THURSDAY, JUNE 16, 1881

THE STORAGE OF ELECTRIC ENERGY

I AM continuing my experiments on the Faure accumulator with every-day increasing interest. I find M. Reynier's statement, that a Faure accumulator, weighing 75 kilograms (165 lbs.) can store and give out again energy to the extent of an hour's work of one-horse power (2,000,000 foot-pounds) amply confirmed. I have not yet succeeded in making the complete measurements necessary to say exactly what proportion of the energy used in the charging is lost in the process of charging and discharging. If the processes are pushed on too fast there is necessarily a great loss of energy, just as there is in driving a small steam-engine so fast that energy is wasted by "wire-drawing" of the steam through the steam pipes and ports. If the processes are carried on too slowly there is inevitably some loss through local action, the spongy lead becoming oxidised, and the peroxide losing some of its oxygen viciously, that is to say, without doing the proper proportion of electric work in the circuit. I have seen enough however to make me feel very confident that in any mode of working the accumulator not uselessly slow, the loss from local action will be very small. I think it most probable that at rates of working which would be perfectly convenient for the ordinary use of fixed accumulators in connection with electric lighting and electric transmission of power for driving machinery, large and small, the loss of energy in charging the accumulator and taking out the charge again for use will be less than 10 per cent. of the whole that is spent in charging the accumulator: but to realise such dynamical economy as this prime cost in lead must not be stinted. I have quite ascertained that accumulators amounting in weight to three-quarters of a ton will suffice to work for six hours from one charge, doing work during the six hours at the uniform rate of one-horse-power, and with very high economy. I think it probable that the economy will be so high that as much as 90 per cent. of the energy spent in the charge will be given out in the circuit external to the accumulator. When, as in the proposed application to driving tramcars, economy of weight is very important, much less perfect economy of energy must be looked for. Thus, though an eighth of a ton of accumulators would work very economically for six hours at one-sixth of a horse-power, it would work much less economically for one hour at one horse-power; but not so uneconomically as to be practically fatal to the proposed use. It seems indeed very probable that a tramcar arranged to take in, say 7½ cwt. of freshly-charged accumulators, on leaving head-quarters for an hour's run, may be driven more economically by the electric energy operating through a dynamo-electric machine than by horses. The question of economy between accumulators carried in the tramcar, as in M. Faure's proposal, and electricity transmitted by an insulated conductor, as in the electric railway at present being tried at Berlin by the Messrs. Siemens, is one that can only be practically settled by experience. In circumstances in which the insulated conductor can be laid, Messrs. Siemens' plan will undoubtedly be the most economical, as it will save the carriage of the weight of the accumulators. But there are many cases in which the insulated conductor is impracticable, and in which M. Faure's plan may prove useful. Whether it be the electric railway or the lead-driven tramcar, there is one feature of peculiar scientific interest belonging to electro-dynamic propulsion of road carriages. Whatever work is done by gravity on the carriage going down hill will be laid up in store ready to assist afterwards in drawing the carriage up the hill, provided electric accumulators be used, whether at a fixed driving station or in the carriage itself.

WILLIAM THOMSON

University, Glasgow, June 13

THE LIFE OF WHEWELL

The Life, and Selections from the Correspondence, of William Whewell, D.D., late Master of Trinity College, Cambridge. By Mrs. Stair Douglas. (C. Kegan Paul and Co., 1881.)

IT is now about four years since the first instalment of a biography of the late Dr. Whewell was published. These volumes, admirably edited by Mr. Todhunter, give us a brief outline of his history, but consist chiefly of a most valuable analytical account of his writings and a selection from his literary and scientific correspondence. In the preface a more complete memoir of Dr. Whewell's personal and domestic history is announced as in preparation. The present volume, edited by Dr. Whewell's niece, Mrs. Stair Douglas, fulfils the promise then given. The preface explains the long interval—fourteen years—which has elapsed since Dr. Whewell's death. A series of untoward events have continued to retard publication. From various causes much delay occurred before the exact plan of the work was determined and the subjects apportioned. At first it was hoped that what may be called the academic life of Dr. Whewell would be undertaken by Mr. Aldis Wright, the present Bursar of Trinity College. But the pressure of heavy and unavoidable engagements has precluded him from proceeding with the task. Mrs. Douglas then endeavoured to work the materials into the selections from Dr. Whewell's personal correspondence which she had nearly completed, with the assistance of Mr. J. Lemprière Hammond, Fellow of Trinity, and one of Dr. Whewell's executors. Before this was accomplished she was deprived of his invaluable aid by his lamented and untimely death. Thus some portions of the present work are a little incomplete. Still, as these are generally of a rather technical nature, and more interesting to members of the University than to the general reader, their absence probably will not be very widely felt. We may be allowed to express our admiration at the tact and good taste with which Mrs. Douglas has executed her task. She allows Dr. Whewell as far as possible to speak for himself, connecting his letters generally with only such brief biographical paragraphs as are necessary for a connected and intelligible narrative. There is little comment and no attempt at the fulsome praise with which biographies are often disfigured. Her descriptions, though brief, are often graphic, while the letters enable us to see the Master of Trinity as he appeared to the inner circle of intimate friends and loved relations.

Of his vast and varied knowledge it is almost needless now to speak. Suffice it to say that the letters now published contain additional testimony to the truth of Mr. Todhunter's remark in his preface to the volumes mentioned above : "I do not think adequate justice can be rendered to Dr. Whewell's vast knowledge and power by any person who did not know him intimately, except by the examination of his extensive correspondence; such an examination cannot fail to raise the opinion formed of him by the study of his published works, however high that opinion may be."

The letters, however, in the present volume, as might be expected, bring out their author in a light which is to many new and unexpected. To most persons that broad forehead with its massive brow seemed indicative of intellectual strength almost gigantic ; the square shoulders, strong bones and muscles, the swinging gait—with which as he swept along he seemed to shoulder aside weaker men by the very waft of his passing—told of irresistible force of will and energy of purpose ; tenderness of heart seemed improbable in one of such Titanic mould; one deemed him a "man of iron," who, had he chosen a field other than literature and science, might also have been one of blood ; but, as we shall presently see, underneath that rough exterior there was a warm and affectionate heart concealed.

Of the childhood of William Whewell but few particulars are recorded. A master-carpenter's son at Lancaster, he was on the point of quitting the Blue Coat School in that town to be apprenticed to his father, when —by a mere chance as it seemed—the head-master of the Grammar School entered into conversation with the boy, and was so struck by his abilities that he persuaded the father to let his son come to that school, generously offering to give him both books and instruction. According to Prof. Owen—probably his sole surviving schoolfellow —the lad's indomitable spirit soon manifested itself, as well as his appetite for work. The latter indirectly raised the standard of the school lessons, and the other boys threatened to "wallop" him as the penalty for preparing more than twenty lines of Virgil. Even then however this was more easily said than done, and the "wallopers" got as good as they gave, until public opinion in the school decided that it was unfair for more than two boys to attack him at once—"after the fate of the first pair, a second was not found willing." Once only did Whewell shock the moral feelings of his revered master, and that was when an undergraduate at Trinity. The crime shall be told in the master's own words : "He has gone and got the Chancellor's gold medal for some trumpery poem, 'Boadicea,' or something of that kind, when he ought to have been sticking to his mathematics. I give him up now. Taking after his poor mother, I suppose." (She had occasionally contributed to the "Poet's Corner" of the local newspaper.) Mrs. Owen, to whom this complaint was made, pacified the worthy man by remarking that "young men must have some amusement, and this seemed to be a very innocent one."

Notwithstanding Dr. Whewell's strong frame he appears to have suffered from some constitutional delicacy when a lad. His mother—evidently a woman of ability and culture above her station—died when he was thirteen ; his father only survived to see him take a degree. A sister also died young ; and of his three brothers two died in infancy and the other at the age of nine. Talent was evidently hereditary in the family, for the little brother, at seven years old, had begun to write English verses, and one of his sisters habitually wrote poems. Of all three of the latter, he says, when referring to his prize poem, in a letter to his father, "I am happy in having sisters who all of them have, I think, a more rational taste for poetry and literature of all kinds than any other girls in the same circumstances."

It is very interesting to note the gradual change in these letters. We see in them not only the unfolding of his great intellectual powers as evidenced by the widening circle of interests, but also the gradual expanding of the moral nature. At first those addressed to his relations and more intimate friends are a little stiff and cold, but as sorrows succeed one another the religious element in Dr. Whewell's character becomes more conspicuous, and the later letters are marked by a depth of tenderness surprising to those who only knew him slightly. He was devotedly attached to his first wife, and almost heartbroken by her loss. In one letter he describes himself as taking no pleasure in success now that she was gone, and tells his niece how, while he sat as Vice-Chancellor in the Senate-house conferring degrees, he felt so lonely and miserable that the tears kept trickling down his face ; "so unlikely a thing in a Vice-Chancellor in his chair that probably nobody saw it. I hope so." The writer of this article, who received his degree on that occasion, well remembers some of his friends commenting on the Vice-Chancellor's obvious "sourness" of manner, and wondering whether he was disgusted at the Senior Wrangler being a man of a rival college. We little thought that this was "none other than sorrow of heart." After some time Dr. Whewell married again, his second wife being Lady Affleck, sister of his friend Robert Leslie Ellis. In her companionship he found great happiness, but after about seven years of married life he was again left alone. This time he appeared unable to rally from the bereavement ; "the future which intervened between him and the grave dismayed him by its dark desolation." After this he visibly declined ; the torpor of age began to steal over his faculties, and many thought that the years of waiting would not be very many ; but they were briefer than any expected. While still comparatively vigorous, a fall from his horse caused fatal injury to the brain, and after lingering for a few days, happily without much suffering, he died on March 6, 1866.

"While life was ebbing fast away on that last morning, blinds and curtains were drawn wide apart in compliance with his wish that he might see the sun shine on the Great Court of Trinity, and he smiled as he was reminded that he used to say the sky never looked so blue as when seen fringed with its turrets and battlements. Almost to the last he was conscious, and the last words intelligibly uttered, when the striking of the clock roused him as day dawned, were, 'The Eternal God is my refuge, and underneath are the everlasting arms.'"

The extraordinary comprehensiveness and versatility of Dr. Whewell's mind is fully depicted in the letters published in Mr. Todhunter's volume, but it is brought out no less, perhaps even more, graphically by some of the brief allusions in his familiar correspondence. This, for example, is one taken from a letter to his sister : "Besides my usual employments [as College Tutor and

Professor of Mineralogy] I have to go to London two days every fortnight as President of the Geological Society, and am printing a book which I have not yet written ["The History of the Inductive Sciences"], so that I am obliged often to run as fast as I can to avoid the printers riding over me, so close are they at my heels. I am, in addition to this, preaching a course of sermons before the University; but this last employment, though it takes time and thought, rather sobers and harmonises my other occupations than adds anything to my distraction." He seemed to be able to turn his hand to anything, and, like a dexterous conjuror, play with half-a-dozen balls at once. Pendulum experiments, theories of the tides, mathematical problems, crystallographic formulæ metaphysics, and various subjects in moral philosophy, classics, modern languages, architecture, geology, with plenty of work in general literature, all make up what we may call in the best sense "farrago libelli." These letters also bring out very clearly another characteristic of Dr. Whewell's mind. He was essentially cautious in regard to change—an advocate of reformation rather than of renovation; in science a systematiser rather than a discoverer; like a navigator who explored to the full the uninvestigated coasts of the Old World, rather than one who steered out into the open ocean in the hope of discovering a New World. This was no doubt partly due to his mathematical training and academic habits of life—but it is very rare, perhaps impossible, to find a memory of extraordinary tenacity and a life essentially studious, combined with originality in one of its highest forms. That requires a good deal of mental fresh air, and is apt to droop a little if too much confined to the atmosphere of a library. This is especially evident in Dr. Whewell's remarks upon the "Vestiges of Creation" and in his essay on the "Plurality of Worlds." The same tone of mind is very conspicuous in his attitude towards the question of University Reform. He was a vigorous opponent of the abuses of private tuition, a zealous advocate of progress in every department of learning, deeply anxious for the improvement of the Classical and Mathematical Tripos examinations, and to him more than to any other single man the recognition of the Natural and Moral Sciences as branches of academic study is due. But he was antagonistic—almost bitterly so—to the appointment of the Royal Commission of 1856 and of its successor, and was hostile in many respects to the changes—now almost universally acknowledged to have been on the whole very beneficial—which were introduced by the statutes of 1859–61. His great hope and desire was that the University should be allowed to reform itself, and be spared any interference from without. That he should have entertained this hope after so many years of academic labour is perhaps the strongest proof of his sanguine temperament.

We must now part from this interesting volume. Perhaps—like the portrait prefixed to it—it slightly fails in depicting the characteristic ruggedness of the man, but it does much to show him as he was to those near and dear to him as well as to the world—a man of immense intellectual power, of intense energy and industry, of high purpose and simple piety, a hard hitter in conflict and a lover of the shout of battle, but too magnanimous to bear ill-will, whether in defeat or victory. Besides this he was a munificent benefactor to his College and his University: one to whom both must long be grateful, and of whom both may well be proud, as having filled a great position in the world of science and literature, and especially as being "the man" (in the opinion of a most competent judge) "to whom, more than to any other single man, the revival of philosophy in Cambridge is due." T. G. BONNEY

OUR BOOK SHELF

Inorganic Chemistry. Adapted for Students in the Elementary Classes of the Science and Art Department. By Dr. W. B. Kemshead, F.R.A.S., F.G.S. Enlarged edition, revised and extended. (London and Glasgow: William Collins, Sons, and Co., Limited.)

THIS work is a typical one. While containing much that is useful and fairly satisfactory, especially from an examination point of view, the whole tendency of the book, considered as an elementary treatise on a branch of natural science, must be strongly condemned.

The leading facts concerning the better-known non-metallic elements and compounds are succinctly stated; the principal reactions of formation and decomposition of these bodies—especially those reactions which unfortunately *must* be "got up" for examination purposes—are arranged in the form of equations; and the simpler arithmetical applications of such equations are illustrated by fully worked-out examples. But chemistry is more than this: facts must be connected together by principles; the connection between fact and theory, and between theory and fact, must be revealed; these two must not be regarded as synonymous, but as mutually dependent; and the reasoning by aid of which theoretical conclusions are reached must be clearly indicated. Chemistry is neither a system of dogmatic assertions nor an accumulation of shibboleths, by the skilful use of which an examiner may make havoc among the Ephraimites crowding to the Jordan of Examination, but a living science.

The principle which is most largely used (or rather misused) in Dr. Kemshead's book is that of Valency; but valency in the hands of this author is deprived of its value as a scientific theory, and becomes an accumulation of fanciful speculations. The basis of the present work is evidently Dr. Frankland's "Lecture Notes"; hence probably the success of the book in preparing examinees for South Kensington (the present is a second and enlarged edition); and is not such success after all of more importance than training chemists or disciplining the mental powers of youth?

The theory of valency is based on the wider molecular theory of matter, which was preceded by the atomic theory of Dalton, itself a development from that system of chemical notation which rested on the combining weights of the elementary bodies. Now it is clear, from many passages, that the author of this book has failed to distinguish combining weights from atomic weights, and atomic from molecular weights: thus on p. 13 we read "these proportions by weight [*i.e.* from the context, these proportions in which "substances unite together chemically"] when reduced to their lowest relative value, and expressed with reference to that of hydrogen, which is usually taken as unity, are called the atomic weights, or combining numbers of the elements." Again, on p. 26, "the *combining weight* of hydrogen being 1, that of oxygen becomes 16; of nitrogen, 14; of carbon, 12," &c. But combining weights are *not* synonymous with atomic weights, and the *combining weight* of oxygen happens to be 8, of carbon 3, and of nitrogen 4·66. The formula weights of compounds are constantly referred to as "atomic and molecular weights." We have such formulæ as $(NH_4O)_2$, CuO_2, &c., stated to be molecular formulæ;

molecule is nowhere defined (in a note on p. 57 a casual statement is made as to the meaning of the term); "Avogadro's law," which lies at the basis of the whole modern edifice of chemistry, is conspicuous by its absence; certain statements as to gaseous combination and to "volume weights" are made, it is true (p. 35), but these are incomplete and misleading.

When a theory of valency is raised on so slender and shifting a molecular foundation as is here laid, no wonder that the edifice should be a strange one; the definition of "atomicity" on pp. 54-55 is incomplete, and cannot be upheld by facts; the statement on p. 58, "it is then a law to which there are no real exceptions, that though the equivalence of an element may vary, it does so always by the addition or subtraction of an even number," is simply untrue. As an "important conclusion" from certain "facts" (? fancies) "on equivalence," it is stated that (p. 59) "a formula which possesses an uneven number of bonds or units of chemical affinity cannot possibly represent a molecule"; without minutely criticising the expression "bond or unit of chemical affinity," suffice it to say that such a formula as, according to Dr. Kemshead, cannot possibly represent a molecule, unfortunately does represent a molecule. The existence of the molecule NO is a case in point: *à propos* of this compound, there is a charming example of the author's method of treating chemical science as a collection of opinions of various authorities to be found in a footnote on p. 169.

Notes on the Crania of New England Indians. By Lucien Carr. From the Anniversary Memoirs of the Boston Society of Natural History, 1880.

THIS is one of the numerous contributions now being made towards our knowledge of the fast-disappearing race of North American Indians. The author, Mr. Lucien Carr, holds the office of Assistant Curator to the valuable Museum of American Archæology and Ethnology at Cambridge, Mass., an institution owing its foundation to the liberality of Mr. Peabody, so well known in England by his benefactions to the London poor, and its scientific excellence to the zeal and organising power of its first curator, the late Dr. Jeffries Wyman, and of his successors.

The object of the present memoir is to collect together such information as is still to be obtained regarding the cranial characters of the native Indians of the New England States, the celebrated "five nations" of the early historians of America, who in consequence of their geographical position were among the first of the race to succumb to the inroads of European immigration. Measurements are given of 67 crania, of which 38 are assigned to males and 29 to females. The averages of these measurements give the following results:—A medium cranial capacity, *i.e.* 1436 cubic centimetres for the males and 1319 for the females. A latitudinal index of ·759, showing mesaticephalism verging upon dolichocephalism. The altitudinal index exactly the same. The principal facial indices show orthognathism, with a strong tendency to mesognathism, a mesorhine nose (index 50), and slightly megaseme orbits (index 88 in the males, and 91 in the females). Although these are the average characters of the whole collection, very few, if any, of the individual crania are to be found presenting them. There is indeed no such uniformity among these skulls as may be seen in certain races, such as Eskimos, Bushmen, Fijians, Andamanese, or even Australians. Perhaps it could scarcely be expected in inhabitants of a large continent, presenting great diversities of climatic and other conditions, and with no natural barriers to free migration and intercourse. The examination of these skulls therefore confirms what has been often remarked before, that although in a broad sense the American Indians present a certain community of type, there is great diversity in detail among them, the result probably of a long series of repetitions of the process of breaking up into distinct groups or tribes and reuniting in various combinations.

LETTERS TO THE EDITOR

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

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Conservation of Electricity

IN a recent communication to NATURE (vol. xxiv. p. 78) Prof. Silvanus P. Thompson very kindly mentions my latest memoir on "The Conservation of Electricity," and, as I am glad to find, confirms my views on this subject by stating that he has independently arrived at the same conclusions with myself.

As regards however the question of priority moved by Prof. Thompson, I think I ought to add that an earlier paper of mine, published five years ago, must have escaped Prof. Thompson's attention. This was printed as an abstract in the *Comptes rendus* of the Paris Academy of Sciences for June 19, 1876, under the title, "Extension du principe de Carnot à la théorie des phénomènes électriques. Équations différentielles générales de l'équilibre et du mouvement d'un système électrique réversible quelconque." I there enunciated the law of the Conservation of Electricity in the same terms as now, and also gave the same analytical method for applying it. I beg leave to quote as a proof the following explicit passage from this extract:—"L'équation $\int dm = 0$ a une autre signification plus simple; elle signifie que de l'électricité peut se déplacer, mais ne peut jamais varier en quantité. Ce principe de la conservation de la quantité d'électricité à été admis par les physiciens dans tous les cas connus jusqu'ici, influence, frottement, etc. . . . . Pour que $\int dm = 0$ pour tout cycle fermé, il faut que dm soit une différentielle parfaite." This method I had already applied in 1875 to the phenomena presented by mercury electrodes (*vide Annales de Chim. Phys.* 1875). In fact my latest memoir is merely a renewed attempt to draw, by means of new applications, the attention of physicists to a fact which I cannot help considering as important for the future, viz. that the principle of the Conservation of Electricity is, *as far as analytical applications are concerned*, the exact analogue to Carnot's Principle for Heat.

Paris, Faculté des Sciences, June 5 G. LIPPMANN

Apparent Decomposition of Sunlight by Intermittent Reflecting Surfaces

IT occurred to me that light might be decomposed by interrupting, with a reflecting surface, a ray of light in such a manner that the interruptions may be proportional to the wave-length period of any particular ray forming a part of a composite ray. The experiment is effected in the following way:—

A wheel having bright spokes (the large wheel of a bicycle answers well) is caused to revolve between an observer and the sun, so that a ray of light is reflected to the observer by a bright spoke; then, when 120 spokes pass before the observer per second, violet light shines out vividly; when 65 pass, red appears, and different rates of revolution give different colours. There seems to be a marked relationship existing between the number of spokes which pass by and the wave-length of the two colours mentioned, that of the violet being $\frac{1}{60000}$ inch, and that of the red $\frac{1}{34000}$ inch.

I am now investigating this apparent relationship between spoke-interruption and wave-length for the other colours of the spectrum of white light, and I hope to be able to make known the results shortly. FREDERICK J. SMITH

Taunton, June 4

Symbolical Logic

I AM sorry that Mr. MacColl should have thought that there was any intention on my part to suggest a doubt as to his having written his papers without having read Boole's "Laws of Thought." I knew that he was very anxious that the fact should be known, and I called attention to it. I could not state it as a fact known to me. His own assurance was the only ground I had, or could have, to go upon, and in assigning this it never occurred to me to doubt his statement, or to think that I was suggesting doubts to others.

As regards my half humorous suggestion that an attitude of slight social repression was desirable towards novelties of mere notation—not towards new conceptions or methods—I feel sure that almost every one who has not a private scheme of his own to protect will agree with me. Few things can be more perplexing to students of any subject than to find one author after another making use of a new notation to express old results (I mean no special reference to Mr. MacColl here, who does not seem to me one of the worst offenders in this way). At the time of writing my "Symbolic Logic" I had between twenty and thirty such schemes before me. Some of these, of course, express really distinct conceptions, or effect improvements in procedure, but most of them do not; we find half-a-dozen different signs standing for the same meaning, and half-a-dozen different meanings assigned to the same sign. I cannot but think that much of this confusion would be avoided if the various authors would take the trouble to inquire what had been already written upon their subject. The only "repression" I should like to see introduced consists in the remonstrances of reviewers and students generally against the mere substitution of a new symbol for one which was already in use for expressing precisely the same process or conception. So far from wishing to discourage any attempts to improve on the results of Boole and others, I rejoice to see them, and think that Mr. MacColl himself has done some good work in this way. It would have been better still if he had not disfigured it by a notation which I think makes him regard his results as more original than they really are.

I need not seriously discuss those parts of Mr. MacColl's letter which give his opinion as to the impression which will be produced in other persons by a perusal of my book, and his "impression" that he has "somewhere seen Mr. Venn quoted as holding an opinion very much at variance with" a statement which he misquotes.[1] (By the way, I heartily agree with his "protest against that spirit of criticism which would offer two or three chipped bricks as a fair specimen of a house," &c., and think the chipping of the bricks a happy turn.) The rest of his letter contains criticisms upon my conclusions on a variety of rather intricate speculative questions. Having stated my own views as fully and accurately as I conveniently could only a few weeks ago, in a systematic work, I really must decline to be drawn into repeating them again, in a condensed form, in the columns of a scientific journal, even if the editor would consent to accept them. J. VENN

Cambridge, June 12

[1] What I spoke of was "those problems in Probability which Boole justly regarded as the crowning triumph of his system." What Mr. MacColl puts between inverted commas is that Boole "justly regarded his problems in Probability as the crowning triumph of his system," and challenges me to say whether or not I agree with Boole's solution of a certain well-known example. This considerably distorts the meaning of what I said.

Telephones in New Zealand, &c.

OBSERVING your paragraph on this subject in NATURE, vol. xxiv. p. 88, it occurs to me that the following may be of interest:—When in Wellington and Dunedin, N.Z., at the end of December last, my opinion was asked by the Government Telegraphic officials there upon a pair of ordinary "Edison-Bell Telephones" (not Edison *bell-telephones*, as they are too frequently called) which they had just received from the United States for purposes of experiment. A careful trial under various conditions showed me that they were very good average instruments of ordinary delicacy, such as I had seen hundreds of previously in England and the States.

With these instruments, however, Dr. Lemon, the Superintendent of the Postal and Telegraph Service, was able to converse clearly between Wellington and Napier, over an ordinary land line 232 miles in length, while battery currents were passing over the wires on the same posts.

In New Zealand, Telegraphic communication is, and Telephonic communication will be, entirely in the hands of the Government. In Melbourne the telephone-exchange is worked by a private company, but the erection and maintenance of wires is carried out by the Victorian Government at the annual rate of 5*l.* per subscriber. In Sydney, I regret to say, nothing was being done in this matter. In Honolulu I found (last January) telephonic communication all over the town, but no telegraphs at all. The King of the Sandwich Islands however, Alii Kalakaua, who is shortly expected in England, told me that he greatly needed submarine cables between the various islands. On my return to England I had the pleasure of sending to Sydney materials for a private telephonic line on sugar plantations in the Fiji Islands, and my friend Mr. Frederick Cobb, manager of the Falkland Islands Company, tells me that the line he took out there at my suggestion is a great success.

At Wellington, where the central N.Z. telegraph office is, I was very much struck by the extreme ease with which duplex circuits were worked. Dr. Lemon informed me that it was scarcely necessary to alter the resistances once a week. He showed me a simple little carbon rheostat of his own invention which appeared to answer admirably; it consisted essentially of two pieces of carbon, the closeness of whose contact was regulated by a screw.

On my way home I paid a hurried visit to the central office of the Western Union Telegraph Company in New York (just at the critical time of the absorption by it of the other two companies and the consequent creation of a monopoly), and was greatly surprised to see the extent to which the 16,000 cells in the battery-room were being replaced by Siemens's dynamo-machines. I was told that one of them would "drive" about fifty wires, and was shown a number of plaster-of-paris cylinders, about five inches long and one inch diameter, which were put into circuit to diminish, when necessary, the intensity of the current. It may be remembered that as a rule American lines are less perfectly insulated than ours, and hence require stronger currents. WM. LANT CARPENTER

6, York Buildings, Weymouth, June 1

Implements at Acton

MR. PERCEVAL'S letter in NATURE, vol. xxiv. p. 101, is an interesting one, but the occurrence of Neolithic implements at and near Acton has been known (if not published) for many years past. In the Pitt-Rivers' collection may be seen Neolithic scrapers and flakes from the Acton district. I have found Neolithic stones in the neighbourhood of Acton and Willesden for many years past; and only a few weeks ago I picked up a beautiful and perfect knife of black flint made from a large flake, five and a half inches long, and one and three quarter inches wide, in the field on the east of Acton Station of the North London Railway. Many of the Neolithic flints from this position are white. A considerable number of Neolithic implements and flakes have at different times been dredged up from the Thames to the West of London, and some of these have been quite recently exhibited. I do not attach importance to the quartzite pebble, as pebbles of quartzite are extremely common in the glacial deposits at the North of London, and very common in the gravels of the Thames and its northern tributaries. They also occur *in situ* at the north of Willesden.

Will Mr. Perceval kindly furnish the heights at the Hammersmith position, and say whether he is positive that the gravel he has in view was dug on the spot, and whether the implements occur there (as his letter implies) in "remarkable abundance"? I have repeatedly examined the low gravels about Hammersmith, Fulham, and Chelsea, but with no result. For more than three years I have never missed an opportunity of looking over the low gravels belonging to these places, together with the positions at West Brompton and Kensington, where thousands of tons of gravel have been excavated. My result has been one dubious flake, probably washed down from one of the higher terraces. I however have heard of two Palæolithic implements having been found—one at Kensington and the other at West Brompton—but whether from the local gravel or not I am uncertain.

I by no means wish to imply that because I have been unable to find implements in the lower gravels therefore some one else may not have found them. Some one may have been always before me and picked them up, or I may have constantly looked over unproductive patches.

The places mentioned by Mr. Perceval are, it must be remembered, frequently ballasted with gravel brought from a distance by the Thames, by the Grand Junction Canal, and by the Great Western Railway. I know of at least five different localities whence the Acton and Hammersmith gravel is brought, one locality being in Kent. It is therefore of the highest importance that one should know for certain whence the gravel has been derived that one sees on the roads.

I live in an implementiferous district, and find Palæolithic implements in the Highbury and Clapton gravels; but a visitor would make a fatal mistake if he supposed that all the gravel on the roads about here belongs to the district. Sometimes many tons of gravel are brought here from Walthamstow; at other times from Ware or Hertford; sometimes from Dartford, and from other places. Unless, therefore, the greatest possible care is taken in ascertaining the exact locality whence the ballast comes, mistakes are certain to occur.

The lowest gravels about here are unproductive of the works of primæval man, with the exception of, at times, a stray flake or two, probably derived from a higher level. The evidence that I have seen in the lower gravels round London points to the correctness of the conjecture made by General Pitt-Rivers, that the Palæolithic age had passed away before the lower parts of the Thames Valley were excavated.

WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

How to Prevent Drowning

I HAVE read with some interest Dr. MacCormac's letters on the subject of water-treading as means of preventing drowning.

I am sorry that I cannot agree with him, as it would be decidedly a matter of congratulation if some practical means of diminishing the number of casualties from drowning were found. Personal experience, however, prevents my agreeing with Dr. MacCormac.

I am a tolerably good swimmer, can swim in all the different fashions, but I can neither float nor tread water.

Shortly after Dr. MacCormac's first letter appeared I went to swimming baths with a view of putting the matter to the test. I had carefully read Dr. MacCormac's letter, and determined to give it a fair trial. I minutely observed all his directions, and invariably sank every time I tried his plan.

Now it must be remembered that I am a swimmer, and so far as swimming goes, perfectly at home in the water. Moreover, I was not in the least flustered. When I sank I made no attempt to rise again by swimming; I remained in what Dr. MacCormac would call the orthodox position for treading water, only opening my eyes in order to see whether I was ascending or descending. As however I found that I continued to do the latter until I reached the bottom of the bath, and there seemed to be no probability that I should rise without some further effort, I was at last compelled to make this effort.

This was the course of affairs every time I made the attempt. Moreover, whenever I essayed to float on the surface, although I carefully assumed the correct position, threw my head well back, and took the deep inspiration, the result was the same.

Arguing from these facts, it seems to me pretty clear that it is not everybody who can tread water or float. Why this is so, appears to me to lie in the fact that the human body is not always lighter, bulk for bulk, than water. Perhaps with plump children and others with plenty of adipose tissue about their frame this may be the case, but with spare people who consist mainly of muscle and bone, the specific gravity must be greater than that of water. The body of a fish when the animal is dead will sink until decomposition sets in and causes it to float.

For these reasons I fear that Dr. MacCormac's suggestion will not be found of so much practical use as he hopes. The apparent ease of the process described by Dr. MacCormac may in itself be the cause of rash proceedings by those who cannot swim, and may so lead to greater loss of life, the very evil which the suggestion is intended to diminish.

W. HENRY KESTEVEN

401, Holloway Road, N., June 7

ON the Continent the facilities are greater than in England, where factories and steam-boats spoil the pleasure of swimming, and everybody is well aware that *all can float upon fresh water without assistance from their hands and feet.* It is what in the Paris swimming-schools is called "faire le mort."

Anybody—stout, lean, cripple, halt—is able to do so, and I taught, myself, a poor little hunchback how to perform this easy feat; but his deformity placing him in a state of unstable equilibrium, he was obliged to keep his arms stretched at an angle from 45° to 60°.

Some minutes are sufficient in fresh water to make a proficient and a live "mort." The way to do it is very simple, and Mr. MacCormac described it very exactly, with the omission of some particulars relating to the way of breathing, which had no direct reference to his chief and beneficial topic, "treading water."

He who wishes to "faire le mort" must first draw a deep breath, and keep it, then put himself on his back, with his head thrown backwards, as recommended by Mr. MacCormac, and allow his limbs to droop slackly without any stiffness, no matter in what position.

The body will sink at first under water, but it will immediately rise nearly on a level with the surface, the only parts quite free from water being the chest and the nose and mouth, around which the water describes an oval, whilst the eyes are at times over, at times under, water.

The "mort" can remain floating in this way as long as his breath allows, though it is better not to wait longer than two or three seconds, to avoid fatigue; then he must quickly emit it, draw another deep breath, and keep it again.

The body sinks as before, rises immediately, regaining its floating position, nose, mouth, and chest emerging again from water.

This can be continued for hours together without the least motion of legs or arms, as your readers will be able to verify for themselves, either at the Pont Royal or Ligny swimming schools, on their visit to the Paris Exhibition of Electricity.

Jersey, June 5 CHATEL

P.S.—I ought to add that whilst floating on fresh water the body is not quite on a level with the surface, but from the chest, that is out of water, to the toes, which are about six or eight inches under water, figures an inclined plane, the slope of which varies with everybody, and that any attempt to bring the toes on a level with the surface makes the body sink. On the contrary, the deeper the head is sunk backwards under water the more the body emerges.

Auroric Light

JUNE 6, faint lights, especially to the northward, between 10 and 12; smart frost.

June 7, at 10, masses of purplish light rising from the north-east and congregating about the zenith; pencils of greenish yellow and white rising to the north; these continued up to 12, after which no observations were made; very smart frost, which bit the potato-stalks.

June 7, from 10 to 12, well-marked and at times brilliant columns, pencils, and masses of red rising all round the heavens at intervals, and congregating at the zenith; a most severe white frost that burnt up all the potatoes on the valley flats and on the uplands. At 5.30 on the 8th the frost was so thick that the ground had the appearance as if it had snowed during the night.

Ovoca, Ireland, June 10 G. H. KINAHAN

A Singular Cause of Shipwreck

IN NATURE, vol. xxiv. p. 106, you mention a "singular case of shipwreck" caused by waves and spray freezing on a steamer and sinking it by its weight. Cases of this kind caused by frozen spray alone are known near the east coast of the Black Sea. North of 44°, where the mountains are not very high, an exceedingly strong and sudden north-east wind is frequent, quite similar to the Dalmatian Bora, and called alike. It descends at a certain angle to the sea, raising a great quantity of spray. In winter this spray immediately freezes, and ships may sink by its weight. On January 25, 1848, a war-ship, anchored in the middle of the Bay of Noerrossiisk, sank in this manner. As the weather was fine before, a great part of the crew were ashore, and the storm arrived with such suddenness that the ship sank from the weight of the frozen spray. On account of the bora this coast is avoided by merchant-ships in winter, and visited only by a line of steamers subventioned by the Government.

St. Petersburg, June 8 A. WOEIKOF

OBSERVATIONS ON THE HABITS OF ANTS

ON Thursday (June 2) Sir John Lubbock read a further paper on this subject at the meeting of the Linnean Society. He said that in one of his former papers

(Linnean Soc. *Journ.* vol. xiv. p. 278) he had given a series of experiments made on ants with light of different colours, in order if possible to determine whether ants had the power of distinguishing colours. For this purpose he utilised the dread which ants, when in their nest, have of light. Not unnaturally, if a nest is uncovered, they think they are being attacked, and hasten to carry their young away to a darker, and, as they suppose, a safer place. He satisfied himself, by hundreds of experiments, that if he exposed to light the greater part of a nest, but left any part of it covered over, the young would certainly be conveyed to the dark portion. In this manner he satisfied himself that the different rays of the spectrum act on them in a different manner from that in which they affect us; for instance, that ants are specially sensitive to the violet rays. But he was anxious to go beyond this, and to attempt to determine how far their limits of vision agree with ours. We all know that if a ray of white light is passed through a prism, it is broken up into a beautiful band of colours—the spectrum. To our eyes it is bounded by red at the one end and violet at the other, the edge being sharply marked at the red end, but less abruptly at the violet. But a ray of light contains besides the rays visible to our eyes others which are called, though not with absolute correctness, heat rays and chemical rays. These, so far from being bounded by the limits of our vision, extend far beyond it, the heat rays at the red, the chemical rays at the violet end. He wished under these circumstances to determine if possible whether the limit of vision in the case of ants was the same as with us. This interesting problem he endeavoured to solve as follows:—If an ant's nest be disturbed the ants soon carry their grubs and chrysalises underground again to a place of safety. Sir John, availing himself of this habit, placed some ants with larvæ and pupæ between two plates of glass about one-eighth of an inch apart, a distance which leaves just room enough for the ants to move about freely. He found that if he covered over part of the glass with any opaque substance the young were always carried into the part thus darkened. He then tried placing over the nest different coloured glasses, and found that if he placed side by side a pale yellow glass and one of deep violet the young were always carried under the former, showing that though the light yellow was much more transparent to our eyes, it was, on the contrary, much less so to the ants. So far he had gone in experiments already recorded; but he now wished, as already mentioned, to go further, and test the effect upon them of the ultra-violet rays, which to us are invisible. For this purpose, among other experiments, he used sulphate of quinine and bisulphide of carbon, both of which transmit all the visible rays, and are therefore perfectly colourless and transparent to us, but which completely stop the ultra-violet rays. Over a part of one of his nests he placed flat-sided bottles containing the above-mentioned fluids, and over another part a piece of dark violet glass; in every case the larvæ were carried under the transparent liquids, and not under the violet glass. Again, he threw a spectrum into a similar nest, and found that if the ants had to choose between placing their young in the ultra-violet rays or in the red they preferred the latter. He infers therefore that the ants perceive the ultra-violet rays, which to our eyes are quite invisible.

Now as every ray of homogeneous light which we can perceive at all appears to us as a distinct colour, it seems probable that these ultra-violet rays must make themselves apparent to the ants as a distinct and separate colour (of which we can form no idea), but as unlike the rest as red is from yellow or green from violet. The question also arises whether white light to these insects would differ from our white light in containing this additional colour. At any rate, as few of the colours in nature are pure colours, but almost all arise from the combination of rays of different wave-lengths, and as in such cases the visible resultant would be composed not only of the rays which we see, but of these and the ultra-violet, it would appear that the colours of objects and the general aspect of nature must present to them a very different appearance from what it does to us.

Similar experiments which Sir John also made with some of the lower Crustacea point to the same conclusion, but the account of these he reserved for a future occasion. He then proceeded to describe some experiments made on the sense of direction possessed by ants, but it would not be easy to make these intelligible without figures. After detailing some further experiments on the power of recognising friends, he gave some facts which appear to show that ants by selection of food can produce either a queen or a worker at will from a given egg. Lastly he stated that he had still some ants which he had commenced to observe in 1874, and which are still living and in perfect health; they now therefore must be more than seven years old, being therefore by far the oldest insects on record.

THE WEATHER AND HEALTH OF LONDON[1]

TO the statistician London affords materials for the prosecution of many inquiries such as could not be obtained from the statistics of any other city either in ancient or modern times. Among the more important of these inquiries are those which relate to questions suggested by the enormous aggregation of human beings over a limited area which London presents on a scale absolutely unparalleled in the world's history. It is one of these questions we bring before you this evening, viz., the influence of the climate on the health of the people of London.

The relation of weather to health is a question which has engaged the attention of Dr. Arthur Mitchell and myself for many years. In an early stage of the inquiry our attention was mainly directed to Scotland, and more particularly to the data supplied by its eight large towns; but it was soon found that, owing to the sparseness and other conditions of the population, and to the fact that the division of time into months only, adopted by the Registrar-General for Scotland, they were not sufficiently minute to show the true relations of weather to the fluctuations of the death-rate through the year. In truth it was only after not a little unsuccessful labour, and what could at best be characterised as no more than partially successful work, that we resolved eight years ago to open the discussion of the whole subject by an exhaustive examination of the meteorological and vital statistics of London and London alone. More specifically our reasons for the selection of London were that it afforded data from (1) an enormous population spread over an area so limited that it might be regarded as having one uniform climate during each of the seasons of the year; (2) full weekly reports of weather and the deaths from the different diseases; and (3) returns extending over a sufficiently long period.

In the case of diseases such as diarrhœa and bronchitis, which seem to be directly and immediately under the influence of temperature, and such epidemics as scarlet fever and whooping cough, the rate of mortality from which is largely determined by season and weather, a comparatively small number of years is required to give a satisfactory approximation to their true weekly curve of mortality. But as regards the great majority of diseases, it quickly became apparent that a thirty years' average was required in the construction of curves which could be accepted as true "constants" for the diseases to which they refer. The thirty years beginning with 1845 were therefore adopted. An examination of the curves shows that some of their striking features, particularly those

[1] Substance of a Lecture delivered at the Royal Institution, March 25.

showing the complications of special diseases and their connections with each other, which the weekly averages disclose, would entirely disappear if monthly averages only were employed.

The curves of the more prominent and interesting of the diseases are shown on the accompanying woodcuts, the straight black line in each figure being drawn to represent the mean weekly death-rate on an average of the fifty-two weeks of the year, and the figures on the margin the percentages above or below the average. With this general average the mean death-rate of each week is compared and the difference above or below cal-

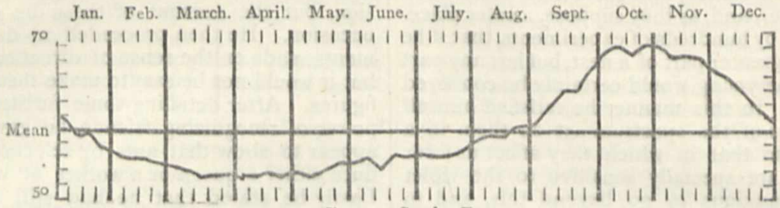

Fig. 1.—Scarlet Fever.

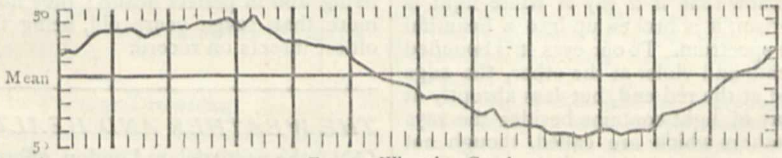

Fig. 2.—Whooping Cough.

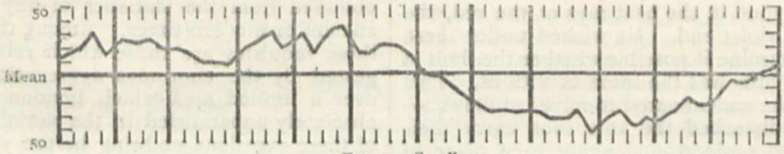

Fig. 3.—Small-pox.

culated in percentages, which, when *plus, are placed* above the mean line of the figure, and when *minus*, below it. Thus as regards scarlatina (Fig. 1), the mean of the fifty-two weeks is 49·6; on the first week of January it is 7 per cent. above the mean, from which time it continues to fall to the annual minimum, 35 per cent. below the mean in the middle of March, thence rises to the mean in the end of August; to the annual maximum, 60 per cent. above the mean, in the end of October, and thereafter steadily falls. The portion of the curve above the mean line thus shows the time of the year when, and the degree to which, the mortality from scarlatina is above its average and the portion below the line when it is under it.

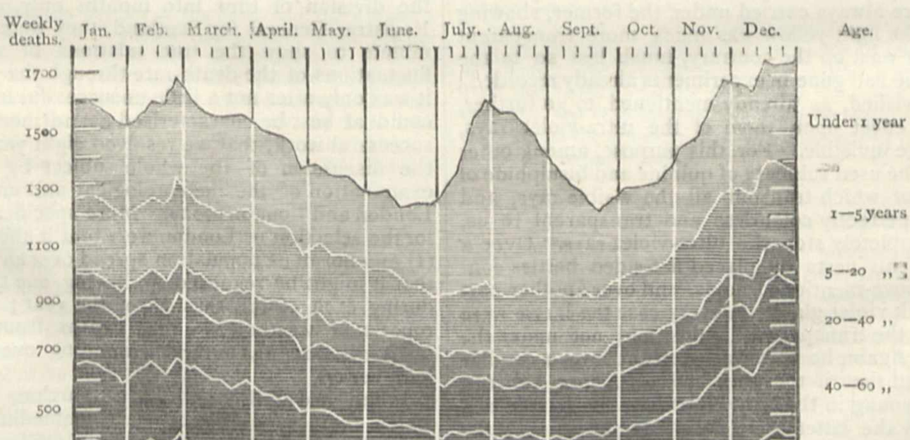

Fig. 4.

Fig. 2 shows similarly the distribution of the mortality from whooping-cough through the weeks of the year, and Fig. 3 the distribution of the mortality from small-pox. It is seen at once that the mortality curve from scarlatina is precisely the reverse of the curve of whooping-cough, the maximum death-rate period of the one corresponding to the minimum period of the other, and *vice versâ*. It is also seen that the mortality curve for small-pox (Fig. 3) is quite distinct from the other two curves.

In order to ascertain the degree of steadiness of these curves, a curve was calculated and drawn for each of the seven epidemics of scarlatina and for each of the

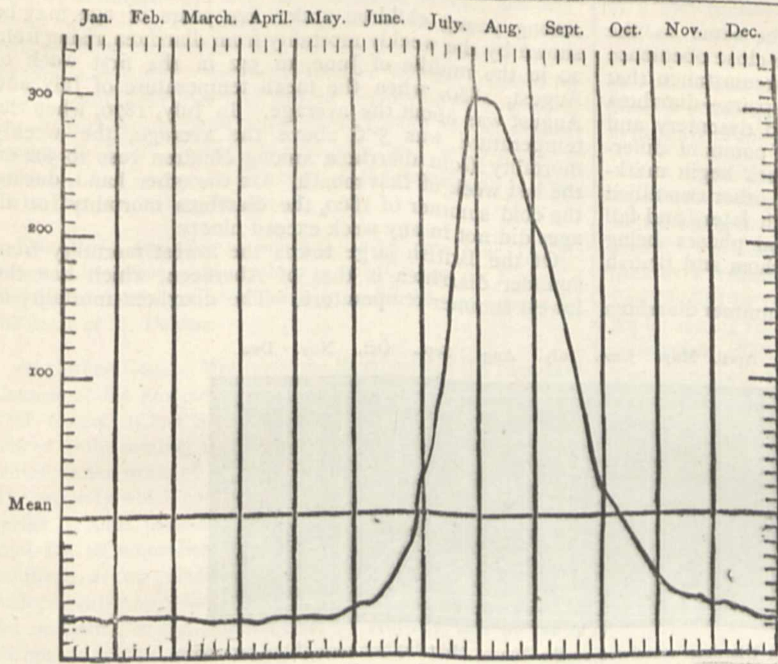

FIG. 5.—Diarrhœa.

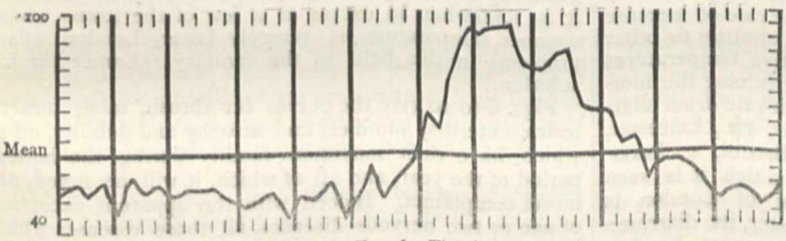

FIG. 6.—Thrush.

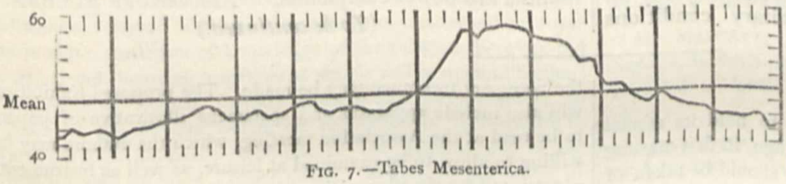

FIG. 7.—Tabes Mesenterica.

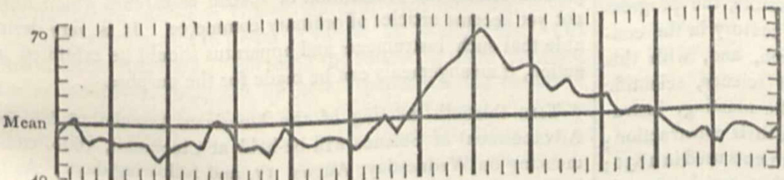

FIG. 8.—Enteritis.

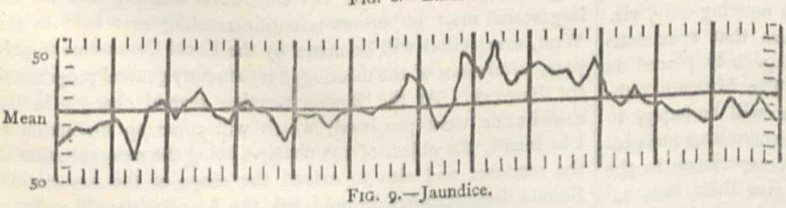

FIG. 9.—Jaundice.

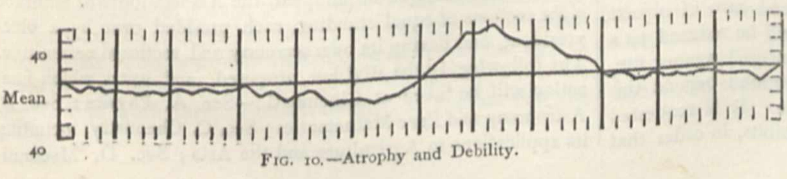

FIG. 10.—Atrophy and Debility.

eight epidemics of whooping-cough during the thirty years, with the instructive result that the curve for each of the separate epidemics was substantially identical with the general curve for the whole thirty years' period, each of the four prominent phases of each curve occurring all within a week of each other. As regards the small-pox curve, if the deaths during the epidemic of 1870-72, by far the most fatal of all the epidemics during the thirty years, be deducted from the general result, we obtain a curve which is substantially the same curve as that for the whole thirty years, but only less pronounced. From these results it follows, and the remark is of general application to all the curves, that the mortality curves for the different diseases arrived at in this inquiry may be regarded as true constants of these diseases for London.

The climate of London, looked at as influencing the health of the people, may be divided into six types of weather according to the season of the year. These are respectively—

Period 1.—Damp and cold, fourth week of October to third week of December.

Period 2.—Cold, fourth week of December to third week of February.

Period 2.—Dry and cold, fourth week of February to second week of April.

Period 4.—Dry and warm, third week of April to third week of June.

Period 5.—Heat, fourth week of June to first week of September.

Period 6.—Damp and warm, second week of September to third week of October.

The outstanding features of the death-rate in its relation to the varying types of weather through the year are shown by the top curve of Fig. 4, which represents the total mortality for all ages. This curve shows two maxima in the course of the year: the one, by far the larger of the two, extending over six months from November to April, and the other embracing the period from about the beginning of July to the autumnal equinox. It will be also observed that the comparatively short-continued but strongly-pronounced summer maximum is restricted to mere infants, whereas the larger winter maximum is a feature of the curves for all ages.

Figs. 5 to 10 are representative curves of those diseases which go to form the summer maximum when "*heat*" is the chief characteristic of the weather. The direct relation of the progress of mortality from diarrhœa to temperature is strikingly seen in the startling suddenness with which the curve shoots up during the hottest months of the year, and the suddenness, equally startling, with which it falls on the approach of colder weather. The curves for dysentery, British cholera, and cholera are substantially the same as the curve for diarrhœa, all showing the same close obedience to temperature. It is a noteworthy circumstance that these four curves group themselves into pairs—diarrhœa and British cholera on the one side, and dysentery and Asiatic cholera on the other. The chief points of difference are that dysentery and Asiatic cholera begin markedly to rise considerably later than the other two allied diseases, attain their maximum a month later, and fall more rapidly than they rose, the annual phases being nearly a month later than those of diarrhœa and British cholera.

The peculiarly malignant character of summer diarrhœa among young children under five years of age may be shown by the weekly mortality from diarrhœa, rising from 20 in the middle of June, to 342 in the first week of August, 1880, when the mean temperature of July and August was about the average. In July, 1876, when the temperature was 3°·6 above the average, the weekly mortality from diarrhœa among children rose to 502 on the last week of that month. On the other hand, during the cold summer of 1860, the diarrhœa mortality for all ages did not in any week exceed ninety.

Of the British large towns the lowest mortality from summer diarrhœa is that of Aberdeen, which has the lowest summer temperature. The diarrhœa mortality of

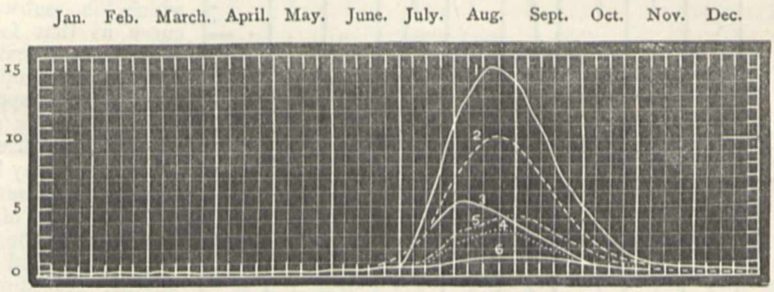

FIG. 11.—Weekly Deaths from Diarrhœa calculated on the Annual Mortality per 1000 of the population.

each town is found from year to year to rise proportionally with the increase of temperature, but the rate of increase differs greatly in different towns, thus pointing to other causes than mere weather, or the relative temperatures and humidities of these towns, as determining the mortality. Fig. 11 shows the weekly death-rate from diarrhœa for six of the large British towns, viz., Leicester, curve 1; Liverpool, 2; London, 3; Bristol, 4; Portsmouth, 5; and Edinburgh, 6; from which it is seen that though the summer temperature of London is hotter than that of Liverpool and Leicester, its diarrhœa mortality is very much less. In this respect London contrasts very favourably with the great majority of British large towns, showing its sanitary conditions generally are at least fairly satisfactory; but inasmuch as it is somewhat in excess of a few of the towns whose summer temperature is scarcely lower, London offers problems in this field to the sanitary reformer for his solution.

Figs. 6 to 10 give the curves for thrush, tabes mesenterica, enteritis, jaundice, and atrophy and debility, all of which have their maximum fatality during the hottest period of the year, and all of which, it will be noted, are bowel complaints. Indeed with the apparent exception of one or two nervous diseases, all those diseases which indicate an increase in their death-rate during the summer months are bowel complaints. ALEXANDER BUCHAN

(*To be continued.*)

NOTES

THE British Association having decided to hold its annual meeting this year at York, where, fifty years ago, its first meeting was held, it has been thought that advantage should be taken of this jubilee meeting to show, as far as possible, the progress which has been made during the past half century in the construction of instruments of scientific research, and, with this view, it has been decided to invite men of science, scientific societies, and manufacturers to exhibit, at the meeting, instruments of the latest patterns, and tools used in their construction, and if the science be fifty years old, the instruments used in 1831; otherwise specimens of the earliest patterns that can be found. The Exhibition will be for the week of the meeting only, viz. from August 31 to September 8. To ensure that specimens intrusted for exhibition shall be as advantageously placed as possible, a special sub-committee, called "The Museum Sub-Committee," has been formed at York, who will be happy to afford every possible information. The sub-committee includes several gentlemen who have had considerable experience in the arrangement of exhibitions, and they will give their personal attention to the unpacking, arrangement, and care of objects intrusted to them, so that the risk of injury will be reduced to a minimum. The articles exhibited will be insured against fire, and provision will be made for police protection; beyond this the committee does not hold itself responsible. It is requested that exhibitors will put a value on their exhibits, in order that the necessary insurance may be made. The proposed Exhibition will also include apparatus and specimens illustrative of papers to be read at the Association meeting, which the authors may be willing to allow to be examined at leisure, as well as instruments constructed for the prosecution of special researches which have not yet become articles of ordinary commerce. It is very desirable that such instruments and apparatus should be exhibited in action, if arrangements can be made for the purpose.

THE thirtieth meeting of the American Association for the Advancement of Science will be held at Cincinnati, Ohio, commencing on Wednesday, August 17, and following days. As it is generally believed that the Cincinnati meeting will be the largest and most important scientific meeting ever held in the West, every effort will be made by the Local Committee to prepare, in advance of the meeting, a satisfactory general programme for the week. At the Boston meeting several changes in the constitution were proposed, which will come up for action at Cincinnati, the object of the changes being the reorganisation of the sections, and also to extend the scope of the Association. Should these changes be adopted, the Association will embrace eight sections of equal standing, each presided over by a vice-president, and having its own secretary and sectional committee. The following is the division proposed, and upon which final action will be taken at Cincinnati:—Sec. A. Physics; Sec. B. Astronomy and Pure Mathematics; Sec. C. Chemistry, including its applications to Agriculture and the Arts; Sec. D. Mechanical Science; Sec. E. Geology and Geography; Sec. F. Biology; Sec. G. Anthropology; Sec. H. Economic Science and Statistics. Also I. A Permanent Subsection of Microscopy, which shall elect its own officers and be responsible directly to the Standing Committee. Several excursions will be arranged for by the Local Committee, and will be announced on their circular. Special excursions will be arranged for the Anthropological Section to Fort Ancient, Madisonville, and other places of interest. The Permanent Secretary of the Association is Prof. F. W. Putnam, Salem, Mass.

THE Paris Academy of Sciences elected, on June 13, M. Fouqué, Professor of Mineralogy to the Collége de France, a Member of the Section of Mineralogy, to fill the seat vacated by the death of M. Delesse.

AT the first General Meeting of the Members and the Honorary Council of the Sanitary Assurance Association on Friday last, Prof. Corfield (Chief Sanitary Officer) and Mr. Judge (Surveyor) related to the meeting the progress of the Association, and reported on the work of sanitary inspection that had been done. The property which had been placed on the Assurance Register varied in value from houses rated at 60*l*. a year, in which the total fee to subscribers for report, supervision of work, and certificate, is two guineas, to houses rated as high as 700*l*. a year, with proportionately increased fees. The Association undertakes the inspection of the smallest class of property, and no fee is charged to subscribers for a single house rated at 20*l*., while the fee is only half a guinea for houses rated at 40*l*. In the discussion which followed, Sir Richard Temple, Capt. Douglas Galton, and Mr. Whichcord spoke strongly in support of the objects of the Association, and the Council were requested to take steps to make the Association as widely known as possible, and particularly to call the attention of the proprietors of large building estates to the advantages which would accrue if the certificate of the Association was made essential to the granting of leases.

THE prize programme of the Belgian Academy of Sciences for 1882 consists of the following subjects (briefly stated):—Distribution between acids and bases, in mixture of solutions of salts which, by their mutual reaction, do not produce insoluble substances; *exposé* of present knowledge of torsion and improvement of it; extension of knowledge of the relations between the physical and chemical properties of simple and compound bodies; description of Belgian Tertiary strata of the Eocene series; influence of the nervous system on regulation of temperature in warm-blooded animals; relations of the pollinic tube to the ovum in one or more Phanerogams. Medals of the value of 600 francs are offered in connection with each question, except the third, for which the medal is valued at 1000 francs. The time-limit is August 1. For 1883 the following three questions are adopted:—1. Establish by new experiments the theory of reactions presented by substances in the nascent state. 2. Prove the truth or falsity of Fermat's proposition: To decompose a cube into two other cubes, a fourth power, and generally any power, into two powers of the same name, above the second power, is impossible. 3. New spectroscopic researches are required, showing especially whether or not the sun contains the essential constituents of organic compounds. A gold medal of 800 francs value is offered for solution of any one of these. The time-limit is August 1, 1883. Memoirs must be written in French, Dutch, or Latin, and sent in with motto and sealed envelope to the secretary.

WE learn that M. Planté, the inventor of the electrical accumulator, intends to organise a factory for the sale of his instruments. M. Planté considers himself obliged to take this step in order to show that the principles of his original apparatus are sufficient to work them with advantage. It appears that the Faure accumulators with oxide of lead cannot be loaded except by a battery, and that the original lead can be worked by a magneto-electric machine, by taking some precautions which will be described shortly. M. Plante is constructing for M. Tissandier an accumulator on his original system, which will be used to direct a small elongated balloon. It is intended to exhibit it in the nave of the Palais de l'Industrie in August next.

OUR Paris Correspondent writes: On June 10 an interesting experiment took place in Paris. A little after midnight a tram-car belonging to the Omnibus Company conveyed forty persons from the Place du Trone to the Boulevard Richard Lenoir and back at a velocity of six miles an hour. The motive power was supplied by 160 Faure accumulators, weighing 18 lbs. each. An interesting feat was accomplished, but not quite such as was anticipated. The work could have been done by two horses. The experiment lasted about one hour, and the power of the motor, although not exhausted, was much diminished.

M. JOSÉ CUSTODIO, Marinha Grande, Leiria, Portugal, writes to say that the centenary of the death of the great Portuguese Minister, the Marquis of Pombal, is to be celebrated on May 8 of next year. In connection therewith it is desired to obtain information about Williams Stephens, who founded the first royal manufactory of glass in Portugal, under the patronage of Pombal. Any information whatever concerning Stephens will be welcomed.

WE would draw the attention of our readers to the announcement of the first general meeting of the Society of Chemical Industry on the 28th and 29th inst., at the Institution of Civil Engineers, with Prof. Roscoe in the chair.

THE Portfolio of Drawings of Living Animals and Plants issued by Mr. Thomas Bolton for June, 1881, is a very creditable production, and we are glad to call our readers' attention to the opportunity there is afforded to them by the labours of Mr. Bolton, of investigating fresh and living specimens of very many interesting forms of animal and vegetable life—for the most part of quite microscopical size—and at the same time of having, by the drawings which accompany these forms, an excellent sketch of what they are to expect to find, and a short but authentic history of what is known about them.

M. MASCART, the director of the French Meteorological Service, is devising a new registering magnetometer, which is intended to have all the indications recorded on one roll of paper.

AN earthquake shock was felt in Switzerland on Thursday morning last. The shock occurred at 12.35 a.m. The direction was south-west to north-east at Geneva, and north-east to to south-west at Lausanne, Martigny, and Bex. Prof. Morel of Morges describes it as having been for one region very intense; it was felt from Martigny and Bex, in the Valais, to the valley of Joux, in Vaud; at Geneva, Chamounix, and all round Lake Leman. Its centre was probably in the valley of the Upper Rhone, where seven or eight oscillations were distinctly perceived, accompanied in many places by subterranean thunder, bells were rung, walls cracked, slates dislodged, and chimneys overturned. It was also felt at Osmondo, notwithstanding the great height of the village above sea-level. A second shock was felt some hours later in the same locality.

INTELLIGENCE received at Constantinople on June 9 from Van states that an earthquake has devastated thirty-four villages in that district. Another shock of earthquake occurred at Chios at half-past nine on Saturday morning, causing the fall of a Turkish minaret and of several ruined houses in the town. Oscillations of the ground are constantly noticed in Croatia. Thus on May 19 at 2 a.m. a violent shock, lasting three seconds, and accompanied by subterranean noise, was observed at Glina.

The shock was felt at Agram also, and at several other Croatian towns. On May 23, at 8.21 p.m., an earthquake was observed at Metkovich (Dalmatia). It lasted eight seconds, its motion was wave-like, and in a south-westerly direction; at 9.45 p.m. a second one followed. At Janina no less than seven different shocks were noticed on the same day; they varied considerably in strength, the first one occurred at 10.15 a.m., the last one at 10.57 p.m. All were accompanied by subterranean noise. At Stagno two violent shocks occurred at 8.23 and 9.3 p.m., and at Slano a moderate one at 8.35 p.m. On May 22 at 6.15 p.m. an oscillation of the ground was observed at Zwickau (Saxony); the direction was from north-east to south-west. On May 21 a moderate shock was noted about 11 p.m. at Copenhagen and in the vicinity. It lasted six seconds.

MR. W. SOWERBY, writing from the Botanical Gardens to the *Times*, states that the fresh-water jelly-fish described in NATURE a year ago by Professors Allman and Ray Lankester, has reappeared in the Victoria Regia tank in the Gardens. It is a curious fact that the date of its first discovery (June 9, 1880) should be so near the day of its reappearance—viz. June 12; as during the winter the tank is cleaned out and remains for some months empty.

THE numbers of the present year's issue of our northern namesake, *Naturen*, under the recent editorship of the eminent Norwegian geologist, Hans F. Reusch, continue to provide well written popular expositions of scientific questions. Dr. Leonhard Stejneger returning to a subject which he had treated of in early numbers, considers the causes which influence the migration of birds, which he is disposed to seek principally in the necessity originally imposed on earlier generations to seek food by change of locality, when the cold in one region, and the heat in another, destroyed the smaller animals, or the plants, from which these birds sought their nourishment, while the sense once developed became in process of time an hereditary instinct. The editor describes the working of the telephone system in Christiania, where, since June, 1880, a central station, in which the work is done by women, has been established in connection with Dr. Bell's Company in New York, and under the direction of Herr Hugo Ullitz. The apparatus used is the so-called Blake's microphone. Herr Geelmuyden draws attention to the expediency of adopting one mean time, viz., that of Christiania, for all Norway. The difficulty of establishing one normal time for the whole country is especially great in Norway, where, for instance, some districts—as Vardö and Vadsö—lie further east than Constantinople, while the west coast has nearly the same W. long. as Marseilles. As one of the curious results that would follow the adoption of the time of Christiania as the normal standard he mentions that the midnight sun at the North Cape would have to be looked for at 11 P.M. A colossal pine which was lately uprooted by an inundation at Pÿhajoki in Oulais, Finland, was found to have 1029 annual rings. The Norwegian Arctic Expedition has yielded a new fish bearing affinity with the Ophidiidæ, but presenting sufficient differences to justify its recognition as a hitherto unknown northern form, for which Dr. R. Collett has suggested the name *Rhodichthys regina*. The entire yield of fish in the trawl-nets at great depths (from 1300 to 1400 fathoms) was 234 individuals, belonging to thirty-two different genera, of which seven had been previously unknown to science.

THE deaths are announced of Dr. Jakob Bernays, Principal Librarian at the Bonn University, and of Dr. Richard Ladislaus Heschl, Professor of Pathological Anatomy at the University of Vienna (the successor of Rokitanski). Both were fifty-seven years of age, and both died on May 26.

THE Highbury Microscopical and Scientific Society gave a *conversazione* at Harecourt Hall, Canonbury, on Thursday, the 9th inst., which was numerously attended.

THERE is a regular mania in Paris at present for publishing periodicals connected with electricity. A new electrical weekly paper called the *Telephone* has issued its first number; it is the fifth in existence. We are told moreover that the first number of another, the *Electrophone*, will be issued in a very few days.

MR. HENRY WALKER has issued a useful little "Guide to the Popular Natural History Societies of London." In London and suburbs there are twenty such associations.

THE *conversazione* to commemorate the fiftieth anniversary of the Harveian Society of London will be held on Wednesday, June 29, at the South Kensington Museum.

EXPERIMENTS have been made during the past few days in lighting the House of Commons by means of the electric light.

SEVEN solar lamps were lighted by electricity about a week ago in Paris by a Siemens machine, situated in the *mairie* of rue Drouat. These lamps, which are perfectly regular, and placed in the most crowded part of the Paris Boulevards, near Passage Jauffroy, have created a sensation.

THE concerts of the Palais Royal will be resumed in a few days. The gardens will be lighted by no less than eighteen Jablockhoff lamps. It is intended to place a miniature electric boat on the basin manned by a little girl.

THE annual Congress in connection with the French Society of Archæology will be opened on June 28 at Vannes (Morbihan). A long and interesting programme has been prepared for the meeting.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*) from South Africa, presented by Miss Agnes Robertson; a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mr. Hamilton Kerr; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr. H. Aylesbury, steam yacht *Albion*; a Common Ocelot (*Felis pardalis*) from America, presented by Mr. P. Leckie; two Common Peafowls (*Pavo cristatus*) from India, presented by Mr. George Stevenson; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from the Moluccas, presented by Miss Rose Hubbard; three Waxwings (*Ampelis garrulus*), European, purchased; a Cape Buffalo (*Bubalus caffer*), a Japanese Deer (*Cervus sika*), born in the Gardens; nine Summer Ducks (*Aix sponsa*), a Jameson's Gull (*Larus jamesoni*), bred in the Gardens.

GEOGRAPHICAL NOTES

AT its last meeting the Russian Geographical Society announced the nomination of M. Yurgens as Director of the Polar Meteorological Station on the Lena, and of M. Eichner and Dr. Bunge as his assistants. The Society also voted sums of money for sending M. Kouznetsoff for the anthropological exploration of Tartarian tribes and for M. Malakhoff, who goes to the provinces of Vyatra and Oufa for the exploration of caverns and of remains of former settlements.

WE find in the *Izvestia* of the Russian Geographical Society the following information as to the geodetical work which was done by the Russian officers on the Balkan peninsula during the last war. The whole of Bulgaria and Eastern Roumelia was covered with a net of trigonometrical triangles, as well as the portion of Turkey between Adrianople, Dede-agatch, and Rodosto, and from Yambol, through Adrianople, to Constantinople and Bourgas. The net goes also into Servia and along the Danube, the total number of geodetically-determined spots being 1289; for all these spots there were also made determinations of heights. The highest determined summits on the Balkan mountains are Youmroutchkal (7791 feet), and Vajan (6217 feet); and in the Rhodope Mountains: Karlyk (9846 feet), Karlyk-moolah and Sutka (both 7189 feet high). The longitudes of eleven principal towns (Rouschouk, Sistova, Tirnova, &c.) were determined with great accuracy, and those of fifty-seven others either by telegraph or by chronometers, and they were chosen in such a manner as to determine the influence of the Balkan chain on the deviation of the pendulum. As to the topographical work, no less than 133,750 square versts were mapped during the war, of which 110,500 square versts were mapped on the scale of $\frac{1}{42000}$, and the heights of more than 110,000 spots were determined, so that there are all necessary data for making an embossment-map of the whole of the mapped parts of Roumelia and Bulgaria.

DR. RAE sends us the following extract from a letter to him from Capt. Howgate, dated May 23, 1881:—"Our Arctic work here is progressing finely, so far as ou fitting is concerned. The *Jeannette* search vessel is to get off early in June, if she fills up her complement of men. For our Lady Franklin Bay work the steam sealer *Proteus* of St. John's, Newfoundland, has been secured. She is a vessel of 688 tons burden, and contracts to deliver, with the colony and supplies, one hundred tons of coals at Lady Franklin Bay, which will guard against failure in the item of fuel, should the coal-seam not turn out so well as expected. The complement of men has been made up, and the shipment of stores to St. John's actually commenced, so there is every reason to expect that the expedition will sail from that port on July 4, as originally intended. The Point Barrow party is nearly filled up, and will be finished this week, I believe."

A SHORT time back it was stated that Mr. James Stevenson of Glasgow had offered to contribute 4000*l*. on certain conditions for the construction of a road between Lakes Nyassa and Tanganyika. The Foreign Missions Committee of the Free Church of Scotland have resolved to do their part by establishing a station among the Chungus at Maliwanda, a place about fifty miles on the proposed line of road from Lake Nyassa. The London Missionary Society have agreed to open a station at Zombé, twenty miles to the south-east of Lake Tanganyika. In order to found the Livingstonia Mission's new stations and superintend the construction of the road, Mr. James Stewart, C.E., left England on May 13 with three artisans, and another is to follow. In the autumn also it is probable that another medical missionary will go out to Lake Nyassa.

BY the aid of a correspondent at the Gaboon who wrote on March 30 a contemporary has received the startling intelligence that M. de Brazza "got to Stanley's Pool from the Ogowé and came down the Congo." Some people however may be aware that this information was made public at the meeting of the French Geographical Society on January 21, when M. Duveyrier tried to make quite clear what is evidently not yet known at the Gaboon, viz. that after he had founded the Ogowé and Stanley Pool stations and descended the Congo, the mission confided to M. de Brazza by the French branch of the International African Association ceased, and the two stations, it is well known, are to be taken charge of by M. Mizon and another Frenchman. M. de Brazza is now engaged on an expedition for which the French Chambers have made a liberal grant, and in which he will be accompanied by his former colleague, Dr. Ballay. These two are to descend the Alima to the Congo in a steam launch, and then to make a thorough examination of the valley of the great river, part of their object being to divert trade to some extent to the Ogowé. The writer of the letter from the Gaboon believes that "Stanley will find de Brazza established there [? Stanley Pool] when he gets up." This of course is a matter of chance, as M. de Brazza has now a sort of roving commission on the Congo, but, no doubt, Mr. Stanley will find some one at the Ntamo station (now called Brazzaville), as Messrs. Crudgington and Bentley in February found a French sergeant and two soldiers there, and by this time possibly M. Mizon or some one else will have arrived to take charge.

IN consequence of the success of the preliminary journey which Mr. Crudgington and his companion have just made along the north bank of the Congo to Stanley Pool, the Baptist Missionary Expedition will now definitely adopt this route into the interior. As the result of a long conversation with Mr. Stanley on the subject, the party consider that it will be best to take advantage of his road as far as Isangila, and then to place a steel boat on the river above the falls there. Afterwards there will be no insuperable difficulty in the navigation of the river, except perhaps in two or three places where the boat will be taken to pieces and carried past the cataracts. A boat is now being built for the expedition in London from the plans and drawings of Mr. Stanley, who has willingly afforded the party the benefit of his advice and assistance. The adoption of this plan will obviate the necessity for passing through the country of the troublesome Basundi, and will materially hasten the progress of the expedition.

HERR ERNST VON HESSE WARTEGG, the well-known traveller, has just returned to Europe from Africa, where he went up the Nile, and then crossed the desert between that river and the Red Sea, making important excavations and discoveries of ancient Egyptian remains, among which were a very interesting sarcophagus, pottery, statuary, &c. He recently gave a lecture before the Geographical Society of Alsace-Loraine at Metz, exhibiting several hundred photographs and ethnographical objects. Some time ago Herr von Hesse Wartegg was elected Honorary Member of the Royal Belgian Geographical Society and Corresponding Member of the Geographical Society of Metz. His travelling companion, Dr. Theodor Hoerner, has gone from Suakin to Kassalla, and from there through the Kunama country to Massawah.

IN the *Colonies and India* we find some particulars respecting a projected expedition from New Zealand to New Guinea for the purpose of exploration and eventual colonisation. The promoters two years ago made a preliminary voyage there in the *Courier*, which for various reasons was not particularly fortunate, but from their past experience they now feel certain of success. The *Courier* then visited Astrolabe Gulf, on the north-east coast, and the natives were found very tractable and disposed to trade. Scented woods were met with in abundance, and tobacco and sugar were seen under cultivation. Mr. R. Mills, who was with this expedition, has brought away with him numerous views taken on the spot, which give a good idea of the natives and the aspect of the country.

LETTERS have been received at Vienna from the African traveller, John Freiherr von Müller. He intends to penetrate into the district south of Fazoglu and Fadazi, which hitherto have never been visited by any European. The geographical problems to be solved in these parts are the discovery of the bifurcation of the Sobat River (a tributary on the right bank of the White Nile), which was suspected by Karl Ritter, and also the discovery of the problematical Zamburu and Baringo lakes. The general circumstances in these districts do not justify the hope of success being oversanguine; yet Freiherr von Müller hopes safely to reach the Indian Ocean at Mombassa or Bagamoyo on his return journey.

ON Monday last week Dr. Ave-Lallemant delivered a lecture to the members of the German Athenæum, Mortimer Street, on the Orinocco River. The lecturer spoke mainly from personal observation, and the lecture was a highly interesting one.

A GENERAL Congress of German geographers, presided over by Dr. Nachtigal, met in Berlin last week. The second volume of Dr. Nachtigal's work on the Sahara and Sudan is expected shortly.

LIEUT. BOVE has just returned from the Argentine Republic, where he has been making arrangements for the projected expedition to the Antarctic regions. The Geographical Institute of the Argentine Republic has unanimously voted 2000 scudi for the enterprise. As soon as the Italian Government has arranged the diplomatic affairs of the expedition with the Argentine Republic, Lieut. Bove will return to Buenos Ayres.

HEFT VI. of *Petermann's Mittheilungen* commences with an interesting article on the Greatest Quantity of Rainfall in One Day, by Dr. H. Ziemer. Letters from Dr. Junker give interesting details concerning his sojourn in the Niam-Niam country, and an article, with map, on East Griqua Land and Pondo Land brings together recent information on these regions. Another article gives the leading results of some recent journeys in Arabia.

NO. 4 of the *Mittheilungen* of the Vienna Geographical Society contains an account, by Dr. Emin Bey, of his journeys in the Upper Nile Region; and Joh. Ritter Stef. v. Vilnovo has a long paper on the side-courses of rivers. In No. 5 Dr. Holub has a useful paper on the industrial aspect of Austrian exploration; Dr. Jettel writes on the scientific exploration of Bosnia and Herzegovina; and Lieut. Kreitner on the Ainos.

THE murder is reported of an Italian exploring party in the Danakil country. According to the latest advices from Aden, the party was composed of the traveller Giulietti, and an escort furnished by the commander of the vessel stationed at Assab. The party, whose object is stated to have been scientific and commercial, left Beilul last April to explore the source of the Gualima. Four days distance from that town they were attacked and slain by the natives. Signor Giulietti was well known for the difficult journey he accomplished from Zeila Hazar. He was asked by the Geographical Society to explore the interior of the west coast of the Red Sea. At first a journey to Lake Aussa was contemplated, but obstacles arising, the plan was changed for an expedition into the Assab Gallas country.

AT the meeting of the Geographical Society on Monday last Capt. W. J. Gill, R.E., read some extracts from a long account of his explorations in Western Szechuen, which has lately been sent home by Mr. E. Colborne Baber, now Chinese Secretary of H.M. Legation at Peking. The extracts chosen dealt chiefly with the amusing side of Mr. Baber's journey, but the paper, nevertheless, contains abundance of solid information respecting the extreme west of China, and, as Lord Aberdare stated in his anniversary address, is considered by competent judges to be a noteworthy contribution to our knowledge of Asiatic geography. The most valuable part of the extracts read is probably that respecting the almost unknown Lolo country, in the neighbourhood of Ning-yüan-fu. Mr. Baber sent home copies of some pages of a Lolo manuscript, no specimen of which, we believe, has ever been seen in Europe before. These have been submitted to the well-known scholar, M. Terrien de la Couperie, who gave the meeting a brief account of the results of his examination of them. Mr. Baber's paper will be published by the Society, together with the valuable cartographical matter which accompanied it.

M. AND MADAME UJFALVY were to leave Simla for Kashmir, *viâ* Kangra, on June 6. From Kashmir they hope to penetrate into Thibet and Central Asia.

THE death is announced of Mr. Andrew Wilson, author of a well-known book of travel in the Himalayas, "The Abode of Snow."

SOLAR PHYSICS—CONNEXION BETWEEN SOLAR AND TERRESTRIAL PHENOMENA [1]

II.

IN my last Lecture I alluded to the complicated periodicity which sun-spots exhibit. It is right here to quote the remark of Prof. Stokes, that until we have applied to solar phenomena a sufficiently rigid analysis we are not certain that this apparent periodicity will bear all the marks of a true periodicity. It cannot however be denied that solar phenomena are roughly periodical, and this apparent periodicity has influenced observers in their attempts to search for a cause. There have been two schools of speculators in this interesting region, consisting of those who imagine a cause within the sun, and of those who imagine one without. The former *may* be right, but apparently they cannot advance our knowledge much. We know very little of the interior of the sun, and no one has yet ventured on any hypothesis regarding the *modus operandi* by which these strangely complicated and roughly periodical surface phenomena may be supposed to be produced by the internal action of the sun itself.

Those who maintain the hypothesis of an internal cause are apparently driven to it by the *à priori* unlikelihood of any cause operating from without. No doubt we have around the sun bodies, the motions of which are strictly periodical, such as planets, comets, and meteors, but they are relatively so small and so distant, that it seems difficult to regard them as capable of producing such vast phenomena as sun-spots.

There is however this difference between the two hypotheses —those who assert internal action cannot convert their views into a working hypothesis. On the other hand, those who look to external sources can take the most prominent planets, for instance, and endeavour to ascertain whether as a matter of fact the behaviour of the sun with regard to spots is apparently influenced by the relative positions of these. Attempts of this nature have been made by Wolf, Fritz, Loomis, Messrs. De La Rue, Stewart, and Loewy, and others. These attempts have been of two kinds. In the first place observers have tried whether there appear to be solar periods exactly coinciding with certain well-known planetary periods. By this means the following results have been obtained by the Kew observers (Messrs. De La Rue, Stewart, and Loewy):—

(1) An apparent maximum and minimum of spot energy approximately corresponding in time to the perihelion and aphelion of Mercury.

(2) An apparent maximum and minimum of spot energy approximately corresponding in time to the conjunction and opposition of Mercury and Jupiter.

(3) An apparent maximum and minimum of spot energy approximately corresponding in time to the conjunction and opposition of Venus and Jupiter.

(4) An apparent maximum and minimum of spot energy approximately corresponding in time to the conjunction and opposition of Venus and Mercury.

Mr. De La Rue and his colleagues make the following remarks upon these results:—

"There appears to be a certain amount of likeness between the march of the numbers in the four periods which we have investigated, but we desire to record this rather as a result brought out by a certain specified method of treating the material at our disposal, than as a fact from which we are at present prepared to draw conclusions. As the investigation of these and similar phenomena proceeds it may be hoped that much light will be thrown upon the causes of sun-spot periodicity."

I may here mention that within the last month I have, in conjunction with Mr. Dodgson, applied a method of detecting unknown inequalities with the view of seeing whether there are any indications of an unknown inequality in sun-spots having a period near that of Mercury, and I find there are indications of such an inequality having a period which does not differ from that of Mercury by more than about three-hundredths of a day. Besides the four periods above mentioned the Kew observers have, they think, detected evidence of a periodicity in the behaviour of spots with regard to increase or diminution depending apparently on the positions of the two nearer planets, Mercury and Venus. The law appears to be, that as a portion of the sun's surface is carried by rotation nearer to one of these two influential planets, there is a tendency for spots to become less and disappear, while on the other hand when it is carried away from the neighbourhood of one of these planets there is a tendency for spots to break out and increase.

The Kew observers regard this latter species of evidence as being well worthy of a more exhaustive discussion when the sun-spot records are more complete. I have already mentioned that the chief difficulty in attributing solar outbreaks to configurations of the planets is the comparative smallness and great distance of these bodies, so that when we reflect on the enormous amount of energy displayed in a sun-spot we cannot but have great difficulty in supposing that such vast phenomena can be caused by a planet like Venus, for instance, that is never as near to the sun as she is to the earth. But this difficulty depends very much on what we mean by the word "cause." If we mean that the planets cause sun-spots in the way in which the blow of a cannon-ball or the explosion of a shell causes a rent in a fortification, the hypothesis is certainly absurd. But if we only mean that the planets act the part of the man who pulls the trigger of the gun, the hypothesis may be unproved, but it is no longer absurd. For we have reason to believe that there may be great delicacy of construction in the sun's atmosphere, in virtue of which a small cause of this kind may produce a very great effect.

We may therefore believe it possible that planets may act in this way on the sun—the energy displayed in a spot being however not derived from the planets, but from the sun itself, just as the energy of a cannon-ball is not derived from the man who pulls the trigger, but from the explosion in the gun.

All this is chiefly historical, and it leads to a very interesting query. If there is such an action of a planet on the sun, must not this have a reaction? If the earth influences the sun, must not the sun simultaneously influence the earth? Perhaps so; nevertheless it is not an influence of this kind which I shall now bring before you. The sun is periodically stirred up—no matter how—and being stirred up there is an increase in the light and heat which are radiated to the earth. This affects the meteorology of the earth, and also its magnetism, after a method which, if we do not fully understand it now, we may ultimately expect to comprehend. It is this kind of influence, and not an occult action, of which I shall now bring the evidence before you

[1] Lecture in the Course on Solar Physics at South Kensington; delivered by Prof. Balfour Stewart, F.R.S., April 27. Continued from p. 117.

And first of all let me speak of the sun's influence on the magnetism of the earth.

Suppose that the chief observatories of the world have each a vault, and that in this vault a magnetic needle is delicately suspended. We may imagine the sun to be shut out altogether, the only light being that of a lamp which enables us to record, either photographically or otherwise, on a magnified scale any small oscillations of the needle. The vault may be supposed to be sufficiently deep down to be practically uninfluenced by the heat of the sun, so that it will exhibit no difference in temperature between noon and midnight. Finally there must be no iron or steel about the place, or anything which might affect the needle. Now under these circumstances you would naturally imagine that the needle would be perfectly stationary, always pointing in the same direction. Such, however, is not the case—it does not move very greatly, but nevertheless it does move, and its position depends on the hour of the day, or, in other words, upon the sun. The sun cannot heat the chamber in the least, nevertheless it can influence the magnet, and we might even tell in a rough way the hour of the day by noting the position of the needle. In this country the needle attains one extreme in its daily progress about five or six in the morning, and the other about one or two in the afternoon, and the difference in position of these two extremes is called the diurnal range of magnetic declination. Here then we have a magnetical phenomenon which depends upon the sun, and which does not take place simultaneously at the various observatories of the earth, inasmuch as the sun travels from east to west, so that when it is six in the morning at one place it may be midnight at another.

In the next place, we have abrupt magnetical changes analogous to the well-known abrupt meteorological changes, and bearing the appropriate name of magnetic storms.

A magnetic storm is not a mere local outbreak, but is felt simultaneously at all the various points of the earth's surface. The various needles in the various vaults of which we have now been speaking will all be affected at the same moment of time, and will be found to be oscillating backwards and forwards in a disturbed state. It thus appears that diurnal ranges and magnetic storms are two distinct phenomena.

To begin with diurnal declination ranges. These have, as their very name implies, a connexion with the hour of the day, and hence with the position of the sun. Again, in middle latitudes declination ranges are greatest in summer when the sun is most powerful, and least in winter. Lamont was the first to observe the signs of a long period inequality in the yearly means of the Munich diurnal declination ranges, and in 1852 Sir Edward Sabine succeeded in showing that this inequality followed that of sun-spots previously discovered by Schwabe, maximum ranges corresponding to years of maximum sun-spots, and minimum ranges to years of minimum sun-spots. In the same year Dr. Wolf and M. Gautier independently remarked the same coincidence.

But there is more than a mere general correspondence between these two phenomena, for it is believed that all inequalities of sun-spots, whether of long or short period, are accompanied by corresponding changes of declination range, a large range invariably accompanying a large number of spots. Perhaps I ought to say a large range following a large outbreak of spots, for the solar phenomenon leads the way and the magnetic change follows after it at a greater or less interval of time. I may add, likewise, that we have some evidence which leads us to suspect that particular states of declination range, like particular states of weather, have a motion from west to east, the magnetical weather moving faster than the meteorological. From a preliminary investigation which I have made, I even think there may ultimately be a possibility of forecasting meteorological weather by means of magnetic weather five or six days before. It will be noticed, that as far as declination range is concerned, we have no evidence of a direct magnetic action of the sun upon the earth, but we have, on the other hand, evidence that the magnetic effect, like the meteorological, lags behind the cause in such a way that we are inclined to attribute the magnetic as well as the meteorological phenomena to the heating effect of the sun's rays.

Let us next take magnetic storms. These, as we have seen, affect the various stations simultaneously, so that the magnetism of the earth appears to change as a whole, and in this respect they are very different from the ordinary diurnal oscillations of the needle. Nevertheless, equally with declination ranges, magnetic storms appear to depend on the state of the sun. In 1852 Sir Edward Sabine showed that in those years when there are most sun-spots there are most magnetic storms, while, on the other hand, years of minimum sun-spots correspond to a minimum number of such storms. The late John Allan Broun, an eminent magnetician, has given reasons for believing that the greater magnetic disturbances are apparently due to actions proceeding from particular meridians of the sun; this when verified will be a fact of the greatest importance.

Again, Prof. Loomis of America, from a discussion of 135 cases of magnetic disturbance, concludes that great disturbances of the earth's magnetism are accompanied by unusual disturbances of the sun's surface on the very day of the magnetic storm. It might at first sight be thought from this last observation that a magnetic storm is due to some direct magnetic influence propagated from the sun to the earth, and accompanying a rapid development of spots, the influence being thus very different from that which may be supposed to cause variations in the magnetic range. But I do not see that this result follows from Prof. Loomis' observations. There is, I think, evidence that the earth before a magnetic storm is in a critical magnetic state—out of relation to its surroundings—and hence a sudden solar outburst may be the immediate occasion of its starting off. But I fail to see any evidence that the influence received from the sun on such occasions is different in kind to that which affects magnetic ranges. For we know that magnetic storms occur most frequently about the equinoxes, or at those times when the sun is crossing the equator. Now were a magnetic storm produced by a magnetic influence immediately proceeding from the sun, it would be difficult to understand why there should be any marked reference in magnetic storms to certain months of the year.

When the magnetism of the earth is in a disturbed state this may of course be rendered visible by means of the oscillations of a delicately suspended magnetic needle. Nevertheless there are associated phenomena of a very conspicuous character which vividly impress us with the reality of the occurrence. One of these is the aurora—displays of which invariably accompany considerable magnetic storms, on which occasions they may be witnessed over a large portion of the globe.

Another of these is the earth currents which on such occasions affect all telegraphic lines connected with the earth. These earth currents are automatically registered at Greenwich by the Astronomer Royal, and their peculiarity is that during magnetic storms they are very violent, passing rapidly and frequently backwards and forwards between positive and negative.

We come now to the meteorological effects produced by the variable state of the sun's surface. More than ten years ago Mr. Baxendell of Manchester pointed out that the convection currents of the earth were apparently altered by the state of the sun's surface, and since that time this peculiar connexion between the sun and the earth has been investigated on an extensive scale by observers in various portions of the globe. Dr. Charles Meldrum of the Mauritius Observatory was one of the first pioneers in this important branch of inquiry. In 1872 he showed that the rainfalls at Mauritius, Adelaide, and Brisbane were greater generally in years of maximum than in years of minimum sun-spots. Shortly afterwards Mr. Lockyer showed that the same law held for the rainfalls at the Cape of Good Hope and Madras. Dr. Meldrum afterwards took twenty-two European observatories, and found that the law held in nineteen out of the twenty-two. It would however appear from the observations of Governor Rawson that at Barbadoes, and perhaps other places, the maximum rainfall does not coincide with the maximum sun-spot years. As locality has a very great influence upon rainfall, it might be supposed that by measuring the recorded depths of water in large rivers and lakes we should be able to integrate the rainfall over a large area, and thus avoid irregularities due to local influence. This too has been attempted. In 1873 Gustav Wex examined the recorded depths of water in the Elbe, Rhine, Oder, Danube, and Vistula for the six sun-spot periods from 1800 to 1867, and came to the conclusion that the years in which the maximum amount of water appeared in these rivers were years of maximum sun-spots, while the minimum amounts of water occurred during the years of minimum sun-spots.

In 1874 Mr. G. M. Dawson, in America, analysing the fluctuations of the great lakes, came to a similar conclusion. This leads me to a very practical and important part of the subject. In countries such as ours we often suffer from excessive rainfall, and are rarely incommoded by excessive heat; but in hot countries such as India a deficient rainfall means a dearth, or even a famine.

This has been brought prominently before us of late years by Dr. Hunter, Director-General of Statistics in India, who has shown that famines are most frequent at Madras about the years of minimum sun-spots—years which were likewise associated with a diminished rainfall.

In summing up the rainfall evidence we ought to bear in mind that the direction as well as the intensity of the earth's convection currents is no doubt altered by solar variability. And if we at the same time reflect how very local rainfall is, we cannot expect that the same rule regarding it should hold for all the various stations of the earth's surface. But on the whole there appears to me to be evidence that we have most rainfall during most sun-spots. Of course we know little or nothing of variations in the rainfall at sea.

I have already mentioned that the magnetic storms of the earth are most frequent during years of maximum sun-spots, and the very same thing may be said of wind-storms. Dr. Meldrum has found that there are more cyclones in the Indian Ocean in years when there are most sun-spots, and fewest cyclones in years when there are fewest sun-spots. M. Poëy has proved a similar coincidence between the hurricanes of the West Indies and the years of maximum sun-spots, and I believe that a similar conclusion has been arrived at with regard to the typhoons of the Chinese seas.

In 1877 Mr. Henry Jeula of Lloyds and Dr. Hunter found that the percentage of casualties on the registered vessels of the United Kingdom was 17½ per cent. greater during the maximum two years than during the minimum two years in the common sun-spot cycle.

We may therefore imagine that the wind as well as the rain of the earth is most violent during years of maximum sun-spots.

We come now to the pressure of the air. If there were no sun the pressure of the air would ultimately distribute itself equally where it is now unequal. This inequality is no doubt caused by the sun, and we should expect it to be most pronounced when the sun has most power. It is also different in summer and winter. In summer we generally find a low barometer in the centres of great continents, and a high barometer over the sea; while during winter we have the converse of this, or a high barometer over continents and a low barometer at sea. I think it likely that the true relation between the variations of sun-spots and of barometric pressure will ultimately be discovered by means of the admirable weather-maps of the United States; meanwhile, however, especially in India, something has already been done in this direction.

If we regard the distribution of isobaric lines, that is to say of lines of equal barometric pressure, we shall find that the Indo-Malayan region is one which for the mean of the year has a barometric pressure probably below the average. Now during years of powerful solar action we might imagine that this peculiarity would be increased. But this is precisely what all the Indian observers have found for years with most sun-spots.

On the other hand Western Siberia in the winter season has a pressure decidedly above the average, and we should therefore imagine that during years of powerful solar action the winter pressure would be particularly high. This again is the state of things that Mr. Blanford has found in his discussion of the Russian stations to correspond with years of most sun-spots.

It therefore appears to me that the barometric evidence as far as it goes is favourable to the belief that years of maximum sun-spots are years of greatest solar power.

I come now to consider the question of temperature. Mr. Baxendell was the first to conclude that the distribution of temperature under different winds, like that of barometric pressure, is very sensibly influenced by the changes which take place in solar activity. In 1870 Prof. Piazzi Smyth published the results of observations made from 1837 to 1869 with thermometers sunk in the rock at the Royal Observatory, Edinburgh. He concluded from these that a heat wave occurs about every eleven years, its maximum slightly lagging behind the minimum of the sun-spot cycle. In 1871 Mr. E. J. Stone examined the temperature observations recorded during thirty years at the Cape of Good Hope, and came to the conclusion that the same cause which leads to an access of mean annual temperature at the Cape leads equally to a dissipation of sun-spots. Dr. W. Köppen in 1873 discussed at great length the connexion between sun-spots and terrestrial temperature, and found that in the tropics the maximum temperature occurs fully a year before the year of minimum sun-spots: while in the zones beyond the tropics it occurs two years after the minimum. The regularity and magnitude of the temperature wave is most strongly marked in the tropics.

The temperature evidence now given appears at first sight to be antagonistic to that derived from the other elements, both of magnetism and meteorology, and to lead us to conclude that the sun heats us most when there are fewest spots on its surface. This conclusion will not, however, be strengthened if we discuss the subject with greater minuteness. Scientifically, we may regard the earth as an engine, of which the sun is the furnace, the equatorial regions the boiler, and the polar regions the condenser. Now this engine works in the following manner. Hot air and vapour are carried along the upper regions of the atmosphere from the equator to the poles by means of the anti-trade winds, while in return the cold polar air is carried along the surface of the earth from the poles to the equator, forming what is known as the trade winds. Now whenever the sun's heat is most powerful, both trades and anti-trades should, I imagine, be most powerful likewise. But we live in the trades rather than in the anti-trades—in the surface currents, and not in the upper currents of the earth's atmosphere. When the sun is most powerful, therefore, is it not possible that we might have a particularly strong and cold polar current blowing about us? The same thing would happen in the case of a furnace-fire—the stronger the fire the more powerful the hot draught up the chimney—the more powerful also the cold draught from without along the floor of the room. It might thus follow that a man standing in the furnace room near the door might be chilled rather than heated when the furnace itself was roaring loudest. In fact temperature is a phenomenon due to many causes. Thus a low temperature may be due

(1) To a deficiency in solar power.
(2) To a clouded sky.
(3) To cold rain.
(4) To cold winds.
(5) To cold water and ice.
(6) To cold produced by evaporation.
(7) To cold produced by radiation.

Now Mr. Blanford, the Indian observer, has recently shown that a low temperature of the air and soil is accompanied in the stations which he has examined by a copious rainfall and by a large number of clouds. If therefore we regard a high rainfall as the concomitant of many sun-spots, we must not be surprised if this is sometimes accompanied with a low temperature, nor hastily conclude from this lowering of temperature that the sun is less rather than more powerful. Considerations of this nature have induced me to think that the true connexion between sun-spots and terrestrial temperature is more likely to be discovered by a study of short-period inequalities of sun-spots than by that of the eleven-year period in which there is time enough to change the whole convection system of the earth. I have accordingly discussed at some length two prominent sun-spot inequalities of short periods (about twenty-four days), and endeavoured to see in what way they affect the terrestrial temperature. From this it appears that a rapid increase of sun-spots is followed in a day or two by an increase of the diurnal temperature range at Toronto. Now an increase of diurnal temperature range surely denotes an increase of solar energy, and we are thus led to associate an increase of solar heat with a large development of spots.

I have thus brought before you a quantity of evidence, chiefly indirect, tending to prove that the sun's rays are most powerful when there are most spots. But you will naturally ask why I have not given you any direct evidence on this point. Is it not possible, you ask, to measure the direct heating effect of the sun's rays, so as to decide the question without further circumlocution? Now, strange to say, this has not been done.

We call an instrument that measures the sun's direct influence an actinometer, and I will now briefly allude to two such instruments, one for measuring the chemical effect of the sun's rays devised by Dr. Roscoe, and another for measuring the heating effect of the sun's rays, devised by myself. (The lecturer here described the mode of action of these actinometers.)

But the use of such instruments is rather a problem of the future than of the past. Hitherto it cannot be said that we have determined by actual observation whether the sun's rays are more powerful or less powerful at times of maximum sun-spots. I may, however, quote the actinometrical observations made in India at Mussooree and Dehra by Mr. J. B. N. Hennessey as confirming, so far as the evidence goes, the hypothesis of greater solar energy at maximum than at minimum epochs.

My trust is that for the future India will throw great light upon the problem we are now discussing. We have a distinguished meteorologist, General Strachey, as member of the Council of India, we have General Walker and the trigonometrical survey staff, and we have Mr. Blandford and the various meteorological and magnetic observers of India, and I am glad to think that neither solar nor actinometric observations are likely to be forgotten.

Let me now briefly recapitulate the conclusions we have come to.

In my first lecture I endeavoured to bring before you theoretical grounds for imagining that the sun is most powerful when there are most spots on its surface.

This has been supported by the evidence of a meteorological nature derived from these observations of rainfall, wind, barometric pressure, and temperature which have now been discussed, and likewise from such actinometric observations as have been made in Mussorree and Dehra. With regard to magnetical observations, we have the fact that diurnal declination ranges are largest in times of maximum sun-spots, and that on such occasions we have likewise a great number of magnetic storms, accompanied with earth currents and displays of the aurora. In fine we have most magnetic activity when there are most spots. There may perhaps be some doubt as to the exact method by which solar phenomena affect the magnetism of the earth, but we have already hypotheses from two distinguished physicists, the late Prof. Faraday and Prof. Stokes, while others have likewise been engaged in similar speculations.

Thus we may hope that eventually the truth will be attained. Meanwhile however we may conclude that the earth is most active both meteorologically and magnetically when there are most spots on the sun's surface. And if this be so, who will say that this is not a problem of great practical as well as of great theoretical importance?

ON GAS SUPPLY BOTH FOR HEATING AND ILLUMINATING PURPOSES[1]

WHEN, within the memory of living men, the gas-burner took the place of the time-honoured oil-lamp, the improvement, both as regards the brilliancy of the light and the convenience of the user, was so great that the ultimate condition of perfection appeared to have been reached. Nothing apparently remained for the engineer to effect but improvements in the details of the works and apparatus, so that this great boon of modern times might be utilised to the largest extent. It is only in recent years that much attention has been bestowed upon the utilisation of by-products, with a view of cheapening the cost of production of the gas, and that the consumer has become alive to the importance of having a gas of high illuminating power and free from nauseous constituents, such as bisulphide of carbon, thus providing a gentle stimulant for steady progress on the part of the gas-works manager.

This condition of steadiness and comfort has been somewhat rudely shaken by the introduction within the last year or two of the electric light, which, owing to its greater brilliancy and cheapness, threatens to do for gas what gas did for oil half a century before. The lighting of the City of London and of many public halls and works furnishes indisputable proof that the electric light is not an imaginary, but a real and formidable competitor to gas as an illuminant, and it is indeed time for gas engineers and managers to look seriously to their position with regard to this new rival; to decide whether to meet it as a foe, and contest its progress inch by inch, or to accept at once the new condition of things, conceding the ground that cannot reasonably be maintained, and to look about in search of such compensating fields as may be discovered for a continuation or extension of their labours.

For my own part I present myself before you both as a rival and as a friend; as a rival, because I am one of the promoters of electric illumination, and as a friend, because I have advocated and extended the use of gas for heating purposes during the last twenty years, and am by no means disposed to relinquish my advocacy of gas both as an illuminating and as a heating agent. Speaking as a gas engineer, I should be rather disposed to regard the electric light as a welcome incentive to fresh exertion, confidently anticipating achievements by the use of gas which would probably have been long postponed under the continued *régime* of a monopoly. Already we observe, both in our thoroughfares and in our apartments, gas-burners producing a brighter and more powerful light than was to be seen previously; and although gas will have to yield to the electric light the illumination of our lighthouses, halls, and great thoroughfares, it will be in a position, I believe, to hold its own as a domestic illuminant, owing to its great convenience of usage, and to the facility with which it can be subdivided and regulated. The loss which it is likely to sustain in large appliances as an illuminant would be more than compensated by its use as a heating agent, to which the attention of both the producer and the consumer has latterly been largely directed.

Having in the development of the regenerative gas-furnace had exceptional opportunities of recognising the many advantages of gaseous over solid fuel, I ventured, as early as 1863, to propose to the Town Council of Birmingham the establishment of works for the distribution of heating gas throughout the town, and it has occurred to me to take this opportunity (when the gas managers of Great Britain hold their annual meeting at the very place of my early proposal) to place before them the idea that then guided me, and to suggest a plan of operation for its realisation which at the present day will not, I venture to hope, be regarded by them as Utopian. The proposal of 1863 consisted in the establishment of separate mains for the distribution of heating gas, to be produced in vertical retorts, that might be shortly described as Appold's coke oven heated by means of "producer" gas and "regenerators." The heat of the retorts was to be increased beyond the ordinary limit in order to produce a coke suitable for locomotive and other purposes; and the gas produced being possessed of less illuminating but of the same heating power, and being, with a view to cheapness, less thoroughly purified than ordinary retort gas, was to be distributed through the town as a heating agent, to be applied to the small boilers and furnaces of the numerous little factories peculiar to the district, as well as for domestic purposes. The Corporation applied for an Act of Parliament, but did not succeed in obtaining it, owing to the opposition of the existing gas companies, who pledged themselves to carry out such an undertaking if found feasible by them. I am ready to admit that at the time in question the success of the undertaking would have involved considerable practical difficulty, but I feel confident that the modified plan which it is my present object to bring before you would reduce those difficulties to a minimum, and open out on the other hand a new field of vast proportions for the enterprise and energy of those interested in gas-works, and of great benefit to the public.

The gas-retort would be the same as at present, and the only change I would advocate in the benches is the use of the regenerative gas-furnace. This was first successfully introduced by me at the Paris Gas-works in 1863, and has since found favour with the managers of gas-works abroad and in this country. The advantages that have been proved in favour of this mode of heating are economy of fuel, greater durability of retorts, owing to the more perfect distribution of heat, the introduction of an additional retort in each bed in the position previously occupied by the fire-grate, and above all, a more rapid distillation of the coal, resulting in charges of four hours each, whereas six hours are necessary under the ordinary mode of firing. The additional suggestion I have now to make consists in providing over each bench of retorts two collecting pipes, the one being set aside for illuminating, and the other for a separate service of heating gas. I shall be able to prove to you from unimpeachable evidence that the gas coming from a retort varies very greatly in its character during progressive periods of the charge; that during the first quarter of an hour after closing the retort, the gas given off consists principally of marsh gas (CH_4) and other occluded gases and vapours, which are of little or no use for illuminating purposes; from the end of the first quarter of an hour, for a period of two hours, rich hydrocarbons, such as acetylene (C_2H_2) and olefiant gas (C_2H_4) are given off; whereas the gases passing away after this consist for the most part again of marsh gas possessing low illuminating power.

M. Ellissen, the late chief of the experimental department of the Paris Gas-works, and actual President of the French Society of Gas Engineers, has favoured me with the result of a most interesting series of experiments, which he carried out in connection with the late M. Regnault, the eminent physicist, some years ago, the object of the experiments being to discover the proper period of time to be allowed for each charge.

The results of these experiments are given in a diagram showing in a striking manner that although the average illu-

[1] Paper read before the British Association of Gas Managers at Birmingham, June 14, by C. W. Siemens, D.C.L., F.R.S., Civil Engineer.

minative power produced by the distillation of the coal did not exceed 1·35 Carcel burners, or 13·5 standard candles, according to our English mode of measurement, the gas given off from the end of the first quarter of an hour, during a period of two hours, possessed an illuminating power of 1·616 Carcel burners, or 16·16 standard candles. According to the figures given in the valuable experiments of M. Ellissen, it appears that nearly two-thirds of the total production of gas takes place in the above period, whilst the remaining third is distilled during the first quarter of an hour and the last hour and three quarters. It hence follows that by changing the direction of the flow of gas at the periods indicated, allowing the first results of distillation to flow into the heating gas-main, then for two consecutive hours into the illuminating gas main, and for the remainder of the period again into the heating gas-main, one-third volume of heating and two-thirds of illuminating gas would be obtained, with this important difference, that the illuminating gas would be of 16·16 instead of 13·5 candle power, and that the heating gas, although possessed of an illuminating power of only 11·05, would be preferable to the mixed gas for heating purposes, in being less liable to deposit soot in its combustion upon heat-absorbing surfaces, and in giving, weight for weight, a calorific power superior to olefiant gas.

These experiments not having been made for the particular objects I have in view, no account was taken of the quantity or quality of the gas coming from the retort during the first quarter of an hour. Judging by the nature of the curves given by M. Ellissen, it is reasonable to suppose that during the first quarter of an hour a considerable quantity of gas of very inferior illuminating power is given off, which, if taken into account, would still further improve the result given in favour of separating the illuminating from the heating gases.

It will be observed that although the candle-power of the illuminating gas would be raised to only 16·25 if two-thirds of the gas were set apart for this purpose, *i.e.* after the first 25 minutes of distillation up to 2h. 35m. from the commencement of the charge, a gas equal to 18·04 candles would be obtained if the proportionate quantity of heating and illuminating gas were reversed, which might be effected by continuing the distillation for illuminating purposes from 0·25m. to 1h. 27m. after the commencement of the charge, whilst if equal quantities of heating and illuminating gas were produced, which would result from allowing the illuminating gas to flow into its receiver from 0·25m. to 2h. 0m., the candle-power of this portion of the gas would be raised to 16·78 candles, as shown in the figures given below.

	Cubic Feet.	Candle-power.	
Total gas produced from ton of coal...	10573·20	of 13·50	
	Illuminating Gas. Cubic Feet.	Illuminating Gas. Candle-power.	Heating Gas. Cubic Feet.
Illuminating gas passing into its main 25 minutes after commencement of charge:—			
If two-thirds the quantity used for illumination from 0h. 25m. to 2h. 35m.	7048·8	16·25	3524·4
If half the quantity used for illumination from 0h. 25m. to 2h. 0m.... ...	5286·6	16·78	5286·6
If one-third the quantity used for illumination from 0h. 25m. to 1h. 27m...	3524·4	18·04	7048·8

These important results are borne out by a series of photographic observations which were made some years ago by Mr. Sugg, which Mr. Sugg has further supplemented verbally in stating that the average illuminative power obtained by the distillation of Newcastle coal might be taken at 14 candle-power, whilst two-thirds of the quantity, if separated in the manner I propose, would produce an average of 16 candles.

The working out of this plan would involve the mechanical operation of changing the direction of the gas coming from each bench of retorts at the proper periods of the charge; this could be accomplished by means of a simple reversing valve similar to that applied for many years in reversing the current of the regenerative gas-furnace, and a sand-glass may be placed in front of each bench of retorts for the guidance of the man in charge as to the time when the reversal should be made. In order to distribute the two gases a double set of gas-mains would certainly be required; but these exist already in the principal thoroughfares of many of our great towns, where at one period or another competing gas companies have been established, and it would not be difficult, I think, to utilise these services for the separate supply of illuminating and heating gas, the latter being taken into such houses and establishments only where asked for by the occupiers.

The public could well afford to pay an increased price for a gas of greatly increased illuminating power, and the increase of revenue thus produced would enable the gas companies to supply heating gas at a proportionately reduced rate. It would not be necessary to employ upon the heating gas the same expense and trouble in purification as is required for illuminating gas, because the products of combustion of the heating gas would not as a rule enter the apartments, but be conducted into the atmosphere through the ordinary chimneys. Heating the retorts by means of the regenerative gas-furnace would, as already indicated, lead to an increased production of gas from each bench of retorts, and thus compensate for the reduced amount of illuminating gas in each operation. The heating gas might without inconvenience be sent through the pipes at a greater pressure than the illuminating gas, in order to make a given plant of mains transmit an increased quantity.

The question may fairly be asked whether a demand would be likely to arise for heating gas similar in amount to that for illuminating gas, and I may state that I am decidedly of opinion that although at the present moment the amount of gas supplied for illuminating purposes exceeds that for heating, the diminution in price of the latter would very soon indeed reverse these proportions. Already gas is used in rapidly increasing quantities for kitcheners, for the working of gas-engines, and for fire-grates. As regards the latter application, I may here mention that an arrangement for using gas and coke jointly in an open fire-place, combined with a simple contrivance (with a view of effecting the combustion of the gas by heated air), has found favour with many of the leading grate builders and with the public; although this arrangement was suggested by me only last winter, several hundred of these grates are already in use in London, Manchester, Leeds, Glasgow, and other towns, showing how fully alive the public are at the present time to that great crying evil, "the smoke nuisance."

It may be as well for me to mention here, that neither the regenerative gas-coke fire-grate just alluded to, nor the plan I here advocate of separating the produce of gas-retorts, has been made by me subject-matter of letters patent, my time being already too much occupied in other directions to give that amount of constant attention to these subjects which the working of a patent necessitates.

As regards the use of illuminating gas, I have one more suggestion to make, which I feel confident will be viewed by you not without interest. The illuminating effect produced in a gas flame depends partly upon the amount of carbon developed in the solid condition in the body of the flame, and partly upon the temperature to which these particles are heated in the act of combustion. Having already shown how by separation a gas of greater luminosity may be supplied, it remains to be seen how the temperature of combustion may be raised. This may be effected to an extent that seems surprising by certain mechanical arrangements, whereby a portion of the waste heat produced by the flame itself is rendered available to heat the gas and air sustaining the combustion of the flame, say to 600° F., or even beyond that point.

The arrangement I have adopted for this purpose is represented on the sectional diagram, and I have also the pleasure to place the burner itself before you to enable you to test its efficiency by actual trial. The burner is of the ordinary Argand type, mounted in a small cylindrical chamber of sheet copper connected with a vertical rod of copper projecting up and through the centre of the burner, and terminating in a cup-like extension at a point about four inches above the gas orifices, or on a level with the top of the flame. A small mass of fire-clay fills the cup, projecting upwards from it in a rounded and pointed form. The copper vessel surrounding the burner is contracted at its upper extremity with a view of directing a current of air against the gas jets on the burner, and on its circumference it is perforated for the admission of atmospheric air. The bottom surface is formed of a perforated disk covered with wire gauze, and wire gauze also surrounds the circumference of the perforated cylinder. The external air is heated in passing through these "regenerative" surfaces, and the flame is thus fed with air, heated to the point above indicated, which by more elaborate arrangements might be raised to a still higher degree. The ball of fire-clay in the centre of the burner, which is heated to redness, serves the useful purpose of completing the combustion of the gas, and thus diminishes the liability to blackening of the ceiling.

This arrangement for transferring the heat from the tip of the flame to the air supporting its combustion is applicable also to an open batswing burner; but I have not yet had time to ascertain accurately the amount of increase of luminosity that may be realised with this class of burner.

I may here mention that another solution of the problem of heating the incoming air by the waste heat of the products of combustion has lately been brought under public notice by my brother, Frederick Siemens, which differs essentially from the plan I have suggested, inasmuch as he draws the flame downwards through heating apparatus, and thence into a chimney. Experiments made officially and with great care have proved that by these methods the luminous effect of gas can be practically doubled. In practice both these methods of intensifying a gas-flame will probably find independent application according to circumstances, the cause of increased luminous effect being in both cases the same.

From a purely theoretical point of view it can be shown that of the caloric energy developed in the combustion of gas, a proportion probably not exceeding 1 per cent. is really utilised in the production of luminous rays, and that even in the electric light nine-tenths of the energy set up in the arc is dispersed in the form of heat, and one-tenth only is utilised in the form of luminous rays. It would lead us too far here to go into the particulars of these calculations, but it is important to call attention to them, in order to show the large margin for practical improvements still before us.

By the combined employment of the process of separation of the illuminating from the heating gas with the arrangement for intensifying the luminosity of the gas-flame, the total luminous effect produced by a given consumption of coal gas may be nearly tripled, thus showing that the deleterious effects now appertaining to gas illumination are not inseparably connected with its use.

My principal object in preparing this communication has been to call your attention generally to the important question of an improved gas illumination, and more particularly to the subject of a separate supply for heating gas, which, if carried into effect, would lead, I am convinced, to beneficial results, the importance of which, both to gas companies and to the public, it would be difficult to over-estimate.

APPENDIX

Paris, June 4, 1881

DEAR SIR,—I send you herewith the result of my experiments, together with tables and curves; the very ingenious proposal that you have made would permit such a division of the total production of gas, that two-thirds could be employed for lighting and one-third for heating purposes, resulting in splendid illumination and much more rational heating.

I am, dear sir, &c.,

Dr. C. William Siemens A. ELLISSEN

Experiments on the Variation of Production of Gas, and of its Illuminating Power at different Periods of the Distillation

Tables I. and II. contain the results of experiments made in a bench of seven retorts of the type of the Compagnie Parisienne, each retort being charged respectively with 100, 110, and 120 kilogrammes (220, 242, 264 pounds).

Table I. corresponds to a distillation of 4 hours.

Table II. corresponds to a distillation of 4h. 48m.

The period of distillation has been divided into intervals of fifteen minutes, and the results recorded on each horizontal line refer to the gas produced during the quarter ending the time mentioned on each line.

In each of the two tables the case of a charge of 110 kilos. (242 lbs.) has been chosen as the standard, and the results have been graphically represented by means of two curves, one in red for the gas produced, and the other in blue for the illuminating power.

The line of abscissæ being divided into equal parts, each representing fifteen minutes, each ordinate of the red curve gives the gas produced during the preceding quarter of an hour, and the corresponding ordinate of the blue curve indicates the illuminating power of this same gas.

The production of the gas been further divided into two portions, the one destined for illumination, and the other for heating and motive power.

The gas produced during the first quarter of an hour is generally of low illuminating power, and varies besides with the hygrometric condition of the coal; it has, in the following calculation, been accordingly classed with the heating gas, and the gas produced during the interval from 0h. 15m. to 2h. 15m. of the working has alone been reserved for illuminating purposes.

Distillation in four hours. Charge of 110 *kilos.* (242 *lbs.*)

I. Gas produced per 100 kilos. of coal distilled—

	Cubic metres.	Per ton, cubic feet.
1. From 0h. 15m. to 2h. 15m. ...	18·062	... 6502·32
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 0m. ...	11·308	... 4070·88
Total	29·370	... 10573·20

II. Gas produced per 100 cubic metres obtained—

	Cubic metres.
1. From 0h. 15m. to 2h. 15m. ...	61·502
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 0m. ...	38·498
Total	100·000

III. Mean illuminating power of the produced gas—

	Litres.	In English standard candles.
1. From 0h. 15m. to 2h. 15m. ...	87·7	... 16·16
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 0m. ...	128·2	... 11·05
Mean of the total mixed gas as per calculation	103·3	
Illuminating power of mixed gas as per direct trial...	105·7	... 13·50

Distillation in 4 *hours* 48 *minutes. Charge of* 110 *kilos.* (242 *lbs.*)

I. Gas produced per 100 kilos. of coal distilled—

	Cubic metres.	Per ton, cubic feet.
1. From 0h. 15m. to 2h. 15m. ...	20·388	... 7339·68
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 48m. ...	9·741	... 3506·76
Total	30·129	... 10846·44

II. Gas produced per 100 cubic metres obtained—

	Cubic metres.
1. From 0h. 15m. to 2h. 15m. ...	67·673
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 48m. ...	32·327
Total	100·000

III. Mean illuminating power of the produced gas—

	Litres.	In English standard candles.
1. From 0h. 15m. to 2h. 15m. ...	101·1	... 14·02
2. From 0h. 0m. to 0h. 15m., and from 2h. 15m. to 4h. 48m. ...	132·4	... 10·07
Mean of the total mixed gas	111·2	... 12·77

It is not proposed to stop at the results obtained by distillation in 4h. 48m., that is five charges per twenty-four hours; experience has proved that the best conditions of working are found in the use of active charges rapidly distilled by raising the temperature of the furnaces.

From these experiments it results that it would be possible to divide the products of distillation of coal into illuminating gas, and gas for heating purposes and motive power.

Thus in place of producing, as is generally done, by means of a distillation of four hours and 110 kilos. (242 lbs.) per retort, a mean result per 100 kilos of coal distilled, of 30 cubic metres of normal gas, which corresponds to an expenditure of 105 litres, to produce the light of a Carcel burner consuming 42 grammes of oil per hour, there may be produced:—

1. About 18·5 cubic metres of illuminating gas of an illuminative power of 87 litres; and

2. About 11·5 cubic metres of heating and motive-power gas of an illuminative power of 128 litres; or per 100 cubic metres

of gas produced, 61·50 cubic metres of illuminating gas, and 38·50 cubic metres of heating and motive-power gas.

This result would be obtained by receiving into separate reservoirs the gas produced during the first fifteen minutes, and during the last 1h. 45m. of the distillation, and in reserving for illuminating purposes the gas made in the interval of 0h. 15m. to 2h. 15m. of the charge from the commencement of the distillation.

STORAGE OF ELECTRIC ENERGY

THE following correspondence on this subject has appeared in the *Times*. By help of this and the communication in our issue of to-day from Sir W. Thomson, the reader will be able to understand the present position of this important question.

THE marvellous "box of electricity" described in a letter to you, which was published in the *Times* of May 16, has been subjected to a variety of trials and measurements in my laboratory for now three weeks, and I think it may interest your readers to learn that the results show your correspondent to have been by no means too enthusiastic as to its great practical value. I am continuing my experiments to learn the behaviour of the Faure battery in varied circumstances, and to do what I can towards finding the best way of arranging it for the different kinds of service to which it is to be applied. At the request of the Conseil d'Administration of the Société de la Force et la Lumière, I have gladly undertaken this work, because the subject is one in which I feel intensely interested, seeing in it a realisation of the most ardently and unceasingly felt scientific aspiration of my life—an aspiration which I scarcely dared to expect or to hope to live to see realised.

The problem of converting energy into a preservable and storable form, and of laying it up in store conveniently for allowing it to be used at any time when wanted, is one of the most interesting and important in the whole range of science. It is solved on a small scale in winding up a watch, in drawing a bow, in compressing air into the receiver of an air-gun or of a Whitehead torpedo, in winding up the weights of a clock or other machine driven by weights, and in pumping up water to a height by a windmill (or otherwise, as in Sir William Armstrong's hydraulic accumulator) for the purpose of using it afterwards to do work by a waterwheel or water pressure on a piston. It is solved on a large scale by the application of burning fuel to smelt zinc, to be afterwards used to give electric light or to drive an electro-magnetic engine by becoming, as it were unsmelted in a voltaic battery. Ever since Joule, forty years ago, founded the thermodynamic theory of the voltaic battery and the electro-magnetic engine, the idea of applying the engine to work the battery backwards and thus restore the chemical energy to the materials so that they may again act voltaically, and again and again, has been familiar in science. But with all ordinary forms of voltaic battery the realisation of the idea to any purpose seemed hopelessly distant. By Planté's admirable discovery of the lead and peroxide of lead voltaic battery, alluded to by your correspondent, an important advance towards the desired object was made twenty years ago; and now by M. Faure's improvement practical fruition is attained.

The "million of foot pounds" kept in the box during its seventy-two hours' journey from Paris to Glasgow was no exaggeration. One of the four cells, after being discharged, was recharged again by my own laboratory battery, and then left to itself absolutely undisturbed for ten days. After that it yielded to me 260,000 foot pounds (or a little more than a quarter of a million). This not only confirms M. Reynier's measurements, on the faith of which your correspondent's statement was made; it seems further to show that the waste of the stored energy by time is not great, and that for days or weeks, at all events, it may not be of practical moment. This, however, is a question which can only be answered by careful observations and measurements carried on for a much longer time than I have hitherto had for investigating the Faure battery. I have already ascertained enough regarding its qualities to make it quite certain that it solves the problem of storing electric energy in a manner and on a scale useful for many important practical applications. It has already had in this country one interesting application, of the smallest in respect to dynamical energy used, but not of the smallest in respect to beneficence, of all that may be expected of it. A few days ago my colleague, Prof. George Buchanan, carried away from my laboratory one of the lead cells (weighing about 18 lbs.) in his carriage, and by it ignited the thick platinum wire of a galvanic *écraseur* and bloodlessly removed a nævoid tumour from the tongue of a young boy in about a minute of time. The operation would have occupied over ten minutes if performed by the ordinary chain *écraseur*, as it must have been had the Faure cell not been available, because in the circumstances the surgical electrician, with his pharaphernalia of voltaic battery to be set up beforehand, would not have been practically admissible.

The largest useful application waiting just now for the Faure battery—and it is to be hoped that the very *minimum* of time will be allowed to pass till the battery is supplied for this application—is to do for the electric light what a water cistern in a house does for an inconstant water supply. A little battery of seven of the boxes described by your correspondent suffices to give the incandescence in Swan or Edison lights to the extent of 100 candles for six hours, without any perceptible diminution of brilliancy. Thus, instead of needing a gas engine or steam engine to be kept at work as long as the light is wanted, with the liability of the light failing at any moment through the slipping of a belt—an accident of too frequent occurrence—or any other breakdown or stoppage of the machinery, and instead of the wasteful inactivity during the hours of day or night when the light is not required, the engine may be kept going all day and stopped at night, or it may be kept going day and night, which will undoubtedly be the most economical plan when the electric light comes into general enough use. The Faure accumulator, always kept charged from the engine by the house supply wire, with a proper automatic stop to check the supply when the accumulator is full, will be always ready at any hour of the day or night to give whatever light is required. Precisely the same advantages in respect of force will be gained by the accumulator when the electric town supply is, as it surely will be before many years pass, regularly used for turning lathes and other machinery in workshops and sewing-machines in private houses.

Another very important application of the accumulator is for the electric lighting of steam-ships. A dynamo-electric machine of very moderate magnitude and expense, driven by a belt from a drum on the main shaft, working through the twenty-four hours, will keep a Faure accumulator full, and thus, notwithstanding irregularities of the speed of the engine at sea or occasional stoppages, the supply of electricity will always be ready to feed Swan or Edison lamps in the engine-room and cabins, or arc lights for mast-head and red and green side lamps, with more certainty and regularity than have yet been achieved in the gas supply for any house on *terra firma*.

I must apologise for trespassing so largely on your space. My apology is that the subject is exciting great interest among the public, and that even so slight an instalment of information and suggestions as I venture to offer in this letter may be acceptable to some of your readers. WILLIAM THOMSON.

The University, Glasgow, June 6.

ALTHOUGH agreeing with every word of Sir William Thomson's letter in the *Times* of to-day, and entirely sympathising with his enthusiasm as regards the marvellous box of electricity, still I feel that it would have been desirable if in pointing out the importance of this new discovery Sir William Thomson had guarded against a very probable misconstruction of the purport of his letter.

The means of storing and re-storing mechanical energy form the aspiration not only of Sir William, but of every educated mechanic. It is, however, a question of degree—of the amount of energy stored as compared with the weight of the reservoir, the standard of comparison being coal and corn. Looked at in this way one cannot but ask whether, if this form of storage is to be the realisation of our aspirations, it is not completely disappointing. Large numbers are apt to create a wrong impression until we inquire what is the unit. Eleven million foot pounds of energy is what is stored in 1 lb. of ordinary coal. So that in this box, weighing 75 lb., there was just as much energy as in $1\frac{1}{4}$ oz. of coal, which might have been brought from Paris or anywhere else in a waistcoat pocket, or have been sent by letter.

When we come to the question of the actual conveyance of energy for mechanical purposes, this view is of fundamental importance. The weight of the same amount of energy in the new form is 800 times greater than the equivalent amount of coal; and as a matter of economy, supposing that energy in this form might be had at a certain spot and no capital were required for its conversion or storage, and that the energy were directly applicable it could not be carried ten miles—that is to say, such energy cannot be economically useful ten miles from its source, although coal had to be carried 100 miles to the spot. This limit, in truth, falls far short of what has been already attained by other means. By wire ropes and by compressed air or steam energy may be economically transmitted from ten to twenty miles. So that if this is the utmost of what is to be done by means of the storage of electricity this discovery adds another door to those which are hopelessly closed against the possibility of finding in Niagara or other water power a substitute for our coal, even when the object is motive power, and much more for that purpose for which five-sixths of our coal is used—the production of heat.

It is very important that the people of this country should not shut their eyes to the fact that, so far from there being a greater prospect of the solution of the problem than when, about twenty years ago, Prof. Jevons raised the alarm, the prospect is now much smaller. In the meantime the capabilities of steel ropes, fluids in pipes, and electricity along conductors have been not only investigated, but practically tested, and found altogether wanting. And now it would seem that the storage of electricity must be added to the list. OSBORNE REYNOLDS

Owens College, June 9

YOUR leading article in the *Times* of yesterday, on the storage of electricity, alludes to my having spoken of Niagara as the natural and proper chief motor for the whole of the North American Continent. I value the allusion too much to let it pass without pointing out that the credit of originating the idea and teaching how it is to be practically realised by the electric transmission of energy is due to Mr. C. W. Siemens, who spoke first, I believe, on the subject in his presidential address to the Iron and Steel Institute in March, 1877. I myself spoke on the subject in support of Mr. Siemens's views at the Institution of Civil Engineers a year later. In May, 1879, in answer to questions put to me by the Select Committee of the House of Commons on Electric Lighting, I gave an estimate of the quantity of copper conductor that would be suitable for the economical transmission of power by electricity to any stated distance; and, taking Niagara as example, I pointed out that, under practically realisable conditions of intensity, a copper wire of half an inch diameter would suffice to take 26,250 horse-power from water-wheels driven by the Fall, and (losing only 20 per cent. on the way) to yield 21,000 horse-power at a distance of 300 British statute miles; the prime cost of the copper amounting to 60,000*l*., or less than 3*l*. per horse-power actually yielded at the distant station. WILLIAM THOMSON

The University, Glasgow, June 9

IF you do me the honour to publish a letter which I wrote to you yesterday regarding the electric transmission of energy it will be seen that I thoroughly sympathise with Prof. Osborne Reynolds in his aspirations for the utilisation of Niagara as a motor, but that neither Mr. Siemens nor I agree with him in the conclusion which he asserts in his letter to you, published in the *Times* of to-day, that electricity has been tried and found wanting as a means for attaining such objects. The *transmission* of power was not the subject of my letter to you published in the *Times* of the 9th inst., and Prof. Reynolds' disappointment with M. Faure's practical realisation of electric *storage*, because it does not provide a method of *porterage* superior to conduction through a wire, is like being disappointed with an invention of improvements in water cans and water reservoirs because the best that can be done in the way of movable water cans and fixed water reservoirs will never let the water-carrier supersede water-pipes wherever water-pipes can be laid.

The $1\frac{1}{2}$ oz. of coal cited by Prof. Osborne Reynolds as containing a million of foot-pounds stored in it is no analogy to the Faure accumulator containing the same amount of energy. The accumulator can be re-charged with energy when it is exhausted, and the fresh store drawn upon when needed, without losing more than 10 or 15 per cent. with arrangements suited for practical purposes. If coal could be unburned—that is to say, if carbon could be extracted from carbonic acid by any economic process of chemical or electric action, as it is in nature by the growth of plants drawing on sunlight for the requisite energy—the result would be analogous to what is done in Faure's accumulator. WILLIAM THOMSON

The University, Glasgow, June 11

DR. MIKLUCHO MACLAY'S ANTHROPOLOGICAL AND ANATOMICAL RESEARCHES IN MELANESIA AND AUSTRALIA [1]

AFTER I had left Sydney in March, 1879, I visited the following islands: New Caledonia, Lifu; of the New Hebrides: Tana, Vate, Tongoa, Mai, Epi, Ambrim, Malo, Vanua Lava; the Admiralty Islands; the groups—Lub (or Hermit), Ninigo (Echiquier), Trobriant, the Solomon Islands, the islands at the south-east end of New Guinea, and the islands of Torres Straits.[2]

Only a very few of the results of the journey can be comprehended in a short *résumé*, of these the first two of the following appear to me to be the most important:—1. Many islands of Melanesia[3] (especially some of the islands of the New Hebrides, of the Solomon Group, of the Louisiades, New Ireland, &c., &c.) possess a well-marked brachycephalic population (the breadth-index of many heads exceeds eighty, and sometimes even eighty-five), which circumstance is assuredly not ascribable to a mixture with another race, and proves that brachycephalism has a much wider range in Melanesia than has been hitherto supposed. This is a result of numerous careful measurements of heads and skulls[4] of the aboriginals of different islands of Melanesia. 2. Although in some villages of the southern coast of New Guinea there is noticeable a Polynesian admixture, yet this circumstance by no means permits of the aboriginals of the south-eastern peninsula (who are a branch of the Melanesian stock) being called a "yellow Malayan race," as has been frequently done of late years. 3. An acquaintance with the languages of the group Lub (or Hermit) and the dialects of the northern coast of the large island of the Admiralty Group, as well as the native traditions of the former, has shown that the population of the group Lub emigrated from the Admiralty Islands. Further acquaintance with the natives of Lub proved that there is among them a Polynesian admixture, which has resulted from the carrying off of the women of the group Ninigo, and from a frequent intercourse with the inhabitants (also a Melano-Polynesian race) of the smaller group Kaniet or Kanies (or Anchorites). My stay among the inhabitants of the Admiralty Islands has afforded me a glimpse of many interesting customs of the islands; but an account of these observations and researches cannot be condensed within the compass of a few sentences. To this series of results belong also the observations which I never neglected to make during the journey in Melanesia whenever the opportunity presented itself—especially observations on their customs, such as the deformation of the head, tattooing, perforation of the septum narium, alæ nasi, lobes and margins of the ears. I have also succeeded in making further observations, and obtaining more information, on the macrodontism in the Admiralty and Lub islands.

On my way back from the islands of Torres Straits I visited Brisbane, where I at first only intended to remain a few days. Here however a favourable opportunity presented itself of acquiring some interesting anatomical material for my anthropological researches, which circumstance induced me to prolong my stay for several months. I found, namely, that there was a possibility of continuing my researches on the comparative anatomy of the brain of the different varieties of the genus *homo*, which were commenced in 1873 in Batavia and resumed in Sydney in 1878. Although the material in question consisted only of three brains, yet I find that this new contribution to our knowledge of race-anatomy supports the view which I may briefly summarise as follows:—The investigation of the brains of representatives of different races of men shows that there occur peculiarities of by no means trifling import, which one cannot regard as individual variations. To this category belong differences in the development of the corpus callosum of the pons varolii, of the cerebellum; differences in the volume of the cranial nerves, and so forth; also the arrangement of the convolutions of the cerebrum is different, and I believe that in

[1] From a paper read before the Linnean Society of New South Wales February 23, 1881, by Dr. N. De Miklucho-Maclay. Revised and transmitted by the author.

[2] A more detailed account of the route, of the time spent at the different places, with sketch-maps of the routes and other details, will be found in my communication to the Imperial Russian Geographical Society, in the Jswestija of the Society.

[3] By the name "Melanesians" I designate exclusively the frizzly-haired inhabitants of the South Sea Islands.

[4] In order to eliminate any doubt as to the correctness of the cranial measurements on living individuals, I have not neglected to collect a considerable number of undoubtedly authentic skulls from New Caledonia, New Guinea, the Admiraltys, Ninigo, and Solomon Islands.

course of time it will probably be discovered that there exist certain definite types of cerebral convolutions corresponding to the principal varieties of mankind. In order to discover those types much material will require to be conscientiously examined; and I hope that my investigation will induce other anatomists to work in this direction to prove or to disprove this statement, which in the present state of our knowledge can only be more or less hypothetical.

On my way from Thursday Island I let slip no opportunity of examining, measuring, and photographing the remnant of the Australian aboriginals; and hearing it stated in various quarters that there were living in the interior of Queensland certain natives described as devoid of hair, I thought the problem of a possible occurrence of a hairless stock among the aboriginals worthy of a personal investigation. I have written to Prof. Virchow of Berlin at length concerning my examination of this hairless family, which I found at Gulnarber Station, near St. George, on the Balonne River. This was made considerably easier for me by the kind assistance of Mr. G. M. Kirk of Gulnarber Station. As regards this instance of natural, and in this case hereditary *atrichia universalis* among the Australian aboriginals, I will only remark that it forms an interesting antithesis to the well-known cases of excessive hypertrichosis.

With a view of pursuing comparative anatomical researches on the brain of the marsupials, I went to Pikedale, near Stanthorpe, where I succeeded during a stay of almost six weeks in acquiring for my cerebral investigations some material which is almost impossible to obtain in the cities, such as Brisbane or Sydney, and which, as I have learnt by my own experience, cannot be obtained even in the bush with great ease and quickness. I succeeded, however, in obtaining a number of brains of some species of the genera—Macropus, Osphranter, Halmaturus, Petrogale, Phascolarctos, as well as a few brains of Ornithorhynchus and Echidna.

At the end of December last year, still availing myself of the kind hospitality of Mr. Donald Gunn, I went on to his other station, Clairvaul, near Glen Innes, with the intention of collecting some fossils, and without great trouble I got a series of interesting remains of *Diprotodon Australis, Nototherium Mitchellii, Phascolomys gigas, Macropus titan*, &c., &c.

When I received in May, 1880, in Thursday Island, a letter from my friend Mr. William Haswell, informing me that the Zoological Station in Sydney was not established, I determined not to leave Australia before the scheme had been carried out. Detained in Queensland by the work already referred to, I only arrived in Sydney in January of this year, and now, after a stay of one month, I have the pleasure to announce that I have every reason to believe that the Zoological Station at Watson's Bay will be opened in a short time. My stay in Brisbane has once more caused me to feel the *necessity* of such an institution for the biologist. I could expatiate at length on the advantages of a zoological station, but I content myself with remarking that, in spite of my great dislike to waste my time, I was obliged to spend many days, even weeks, in Brisbane and Sydney without the possibility of working, on account of the want of a suitable place.

I repeat again my conviction, grounded on long experience, that "the immediate need is not of apparatus or libraries, but of a place for undisturbed work."[1] I hope to be able, not later than in two months, to work in the Zoological Station in Watson's Bay. I am convinced that many men of science will avail themselves of it in future years; and I am satisfied to leave for future generations such a memento of my stay in Sydney as the first zoological station in Australia.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Physiological Laboratory (Dr. Foster's) will be open during the Long Vacation, and a series of repetitions of lectures and demonstrations will be given by Mr. Waters, the Assistant Demonstrator, in Elementary Biology, Histology, and Physiology.

The Cavendish Laboratory will be open during July and August, and the Professor of Experimental Physics or one of the Demonstrators will attend daily.

Prof. A. C. Haddon, of the Royal College of Science, Dublin, has been nominated by the Board of Natural Science Studies to study at the Zoological Station at Naples during the ensuing autumn.

The Board of Mathematical Studies has issued a report showing that in the last Mathematical Tripos the total of marks was 33,541, of which the first ten wranglers averaged 8582. In the last five days 11,753 marks were assigned to riders, and 7770 to problems; of which the first ten wranglers averaged 2388 and 936 respectively. The additional examiner stated his satisfaction with the answering; and he considered that much of the time formerly occupied by the study of astronomy, including the Lunar and Planetary Theories, Figure of the Earth, and Precession of Nutation, was now devoted to Heat, Electricity, and Magnetism. Comparing the progressive nature of the latter subjects with the stationary nature of the former, the latter afford the best means of testing the mathematical ability of the candidates.

Prof. Cayley will lecture in Michaelmas Term on Abel's Theorem; Dr. Ferrers (Master of Caius) on the Theory of the Potential; Mr. Niven (Trinity) on Electrostatics; Mr. Glaisher (Trinity) on Definite Integrals and Differential Equations; Mr. Hobson (Christ's) on Rigid Dynamics; Mr. Stearn (King's) on Conduction of Heat and Electricity; Mr. Allen (St. Peter's) on Magnetism; Mr. Dickson on Dynamics of a Particle.

The annual report of Prof. Adams to the Observatory Syndicate states that, notwithstanding the exceptionally unfavourable weather for observing, there had been made 2834 determinations of Right Ascension and North Polar Distance with the transit circle, including 2151 observations of zone stars which were made on eighty nights. Satisfactory observations of the partial solar eclipse, December 31, 1880, were obtained with the Northumberland equatorial, employing the wire micrometer. The observations with the transit circle for nadir point and level have been facilitated and rendered much more satisfactory by an alteration in the mode of illumination of the wires through the Bohnenberger eyepiece. Instead of placing a small hand-lamp on a stand close to the eyepiece, which gave an uncertain image at the best, the illumination is now effected by means of a paraffin lamp placed on a platform at the requisite elevation about ten feet from the eyepiece. The rays for the lamp are rendered parallel by passing through the system of lenses intended for the illumination of the microscopes of the eastern circle, which is not in ordinary use. There is now no difficulty in getting the light properly directed, and the images both of the Right Ascension and Declination wires are dark and very distinct. The observations of standard stars are completely reduced in R.A. and N.P.D. to the end of 1879 and part of 1880, as to the zone stars, the true R.A. and N.P.D. are obtained to the end of 1878, the approximate N.P.D. to the end of 1879. The calculation of reduction of apparent place to mean is completed to the end of 1876, and is far advanced for 1877. Meteorological observations are regularly made. A third assistant in the Observatory is urgently needed.

The following awards have been made by the Master and Seniors of St. John's College for proficiency in Natural Sciences:—To Samways, a Wright's Prize, with 100*l*. for the year; to Weldon, Edmunds, Love, T. Roberts, Foundation Scholarships; to Pagan, Goodman, Exhibitions. The Open Natural Science Exhibition was awarded to H. Wilson of the Leys School, and another Open Exhibition to J. Kerr of Manchester Grammar School.

SCIENTIFIC SERIALS

Trimen's Journal of Botany, June, 1881, contains:—Notes on *Carex flava*, L., by F. Townsend, M.A.—A revision of the Indian species of Leea, by C. B. Clarke, M.A.—Notes on Irish plants, by H. C. Hart, B.A.—Short notes.—Extracts and notices of books and memoirs.—Botanical notes.

THE *American Naturalist*, June, 1881, contains:—The archæology of Vermont, by Prof. Geo. H. Perkins.—On the larval habits of the Bombyliidæ, by C. V. Riley (with a coloured plate).—On the late explorations in the Gaboon, by H. von Kopenfels.—On the Pueblo pottery, by Edwin A. Barber.

Kosmos, Jahrgang v. Heft 2, contains:—Prof. Dr. Fritz Schultze, on the relations of sceptical naturalism to modern natural science, with especial reference to the evolution theory (conclusion).—Henry Potonié, on the relations of morphology to physiology.—Dr. Fritz Müller, *Atyoida Potimirim*, a mud-eating fresh-water shrimp (with twenty woodcuts).

[1] *Vide Proceedings* of the Linnean Society of New South Wales, August 25, 1878.

Revue Internationale des Sciences, May, 1881.—M. Debierre, on physical dynamism and biological dynamism (concluded).—A. Charpentier, on the examination of the powers of vision, from a general medical point of view.—J. Morton, the city of Gheel, in Belgium, and its asylums.—H. Müller, on the pretended refutation of Boumer of the theory of flowers (translated from *Kosmos*).

Nyt Magazin for Naturvidenskaberne, Christiania, 1880-1881. Band 26, Heft i.—Herr Leonhard Stejneger continues his contributions to the ornithology of Madagascar, and describes a new *Tylas*, which appears to be closely allied to the *T. madagasc.* of Grandidier.—L. Meinich gives the result of his examination of the quartz and sandstone formations of the Trysilfjeld near Kongsberg, Norway, and Herr H. Rensch, editor of *Naturen*, describes the geological character of the strangely dislocated and fissured fjeld known as the Torghattee, on an island off the Heligoland coast. The same writer occupies nearly all the pages of Heft 2, first in giving the remainder of his observations of the Torghatten caverns and rocks, and next in a comprehensive and elaborate description of the character of the conglomerate sandstones and metamorphosed schists in the Nordfjord and Göndfjord districts near Berglun, to which he adds the analyses and histological results obtained from the examinations of these rocks in the Leipzig mineralogical laboratory. These numbers of the magazine contain, however, some specially interesting communications by Herren Danielssen and Koren of the various new forms of Gephyreæ and Echinodermata obtained in the Norwegian Arctic Expedition. These observers describe a form of Bonellia, to which they have given the name of *Hamingia Arctica*, which approximates closely to *Bonellia viridis*, first found in the northern seas about forty years ago by Herr Koren. Only one specimen was obtained of Hamingia. In regard to echinoderms the expedition has proved more fortunate, and Herren Danielssen and Koren describe several new forms of Asterias, Solaster, and Asterina.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, June 9.—S. Roberts, F.R.S., president, in the chair.—Prof. Mannheim and Mr. T. Craig (United States Coast Survey) were admitted into the Society, and Mr. G. R. Dick, Professor of Mathematics in the Royal College, Mauritius, was elected a member.—Much interest was excited at the meeting by the fact that one of the Society's Foreign Members was present, and proposed to read a paper. M. Mannheim is well known in this country to be a most elegant cultivator of the modern geometry on the lines of Poncelet and Chasles. He has more especially worked at the following subjects :—(1) The method of geometrical transformation, following out in this direction Poncelet's researches in the theory of reciprocal polars; (2) the plane representation of certain space-figures; (3) the wave surface (his early papers form the subject of an article in the *Quarterly Journal* for 1878 by Prof. C. Niven, F.R.S.); but lastly, he has been more particularly engaged upon the study of properties relative to the displacement of figures in space; to this he has given the name of "Géométrie Cinématique" (Dr. Ball in his "Theory of Screws" says, "To M. Mannheim belongs the credit of having been the first to study geometrically the kinematics of a constrained body from a perfectly general point of view") :—his recent work with this title has obtained a warm recognition *propter merita* in this country—on this occasion Prof. Mannheim communicated a paper "Sur les surfaces parallèles," which was characterised by all the clearness and power of exposition so well known to belong to mathematicians of the French school. Dr. Hirst, F.R.S., in proposing a vote of thanks, lightly touched upon the novelties of the communication, and expressed the pleasure it gave him and the meeting to see his fellow-student and friend present in the Society's rooms. A cordial vote of thanks having been carried, M. Mannheim briefly thanked the members present for their kind reception of him.—Other communications were :—On certain symbolic operators, by Mr. J. W. L. Glaisher, F.R.S.—On a system of co-ordinates, by Prof. Genese.—Note on a system of Cartesian ovals passing through four points on a circle, by Mr. R. A. Roberts.—On the Gaussian theory of surfaces, by Prof. Cayley, F.R.S.—On a theorem in the calculus of operations, by Mr. J. J. Walker.—On spherical quartics, with a quadruple cyclic arc and a triple focus, by Mr. H. M. Jeffery, F.R.S.—Note on the wave surface, by Prof. Mannheim.

Chemical Society, June 2.—Prof. Roscoe, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting of the Society (June 16).—The following papers were read :—Experimental researches on the amalgamation of silver ores, by C. Rammelsberg.—On the action of solvents on saponified oils and waxes, by A. H. Allen and W. Thomson. The authors have made many experiments with a view of discovering a correct method for the analysis of mixtures of hydrocarbons with animal and vegetable fatty matters. Two methods are suggested. In both, the sample is boiled with a solution of caustic soda in alcohol, which is, in one case, diluted with water, and then shaken up with ether, to dissolve out the unsaponifiable matter, leaving the soap in the solution; in the other method alcohol is added to the mixture, and then some sodium bicarbonate and ignited sand; the whole is dried and extracted by petroleum spirit in a Xoxhettes apparatus. Some analyses are given in the paper; good results were obtained.—On the sulphides of copper and a determination of their molecular weight, by S. U. Pickering. The author has heated cupric sulphide alone, in a current of hydrogen and in a current of carbonic acid, and concludes that the sulphur is given off in two separate and equal portions at totally different temperatures; therefore the molecule contains two atoms of sulphur. Hydrogen reduced the sulphide to the metallic state.—Chemical examination of the Buxton thermal water, by J. C. Thresh. The author has disproved the extraordinary statements of Playfair and Muspratt that one gallon of this water contains 206 and 504 cubic inches of nitrogen. The water really contains 22·98 c.c. of nitrogen per litre. He points out how the error originated. A complete analysis of the mineral constituents is given; amongst them are molybdic acid, cobalt oxide, &c.—On potable waters; determination of total solids, by E. J. Mills. This determination is made by carefully noting the time required by a glass bulb to rise a given distance through the water.—On the estimation of the value of zinc powder and on a gauge for measuring the volume of gases without calculation for temperature and pressure, by J. Barnes.

Zoological Society, June 7.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—The Secretary called the attention of the meeting to the opening of the Insectarium in the Society's Gardens, which had taken place on April 25, and read a report on the insects that had been reared and exhibited there, drawn up by Mr. W. Watkins, the Superintending Entomologist.—Mr. F. M. Balfour, F.Z.S., read a paper on the development of the skeleton of the paired fins of Elasmobranchs considered in relation to its bearings on the nature of the limbs of the Vertebrata. The object of the investigations recorded in this paper was explained by the author to be twofold—viz., on the one hand to test how far the study of the development of the skeleton of the fins supported the view which had previously been arrived at by the author, to the effect that the paired fins were the specialised and highly-developed remnants of a once continuous lateral fin on each side, and on the other to decide between the views of Gegenbaur and Huxley and Thacker and Mivart as to the primitive type of fin-skeleton. The author pointed out that the results of his researches were entirely favourable to the view that the paired fins were structures of the same nature as the unpaired, and that they gave a general support to the views of Thacker and Mivart. They clearly showed that the pelvic fins retain more primitive character than the pectoral. Conclusions were drawn somewhat adverse to the views recently put forward on the structure of the fin by Gegenbaur and Huxley, both of whom considered the primitive type of fin to be most nearly retained in *Ceratodus*, and to consist of a central multisegmented axis with numerous rays on its two sides. It appeared, in fact, that the development of the skeleton demonstrates that a biserial type of fin like that of *Ceratodus* could not have been primitive, but that it must have been secondarily derived from a uniserial type, by the primitive bar along the base of the fin (the *basipterygium*) being rotated outwards, and a second set of rays being developed on its posterior border.—Mr. W. T. Blanford, F.Z.S., read some notes on a collection of Persian reptiles recently added to the British Museum, amongst which was an example of a new species of lizard, proposed to be called *Agama Persica*.—A communication was read from the Rev. O. P. Cambridge, C.M.Z.S., on a new spider of the family *Theraphosidæ*. The chief interest attaching to this spider was the fact that it had lived in the Gardens of the Society from March to October, 1880. Mr. Cambridge proposed to name the species *Homœomma Stradlingii*, after Dr. Stradling, who had brought the specimen in question home from Bahia.—Mr. G. E. Dobson, C.M.Z.S., read a paper on the pharynx, larynx, and hyoid bones in the *Epomophori*, indicating some very remarkable peculiarities of structure, in which these bats appear to differ not only from all other Chiroptera, but from all other mammals. Pharyngeal air-sacs were also described in the males of *Epornophori monstrosus*, *franqueti*, and *comptus*.—Mr. J. Gwyn Jeffreys, F.R.S., read the third of the series of his memoirs on the Mollusca procured during the *Lightning* and *Porcupine* expeditions, 1868-70. The present paper contained an account of the families from *Kelliidæ* to *Tellinidæ*. Eleven new or hitherto unfigured species were described. The geographical, hydrographical, and geological distribution of the species enumerated were fully given.—Mr. F. C. Selous read a paper on the South African Rhinoceroses, based upon specimens collected and observations made during nine years' hunting in Southern and South-Central Africa. Mr. Selous had come to the conclusion that in these countries only two well-marked species of *Rhinoceros* existed—namely, the square-mouthed *Rhinoceros simus*, and the prehensile-lipped *R. bicornis*.

Entomological Society, June 1.—Mr. H. T. Stainton, president, in the chair.—Rev. E. N. Bloomfield, M.A., was elected a Subscriber to the Society.—Mr. J. Jenner Weir, on behalf of Mr. J. W. Douglas, exhibited, and read remarks on, various British species of *Aleurodes* and *Orthezia*, one of which was described as new to science under the name of *O. Normani*. —Mr. T. R. Billups exhibited specimens of *Crabro clavipes*, L., and *Molorchus minor*, L.—Mr. J. Sang exhibited some interesting varieties, &c., of British *Lepidoptera*.—The Secretary read a communication from Mr. G. E. Piercey respecting a creature stated to be noxious to travellers in Turkestan. It was suggested that this was probably identical with the well-known *Argas Persicus*.—The Secretary also read a report from the Committee appointed at the last meeting of the Society to inquire into the supposed presence of *Phylloxera* on the vines in Victoria; also a communication from the Colonial Office respecting an insect stated to be destructive to the eggs of locusts in the Troad.—Lord Walsingham read a paper on the *Tortricidæ*, *Tineidæ*, and *Pterophoridæ* of South Africa.—Mr. A. G. Butler communicated a memoir on the genus *Sypna*.—Mr. W. L. Distant communicated descriptions of *Rhynchota* from the Australian and Pacific regions.

PARIS

Academy of Sciences, June 6.—M. Wurtz in the chair.—The Secretary read telegrams from the Emperor of Brazil (of May 31 and June 2) announcing the discovery of a comet.—On the right ascensions of the moon observed at Algiers by M. Trepied, by M. Faye. Hansen's tables, defective for long-period inequalities, are shown to be perfect for ordinary inequalities. Newcomb's correction, instead of being too great, has to be increased 1″.—Researches on sulphide of nitrogen, by MM. Berthelot and Vielle. *Inter alia*, it detonates with violence under the hammer, but it is less sensible to shock than fulminate of mercury or nitrate of diazobenzol. In heating, it deflagrates about 207°. The heat of formation is negative. The pressures arising from explosion in a closed vessel are very near those from fulminate, but the velocity of decomposition is very different.—On the report of M. Roudaire, on his last expedition in the Tunisian Chotts, by M. de Lesseps. Further examination confirms the feasibility of the inland sea project, the political advantages of which (with others) are noted.—On the geological results of M. Roudaire's mission, by M. Hébert. Tunisia seems to have emerged during the long periods between the deposit of the Senonian Chalk and that of the Middle Miocene. The basin of the Chotts, with the Cretacean masses bordering it on either side (their strata anticlinal), are like a button-hole, the Chotts forming the aperture.—New analyses of jadéite and some sodiferous rocks, by M. d'Amour. It is proved that beds of jadeite (which is largely used in India and China for ornaments, and found in the form of coins, hatchets, &c., in European dolmens and caves), exist in Asia, especially the Thibet region; also in North and South America. From analysis of some European rocks the author finds reason to suppose that beds of jadeite may also be found in the Alpine chain, or region near; thus the prehistoric articles in Europe would be naturally explained, without migration of Asiatic peoples. MM. Boussingault and Daubrée made remarks on the subject.—Study of electricity on board modern ships; incidental remarks (1) on the influence of the mode of junction in complex electric circuits, and (2) on the principle of an electric hygrometer and a fire-alarm, by M. Ledieu. The iron hulls of fast ships, sheathed with wood, then copper, both fixed metallically, form a complex pile, which the author studied. Experimenting with a moist piece of wood having copper nails in it, he found a battery current had much greater intensity when the rheophores were applied to the nails than when applied to the wood. Using dry wood with the former arrangement, the intensity varied with the atmospheric moisture. This might be applied *e.g.* in measuring dew-formation. For a fire-alarm he would keep the wood slightly moist by means of spongy matter on its surface connected with water. A galvanometer wonld indicate the degree of dryness of the wood, and if a certain limit were reached the needle would cause a bell to ring.—On the *rôle* of phosphoric acid in volcanic soils, by M. de Gasparin. He controverts Prof. Ricciardi's view that the fertility of the eruptive strata of Etna is due to presence of this mineral. The concomitance of muddy formations, and the climate, hastening the decomposition of lava, are the chief factors.—The vines of Soudan of the late M. Légard, by M. Planchon.—The solar parallax deduced from American photographs of the transit of Venus of 1874, by Mr. Todd.—On the functions of two variables arising from the inversion of integrals of two given functions, by M. Fuchs.—On the expressions of co-ordinates of an algebraic curve by Fuchsian functions of a parameter, by M. Picard.—On a property of uniform functions, by M. Poincaré.—On the liquid state and the gaseous state, by Mr. Hannay. He claims to have proved, more than a year before, for all pressures, what MM. Cailletet and Hautefeuille have lately established for a single pressure: viz., that the continuity of the liquid and gaseous states (Andrews) is only apparent.—Cyanides of sodium and barium, by M. Joannés. —On the combinations of iodide of lead with alkaline iodides, by M. Ditte.—On the *rôle* and the origin of certain microzymas, by M. Béchamp. The microzymas in rocks, earth, mould, street-dust, or the slime of marshes, have no other origin than those forming an integral part of every living organism, and whose *rôle* is the total destruction of this after death; after which they remain in the soil or the air, ready chiefly for transformation of organic matter for vegetation.—On the non-existence of *Microzyma Cretæ*; reply to M. Béchamp, by MM. Chamberland and Roux.—On the mechanism of troubles produced by cortical lesions, by M. Couty. Unilateral and limited cortical lesions involve profound modifications of the various functions of the medulla oblongata and the spinal cord opposite, leaving intact the brain-functions.—On the embryogeny of Ascidians of the genus *Lithonephora*, by M. Giard.—On the stomatorrhiza of *Succulina carcini*, Thompson, by M. Jourdain.—On the morphology of the fœtal envelopes of Chiroptera, by M. Robin.—Contributions to the cryptogamic flora of Banks's Peninsula (New Zealand), by M. Crié.—Mr. Stone's star-catalogue was presented by M. d'Abbadie.

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