′ at Epernon, −18′ at Orleans. Thus, contrary to all expectation, the declination is less at Orleans than at Montargis and Gien, less at Epernon than at Paris.

The facts are consistent with the view that the north pole of the needle is attracted from both sides towards a line which passes through or near Fécamp, Elbeuf, Ram-

[1] This article is a *résumé* of a communication made by M. Mascart to the British Association at Leeds.

bouillet, and Châteauneuf-sur-Loire, making an angle of from 25° to 30° with the geographical meridian. The maps of the other elements also show effects along this line, viz. an increase in the dip, and a decrease in the horizontal force.

On comparing this anomaly with a geological map, we find that it extends exclusively over calcareous and cretaceous soils. In the region under consideration, the isogonals also present a regular deformation, which is not found over rocks of a kind to produce local effects on the compass. In the latter case the results are discordant, and it is seldom possible to represent them by curves. The totally unexpected phenomenon brought to light by this first series of observations seems due to some more general cause, the nature of which has yet to be determined. Before attempting an explanation, it will, no doubt, be necessary to study it more completely, and to extend the network of stations southwards. The irregularities in the isomagnetic lines which Messrs. Rücker and Thorpe have discovered in the south of England seem to be connected with those which M. Moureaux has observed in France.

On a Possible Connection between the Ridge Lines of Magnetic Disturbance in England and France. By A. W. Rücker.

The possible connection between the French and English surveys, referred to by M. Mascart, can only be appreciated in view of facts which have been brought to light in the magnetic survey of the United Kingdom. It may therefore be desirable that a few lines should be added as to the phenomena observed on this side of the Channel.

The most salient feature in the magnetic constitution of the south of England is the existence of a ridge line (*i.e.* a line towards which the north end of the needle is attracted) the direction of which coincides generally with that of the Palæozoic axis. It runs through South Wales, passes thence to the valley of the Thames, and deviates to the south through Kent.

Near Reading the disturbance reaches a local maximum, and from this point a spur is thrown off, which passes due south to the Channel. In the maps which illustrate the magnetic survey of Dr. Thorpe and myself, it is shown entering the sea near to, but a little to the west of, Selsey Bill.

If the direction of the ridge line described by M. Mascart as passing through Rambouillet, Elbeuf, and Fécamp, be prolonged, it cuts the English coast near Portsmouth.

Putting aside, therefore, all other considerations, we may at the very least conclude that as the directions of the English and French ridge lines intersect in the Channel, they may possibly be connected. There are, however, various arguments which increase the probability of the connection.

We have traced in the United Kingdom two main ridge lines which run through districts covered by sedimentary rocks, and where, therefore, the source of attraction, if due to rocks, must be deep seated. In both cases the general directions of the lines remain unchanged for long distances.

A line, about 150 miles long, drawn from St. Bride's Bay to Kew would fairly represent the direction of the main magnetic ridge in the south.

Two lines, each 75 miles long, drawn from Wainfleet to Market Weighton, and from Market Weighton to Ribblehead, would fairly represent the main features of the ridge line which runs from the Wash to South-east Yorkshire, and thence to Craven. They display, in fact, the same kind of continuity of direction as is shown by many mountain ranges. From Châteauneuf to Fécamp the ridge line follows an easy curve for 150 miles. The three most northerly stations lie nearly on a straight line 100 miles long, and it is quite in accord with what has been observed elsewhere that this general direction should be preserved for another 80 miles across the Channel. There are very similar magnetic indications that the Palæozoic axis extends across the Irish Channel, where it is 60 miles wide.

If this be so the direction of the French ridge alters when it approaches the English coast.

This might be expected. M. Moureaux finds that the magnetic disturbance diminishes as the latitude increases, and the disturbances in South Sussex and Hampshire are at all events not greater than those he finds in the north of France.

The declination disturbances at Neufchâtel-en-Bray and near the mouth of the Seine are + 14′ and − 6′ respectively; at Worthing and Ryde they are + 8′ and − 10′. When the line of the North Downs is passed, the phenomena are complicated by the meeting of the two lines of disturbance which run east and west and north and south respectively. That a locus of centres of attraction of diminishing intensity should be diverted by approach to another well-marked ridge line is *a priori* probable.

If, however, we take a further step, and attempt to seek a physical cause for the facts, it is noticeable that the English and French lines of disturbance meet a little to the east of the Isle of Wight, and almost in the prolongation of the great fault which traverses that island.

On the whole, there appears to be good reason to believe that the ridge line which is thrown off from the Palæozoic axis at Reading crosses the Channel, and is continued for 150 miles and for an unknown distance beyond into the heart of France. The magnitude of the declination disturbance at Cosne (the most southerly point to which it has been followed) is 36′. This is greater than any disturbance hitherto observed in England, and is surpassed only by those obtained in Scotland and Ireland relatively near to basaltic rocks. The largest similar disturbances in England were found at Melton Mowbray and Loughborough, on good ground, but not far from the igneous rocks of Charnwood Forest. M. Moureaux's future investigations will, therefore, be followed with interest on this side of the Channel. Valuable evidence may be gained as to the causes of these disturbances when the clue he is following southwards has been followed to the end.

NOTES.

At the next evening meeting of the Zoological Society, on Tuesday, May 5, a special conference on the fauna of British Central Africa will be held, in order to encourage the new Commissioner, Mr. H. H. Johnston, C.B., and his associates in their exploration of that country. The subject will be introduced by the Secretary, but many other members are expected to take part in the discussion. Mr. Johnston has already left for Nyassaland, but his naturalist, Mr. Alexander Whyte, and his Chief of the staff, Mr. B. L. Sclater, R.E., are expected to be present at the meeting.

An important addition has been made to the Zoological Society's living collection in the shape of a fine adult specimen of the Lesser Orang (*Simia morio*) which has been received as a present from Commander Ernest Rason, R.N. The Lesser Orang was discriminated from the larger and more common form by Sir R. Owen as long ago as 1836, but this is the first example of it that has been acquired alive. It inhabits the swamps at the mouth of the Sarawak River in Borneo, where it is known to the natives as "*Mias Kassar*." An interesting account of its habits was sent home by the late Sir James Brooke in 1836, and was published in the Zoological Society's Proceedings for that year. The Lesser Orang is distinguishable from the larger Orang by its smaller size and by the absence of callosities on the face. The present specimen has been lodged in the Ant-eater's House, next door to the compartment occupied by "Sally," the bald-headed Chimpanzee, which has now lived seven years and six months in the Society's Gardens.

THE annual meeting of the Royal Society of Canada opens at Montreal on Wednesday, May 27, the sessions being held in the buildings of the McGill University. There will be the usual local excursions, receptions, and entertainments, in addition to the more serious work of the week. The Allan Line issues return tickets at £20 to £30, the Dominion Line at £16 to £30, and the Beaver Line at £16 to £18. The Committee are engaged in the preparation of a hand-book for gratuitous circulation among intending visitors, which will include an historical account of the Society, together with other interesting scientific and local information.

THE hand of Fate has been heavy of late upon the secretariat of the Reale Istituto Veneto. The Secretary has been seriously ill, the Vice-Secretary absent from heavy domestic calamity, and now Signor G. Berchet, who has been nominated by the President to undertake the secretarial work *pro tempore*, communicates to the Correspondents of the Istitute the death of Prof. Giovanni Bizio, an eminent member, who was formerly Secretary, and held that office for seventeen years.

THIS evening, a *conversazione* will be given by the Royal Microscopical Society.

THE Council of the City and Guilds of London Institute has issued its Report for the year 1890 to the Governors. It is to the electrical department that the greatest number of students have been attracted both at the Central Institution and at the Technical College, Finsbury. At both Colleges this department is overcrowded, and the Council is able to state that all those who completed their course at the end of last session have obtained employment. Unfortunately, the success of the electrical department has been to some extent achieved at the expense of the chemical department. "The great impetus which has been given of late years to the use or application of electricity," says the Council, "has made this branch of industry particularly attractive to young men in choosing their profession. There is, however, as great a want and as great scope for well-trained technical chemists, and the Council trusts that this important department may receive a larger share of new students in the future."

MR. G. J. ROMANES, F.R.S., is one of three gentlemen who were elected last week by the Committee of the Athenæum Club, in accordance with the rule which empowers the annual election by the Committee of nine persons of distinguished eminence in science, literature, or the arts, or for public services.

A REPORT from New York says that Lieutenant Robert Peary, of the United States Navy, who has obtained 18 months' leave, is making preparations for an Arctic expedition to start about the end of May. It is being sent by the Academy of Natural Sciences, and the duty of making observations will be confided to six persons. The explorers will start from St. John's Land, on Whale Sound, and scale the glaciers near the coast, up to a high altitude where the hard snowy plains will enable them to observe the shore formation until the spring, when they will start for the North Pole.

THE Geologists' Association has made arrangements for three geological excursions—which, if the weather is favourable, ought to be very pleasant—in the month of May. The first is an excursion to Guildford, and will take place on May 2. The next will last from May 16 to 19, when the members will have an opportunity of exploring the neighbourhood of Northampton. On May 30 there will be an excursion to West Surrey.

A COURSE of six lectures on photography is to be delivered at the Central Institution, Exhibition Road, by Mr. H. Chapman Jones. The lectures will be given on Wednesday evenings at 7.30, and will be begun on May 6. The subject will be treated both from a practical and from a scientific point of view.

A COURSE of six lectures (in connection with the London Society for the Extension of University Teaching) will be delivered in the lecture-room in the Gardens of the Zoological Society, Regent's Park, on Thursdays, at 5 p.m., beginning Thursday, May 23, by Mr. F. E. Beddard, Prosector to the Society and Davis Lecturer. The subject will be "The Animals living in the Zoological Society's Gardens." The lectures will be illustrated by diagrams, as well as by specimens.

ACCORDING to *Modern Light and Heat*, of Boston, something like an exodus of electricians from the United States to Europe is now going on. Many representatives of the leading American electrical companies are here already, and "many more are to follow." "It takes Americans," says our contemporary, "to stir up business of an electrical character in Europe, as is evidenced by the big shipments now being made from this side."

IN the current number of *Cosmos* there is a good reproduction of the interesting old portrait of Columbus, recently found at Como.

THE Rugby School Natural History Society has issued its report for 1890. The number of members and associates is now larger than it ever was, and the work during the year seems to have been generally most satisfactory. The editors complain, however, that there has been a lack of interest in geology. "In the early years of the Society," they say, "this section was the most popular of all; but later it led a lingering existence, and for some years has been practically extinct. We have a valuable geological collection, accumulated in the neighbourhood; and with the increased numbers on our lists we feel justified in calling on volunteers to continue this work of our predecessors."

THE new Quarterly Statement of the Palestine Exploration Fund contains an interesting paper, by the Rev. George E. Post, on land tenure and agriculture in Syria and Palestine, and on the physical, mental, and moral characteristics of the people. In the same number, besides various other contributions, there is a valuable comparison, by Mr. James Glaisher, F.R.S., of the highest and lowest temperatures of the air, and range of temperature, in Palestine and in England in the ten years ending 1889.

MM. GAUTHIER-VILLARS ET FILS have just issued "Les Théories Modernes de l'Électricité," a translation, by M. E. Meylan, of Prof. Oliver Lodge's well-known "Modern Views on Electricity." The same publishers, with M. Léon de Thier, Liége, issue a second edition of "Leçons sur l'Électricité," by M. Eric Gerard, the first edition of which was reviewed in NATURE. A second edition of M. J. Joubert's "Traité Élémentaire d'Électricité" has been issued by M. G. Masson, Paris.

TWO Annual Reports of the Board of Regents of the Smithsonian Institution have lately been issued. One is for 1887–88, the other for 1888–89—counting the year from July to July. In each case Dr. Langley, the Secretary, is able to give a very satisfactory account of work done—work of which any institution might be proud. After the Report, each volume includes, as usual, a number of valuable scientific papers. In the volume for 1888 there are summaries of progress in various branches of science, a series of miscellaneous papers, and several biographical memoirs. In the volume for 1889 the best papers are reprints of writings by British and German men of science.

The Report of the U.S. National Museum, under the direction of the Smithsonian Institution, for the year ending June 30, 1888, has also been issued. It is presented in a massive volume, which takes in, besides the report of the assistant secretary in charge of the Museum, reports of the curators, papers describing and illustrating the collections, a bibliography of the institution during the year, and a list of accessions. The papers relating to the collections belong chiefly to the department of anthropology, and are of more than usual interest. They are carefully illustrated. In one of them Ensign A. P. Niblack, of the U.S. Navy, gives an elaborate account of the Coast Indians of Southern Alaska and Northern British Columbia. Mr. Walter Hough deals with the fire-making apparatus in the Museum, and Mr. P. L. Jouy with Corean mortuary pottery. Mr. Thomas Wilson contributes a study of prehistoric anthropology, and an essay on ancient Indian matting. He also presents the results of an inquiry as to the existence of man in North America during the Palæolithic period of the Stone Age.

Among the contents of the current number of the Journal of the Straits Branch of the Royal Asiatic Society, is a paper on the Sphingidæ, or hawk moths, of Singapore, by Lieut. H. L. Kelsall, R.A. Mr. H. N. Ridley contributes papers on the Burmanniaceæ of the Malay Peninsula; on the so-called tiger's milk, "Susu Rimau," of the Malays; and on the habits of the red ant commonly called the Caringa. These ants, although very ferocious, are remarkably intelligent; and Mr. Ridley gives a striking account of the way in which they make leaf nests. They have also great courage, and do not scruple to attack any insect, however large. Mr. Ridley once saw a fight between an army of Caringas, who tenanted the upper part of a fig-tree, and an advancing crowd of a much larger kind of black ants. The field of battle was a horizontal bough about 5 feet from the ground. The Caringas, standing alert on their tall legs, were arranged in masses awaiting the onset of the enemy. The black ants charged singly at any isolated Caringa, and tried to bite it in two with their powerful jaws. If the attack was successful, the Caringa was borne off to the nest at the foot of the tree. The red ant, on the other hand, attempted always to seize the black ant and hold on to it, so that its formic acid might take effect in the body of its enemy. If it got a hold on the black ant, the latter soon succumbed, and was borne off to the nest in the top of the tree. Eventually the Caringas retreated to their nest. The last to go had lost one leg and the abdomen in the fight; nevertheless, Mr. Ridley saw it alone charge and repulse three black ants one after the other, before it left the field.

The Meteorological Council have just published an atlas of cyclone tracks in the South Indian Ocean, from information collected by Dr. Meldrum, of Mauritius, during a period of thirty-eight years, from 1848 to 1885 inclusive, with the exception of three years for which no reports of cyclones were received. The tracks are represented in two sets of charts, one set showing the distribution in each year, and the other grouping the storms according to months, excepting for August and September, in which months no cyclones were recorded. In dealing with these cyclones, Dr. Meldrum has divided them into progressive and stationary. It is admitted, however, that some of the latter may have moved, but that their progress may not have been detected from lack of observations. The relative frequency of both classes of storms for the whole period is very small, varying from 1 in 18 years for July, to 5 in 3 years during February and March; but, although the number of storms is so small, it does not appear likely that many have been missed, considering the untiring persistence with which Dr. Meldrum has pursued his investigations. The tracks of the several cyclones will afford much valuable information, and lead to a better knowledge of the latitude in which the recurvature of the storms in that ocean takes place. A cursory examination shows that the range of latitude over which the points of recurvature extend varies considerably, being from about 15° to 25° S.

The Meteorological Department of the Government of India has published Part 3 of "Cyclone Memoirs," containing an elaborate discussion of the two most important storms in the Bay of Bengal during the year 1888—viz. those of September 13–20 and of October 27–31—and also of the cyclone in the Arabian Sea of November 6–9, 1888, accompanied by tables of observations during and before the storms and by 29 plates. The following is a very brief *résumé* of some of the more important conclusions arrived at by Mr. Eliot with regard to these storms, and with regard to cyclones generally in India: (1) that the difference of intensity in different quadrants is chiefly due to the fact that the humid winds which keep up the circulation enter mainly in one quadrant; (2) that the ascensional movement is usually most vigorous in the advancing quadrant, a little distance in front of the centre; (3) in consequence of this, and of rainfall taking place most vigorously in front of the cyclone, the isobars are oval in form, and the longest diameter coincides approximately with the direction of the path of the centre; this is not in the middle of the diameter, but at some distance behind; (4) that the cyclonic circulation cannot be resolved into the translation of a rotating disk or mass of air, and that its motion is somewhat analogous to the transmission of a wave; (5) that the direction of advance of these storms is mainly determined by rainfall distribution, and there is a marked tendency for storms to form in and run along the south-west monsoon trough of low pressure; (6) the lie of this trough depends upon the relative strengths and extension of the two currents.

At the Iowa Experiment Station great efforts were made in the spring of last year to find out the best way of preventing the striped squirrels from taking corn. According to Mr. C. P. Gillette, some notes by whom are printed in *Insect Life* (U.S. Department of Agriculture), the corn was treated in the following ways:—Smoked with meat in an ordinary smoke-house until the kernels were black; smoked in a barrel with tobacco dust; smoked over night in strong decoctions of tobacco and of quassia chips; soaked in a dilute carbolic acid mixture, in strong alum water, in salt water, and in kerosene. The squirrels would take the corn treated in any of these ways, though the carbolic acid treatment and the smoking with tobacco made the corn distasteful, and when in the vicinity of other grain would be left till the last. The best remedy seems to be to harrow the ground immediately after planting to cover the planter tracks, and then to scatter corn about the border of the fields and in the vicinity of the squirrel holes as soon as the corn begins to come up.

We have received the first number of the *London and Middlesex Note-book* (Elliot Stock). It is edited by Mr. W. P. W. Phillimore, and will be published quarterly. The periodical is likely to be very welcome to all students of the local history and antiquities of the cities of London and Westminster and the county of Middlesex.

During May, the lectures at the Royal Victoria Hall will be as follows:—5th, Dr. W. D. Halliburton, skin and bones; 12th, Mr. F. H. Blandford, silk and silkworms; 19th, Rev. J. Freeston, Galileo; 26th, Mr. W. North, the physiology of a dinner.

A paper upon the salts of the sub-oxide of silver is contributed by M. Güntz to the current number of the *Comptes rendus*. The question of the existence of these salts has been much discussed of late years, but very little trustworthy experimental evidence has been hitherto forthcoming. M. Güntz now claims to have prepared the sub-fluoride Ag_2F, sub-chloride Ag_2Cl, sub-iodide Ag_2I, and sub-sulphide Ag_4S. The sub-fluoride, Ag_2F, was described by M. Güntz about a year ago. It was obtained in the form of a crystalline powder, resembling bronze filings in appearance, by the electrolysis of a saturated solution of silver fluoride by means of a very strong electric current. These crystals of the sub-fluoride are unalterable in dry air, but are more or less rapidly decomposed by moisture. Water itself effects the decomposition at once with evolution of heat, one equivalent of silver being precipitated, and one molecule of ordinary silver fluoride passing into solution. This well-defined crystalline salt serves for the preparation of the others. When dry hydrochloric acid gas is led over the sub-fluoride the latter rapidly changes, becoming coloured a deep violet tint, and gradually becoming converted into the sub-chloride, Ag_2Cl. The vapours of the chlorides of carbon, silicon, and phosphorus act very much better than hydrochloric acid, fluorides of carbon, silicon, or phosphorus, and comparatively pure sub-chloride of silver being produced. Similarly, by leading a current of gaseous hydriodic acid over the sub-fluoride, the sub-iodide, Ag_2I, is obtained, the reaction in this case being accompanied by a large rise of temperature. The sub-sulphide, Ag_4S, is also obtained in like manner by passing sulphuretted hydrogen over the sub-fluoride. It is of considerable interest, moreover, that if the sulphuretted hydrogen is replaced by water vapour, and the tube containing the sub-fluoride is heated to 160°, the sub-oxide itself, Ag_4O, is obtained as the product of the reaction. As regards the initial preparation of the sub-fluoride, it is somewhat important to note that if a weak current is employed, and the temperature of the saturated solution of silver fluoride prevented from rising, only metallic silver is deposited at the negative pole. But if, on the contrary, a very strong current is employed, such as will cause considerable heating effect, the bronze-like crystals of the sub-fluoride make their appearance. M. Güntz announces that he now proposes to compare his sub-salts thus prepared with the products of the action of light upon the normal salts, a comparison which can scarcely fail to lead to results of interest from a photographic standpoint.

THE additions to the Zoological Society's Gardens during the past week include a Sooty Mangabey (*Cercocebus fuliginosus* ♂) from West Africa, presented by Mr. F. J. Bennett; a Brown Howler (*Mycetes fuscus*) from Brazil, presented by Mr. E. Luxmore Marshall; an Azara's Agouti (*Dasyprocta azaræ*) from Brazil, presented by Lord Hebrand Russell, F.Z.S.; two Wild Swine (*Sus scrofa*) from Spain, presented by Mr. Alex. Williams; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, a Rock-hopper Penguin (*Eudyptes chrysocome*) from the Falkland Islands, presented by Mr. H. B. Bingham; two Ring-necked Parrakeets (*Palæornis torquatus*) from India, presented by Miss E. Ogilvie; a Common Barn Owl (*Strix flammea*), British, presented by Mr. H. Bendelack Hewetson; a Common Fox (*Canis vulpes*), two White Pelicans (*Pelecanus onocrotalus*) from Southern Europe, deposited; an Egyptian Cat (*Felis chaus*) from North Africa, a Nankeen Night Heron (*Nycticorax caledonicus*) from Australia, a Great-billed Tern (*Phaethusa magnirostris*) from South America, purchased.

OUR PRESENT KNOWLEDGE OF THE HIMALAYAS.

THIS was the subject of an able paper read at Monday's meeting of the Royal Geographical Society, by Colonel H. C. B. Tanner (Indian Staff Corps), who for many years has been one of the officers of the Indian Survey, most of his time having been spent in various parts of the Himalayas from north-west to south-east. The paper was illustrated by a large number of admirable drawings by the author, which afforded an excellent idea of the physical and picturesque aspects of this great mountain system.

With regard to avalanches, Colonel Tanner stated that they play a great part in the conformation of the topography—a greater part, indeed, than is generally supposed, and this factor has not received the attention it deserves at the hands of geologists.

"I became acquainted," he said, "with four distinct kinds of avalanche, which, perhaps, are called by distinctive names by mountaineers, though I have been unable to ascertain them. The first, and the most common, is the precipitation of a mass of new snow from slopes which, from their steepness, are unable to retain more than a limited quantity of snow on them. They occur generally in winter and in early spring, and are the cause of the results just described. The second kind of avalanche is a descent of *old* snow, which is loosened by the heat of the sun. They may be heard throughout the summer and autumn, and are dangerous from the unexpected and irregular manner in which they slide off. The sportsman and traveller should guard against them by intelligently placing his camp in some sheltered spot out of their reach. This class is not usually of any great extent or weight, but such avalanches are of constant occurrence. The third kind can only be seen when the mountains are of peculiar formation or structure, and are really ice and not snow avalanches. They are of very constant occurrence in some localities, more particularly where small glaciers are situated high up on the crest of mountains, and are gradually pushed over the edge. In Lahaul, in the company of a friend, we watched the face of the well-known Gondla cliffs from the right bank of the Chandra River, and saw a number of these ice-falls, which came down every few minutes, filling the air with the noise of the loosened rocks and ice-blocks. The fourth kind of avalanche is one that I have only once seen, and have never known described. It is very curious, being the movements of billions of snowballs, which, in a stream a mile or half a mile long, I saw slowly wind down the upper part of an elevated valley in the Gilgit-Darel mountains. I was after ibex at the time of the occurrence, and was watching a herd of these animals, when I became aware of a low but distinct and unusual sound, produced by a great snake-like mass of snow winding down one of the valleys in my front. It occasionally stopped for a moment, and then proceeded again, and finally came to rest below me. I found this curious movement of snow was produced by countless numbers of snowballs, about the size of one's head, rolling over and over each other. The torrent-bed was full of them, an accumulation formed by numerous similar freaks of nature. I am quite unable to account for such an avalanche as the one now described. How does it originate, or by what process is the snow rolled up into these innumerable balls?"

Colonel Tanner made some interesting remarks on the subject of the line of perpetual snow. "Various authorities," he stated, "lay down such a line with great assurance, but for myself I find that circumstances of position, of climate, and of latitude, play so great a part in the position of this line that I am unable to define it even approximately. No sooner in one locality, or during one particular season, have I settled, to my own satisfaction, the line of perpetual snow, than I presently have been obliged completely to modify my views on the subject. On p. 154 of the 'English Cyclopædia,' vol. v., I read that snow lies 4000 feet higher in the northern than in the southern side of the Himalayas. On p. 281, vol. x., of the same work, it is stated that the snow-line on the northern slope is at 19,000 feet, which I should have been inclined to say is 1500 or 2000 feet too high. In Gilgit, during the end of summer, I found masses and fields of snow at 17,200 feet, and they extended down the northern slope certainly 2000 feet or even more below that altitude. In Kulu, which has many degrees of latitude less than that of Gilgit, avalanche snow lies in valleys above 8000 feet throughout the year after a good winter snowfall, but during the past spring, following a very mild winter, I found no snow at all at 8000 feet. There had been no avalanches, and even in June, at 14,000 feet, snow lay only in patches. I think that, in determining the snow-line with greater precision than has been done hitherto, scientific men should ascertain those altitudes on which perpetual snow lies on flat places in the position where it first falls, and should neglect the occurrence of a snow-field where it may have been protected from the sun's rays by its occurrence on the north face of a mountain. From memory I can state that there are a considerable number of typical localities which would help out such an inquiry. There is a peak (without name) about thirty miles north of Gilgit, with rounded summit, which, though only 17,500 feet high, is covered with a cap of perpetual snow."

Speaking of the Himalayan glaciers, Colonel Tanner stated that the most extensive and the most picturesque he has seen are in the Sat valley, which drains the southern face of Rakaposhi mountain in Gilgit. Three great glaciers come down into this valley, and dispute with the hardy mountaineers for the possession of the scanty area of the soil. Here may be seen forests, fields, orchards, and inhabited houses all scattered about near the ice heaps. The only passable route to the upper villages in this valley crosses the nose of the greatest of the three glaciers, and threads its way over its frozen surface. This glacier is cut up into fantastic needles of pure green ice, some of which bear on their summits immense boulders. About half a mile from its lower end or nose, Colonel Tanner found an island bearing trees and bushes, and at one place above this a very considerable tarn of deep blue-green water. The glacier had two moraines parallel with each other, and both bearing pine-trees; and from the highest point Colonel Tanner reached he fancied he saw the ice emerging from the *névé* at its source, far away up the slopes of Rakaposhi. In this glacier the pinnacles, wedges, blocks, and needles of ice were of the most extraordinary appearance, and the whole formed a weird and impressive view which he can never forget. Though the largest glacier Colonel Tanner has ever approached, it is very small indeed when compared with those described by Colonel Godwin-Austen in a locality not very far from the Sat valley. Insignificant though it is, it was more than Colonel Tanner could take in during his visit of two days' duration. It struck him at the time of his inspection that the peculiar stratified appearance of the ice needles, which in the case of the Sat glacier is very strongly marked, must have been caused by the different falls of avalanche snow on the bed of *névé* at the source of the glacier.

The lowest glacier Colonel Tanner has seen in the Himalayas is one that reaches the foot of the range near Chaprot Fort in lat. 35½°, in Gilgit. It is formed of beautiful clear ice and has no dirt. In Kulu and Labaul, lat. 32°, glaciers do not come down below 12,000 or 13,000 feet, and all are very dirty; and in Sikkim, lat. 28° or 29°, without having visited the glacier region himself, Colonel Tanner would say that the lowest limit reached by the Kinchinjanga group must be considerably higher, perhaps by 2000 feet or even more. The smallest mountain he has ever met with capable of giving rise to a glacier, is one on the Gilgit-Dareyl range, whose height is 17,200 feet, and in this case the mass of ice formed is of very inconsiderable size. Of the glaciers round Mount Everest and its great neighbours we know next to nothing, and the little we have learnt is derived from the itineraries of native explorers, who, of all classes of travellers, seem the least capable of furnishing trustworthy information regarding any subject lying at all outside their actual angular and distance measurements. But with his telescope, when employed on the survey of the Nipal boundary, Colonel Tanner has gazed long and earnestly at the icy region at the foot of Everest, and Peak No. XIII., where the glaciers extend over a very large area.

With regard to our actual knowledge of the Himalayas, Colonel Tanner thinks that perhaps our botanical knowledge is far ahead of other branches of science. Many eminent botanists have been at work for a long time past, and of late Dr. Duthie has been allowed to travel on duty into tracts not before visited by anyone possessing the requisite knowledge. It is likely that Dr. Duthie's museum at Saharunpur will, within a moderately short time, become an almost complete depository of the chief vegetable products of the Himalayas. The geologists, Messrs. Blandford, Edwin Austen, Richard Strachey, Stolitzka, and Lydekker, have been pretty well over those tracts open to Europeans, and are now well acquainted with all the leading features of their branch of science presented by the mountains of Kashmir, Kumaon, Kangra, and Sikkim. Ornithology has found many votaries, and the birds of these mountains are now probably all or nearly all known, though the late Captain Harman, only a few years back, discovered a new and handsome pheasant in the extreme eastern end, either of Bhutan or Tibet. The mammals, Colonel Tanner supposes, are all known, though one, at least, the Shao, or great stag of Tibet, has not yet even been seen by any European, and the famous *Ovis poli* has been shot by not more than two or three sportsmen.

With regard to the work of the Survey, Colonel Tanner stated that the maps of Kashmir and Gilgit, without being free from error, are of the greatest use to a large class of officials. Incomplete though they may be, they were not brought up to their present state without taxing to the utmost the endurance of a hardy set of men. Adjoining Kashmir to the eastwards comes Kangra, with its subdivisions of Kulu, Lahaul, and Spiti. Kangra had once been roughly surveyed prior to the arrival there of Colonel Tanner's party, who are now at work on a very elaborate contoured map, which will take a long time to complete, owing to the intricacy of the detail demanded. Between Kangra and Kumaon occur various native States whose territories are being surveyed on the scale of 2 inches to 1 mile, also contoured work, resulting in very elaborate and trustworthy, though somewhat expensive, maps. Eastward of Kumaon, Nipal stretches along our border for some 500 miles till Sikkim is reached, and eastward again of Sikkim comes Bhutan, and various little-known semi-independent states which lie on the right bank of the Sanpo River. Nipal marches with the Kumaon border for many miles, and advantage was taken of the existence of the trigonometrical stations on the Kumaon hills to extend our knowledge of the adjacent topography of Nipal, and this was done about four years ago with some little result. The more prominent peaks in Nipal within a distance of about 100 miles were fixed trigonometrically, and some slight topographical sketching was done. From the trigonometrical stations near the foot of the lower hills, both in the North-West Provinces and in Bengal, trigonometrical points have lately been fixed, and some distant sketching done in Nipal, for 500 miles between Kumaon on the western and Sikkim on the eastern extremity of this kingdom; and, again, from the trigonometrical hill stations along the western boundary of Sikkim more points and hazy topography of Nipal was secured. This very meagre topography, sketched from very great distances, comprises all the geography of Nipal other than the sparse work collected by Colonel Montgomerie's explorers, or by explorers trained to his system who have worked since his death. All the existing data, whether trigonometrical, distant sketching, or native explorers' routes, are now being combined, as far as the often conflicting and contradictory materials admit. The resulting map of the country, though at most little better than none, is all we have to expect until some of the strictures on travelling in Nipal are lessened by the Nipal Government.

The whole of the Nipalese border, which marches with British territory for some 800 miles, is jealously guarded, and no European is allowed to cross it, except when the Resident of Kashmir or his own personal friends are permitted to proceed by a certain and particular route, between the military station of Segowli and Katmandu. Sikkim flanks the eastern boundary of Nipal, and the, until lately, indefinite western boundary of Shutan. British Sikkim is a small tract, which has twice been surveyed on suitably large scales. Independent Sikkim, which contains Kinchinjangee, one of the highest mountains, and some famous passes—the Donkhya, visited by Sir Joseph Hooker and a few others; and the Jelap, where our forces, under General Graham, have lately been employed, was surveyed in reconnaissance style by Mr. Robert, an energetic and hardy assistant of the Survey of India Department. The sketch map obtained by this gentleman is complete, and similar in character to that of Gilgit by Colonel Tanner, and to that of Nari Khorsam and Hundes by Mr. Ryall. It does not pretend to any exhaustive detail.

Our knowledge of Bhutan, or, rather, our ignorance of it, is about on a par with that of Nipal, but in Bhutan we have the valuable information left by Captain Pemberton, who forty-three years ago traversed the greater portion of the country from west to east. Besides Pemberton's work, Colonel Godwin-Austen, while he accompanied Sir Ashley Eden's mission to the court of the Deb Raja in the year 1863, executed a route survey in Western Bhutan. The engineer officers who were attached to the military force at Pewangiri also did some little topographical sketching, and beyond this we have distant sketching and trigonometrical work, as in Nipal, which, also, has yet to be combined with the route surveys of native explorers, some rather recent, and some of greater date. The difficulties which are presented to further researches in the direction of Bhutan geography seem unlikely to diminish. Our knowledge, then, of Bhutan is as unsatisfactory as that of Nipal. Eastward of Bhutan occur those numerous semi-independent hill States who sometimes, when necessity presses, own allegiance to Tibet, and at others assert their complete freedom from control. Colonel

Tanner himself has sent in two maps of this region derived from native sources, and both upset maps previously accepted, and it is highly improbable that we have any but the most rudimentary and vague knowledge of the course of the Sanpo below Gyala Sindong, and not even that of the course or limits drained by the Dibong. Colonel Tanner then referred in some detail to the great rivers that have their sources in the Himalayas, and concluded by giving some advice to tourists as to the best routes to take.

SCIENTIFIC SERIALS.

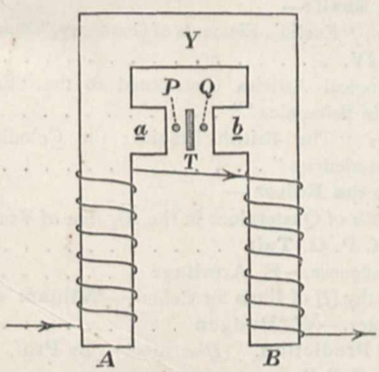
THE numbers of the *Journal of Botany* for February, March, and April, do not contain many articles of interest to those not concerned with systematic botany. Miss Barton has an interesting note on the occurrence of structures which have the appearance of galls on a common red seaweed, *Rhodymenia palmata*. Very few examples of this phenomenon have hitherto been described in connection with Algæ. Messrs. Britten and Boulger's biographical index of British and Irish (*sic*) botanists is now nearly completed, and we are glad to see that the editor of the *Journal* has not yet abandoned the idea of reprinting it in a separate form.

IN the *Botanical Gazette* for February, Dr. D. H. Campbell continues his useful series of papers on the histology of the higher Cryptogams, with a note on the apical growth of *Osmunda* and *Botrychium*, in which he shows the large range of variation on this point within the Osmundaceæ.—In the March number, Mr. G. F. Atkinson discourses on the black rust of cotton, a disease due to the attacks of several parasitic fungi, of which the most destructive is *Cercospora Gossypina*.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, April 17.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—The following communications were made:—On a property of magnetic shunts, by Prof. S. P. Thompson. After referring to a few instances in which magnetic shunts are employed, as, for example, the ordinary magnetic medical coil, and Trotter's constant current dynamo, he said that the particular property he wished to speak of was the time taken for such a shunt to lose its magnetism, as compared with the other branch of the magnetic circuit. If these times were very different, unexpected results might be produced, and these might be regarded as being due to a kind of magnetic time-constants. Short pieces, as was well known, demagnetize much more rapidly than long ones, particularly if the latter



form, or be part of, a closed magnetic circuit. Hence, in alternators, such as Kingdon's keeper dynamo, in which both magnets and armature are stationary, it was important that the revolving keepers should be short. The most important application of a magnetic shunt occurred, he said, in D'Arlincourt's relay, described in vol. iv. of the Journal of the Society of Telegraph Engineers and Electricians, and shown diagrammatically in the figure. In this relay the polarized tongue T plays between two projections, *a* and *b*, near the yoke Y, and it is claimed to have a quicker action than ordinary kinds. The reason of this the author explained as follows. When a current flows through the coils, the greater part of the magnetic lines pass through the yoke, but a few leak across from *a* to *b*, and move the tongue against the contact P. On stopping the current, the magnetism in the extremities A and B dies away much more rapidly than in the yoke; consequently, the direction of the field between *a* and *b* is reversed, and the tongue T thrown back against the stop Q. Prof. Perry asked if any experiments had been made to test whether the throwing back actually occurred. He also inquired whether such action would be augmented, or otherwise, by having a thick copper tube round the yoke, or by laminating the iron. Mr. Blakesley asked if placing a yoke across A B would not improve the action. The President said he would be glad to know whether the relay was any more sensitive than an ordinary one as regards the ampere-turns or the watts required to actuate the instrument. In India, he remembered that they used inductive coils shunting the ordinary relays, in order to expedite the action and to avoid confused signals arising from the electrostatic capacity of long lines. In reply, Prof. Thompson said he had tried an experiment on a horseshoe electromagnet, and found evidence of throwing back when working near the bend or yoke. Putting a yoke across A B would, he thought, tend to neutralize the effect desired.—An alternating current influence machine, by James Wimshurst. This machine consists of a varnished glass disk, with or without metallic sectors, mounted on an axis, and rotating within a square wooden frame fixed in the plane of the disk. The frame carries four square glass plates, each of which has one corner cut away so as to clear the boss of the disk. These plates are placed one at each corner of the frame, alternately on the two sides, and the disk revolves between them. There are thus two plates on one side of the disk situated at opposite ends of a diagonal of the frame, and two on the other side of the disk, at opposite ends of the other diagonal. Tin-foil sectors fixed to the outer sides of the plates act as inductors, and wire brushes connected with them touch the disk about 90° behind the centre of the inductors. The peculiarity of the machine is that, although sparks can be readily obtained from it, a Leyden jar cannot be charged by bringing it to one of the terminals. From this the author concluded that the electricity produced was alternately of positive and negative sign, and this he showed to be the case by means of an electroscope. The alternations, he said, occurred about every three-quarters of a revolution, the suspended paper disks which he used as an electroscope remaining apart for that period and collapsing during the next quarter of a turn. Using disks with various numbers and sizes of sectors, the author finds that the smaller the sectors and the fewer the number the greater the quantity of electricity produced. Plain varnished glass is the best in this respect. Such a disk, however, does not excite itself quite so freely as one having numerous metallic sectors. By removing two of the inductors and placing an insulating rod carrying collecting brushes at its ends, across a diameter of the disk, the machine was used to produce direct currents. Numerous disks and various shaped inductors accompanied the machine, by means of which a Holtz, Voss, or ordinary influence machine could be imitated. Prof. S. P. Thompson congratulated the author on the most interesting and puzzling machine he had brought before the Society. He inquired if the machine would work if the direction of rotation was reversed, or if two of the inductors were removed, and also whether all the four inductors are electrically of the same sign at the same instant. In reply, Mr. Wimshurst said the machine would not excite if the direction of rotation be changed without also changing the direction of the brush arm, but it would work as a direct current machine when two inductors were removed.—On erecting prisms for the optical lantern, and on a new form of erecting prism made by Mr. Ahrens, by Prof. S. P. Thompson. The ordinary form of erecting prism, viz. a right-angled isosceles one, was, the author pointed out, open to the objection that the top halves of the faces inclosing the right angle were nearly useless, for only the light which, after the first refraction is totally reflected by the hypotenuse face, can be utilized. The fraction of the side which is useful varies with the refractive index, being 0·46 when $\mu = 1·5$, and 0·525 when $\mu = 1·65$. To increase these proportions, prisms with angles of 105° and 126° have been used by Wright and others, and in some cases the prisms have been truncated. With such large angles, much light is lost by reflection. Bertin employed two truncated right-angled prisms placed base to base with an air-film between them. Nachet has also made erecting prisms for microscopes in which internal reflection occurs from faces inclined at an angle of 81° to each other. This form of prism suggested to Mr. Ahrens the new form now shown, which may be described as a long right-angled prism whose ends are cut off so as to be parallel to each other and inclined at 45° to the hypotenuse face. The longitudinal acute angles not being required, are truncated. Light falling parallel to the axis, on one end of the prism is refracted, and after internal reflections, emerges parallel, but perverted. It is claimed that this form of prism gives, weight for weight, a larger angular field than any previously made. The performance of the new form of prism, and also of the ordinary forms, was tested before the Society.—At the close of the meeting, Dr. Atkinson, who had taken the chair, announced that the next meeting would be held at Cambridge on May 9, instead of May 8, as previously intended.

PARIS.

Academy of Sciences, April 20.—M. Duchartre in the chair.—On some calorimetric data, by M. Berthelot. (1) Aspartic acid and its mixed function. This acid is a bibasic acid in which the heat of neutralization for the first addition of a molecule of NaOH is about four times that for the second; this unequal heat liberation is connected up by the author with the presence in the molecule of the (NH_2) group. (2) Malonic chloride. (3) The formation of isomeric soluble and insoluble tartrates.—On the crystalline form and optical properties of M. Engel's new variety of sulphur, by M. C. Friedel.—An excursion from the Arago Laboratory to Rosas, Spain, by M. de Lacaze-Duthiers. An account is given of a zoological excursion into Spain taken by the pupils of the Arago Laboratory during the Easter holidays.—On the endothelium of the peritoneum, and some modifications which it undergoes during experimental inflammation; how we must understand the recovery of wounds by immediate healing, by M. L. Ranvier.—New nebulæ discovered at Paris Observatory, by M. G. Bigourdan. The list now given is in continuation of previous ones, and includes thirty-five newly-discovered objects between 16 hours and 24 hours of right ascension.—On the deformation of spiral surfaces, by M. L. Raffy.—On the theory of light, by M. C. Raveau.—Dissociation of amylene hydrobromide under reduced pressures, by M. Georges Lemoine. The conclusion is drawn that in the case of this body "dissociation is facilitated by a diminution of pressure." The curves given show the relationship between dissociation at $\frac{1}{10}$ atmosphere and that at the ordinary atmospheric pressure.—On the preparation and the reaction of ammoniacal chlorides of mercury, by M. G. André.—On the sub-salts of silver, by M. Güntz. Starting from the perfectly definite and crystallized sub-fluoride, Ag_2F, previously prepared by the author, the salts Ag_2Cl, Ag_2I, Ag_4S, have now been obtained, and their composition established by the analyses given.—On the sulphide of boron, by M. Paul Sabatier. The heat of formation of this body (prepared by the method of Sainte-Claire Deville and Wöhler) is found to be 82·6 calories; less than that of the oxide or chloride.—On boron hydride, by M. Paul Sabatier.—On two new forms of sulphur, by M. Engel.—A strong solution of sodium thiosulphate is decomposed by strong HCl; the filtrate on standing becomes yellow; extracted with $CHCl_3$ and the exract evaporated, orange-yellow crystals of the rhombohedral ystem are obtained. A variety of sulphur soluble in water is also described.—Action of urea on sulphanilic acid, by M. J. Ville. The following equation sums up the results obtained—

$$C_6H_4\begin{cases}NH_2\\SO_3H\end{cases} + CO\begin{cases}NH_2\\NH_2\end{cases} = NH_3 + C_6H_4\begin{cases}NH—CONH_2\\SO_3H\end{cases}.$$

—New combinations of metallic sulphites with the aromatic amines, by M. G. Denigès.—Estimation of acetone in methylated alcohols, by M. Léo Vignon.—On the purification of industrial waters and sewage, by MM. A. and P. Buisine. The authors propose the use of ferric sulphate as a purifying agent.—Contribution to the history of fecundation, by M. Hermann Fol. The changes which two corpuscles of the ovule undergo after fecundation are described.—On the gustatory organs of the sea-devil (*L. piscatorius*), by M. Frédéric Guitel.—On the innervation of the proboscis of Glycera or Rhynchobolus, by M. E. Jourdan.—On an artificial melanine, by M. Georges Pouchet. The author has prepared a body which has the general properies of the black matter in the blood known as melanine.—New researches on olfactometry, by M. Charles Henry.—On the assimilation of lichens, by M. Henri Jumelle. Numerous experiments lead the author to the conclusion that when certain favourable conditions of light, humidity, and season are realized, all lichens are capable of decomposing the carbon dioxide in the air very energetically, notwithstanding their respiration of carbon dioxide. Lichens thus increase in carbon. It is also shown that, *cæteris paribus*, direct sunlight is preferable to diffused light.—Influence of salinity on the quantity of starch contained in the vegetable organs of *Lepidium sativum*, by M. Pierre Lesage. The experiments indicate that when plants are watered with solutions containing from 12 to 15 grams of salt per litre the starch disappears completely. The diminution of starch is not proportional to the increase of salinity.—On some rye possessing peculiar poisonous properties, by M. Prillieux.—On the discovery of a spring at the bottom of the Annecy Lake, by MM. A. Delebecque and L. Legay.—On the soundings executed in the Pas-de-Calais in 1890, by M. J. Renaud.—On the metamorphic rocks of the Savoy Alps, by M. P. Termier.—Contribution to the study of the culture of colza, by MM. E. Louise and E. Picard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

An Introduction to the Study of Mammals: W. H. Flower and R. Lydekker (Black).—Forty Years in a Moorland Parish: Rev. J. C. Atkinson (Macmillan).—Elementary Text-book of Botany: E. Aitken (Longmans).—Proceedings of the Seventh Annual Convention of the Association of Official Agricultural Chemists (Washington).—The Missouri Botanical Garden (St. Louis).—Die Organisation der Turbellaria Acocla: Dr. L. von Graff (Leipzig, Engelmann).—A History of Chemistry: Dr. E. von Meyer: translated (Macmillan).—The Best Books: W. S. Sonnenschein; 2nd Edition (Sonnenschein.—General Report on the Operations of the Survey of India Department, 1888–89 (Calcutta).—Grundzüge der Geologie und Physikalischen Geographie von Nord-Syrien: Dr. M. Blanckenhorn (Berlin, Friedländer).—Ueber den Schädel eines fossilen dipnoërs Ceradotus Sturii, nov. sp.: F. Teller (Wien, Hölder).—Coal, and what we get from it: R. Meldola (S.P.C.K.)—Eighteen Years of University Extension: R. D. Roberts (Cambridge University Press).—The Monist, vol. i., No. 3 (Watts).—Willing's British and Irish Press Guide (Willing).

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