THURSDAY, AUGUST 15, 1889.

THE THREE CRUISES OF THE "BLAKE."

The Three Cruises of the "Blake." Two Vols. By Alexander Agassiz. *Bulletin of the Museum of Comparative Zoology at Harvard College*, Vols. XIV. and XV., Cambridge, Mass. (Boston and New York: Houghton, Mifflin, and Co., 1888.)

IN these profusely illustrated volumes there is presented to the general reading public the best and most comprehensive account of recent oceanographical investigations and speculations that has as yet been attempted. These volumes have, moreover, a special value for all who interest themselves in deep-sea researches, from the descriptions that are given of the work carried on with so much ability and industry by Mr. Agassiz and his fellow-countrymen on the eastern and southern sea-boards of the United States and in the West Indian seas. The volumes abound with novel and ingenious views bearing on nearly all the physical and biological phenomena of the ocean; and, whether we agree with the writer or not, his opinions are none the less welcome and suggestive, coming as they do from one who has for many years taken a large part in the practical work connected with the observations which he here undertakes to describe and discuss. It does not seem possible to over-estimate the credit due to the Government and the men who initiated and have carried through this excellent and extensive hydrographical survey of the deeper waters surrounding the eastern shores of North America, nor to value too highly the resulting additions to human knowledge. These positive additions to our knowledge of the ocean will be fully acknowledged and appreciated by all who desire to trace the causes that have led to the development of the surface features of the earth and the existing conditions of life on our globe.

After an attentive reading of the twenty-three chapters into which this publication is divided, it is possible to point out some errors; but slips cannot be avoided in a work dealing with such a wide range of subjects. It is difficult to follow the author in all his speculations, or to agree with him in all the deductions drawn from his excellent and extensive observations, but such agreement was in no way to be expected in these wide fields of research. Almost all the writings and opinions of previous and contemporaneous workers are in some way noticed, and discussed in an appreciative spirit. Mr. Agassiz is in every respect to be congratulated on the completion of this praiseworthy contribution to the growing science of oceanography. So many subjects are presented for discussion and remark that the reviewer is at a loss to know which may with most advantage be touched upon within the limits of a short notice. An attempt may at all events be made to point out the arrangement of the work, and the nature of the varied investigations treated of in the several chapters.

An introduction gives a brief sketch of the cruises of the *Blake*, the extent of the work undertaken, and indicates the localities in which these were conducted. Acknowledgment is duly recorded for the assistance obtained from naturalists in all parts of the world in working out the results of the dredgings. By arrangement the specimens were as far as possible sent to the same specialists as were engaged in describing the *Challenger* collections.

The first chapter deals with the equipment of the ship, which has already been made known through Captain Sigsbee's publication. Mr. Agassiz, from his engineering training in the copper mines of the West, was able to render very valuable assistance in modifying the apparatus for deep-sea work. To him we owe the introduction of wire dredging rope, improvements in the trawl, tow-net, and other apparatus. The use of tangles on rocky ground was very successful, as it was when used in similar places by the *Challenger*. The tow-net for intermediate depths is ingenious, but the experiments with it are in no way sufficient to prove that no living animals are to be found at intermediate depths, as is sometimes asserted. The *Challenger* experiments clearly showed that when tow-nets were dragged for considerable distances at depths of 1000 and 500 fathoms, they always contained animals (Challengeridæ and other Radiolarians and fishes, &c.) never taken in the nets dragged down to 100 fathoms from the surface. While the great development of life in the ocean is in the surface and sub-surface waters, where Algæ abound, and at and near the bottom, where the organic matters are settling on the mud or ooze, yet it appears to me that the *Challenger* has also shown that there is no intermediate lifeless region.

A short chapter gives a fair and impartial historical sketch of deep-sea work, with special reference to the work off the American coast; and a longer chapter gives a very complete account of the origin, development, and present condition of the Florida reefs—the most thorough account of a series of coral reefs to be found in the literature of the subject. Mr. Agassiz successfully explains the phenomena without calling in subsidence, indeed, he found Mr. Darwin's theory quite inapplicable. He rightly places stress on the vigorous growth of the reefs in all situations where they are bathed by currents coming directly from the ocean, and traces this vigorous growth to the abundant pelagic food brought to the reef-forming corals by these oceanic currents. He also dwells on the formation of submarine banks by the dead shells of these pelagic and other marine organisms. Speaking of the Pacific, he says: "It is difficult to account for the great depth of some of the lagoons—40 fathoms—on any other theory than that of subsidence." It appears to me that these depths are only found in very extensive atolls and barrier reefs, and that marine animals, other than the ordinary reef-building species, can build up submerged banks from much greater depths than 40 fathoms. Buchanan, on such a bank in the Atlantic, found *Lophohelia prolifera* growing in large quantities, together with Polyzoa, Crinoids, and other lime-secreting organisms. Besides, in the central parts of the lagoon of completely formed atolls, like Collomandu or Suadiva atolls, the solution of lime by the sea-water probably exceeds in quantity that secreted by organisms, and this process would of itself result in a deepening of the lagoon.

The topography of the Caribbean Sea, Gulf of Mexico, and eastern coasts of North America are illustrated by the admirable hydrographic charts of the Coast Survey. With a thorough knowledge of the various basins, their depths, currents, passages, and contour lines, Mr. Agassiz is well qualified to enter on a discussion of the relations of the American and West Indian fauna and flora, which he does in a most suggestive and instructive chapter. He says:—

"The deep soundings south of Cuba, between that island and Yucatan and Jamaica, do not lend much support to the theory of an Antillean continent as mapped out by Wallace, nor is it probable that this continent had a much greater extension in former times than now, judging from the depths found on both sides of the West Indian Islands. This would tend to prove the want of close connection between the West Indian Islands and the adjoining continent. It leads us to look for the origin of the fauna and flora of those islands to causes similar to those which have acted upon oceanic islands. The proximity of these islands to a great continent has, moreover, intensified the efficiency of these causes."

Since the return of the *Challenger*, the existence of Tertiary continents in the Atlantic, Indian, and Pacific Oceans does not appear to have been seriously advocated. These views have been generally replaced by that which looks upon the continents and ocean basins as holding positions of great permanence on the surface of the earth. Mr. Agassiz adopts this latter view, and illustrates it by special reference to the geological structure of the American continent and its adjoining oceans. In discussing this matter, he expresses the opinion that the "Blake Plateau" was once within the 100-fathom line, and that it has been cut away to its present depth of 500 or 600 fathoms "by the action of the Gulf Stream acting upon the 'Blake Plateau' from a geological time which we can trace with a certain degree of accuracy." This is a most important conclusion, but I cannot think it will be accepted till more evidence of the action of oceanic currents at these depths can be produced. The deposits I have examined from the bed of the Gulf Stream are principally composed of the shells of pelagic Foraminifera, Pteropods, and other organisms living in the present seas of the region, together with much glauconite and many phosphatic concretions. These would lead one to think that the bed of the Gulf Stream was now growing upwards by these accumulations, rather than being washed away.

All the new and valuable observations on the temperatures of the West Indian seas and on the Gulf Stream are presented to the reader with a wealth of illustration in the way of diagrams and maps that leaves little to be desired, and the chapters on these subjects give to the physical geographer many much-needed data.

Mr. Agassiz has long been known to the scientific world for his special researches on the pelagic animals of the eastern North American coasts, conducted chiefly at Newport since 1866. It was therefore to be expected that his observations in this direction would be attractive and important. Nearly all the principal organisms met with in the tow-nets are illustrated in the long chapter devoted to this subject, and the naturalist will here find much new matter and many novel views concerning the origin of this fauna. He says:—

"It seems most natural to look upon the pelagic fauna of to-day and that of former geological periods as made up of embryonic types removed from the influences necessary for their full development, even after a time reproducing themselves as other larval forms are now capable of doing. But to consider that the littoral forms were developed from pelagic types, as has been suggested by Moseley, does not seem to be warranted by the embryological history of marine invertebrates."

The chapters on marine formations, deep-sea deposits and deep-sea fauna contain the latest information and views as to their origin, and the first volume is concluded with a chapter on the physiology of deep-sea life, dealing with the gases in sea-water, the effects of pressure and temperature, phosphorescence, effects of the absence of sunlight, colours of deep-sea animals, source of their food, and other kindred relations.

It is now recognized that the inhabitants of the abysmal regions differ more from the shore species than they do from one another. Perhaps the most striking characteristic of deep-sea species is that they live in a region where there is no plant-life, and that their food consists primarily of the dead remains that fall to the bottom from the surface. All these animals, therefore, either live by eating the mud or ooze of the bottom, or by devouring each other. It appears to me probable that these deep-sea animals are derived from the shore ones, some species descending into these deep regions and establishing a home there at each geological period, while the forms from which they were derived have died out in the shallower waters.

This and all similar questions Mr. Agassiz discusses in his second volume, where he deals specially with the West Indian fauna of the deep sea. He writes:—

"We may safely assume that but little will hereafter be added to our notions of the association of the sponges, polyps, corals, echinoderms, crustacea, and mollusks, comprising the West Indian deep-sea fauna, and making it in certain groups by far the richest in the world. The number of new forms from the West Indian region constitutes such a vast addition to our knowledge of the principal classes of invertebrates of that fauna as to revolutionize our ideas of geographical as well as of bathymetrical distribution. No other region of the ocean has yielded so abundant a harvest."

I should think that in proportion to the number of dredgings, the regions in the Southern Ocean investigated by the *Challenger*, or off the north of Scotland worked over by the *Porcupine* and *Triton*, might be held to be quite as rich as those of the West Indian Islands. It is to be hoped that this will be shown before long by an expedition thoroughly equipped for examining the deep waters around Britain.

In a series of nine chapters in his second volume Mr. Agassiz attempts for the first time to give a general account of the deep-sea fauna in the areas explored by the *Blake*, commencing with the fishes, and ending with the Protozoa. In this he has been remarkably successful by the help of numerous illustrations. For details the reader must be referred to the volumes themselves, which will be widely consulted, and will well repay all who give them attentive study.

JOHN MURRAY.

KANT'S "KRITIK."

Kant's Critical Philosophy for English Readers. By J. P. Mahaffy and J. H. Bernard. (London: Macmillan and Co., 1889.)

THE abundance of Kantian literature within recent years shows no signs of abating. In Germany itself there is quite a school of students who have taken for their motto "We must go back to Kant," while in England the able commentaries of Prof. Caird and Prof. Watson have been succeeded by the studies of Dr. Hutchinson Stirling, Prof. Max Müller, and Dr. Mahaffy. Mr. Meiklejohn's translation of the "Kritik" still holds its own, though Mr. Belfort Bax and others have tried their hands on Kant, and made many improvements in detail. It is clear that the famous "Kritik of Pure Reason" is still regarded as a necessary element in all philosophic education, and as likely to continue longer in fashion than the more brilliant but less solid speculations of some of the successors of the Kantian school.

There are certain well-known difficulties in Kant's philosophy to which the reader naturally turns when he has in his hands a new commentary on the "Kritik." Is any fresh light thrown on the schematism of the categories, or on the number and derivation of the categories themselves? Are we enabled to understand better the precise value of the principle of causation, or the principle of the permanence of substance? Above all, shall we be satisfied to accept the second and first editions of the "Kritik" as alike containing the true gospel of Kantianism, or are we to be left to suppose that there is a serious divergence between the earlier and later edition, especially in reference to idealism and the refutation of Berkeley? These are a few salient points out of many others on which we look for guidance to some fresh commentator. In the case of one of these, we have for some time been aware of Dr. Mahaffy's opinion. We know that in his early version of Kuno Fischer's "Commentary" he was dissatisfied with that philosopher's treatment of Kant on the subject of idealism. Dr. Mahaffy returns to the charge in the present edition (in which he has the collaboration of Mr. Bernard), perhaps in some measure stirred to resume the controversy by the discovery that Kuno Fischer in his recent "Critique of Kant" had not found reason to alter his original views. As the point is an important one, and as Dr. Mahaffy would probably himself select its treatment as the most characteristic contribution which his new edition has to offer to the better comprehension of Kant, we shall attempt as briefly as possible to lay the questión at issue before our readers.

It was Schopenhauer who decisively stigmatized the second edition of the "Kritik" as inferior to its predecessor. Kant, he declared, had become alarmed at the idealistic conclusions which had been drawn from his principles, and proceeded to mutilate the earlier version of his doctrines by suppression of some passages, and alteration of others. There was one paragraph especially, inserted into the deduction of the categories, which stated in the most explicit terms that the "matter" of our intuitions is derived from a source independent of the understanding; and there was the comparatively long excursus introduced under the heading of a "Refutation of Idealism," which seemed expressly intended to reassure those who thought that Kant had taken up the position of Berkeley. Hence the conclusion was drawn that Kant, "in the weakness of old age," had compromised with the Realists, and that the second edition, in consequence, was by no means so clear and consistent an exposition of Kant's own opinions as the one it was intended to supersede. It is this opinion (which others besides Schopenhauer have entertained) which Dr. Mahaffy sets himself to oppose. He draws attention, in the first place, to Kant's own words in his preface, which contain the most unqualified statement of the practical identity of the two editions. "In the positions themselves," says Kant, "and in the grounds of proof, as well as in the form and completeness of the plan, I have found nothing to alter;" and again, "my present exposition, in substance, as regards the propositions, and even in their method of proof *changes absolutely nothing*, while it varies from the former here and there in the method of the exposition in such a manner as could not be managed by interpolation." These words are certainly explicit enough, and if we are to venture to disregard them, as Kuno Fischer has done, it can only be because on such a question the opinion of the author himself is perhaps not wholly trustworthy, or at all events ought not to be allowed to overbalance the evidence derived from a comparison of the editions. Such a comparison Dr. Mahaffy himself undertakes, and is thereby led to the conclusion that Kant's own judgment was right, and that of his critics was wrong. On the whole, it may be conceded that Dr. Mahaffy makes out his case, but even he would probably allow that the general tendency of the second edition is to accentuate Kant's avoidance of the idealistic position, and to effect, by a not too successful criticism of Berkeley, a reconciliation with the realistic position. Such a tendency is undoubtedly absent in the first edition. Indeed, Dr. Mahaffy admits, in a note on p. 23, that "it may be that Kant was somewhat frightened at the charge of Berkeleianism"; and the history of the controversy on this point given by Dr. Stirling in the eighth volume of *Mind* entirely confirms the opinion that in some fashion the effect produced by the first edition was one which Kant set himself to alter.

One of the important passages is, of course, the "Refutation of Idealism," which, although it was not wholly new, but had already been outlined in the first edition among the "paralogisms," still deserved the attention which the German critics gave to it because of the new position and importance which it assumed in the revised version. Is it the case, as the Idealists assumed, that the intimations of the inner sense (internal experience) are more trustworthy than the intimations of the outer sense (external experience)? May we rightly argue from sensations to the percipient who is the subject of the sensations, although we are debarred from arguing from sensations to the "matter" or external object to which the sensations are referred as their cause? According to the "Refutation of Idealism," Berkeley is clearly wrong: both inner sense and outer sense have precisely the same validity, inasmuch as both yield us "phenomena" of the same value: while it is also suggested that were it not for the *permanent* object of sensation, no sense of change, no sequence in the intimations of the inner sense, would be possible at all. Dr. Mahaffy devotes a chapter (chap. xiv.) to the discussion of this question, and we venture to think that there is no part of his work which more deserves an attentive perusal. We would especially point to the passages in which he discusses the precise meaning of Kant's principle of permanence (pp. 212 *et seq.*). Berkeley's polemic against matter was a disproof of the supposed substratum of qualities. But, according to

Kant, Berkeley confused matter as a substratum of qualities with matter as a thing *per se*. No doubt we can never prove the existence of matter as a thing *per se*. On the other hand, the notion of a substratum is necessary for our knowledge in order to account for that permanence without which there would be for us no real world at all. This permanent substratum is itself *phenomenal*, because we can form no notion of permanence *except in space*, and space is a form of sensible intuition. Hence Kant would be the last to sanction any speculations on the permanence of unknown things *per se:* he is no materialist in the vulgar sense of the word; but he is, to the extent above explained, a problematical realist, in so far as he accepts the necessary principle of a permanent phenomenal substratum. If we further ask, Whence do we obtain this notion of permanence? Kant answers that we derive it "from the fact that all our experience is comprised in one time, which time cannot be perceived in itself, but only when occupied by some perception. Hence we infer the permanence of the matter of experience, of phenomenal substance, the changing states of which correspond to the various portions of changing time comprised in the one great complex of time. Thus we represent to ourselves the permanent, even though we have no permanent representation; and as an empirical criterion of this permanence in time, we use impenetrability, or modes of resistance in space" (p. 213).

Kant was, however, not content with merely indicating the equal authority of external experience as compared with internal experience. He advances to a still bolder position. While both external and internal experience, although they can give us no information concerning, objects *per se*, are equally immediate and equally certain Kant thinks that it can also be proved that in some respects external experience is the more important of the two. For internal experience is, after all, only possible if we presuppose external experience. Internal experience is subject to the form of time; it is made up of changing modes of consciousness. But change can only be understood if we already have the notion of permanence (" only the permanent can change," says Kant), and the notion of permanence is to be found in that permanent substratum which underlies all our external experience. Hence it is so little true that internal experience is more certain than external that the reverse is almost the case. Without external experience there would be for us no possibility of internal. Where Kant's critics have generally gone wrong is in assuming that where Kant speaks of a permanent substratum he means a thing *per se*. But this is not the case. What he means is a phenomenal substratum, the non-recognition of which is the fatal error of Berkeley.

We have spent so much time and space over this point that we have left ourselves but little opportunity to speak of others. But we do not think we are wrong in assuming it as the point of capital importance in Dr. Mahaffy's new edition, especially as it is at once the most original and the most effective part of his polemic against Kuno Fischer and other German critics. But there are many other features which deserve attention, although we can do no more than refer to them. We would especially direct the reader to the following. Let him observe Dr. Mahaffy's clear explanations of Kant's passage from the ordinary table of logical judgments to that of the categories (pp. 80 *et seq.*), his treatment of the categories themselves (pp. 88 *et seq.*), as well as his vindication of Kant against Mansel, Fichte, and other critics of the categories (pp. 100 *et seq.*). Chapter vii., on the deduction of the categories, is an important one, especially as it compares the " Prolegomena " with the " Kritik." If Dr. Mahaffy has not been able to bring into clearer light the difficult and unsatisfactory treatment which Kant has given to his principle of causality (pp. 180 *et seq.*), he at all events has effectively criticized Schopenhauer's carpings at the Kantian categories and schemata (pp. 151 *et seq.*). Mr. Bernard's contributions to the commentary begin with chapter xvii., and deal with Kant's " Dialectic of the Pure Reason," but we believe that for most students the chief interest of this new edition will be found to be concentrated in the commentary on the "Analytic." It should be added that the second volume is a translation of Kant's " Prolegomena," and contains also, in the appendix, the suppressed passages of the first edition of the " Kritik." The whole edition forms a striking and valuable version of the logical views of Kant, and we can imagine no text-book more helpful both for older and younger students of Kant's immortal work. W. L. COURTNEY.

OUR BOOK SHELF.

Monograph of the Marine and Fresh-water Ostracoda of the North Atlantic and of North-Western Europe. By Dr. G. S. Brady and Rev. A. M. Norman. (London: Williams and Norgate, 1889.)

ABOUT twenty-one years ago Dr. G. S. Brady published, in the Transactions of the Linnean Society of London, a monograph of recent British Ostracoda. The present monograph is to some extent a supplement to the former one, but as it embraces the description of the forms belonging to a greatly extended area, it may be regarded in the light of a new work, in the publication of which the authors have been ably assisted by the contributions of most of those zoologists interested in this group of Crustacea. The present memoir deals only with the section of the Podocopa. The geographical area embraces the Arctic Seas, the North Atlantic Ocean, and North-Western Europe. The North Atlantic area is fixed at 35° N., thus excluding the tropical species of the West Indies and the Gulf of Mexico; the Mediterranean is not included, as the doing so would have too greatly extended the limits of the work; and the North-Western European area embraces Austria, Germany, France, Belgium, Holland, Denmark, Scandinavia, and the British Islands. The distribution of the living species, as far as known, is recorded. One hundred and eighty-eight species inhabiting salt water are recorded, and sixty-one fresh-water forms, and yet it is certain that the record is still very incomplete. While the marine species of Norway and Sweden have been in part studied, little has been done with respect to the marine species of Denmark and Germany. The knowledge of the Dutch marine forms has been derived from some dredgings in the Rivers Maas and Scheldt. Those of the Belgian and French coasts are also but little known, and the same may be said of the truly Arctic species, while nothing is known of the forms inhabiting the coasts of the United States or Canada.

The fresh-water Ostracods of Norway and Sweden have been more or less investigated by G. O. Sars and Lilljeborg; in Denmark by no one since the time of O. F. Müller (1785); in Holland not at all; in Belgium only by Plateau: in Germany by Koch, W. Müller, and Zaddach, whose type species were very generously intrusted to Brady and Norman by Prof. Seeliger, of Königsberg. In France, the species have only quite recently been investigated by Moniez. Those of Great Britain are the only ones at all extensively worked out.

This monograph, which consists of 200 quarto pages, is illustrated with fifteen plates, and is published as Part II. of the fourth volume of the Transactions of the Royal Dublin Society.

The Harpur Euclid. Books I.—IV. By E. M. Langley and W. S. Phillips. (London: Rivington, 1889.)

THIS is an edition of the "Elements" "revised in accordance with the Reports of the Cambridge Board of Mathematical Studies, and the Oxford Board of the Faculty of Natural Science." The favourable impression made upon us by the two previous instalments of Books I. and II. is thoroughly confirmed by the additional matter now before us. We are not going to say that it is the best edition we have seen, for lately we have had under our notice two or three excellent works on much the same lines, but it is certainly not inferior to any of these, whilst for the arrangement of the text, the variety of type, and boldness and *correctness* of the figures, it is admirably adapted for school purposes. The editors, following on the lines of Mackay's interesting edition, have supplied ample store of illustration and historical matter, which will render the study of "Euclid" interesting to the intelligent boy. Not content with embracing within their net an account of Simson's line and the nine-point circle, Mr. Langley, in an article on the principal circles of a triangle, has given an excellent though brief description of the properties of symmedian lines and of the Brocard, cosine, and other modern circles, thus bringing the teaching of these circles within the range of the school curriculum. The exercises now reach the respectable total of 677, and to many of them useful hints for their solution are appended. We await the completion of the authors' task with interest, for a good work on elementary solid geometry is not altogether superseded by Nixon's admirable manual.

An Elementary Treatise on Mechanics. Part I. Statics. By Rev. J. Warren. (London: Longmans, Green, and Co., 1889.)

THIS is an elementary treatise intended for the use of schools and University students. The arrangement and methods of reasoning are best adapted for students who have made some acquaintance with trigonometry. At the same time a considerable portion of the book may be read by those students not possessing such mathematical knowledge. Each chapter contains a good selection of examples, some of which are worked out. This feature, together with the fact that the various theorems and results are very clearly established, will help to render the book a useful one. An experimental proof only is given of the parallelogram of forces, other proofs of which will be found in the second part of the work, which is devoted to dynamics. G. A. B.

LETTERS TO THE EDITOR.

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

The Earlier Eruptions of Krakatão.

AT the time when a Committee of the Royal Society entrusted me with the preparation of a sketch of the geological facts bearing upon the great volcanic eruption of Krakatão in 1883, I was quite unaware that any reliable records concerning the early history of the East Indian Archipelago were in existence. I stated, therefore, that authentic history commenced only about three centuries ago, when the district began to be visited by the Dutch and Portuguese navigators; and that the earliest eruption of which we have any detailed account was that of 1680, described by Vogel and Hesse.[1]

At the same time, however, I pointed out that the study of the geological structure of the district—so well described by Mr. Verbeek and by MM. Bréon and Korthals—especially when that structure was considered in the light afforded by other volcanic areas that have been investigated by vulcanologists, led to very definite conclusions as to what must have been the early history of the great volcano.

It was shown that Krakatão is situated on a fissure traversing the Sunda Strait and running at right angles to the great band of volcanic activity which traverses the islands of Java and Sumatra; and it was inferred that at a very early period a volcano of great height and bulk was built up at the point marking the intersection of the two lines of fissure; it was then stated that—

"At some unknown period this volcano became the scene of an eruption, or series of eruptions, which, judging from the effects they have produced, must have been on even a far grander scale than that which four years ago attracted so much interest. By these outbursts the whole central mass of the volcano seems to have been blown away, and only an irregular crater-ring left behind. The great crater thus formed must have had a diameter of three or four miles, and its highest portions could have risen but a few hundreds of feet above the present level of the sea."

After pointing out that this crater-ring became to a great extent filled up by a succession of smaller eruptions, I proceeded to say:—

"Whether the tract now constituting the Strait of Sunda was then dry land uniting the islands of Java and Sumatra, we have no means of determining; but I may point out that there are some grounds for believing that the formation of the depression occupied by the straits was subsequent to the evisceration of the volcano."

After stating what these grounds are, I added:—

"It seems not improbable that the depression between the islands of Java and Sumatra may have resulted from subsidences accompanying or following the ejections taking place at the great central volcanic focus of Krakatão" (*loc. cit.*, pp. 7–8).

I am greatly indebted to Mr. C. Baumgarten, of Batavia, who, through Dr. R. Rost, the Librarian to the India Office, has called my attention to some ancient records that seem to confirm, in a singularly striking manner, these conclusions arrived at by scientific reasoning.

Mr. Baumgarten writes as follows:—

"In a Javanese book called 'Pustaka Raja,' the 'Book of Kings,' containing the chronicles of the island, kept secret during centuries in the Royal Archives, and only recently made public, we find the following interesting and curious account of an eruption of the mountain Kapi:—

"'In the year 338 Saka [*i.e.* A.D. 416], a thundering noise was heard from the mountain Baṭuwara,[2] which was answered by a similar noise coming from the mountain Kapi, lying westward of the modern Bantam. A great glaring fire, which reached to the sky, came out of the last-named mountain; the whole world was greatly shaken, and violent thundering, accompanied by heavy rains and storms, took place; but not only did not this heavy rain extinguish the eruption of fire of the mountain Kapi, but it augmented the fire; the noise was fearful, at last the mountain Kapi with a tremendous roar burst into pieces and sunk into the deepest of the earth. The water of the sea rose and inundated the land. The country to the east of the mountain Batuwara, to the mountain Kamula,[3] and westward to the mountain Raja Basa,[4] was inundated by the sea; the inhabitants of the northern part of the Sunda country to the mountain Raja Basa were drowned and swept away with all their property.

"'After the water subsided the mountain Kapi and the surrounding land became sea and the Island of Java[5] divided into two parts.

[1] "The Eruption of Krakatão and Subsequent Phenomena," Report of the Krakatão Committee of the Royal Society (1888).

[2] "Now called Pulosari, one of the extinct volcanoes in Bantam, and the nearest to the Straits of Sunda."

[3] "Now called the 'Gedé' mountain."

[4] "The most southern volcano of Sumatra, and situate in the 'Lampung' country."

[5] "The Sanskrit *Yawa-dwipa*."

"'The city of Samaskuta, which was situate in the interior of Sumatra, became sea, the water of which was very clear, and which was afterwards called the lake Sinkara.[1] This is the origin of the separation of Sumatra and Java.'"[2]

Whether we are justified in accepting this date (A.D. 416) as that at which the very grand eruption of Krakatão, and the accompanying subsidence which led to the separation of Java and Sumatra, actually took place, I am not prepared to say. It may be that, as in many similar cases, the floating traditions of a grand catastrophe attached themselves to a subsequent event of a similar character. It is certainly very interesting to learn, however, that in the fifth century very grand volcanic outbursts were taking place in the district in question; and that a belief existed in the former connection of the islands of Java and Sumatra. Nor is it unimportant to discover that tradition is in complete harmony with scientific reasoning in assigning the separation of the two islands to actions occurring concurrently with great volcanic outbursts.

In concluding this note, I must express my great obligations to Mr. Baumgarten and Dr. Rost for bringing these important records under my notice. JOHN W. JUDD.

[1] "The well-known Lake of the 'Menang-Kebo' country."

[2] See the "Krakatão Eruption and the Javanese Chronicles" in *Trübner's Record* for August 1889 (third series, vol. i. pt. 3).

On some Effects of Lightning.

PROF. MCMILLAN's interesting letter in NATURE of July 25 (p. 295) contains some minute details of the effects of a lightning-stroke on a house near Calcutta, on June 8 last. I agree with the writer that such cases are of value to electrical science, especially when reported by a competent observer. In Prof. McMillan's excellent letter there is one word to which I object, and that is "vagaries," as applied to electricity at high potential. When lightning enters a house, it is as much subject to law as when it flashes from the cloud to the earth, and does not behave with the whim, caprice, or freak implied by the word "vagary." In the absence of continuous conductors, the electrical discharge drags into its path light, conducting substances, which assist its progress, and by means of which it can strike through considerable distances and in various directions, as in the case before us. As to the effect of the discharge on the air of the house, Mr. McMillan appears to have made a real advance towards the solution of a difficult problem—namely, What is the origin of the powerful odour produced by a lightning-discharge within an inclosed space, such as a room or a ship? In most cases, the odour is compared to that of burning sulphur—"the ship seemed to be nothing but sulphur," was entered in the log of the *Montague*, after having been struck by lightning. Now as far back as 1785, Cavendish's famous experiment proved that electrical discharges in a confined mass of air lead to the formation of nitric acid, and Liebig found that acid in seventeen samples of rain-water collected during thunder-storms. Nevertheless, with these facts before him, Snow Harris wrote: "From whence this odour arises is still an interesting problem in physics," and he declines to discuss "those chemical views which some able philosophers have entertained of the nature of the odour emitted." Arago also states that the odour is generally compared to that of burning sulphur; but he adds: "others compare it to phosphorus, others to nitrous gas"; and significantly remarks: "L'odeur de gaz nitreux serait le plus facile à expliquer." Now, Prof. McMillan has shown that nitrogen teroxide, more or less diluted with air, was sufficient in the case so ably reported by him, to account for the colour and odour of the atmosphere produced within the house by the electric discharge. "The whole house seemed to be filled with an orange-coloured gas, mixed with clouds of dust affecting the breathing like fumes of burning sulphur," is the description given by the occupier of the house.

Another point of interest in this valuable communication is the introduction of ball-lightning. Arago is sceptical as to the existence of ball-lightning (*éclairs en boule*), or that which moves through the air at a comparatively slow rate, appearing like a luminous ball or globe of fire. Faraday is also equally sceptical. But the well-attested cases of what we name ball-lightning, and the Germans *Kugelblitz*, are so numerous that they can no longer be termed, in Arago's language, "a stumbling-block (*pierre d'achoppement*) for meteorologists." Snow Harris properly describes these luminous balls as a kind of brush or glow discharge. In the well-known case of the *Montague*, the luminous ball was seen rolling on the surface of the water towards the ship from to windward; evidently a brush discharge, or St. Elmo's fire, produced by some of the polarized atmospheric particles, yielding up their electricity to the surface of the water. On nearing the ship, the point of discharge became transferred to the head of the mast, and, the striking distance being thus diminished, the whole system returned to its normal state—that is to say, a disruptive discharge ensued between the sea and the clouds, producing the usual phenomena of thunder and lightning, described by the observers as "the rising of the ball through the mast of the ship." In Prof. McMillan's case I do not understand his remark that "no second ball was seen to enter from the opposite side to meet the first, and so produce the apparent explosion." Surely a second ball was not necessary to produce the effect described—namely, "an intensely brilliant ball of yellow fire, about 6 or 7 inches in diameter, which passed from one end of the room to the other at a pace just sufficiently slow to allow it to be readily followed by the eye: it appeared to be momentarily checked, then burst with a deafening report, which shook the whole house." In other words, it passed from a brush into a disruptive discharge.

Lastly, we have another remarkable confirmation of the fact that a lightning-conductor does not afford protection to surrounding objects. According to the French theory, a lightning-rod affords protection over a circle equal to twice its radius. But there are numerous cases to prove that no such radius of protection exists. The *Endymion* frigate, at Calcutta, in March 1842, was furnished with a chain conductor on the mainmast, but the lightning struck the foremast, shivered the topgallant and topmast, and damaged the lower mast. The mast struck was not above 50 feet from the mainmast. A somewhat similar accident happened to the *Etna* in Corfu, in January 1830. So also in the case that excited so much discussion at the time, the Board-house at Purfleet was struck on May 12, 1777, at a point about 40 feet from the conductor. A similar case occurred at the Poor-house at Heckingham in June 1781. So also in the recent Calcutta case, there was a conductor at one end of the building, projecting 8 or 9 feet above the roof-level. But the lightning entered the house by an iron-covered hatchway, 70 feet from the conductor, and near to a shell factory, which bristled with conductors.

Prof. McMillan properly attaches great value to such cases as the one he reports, leading as they do to the conditions which should govern the protection of buildings. In the course of a long experience, I have noticed that the profession which should be the best instructed on the subject, is—I hope I may say was—the worst. When the new buildings for the Cholmeley School at Highgate were being erected, the head master consulted me as to the erection of a lightning conductor, and asked me to see the architect. That gentleman called on me and said, "We never put up these things; we don't approve of them. I never erected one in my life, and don't know how." I once visited a church in Rutlandshire, that had been restored by Sir Gilbert Scott. The rector took me to one of the gable ends, and said, "You see, we have a lightning conductor, properly insulated by means of glass rings." I replied that on visiting the granite lighthouse at the end of the Plymouth breakwater, I noticed that Faraday, in fixing a lightning conductor, had caused a spiral groove to be cut inside the shaft from top to bottom, for the insertion of a massive copper band, so as to make the conductor an integral part of the building. Snow Harris's method of protecting a ship applies also to a building. At whatever part of the ship or building the lightning may strike, it ought to find an easy metallic path to the sea, or to the earth. The late Prof. Clerk Maxwell, writing to me as to the best method of securing a building, proposed to inclose it with a network of good conducting material, such as a copper wire, No. 4 British wire gauge, to be carried round the foundation of the house, up each of the corners and gables, and along the ridges. Further details would occupy too much space on the present occasion.

Highgate, N., August 1. C. TOMLINSON.

SOME weeks ago, two trees were struck by lightning near St. Albans, in Hertfordshire, the effects of which are most unusual. The two trees stood near each other in a wood called Symonds Hyde Wood. Assuming that the lightning struck downwards, it is easy to see in one case where the damage began—namely, at a place where a branch had by some means been broken off formerly, leaving a ragged break, into which no doubt water had soaked. Thence for some feet downward the effect was apparently merely to split the bark in the usual way. But, at a height of about 20 feet from the ground (as I judged), something in the nature of an explosion must have taken place. Not only is the bark stripped absolutely clean off in large sheets from the level of the ground up to a height of about 30 feet, some of the sheets having been shot to a considerable distance, but where the explosion seems to have occurred, and for a considerable height above and below, the solid timber is burst open and broken into shivers, and the tree, which was a very fine one, is broken short across at the point where the greatest amount of splintering has taken place. Here, too, a considerable branch seems to have been wrenched off by the explosion.

The other tree is at a distance of perhaps 30 yards, and was, if anything, a still finer one. The appearances here are precisely similar, except that the lightning, which I imagine was conducted along the wet surface of the twigs and smaller branches at the top of the tree down to the junctions of the main branches with each other and the stem, here appears to have struck into the wood, tearing the bark up into rough filaments, which still remain attached to the surrounding bark. Most of this tearing occurs at the angles where the main branches join; but I noticed two places, each about the size of a five-shilling piece, each of which was at some distance above an angle. One such "bruise" appears also in the first-mentioned tree, some distance above the broken bough where the main body of the electricity appears to have entered. The "explosion" in the second tree was at a less height than that in the other.

It has been suggested that a good deal of the splintering may have been caused by the trees, weakened by the stroke, having been broken short off by the storm of wind which presumably raged at the time. I do not think this a probable explanation, for the following reasons. The trees are well within a considerable wood, where the effect of such a gale would not be fully felt, while the two tops are now lying almost at right angles to one another; not parallel as one would expect if the same gale of wind had overthrown them. It appears most probable that a violent explosion occurred, not exactly in the middle of the stem, but rather to the side remote from that to which the tops have fallen. I think it will be found that all the appearances agree with this explanation.

The destructive nature of the strokes cannot be adequately described by words: long splinters, of wedge-shaped section, are sticking up everywhere at the place of fracture, while many feet below it a pen-knife can be inserted easily in numbers of crevices which run up and into the stem along the radial lines which are always formed in the growth of oak timber, and which have here been split open.

Some black stains on the soil at the foot of the trees are pointed out as the effect of the "fire." It will be seen, however, that these are caused by the tannin in the oak sap staining the iron-impregnated clay soil.

I have seen numbers of lightning-struck trees, but have never seen anything to compare with these, and I much hope that some one, with more knowledge of lightning and its effects than I have, will take the train to St. Albans, with a photographic apparatus, and see and judge for himself, and give us some explanation. If he will write to my father (Rev. Dr. Griffith, Sandridge Vicarage, St. Albans, Herts), with a couple of days' notice or so, he will be happy to do all in his power to help, and will no doubt drive him to the spot.

Readers of NATURE may possibly remember a former letter of mine, written, if I recollect aright, in the autumn of 1879, describing a sparrow and her nest, flung out of a crevice in a chimney struck by lightning—the bird almost entirely plucked. The bird I placed in spirits, and deposited in the Cavendish Museum at Cambridge. The barking of these trees reminded me of the plucking of that bird.

A. F. GRIFFITH.

15 Buckingham Place, Brighton, August 8.

A Brilliant Rainbow.

FROM the veranda of this Club, several of the members, including myself, have, within five minutes of the time of writing this (6.30 p.m.), witnessed a phenomenon the parallel to which we have never seen before—a most brilliant rainbow, the usual parallel reflection, and another rainbow *or* reflection quite as brilliant as the ordinary and usual reflection, at an angle which I sketched at the moment, but which, as I have no compasses, I cannot draw semicircular. The tide is out, and nothing but little puddles of sea are left.

E. BURTON DURHAM.

Alexandra Yacht Club, Southend-on-Sea, August 11.

THE 1851 COMMISSIONERS' ESTATE AT KENSINGTON.

A NEW light has been thrown upon the proposals of the Commissioners of 1851, to make grants for provincial Museums, and to found Scholarships of science and art, which were discussed in NATURE of July 18 (p. 265). As was then explained, the carrying out of these proposals would necessitate the sale of a considerable portion of the Commissioners' estate at Kensington Gore. We also showed that public protests had been raised during the last three months against any such sale; further, that State aid in co-operation with local effort has provided the country with a system of science and art Scholarships and grants for provincial institutions, and that the Commissioners are not in a position also to launch and administer a corresponding system of grants and Scholarships themselves, for the benefit of the country. But as the Commissioners have published a notification of their intention to proceed with their scheme, it is perhaps but natural that, when it is attacked, they should defend their position as ably as may be. The attack which has been made upon their scheme is twofold in character, but it aims chiefly at the preservation of the inner gardens of the estate from the degradation of being sold for and used as a common site for private houses, to the unavoidable detriment of the institutions upon the estate.

As regards the scheme for grants and Scholarships, Mr. Samuelson, M.P., gave notice of a question in the House of Commons to Sir Lyon Playfair, the gist of it being whether, in formulating their scheme, the Commissioners had examined, and taken account of, the scheme (identical in direction) which has been for years and is in operation under the Science and Art Department. Sir Lyon Playfair transferred the answering of this question to the Home Secretary, who stated accordingly that the Commissioners' scheme was being prepared, and would be printed in due course and laid before the House of Commons. Under these circumstances, we may postpone further remarks upon the Commissioners' grants and Scholarships scheme until it is printed in a complete form and submitted as promised.

In the meantime, however, we must take notice of another incident connected with the Commissioners and their proposals. On July 19, Mr. Bartley, M.P., gave notice of a question to be asked of Sir Lyon Playfair on July 22. The question was, whether Sir Lyon would, as a Commissioner of 1851, state the names of the different persons, public bodies, and institutions which had sent protests to the Commissioners for the Exhibition of 1851, against the proposed sale of part of the inner gardens of their estate at Kensington Gore for private buildings. Sir Lyon Playfair, however, was not in his place when the question should have come on on the 22nd. It was postponed to the 23rd; and when the printed notice paper for that day appeared, it was found that an arrangement had been made for the question to be answered, not by Sir Lyon Playfair, but by his brother Commissioner, the Home Secretary. The question was therefore asked of that Minister, and, before the answer to it could be given, Sir Lyon Playfair got up and asked whether the right hon. gentleman was aware that a deputation of the Mayors of nearly all the largest towns in England and Wales, representing a population of more than 3,000,000, had waited on the Commissioners to urge that the property in South Kensington should be sold and realized, in order to be applied to provincial Museums. Mr. Matthews replied to both the questions in one answer.

There had, he said, been protests from public bodies and towns against the sale of the estate, and it was also the case, as indicated in the question of Sir Lyon Playfair, that a deputation had waited on the Commissioners and urged the sale. The impression thus conveyed was, that the Commissioners had quite recently been between two fires, and had given way under the hottest. This would be a poor enough plight for a body of public men, who, up to within the last three or four years, had been consistent and strong enough to adhere to the main lines of a policy traced out for them in 1852, when they were charged with a public trust. But it is worse than a poor plight when considered, as it has to be, in conjunction with matured opinions and resolutions published by that same body of men in 1878.

The deputation to which Sir Lyon Playfair directed attention waited on the Commissioners, not, as was imagined, quite recently, but as long ago as 1877. The deputation was received at Marlborough House on June 20, 1877, and was headed by Mr. Chamberlain. The Prince of Wales made a very brief reply to it; and Lord Granville delivered a full exposition of the Commissioners' reasons for disagreeing with the views of the deputation. The deputation "proposed, as the best method of dealing with the Commissioners' trust: (1) the realization of the estate to as great an extent as possible; and (2) the application of the realized funds, in grants to provincial Museums for buildings, and for the purchase of suitable objects for exhibition therein." Amongst other statesmanlike views of their responsibilities towards their trust and to the public at large, Lord Granville, on behalf of the Commissioners, pointed to the importance, in the founding of provincial Museums, of voluntary subscriptions, and to the "danger of stopping the flow of them if you [the deputation] get a great central body supplying everything. There is the consideration how far even a large sum would go." The possibility of realizing a portion of the estate was mentioned; but it should be clearly stated that that portion of the estate was a site lying to the *south* of the present Imperial Institute. No portion of the inner gardens, upon which the Albert Hall abuts, was to be put up for sale.

It will be useful to now give extracts from the Sixth Report of the Commissioners published in 1878, shortly after the deputation above referred to had waited on them.

"The claim of the provincial towns to share in the application of our funds was supported by two considerations. In the first place, it was said that the support which enabled the Exhibition of 1851 to be held was obtained, to a large extent, from our great manufacturing centres. As a fact, the metropolis subscribed £35,108 16*s*. 11*d*., and the provinces £34,057 12*s*. 8*d*. The second consideration urged was the difficulty which is felt in provincial towns in raising, by means of rates, amounts adequate to meet the expenses of building and maintaining scientific museums and galleries of art.

"That we are not insensible to the claims of the provinces to a share of the benefits to be derived from the resources at our command will be seen from the resolutions at which we have arrived, but as to the form in which such benefit should accrue we hold different views from those expressed by the deputation. In the first place, we consider that the proposal to capitalize the whole of our property cannot be entertained, because, apart from all other reasons, it is sufficient for our present purpose to observe that it would clearly be at variance with the appropriation of the land originally contemplated, and a reversal of the whole past policy of our body.[1]

"The object with which, under the guidance of the Prince Consort, we purchased the estate was to provide a remedy for the want so often felt in this country of an extensive site for the development of great institutions for the promotion of industrial art and science amongst the manufacturing population. The South Kensington Museum and the Museum of Natural History are two great monuments of the prudence of the course adopted, and, so long as the wants of technical education are so inefficiently provided for in this country as they are at present, we think that we ought to keep in our hands the means of meeting the possible requirements of institutions for that purpose. . . .

"The suggestion that the resources at our command might be applied to the promotion of local Museums of Science and Art, by grants in aid of buildings or collections of suitable objects, had received our full consideration before the deputation of provincial municipal representatives, already referred to, pressed this course upon us. The establishment of local Museums is an object which has long commended itself to us. In the original scheme, drawn up in the year 1868, for the series of annual International Exhibitions, it was proposed that 'a sum of money might be annually devoted to make purchases of remarkable objects, which might be sent to local Museums throughout the country.' If this scheme had proved permanently successful, we might have been able to supply, by degrees, each of the principal centres of industry with collections of objects illustrating the manufactures in which they are chiefly interested. But, in the present condition of our trust, we see several objections to the promotion of local institutions as a method of applying our resources. Firstly, the amount of the funds at our disposal is very limited, as compared with the numerous demands which might legitimately be made upon us in case we announced our readiness to make grants in favour of local institutions. Secondly, we fear the risk that the knowledge that a central body was ready with funds to assist local objects would have the effect of decreasing rather than stimulating private local subscriptions, and of producing a lukewarmness in local efforts which would far more than counterbalance the moderate amount of assistance which a share of our funds would provide. Thirdly, and chiefly, it is evident that such grants, while exhausting our funds, would result in mere temporary help to science and art."

The above extracts relate to the deputation to which Sir Lyon Playfair has drawn attention, and upon whose views, very distinctly controverted by the Commissioners, he seems to rely for justification in setting aside the protests made within the last three months against a sale such as the Commissioners have been opposed to.

It is therefore difficult to escape from some such conclusion as this: that the projected sale of the inner gardens of the estate for private building speculations will be an autocratic act utterly ruinous to the character of the Commissioners; and that the Commissioners persist in defending and urging this act by pleas and arguments stoutly opposed by themselves twelve years ago.

[1] "The following is an extract from our Second Report, dated in 1852:—'A large number of suggestions and applications, in reference to the disposal of the surplus, have been made to the Commissioners. . . . The answer which the Commissioners have returned to the different applications submitted to them has been to show, by reference to their preliminary Report to Her Majesty, of the 6th of November of last year, that they do not feel themselves to be in a position to comply with proposals which involve the surplus being applied to purposes of a limited, partial, or local character, or to returning to the different localities, in order to be there appropriated to local public objects connected with the progress of art, science, and education, the amount of subscriptions originally raised in each place, which subscriptions were at the time made on the clear understanding that they must be "absolute and definite." The Commissioners would call especial attention to the memorials from the important manufacturing and commercial towns of Birmingham, Bristol, Halifax, Hull, Oldham, Sheffield, and the Staffordshire Potteries, which are appended to this Report, and indicate clearly the strong feeling entertained by those well entitled to form an opinion on this subject, of the importance of establishments for instructing those engaged in trade and manufacture in the principles of science and art on which their respective industries depend.' The towns mentioned presented memorials praying for the establishment of a Central Institution of Arts and Manufactures."

THE INTERNATIONAL CHEMICAL CONGRESS.

An International Chemical Congress has just been held in Paris under the presidency of M. Berthelot. The Congress was composed of some 300 members, including the most eminent French chemists, and a certain number of distinguished foreigners. It was much regretted that foreign countries should not have been more numerously represented, and it was explained that owing to the protracted illness of M. Hanriot, Secretary of the Paris Chemical Society and of the Congress, the invitations were not sent out till very late, and still further delay was caused by their passage through the Ministry of Public Instruction. Much credit is due to M. Fauconnier, who undertook to replace M. Hanriot at the last moment.

The opening meeting was held on the 29th ult. at the Conservatoire des Arts et Métiers, the proceedings being of a purely formal character. M. Berthelot, who occupied the chair, said that a large number of communications had been received by the organizing Committee, and that from the nature of these communications it had been found expedient to divide the Congress into four Sections, which he proceeded to enumerate, together with the list of Sectional Presidents and Secretaries proposed by the Committee :—

Section I. Analysis of Food-Products: President, M. Riche; Secretary, M. Bishop.

Section II. Analysis of Agricultural Products: President, M. Joulie; Secretary, M. Demoussy.

Section III. Analysis of Pharmaceutical Products: President, M. Petit; Secretary, M. Bocquillon.

Section IV. Unification of Chemical Nomenclature: President, M. Friedel; Secretary, M. Bouveault.

The Sectional meetings took place in the day-time; the evenings of July 30, August 1, 2, and 3, being reserved for the discussion of the reports presented by the various Sections.

Section I.—The Section discussed a certain number of methods used in the analysis of flour, bread, wine, coffee, &c. At the general meeting the following resolutions were passed :—

(1) That the Government be requested to inspect regularly the factories of salted meats, as at present the brines used are added to constantly, but never changed (report by M. Combes).

(2) That the Government be requested to inspect all teas on their entry into France.

(3) That not more than 0·3 per cent. of lead should be permitted in the alloy used for "tinning," nor more than 5 per cent. in the alloy for tin vessels.

(4) That it is desirable that the methods of wine analysis should be verified and codified.

Section II.—After discussion of the methods at present in use for the analysis of earths, manures, &c., it was resolved that in the case of judicial disputes no single method should in any case be obligatory,[1] but that the fullest latitude should be allowed to the experts.

Section III.—It was resolved that a certain number of permanent Commissions should be appointed for the inspection of some of the most important substances used in pharmacy—quinine, morphine, chloroform, phenol, salicylic acid, analgesine, &c. The Commissions should endeavour to investigate and improve the methods of analysis used for these substances.

Section IV.—Section IV. was the one of by far the greatest general interest, as was testified by the presence of Profs. Alexeieff (Kiew), Calderon (Madrid), Franchimont (Leyden), Graebe (Geneva), Istrati (Bucharest), Noelting (Mulhouse), Boukowski Bey (Constantinople), and Colonel da Luz (Rio Janeiro), who were elected Vice-Presidents.

[1] The Comité Consultatif des Stations Agronomiques et des Laboratoires Agricoles has issued a code of the methods of analysis of manures, and these are at present obligatory.

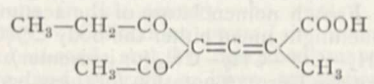
The following is a short account of the discussions held at the Sectional meetings :—

The Nomenclature of Unsaturated Hydrocarbons.—M. Calderon read a paper, in which he proposed to replace the present arbitrary names of the hydrocarbons by a series of names derived from the fundamental terms of each series. He proposes to name paraffins, *proto-methane*, *deuto-methane*, &c., the olefines *deuto-ethylene*, *trito-ethylene*, &c. The radicles are derived from the hydrocarbons by the insertion of Latin numerals between the Greek numeral and the generic name; thus, methyl becomes *proto-uni-methane*, &c. The author advocated these changes as calculated to facilitate the teaching of organic chemistry.—MM. Friedel, Grimaux, Graebe, and Maquenne expressed the view that it was impossible to make changes so radical, and that Hofmann's nomenclature was quite sufficient so far as the normal hydrocarbons are concerned.

M. Béhal presented a report to the Section on a system for the nomenclature of complex open-chain compounds. He regards them as formed by the addition of characteristic groups (*groupements fonctionnels*),[1] in which certain atoms of hydrogen are replaced by various radicles. These radicles are given in the order of the number of carbon atoms they contain, starting with the lowest. To denote different isomers the Greek letters, α, β, γ, δ, are employed. One or two examples may be given. The body

$$CH_3—CO—C\equiv C—CH_2—CHO$$

would be called acetyl-acetylene-ethaldehyde (acétyl-acétylène aldéhyde éthylique). The body



would be called acetyl propionyl allene[2] methyl formic acid.—Many objections were raised to M. Béhal's proposition by MM. Friedel, Graebe, Grimaux, and others, and it was finally decided to refer it to the Permanent Committee.

Derivatives of Ethylene and Urea.—M. Graebe proposed to denote the two atoms of carbon in ethylene derivatives, united by the double bond, by the letters *a* and *b*. If two similar radicles are introduced into the ethylene molecule, as, for example, in dimethyl ethylene, the two isomers would be called *ab*-dimethyl ethylene and dimethyl ethylene, or for greater clearness *a*-dimethyl ethylene. In the case of trisubstituted compounds it should be understood that the first two radicles were linked to the same carbon atom.—M. Franchimont proposed to adopt the same nomenclature for the urea derivatives.—Attention was drawn to the possibility of new isomers, if Wislicenus's extension of the Van 't Hoff-Lebel hypothesis should be proved to be of general application, but it was decided for the present not to number the four atoms of hydrogen in ethylene, and M. Graebe's proposition and M. Franchimont's rider were adopted.

Aldehydes.—M. Grimaux proposed a resolution recommending that in future aldehydes should be named after the corresponding alcohols, and not after the acids. He pointed out that this usage was consistent with the etymological origin of the word, and that the practice of giving the same name to alcohols and their corresponding acids would give rise to confusion in the names of the acid and hydrocarbon radicles which they contain; the names corresponding to ethyl and acetyl, to benzyl and benzoyl, would become identical if we used the names

[1] In France the word function is used in the following sense. Methyl alcohol gives rise by substitution to compounds whose chemical behaviour is similar to its own, and are therefore said to have the same chemical *function*. The group $CH_2 . OH$ is thus characteristic of the *primary alcohol function*; similarly, the group $C\!=\!C$ of the ethylene function, the group $H . C\!=\!O$ of the aldehyde function, &c., &c.

[2] Or isoallylene.

ethylic alcohol, ethylic acid, benzoic alcohol, benzoic acid. —M. Grimaux's proposition was adopted.

The Use of the Prefixes bi- and di-.—M. Bouveault proposed, in the name of M. Hanriot, that the prefix *bi* should be reserved for the denomination of bodies formed by the duplication of organic radicles; the words dipropargyl, diphenyl, dinaphthyl to be replaced by bipropargyl, biphenyl, binaphthyl.[1] M. Maquenne preferred to use *di* instead of *bi* for these double radicles; but it was pointed out that there were only about twelve bodies whose names would need to be changed to be in harmony with M. Hanriot's proposition, whereas the converse proposition would be much more difficult of adoption.—M. Grimaux thought that in any case it would be difficult to alter existing nomenclature at once, but proposed that the Congress should express a wish that M. Hanriot's system of nomenclature should be adopted for all new bodies, and should gradually replace the existing one.—M. Grimaux's modification of M. Hanriot's proposition was adopted.

The Use of the Suffix -ol.—M. Grimaux proposed that the suffix *-ol* should be restricted as far as possible to the alcohols, and that the names of all hydrocarbons should contain the letter *n*. He proposed that the ending *-ene* employed by English chemists, should be made use of for the aromatic hydrocarbons. *Benzine* and *naphtaline* in French, *benzol* in German, would be replaced by the English terms benzene and naphthalene.—M. Graebe warmly supported this proposition, and further suggested the abbreviation of naphthalene to *naphthene*.—M. Grimaux's proposition was adopted.

Ketones.—M. Friedel pointed out the ambiguity which exists in the French nomenclature of the acetones. Dimethyl acetone might mean either the body $CO(CH_3)_2$ or else $CH_3-CH_2-CO-CH_2-CH_3$ (or its isomer). He proposed to adopt the German notation for these bodies, and in general to replace the word *carbonyl* by the word *cetone* (this form being more adapted to the French language than ketone). Dimethyl cetone is then ordinary acetone. —The proposition was adopted.

The Group —C≡N.—M. Bouveault proposed to name this group *carbazil*.—M. Calderon and other members of the Congress were in favour of this name. It was also proposed to use the word *nitrile* universally.—M. Grimaux then pointed out that, as we made no distinction of nomenclature between the molecule and the half-molecule of chlorine, it would be unnecessary to do so in the case of cyanogen, and he proposed that the group —C≡N be denoted by the name cyanogen.—M. Grimaux's proposition was adopted.

The Sulphur Compounds.—M. Chabrié read a report on the sulphur compounds. He complained that a certain number of chemists used the term *sulphine* for compounds containing no oxygen, and which should properly be termed sulphides. He proposed that the name sulphine should be reserved for compounds of the formula $(SO)R_2$, sulphones for compounds $(SO_2)R_2$, derived respectively from sulphurous and sulphuric acids. This nomenclature is used by Beilstein in his invaluable compilation. It is naturally applicable to corresponding selenium and tellurium compounds.—It was decided to refer M. Chabrié's report to the Permanent Committee.

Ring Formulæ containing Nitrogen.—M. Bouveault read a long report on the subject. He proposed to adopt Widmann's nomenclature, with certain modifications in the details.—A protracted discussion ensued, and finally M. Graebe proposed that M. Bouveault's report should be printed and widely distributed, so as to elicit the opinion of all the chemists who had specially devoted themselves to the study of these compounds, and this was agreed to.[1]

Naphthalene.—M. Noelting read a report on the nomenclature of the naphthalene derivatives. He began by describing the various notations of Beilstein, Graebe and Noelting, Meldola, Weinberg, and the Swedish chemists. He proposed to denote the carbon atoms of the naphthalene ring by figures and to extend this system to anthracene, phenanthrene, &c. M. Noelting's proposal was adopted after discussion. The figures will be found below in the summary of the resolutions of the Congress. —It was agreed, on the suggestion of M. Auger, that when it was known that the radicles of a given di-substituted compound were in different rings, but their exact position was uncertain, they should be denoted by the symbol AB.

Benzene.—M. Combes read a report on the numeration of substituted benzene derivatives. He proposed to assign the place (1) to the hydrocarbon radicle containing the smallest number of atoms of carbon: in the absence of a hydrocarbon radicle he proposed to begin with the group having the lowest "molecular weight."—M. Combes gave as the reason for choosing the simplest hydrocarbon for the position (1) that it was this one which was generally attacked last by oxidizing agents.

M. Alexeieff pointed out that the oxidation of paracymene, $C_6H_4 . CH_3 . C_3H_7$, by the action of air and caustic soda, gives rise to cuminic acid, $C_6H_4 . CO_2H . C_3H_7$, and that in this case the position (1) would be changed, although there was no transposition in the molecule.[2]

M. Combe's report was referred to the Permanent Committee.

The general meeting for the discussion of the report of Section IV. was held under the presidency of M. Friedel, in the absence of M. Berthelot.

The following resolutions were adopted:—

(1) That an International Committee, with power to add to its number, be formed, whose object shall be to promote uniformity of chemical nomenclature.

The following gentlemen were nominated to serve on the Paris Committee:—MM. Berthelot, Friedel, Gautier, Grimaux, Jungfleisch, Béhal, Bouveault, Combes, Fauconnier.

That the Committee shall immediately request the following gentlemen to join them:—MM. Alexeieff (Russia), Armstrong (England), Baeyer (Germany), Beilstein (Russia), Boukowski Bey (Turkey), Calderon (Spain), Clève (Scandinavia), Franchimont (Holland), Graebe (Switzerland), Istrati (Roumania), Lieben (Austria-Hungary), Noelting (Germany), Paternò (Italy), Ira Remsen (United States).

(2) The two carbon atoms in ethylene, and the two hydrogen atoms in urea, shall be distinguished by the letters *a* and *b* (proposed by MM. Graebe and Franchimont).

(3) The aldehydes shall be named after their corresponding alcohols (proposed by M. Grimaux).

(4) The word carbonyl shall be replaced by the word cetone[3] (proposed by M. Friedel).

(5) The group —C≡N in organic compounds shall be called cyanogen, instead of nitrile (proposed by M. Grimaux).

(6) The suffix *-ol* shall be reserved as far as possible for alcohols. In the hydrocarbons it is to be replaced by the ending *-ene* (proposed by M. Grimaux).

(7) The prefix *bi-* shall in future be reserved for bodies formed by the union of two radicles such as biphenyl $(C_6H_5)_2$, bipropargyl $(C_3H_3)_2$, and the Congress expresses

[1] It may be mentioned that whereas in England the use of the prefix bi- has been almost entirely abandoned, in France it is still employed, though no rules for its use have hitherto been given; the terms *di-nitro benzine* and *benzine bi-nitré* denote the same body.

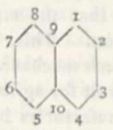
[1] The nomenclature of the pyridine and quinoleine derivatives agreed to by the Section was afterwards referred back to the Committee.

[2] It may be remarked that this difficulty is inherent to the case, since the oxidation of paracymene by chromic acid gives rise to paratoluic acid.

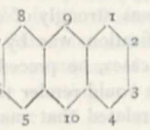
[3] This applies only to French nomenclature.

the desire that this nomenclature may be gradually adopted for bodies already known; the prefix *di-* to be used as at present, to denote bodies formed by double substitution (proposed by M. Hanriot).

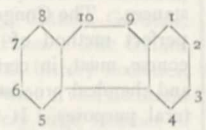
(8) The different carbon atoms of naphthalene anthracene, phenanthrene, fluorene, carbazol, acenaphthene, acridine, shall be numbered as follows:—



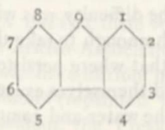
Naphthalene.



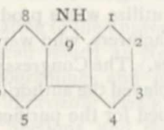
Anthracene.



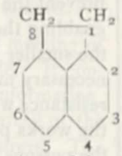
Phenanthrene.



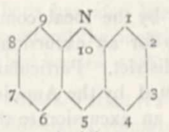
Fluorene.



Carbazol.



Acenaphthene.



Acridine.

(Proposed by M. Noelting.)

(9) The proposition to denote the carbon atoms of quinoleine by a, β, γ for the pyridine ring, and o, m, p, a (ortho, meta, para, ana) in the benzine ring,[1] which had been voted at the Sectional meeting, was adjourned by a vote of 42 against 26, and referred to the Permanent Committee.

After the carrying of the resolutions, M. Friedel made a short speech, in which he thanked especially the foreign members for their attendance and help. Some people might, he said, think that the Section had accomplished but little; that, however, was not his opinion: their aim had been to help workers in their work, and nothing could be more conducive to this aim than the use of the same language among the chemists of all countries. He felt sure that their efforts would prove fruitful in the future, and he hoped that by next year the International Committee would be able to report such serious progress as to justify the summoning of another Congress.

Thus terminated the formal proceedings of the Section and Congress, which had been marked throughout by the greatest good feeling among the *savants* of the various nations represented. On Sunday the Congress was brought to a final close by a banquet offered to the foreign members at the Terminus Hotel.

[1] Lellmann's notation.

THE REV. M. J. BERKELEY.

THE death of our great English mycologist has followed very close upon that of our great English lichenologist. Both of them were country clergymen of the Church of England, both were over eighty, and the career of both as botanical authors has extended over half a century.

The Rev. Myles Joseph Berkeley, M.A., F.R.S., was born at Biggin, in the parish of Oundle, in the year 1803. He was a descendant of the old historic family of that name. He was educated at Rugby, and at Christ's College, Cambridge, and graduated as fifth Senior Optime in the year 1825. After holding a curacy at Margate, he was appointed, in 1833, incumbent of two small parishes near Wansford, in his native county. Here he remained for thirty-five years, actively engaged in the performance of his parochial work. His stipend was small and his family large, and he had to supplement his clerical income by taking private pupils. This of course absorbed a great deal of his leisure, but his industry and force of character were so great that he got through, in addition, an enormous amount of scientific work. In 1868 he was appointed to the more valuable living of Sibbertoft, near Market Harborough, which he held until his death, on July 30. During the last ten years his health has failed, and in 1879 he presented his botanical collections to Kew, and, since that time, has published scarcely anything.

His attachment to botany must have begun very early in life, for I remember him saying, when we were speaking about a certain botanical examination, that he had not set any questions that he could not have answered when he was six years old. His friends thought he would have taken a higher degree at Cambridge if he had not given so much attention to natural history. His first book, "Gleanings of British Algæ," appeared in 1833. It deals mainly with minute microscopic types. The book which made his reputation was his "Monograph of the British Fungi," which forms the third volume of Hooker's "British Flora," published in 1836. This was the only hand-book of the British species in existence up to 1871, so that for thirty-five years it was the indispensable companion of every worker. The "Systema Mycologicum" of Fries, which summarized most ably all that was then known about genera and species, came out —the three volumes from 1821 to 1829, its "Supplement" in 1830, and the "Elenchus" in 1828; so that these were just in time to serve Berkeley as a foundation to build upon. From 1836 to 1870 he was the universal referee for everyone in this country who wanted information about fungi. Collections poured in upon him from home and abroad, and he described many thousands of genera and species, a large proportion of which were new, in Hooker's "Antarctic Floras," Hooker's *Journals of Botany*, the Transactions and Journal of the Linnean Society, and in the *Annals of Natural History*. During the latter part of the time he worked a great deal in conjunction with the late Mr. C. E. Broome, of Bath, who had abundant leisure and industry, combined with an unconquerable disinclination to publish on his own account, and in every Fungus-list "Berk. et Broome" is an often-quoted authority. Beginning with *Oidium Tuckeri*, he gave special attention to the fungoid pests of agriculture and horticulture; and it was, more than anything else, his papers on the potato disease that obtained for him the small pension that was granted to him during the last twenty years of his life. In 1857 he published a general "Introduction to Cryptogamic Botany," which has had a wide circulation. There has been no other book of a similar scope in the English language till this present year. His "Outlines of British Fungology," published in 1860, contains twenty-four plates, illustrating a series of about 150 typical forms. The text deals specially with the Hymenomycetes, and, for the other orders, does not go much beyond a catalogue of the British genera and species. His "Hand-book of the British Mosses," published in 1863, contains descriptions and plates of all the species then known in Britain. In the same year he was awarded the Biological Gold Medal of the Royal Society, of which he was elected a Fellow in 1879. But by this time his working days were over, and in that year he presented to Kew his entire fungus herbarium, followed, not long after, by his books. His herbarium contains specimens of upwards of 10,000 species, duly named and classified; and it has been estimated by Mr. G. E. Massee that it contains type specimens of 4866 species described by himself, and that the full number of new species which he described will not fall far short of 6000.

For many years he acted as one of the botanical examiners of the University of London, generally in conjunction with Sir J. D. Hooker and the late Dr. Thomson; and he acted also in the same capacity for the University of Cambridge and the Apothecaries' Company.

From the commencement of the *Gardener's Chronicle*, in 1841, till his health failed, he was a regular contributor to its pages. His most important papers were a series of articles on vegetable pathology, commenced in 1854 and continued at intervals till 1857. He also contributed a series of articles on the diseases of plants to the "Cyclopædia of Agriculture." He was one of the first to lay special stress upon the need for studying the full life-history of a fungus in order to understand it properly, the carrying out of which in the last generation has so completely changed many of our old ideas.

After the death of Lindley he acted for many years as botanical referee and general counsellor to the Royal Horticultural Society, a post for which he was well fitted, from his thorough knowledge of vegetable physiology and his acquaintance with practical gardening. It was in the Journal of this Society that his papers on the potato disease appeared. He was an excellent classical scholar, and read through all the proof-sheets of Bentham and Hooker's "Genera Plantarum," specially as linguistic critic. He was a man full of geniality, always willing to impart freely his wide store of information, and will be greatly missed by those that had the pleasure of his personal acquaintance. He had a commanding presence and a robust physique. His portrait, painted in oil by Peel in 1878, now hangs in the rooms of the Linnean Society, and a capital full-page engraving, by Mr. Worthington Smith, has twice appeared in the *Gardener's Chronicle*.

J. G. B.

NOTES.

We mentioned last week the *fêtes* in connection with the opening of the new Sorbonne, to which students from all the Universities of Europe had been invited. The following is a complete programme of the ten days' festivities—held under the auspices of the Association Générale des Étudiants de Paris—in which all the students of Paris, as well as their foreign guests, were invited to take part:—August 4, gala performance at the Opera. August 5, inauguration of the Sorbonne; reception by M. Fallières, Minister of Public Instruction. August 8, reception by the Municipality of Paris at the Hôtel de Ville. August 9, *matinée* at the Comédie Française; reception by M. Yves Guyot, Minister of Public Works; reception of the English and American students by M. Beljame, Professor of English at the Sorbonne. August 10, speeches by the chiefs of the foreign delegations; performance at the Gaîté Theatre. August 11, presentation of the chiefs of the foreign delegations to M. Carnot. August 12, ascent of the Eiffel Tower; excursion to Meudon; banquet on the terrace, under the presidency of M. Janssen. The *fêtes* have been throughout a magnificent success, and the students of all countries must carry away with them the most pleasant of remembrances of their French comrades. The Government and the City of Paris had voted a sum of about £3500 to the Association Générale des Étudiants, and it is to the organizing powers of the President, M. Chaumeton, and his devoted lieutenants, that the success of the meeting is due. At the Meudon banquet, at which there were nearly 2000 covers, M. Lavisse, Professor of Modern History at the Sorbonne, in an eloquent speech, declared that the principle of the future must be, not cosmopolitanism, but "*le respect de chaque patrie par toutes les patries*."

The Congress of Physiological Psychology was held in Paris last week, and the meeting is considered to have been very successful. It was decided that a second meeting should be held in 1892, either in London or in Cambridge, during the month of August.

The Hygiene Congress at Paris brought its labours to a close on Saturday last. Among the subjects discussed during the week was that of the pollution of rivers. The Congress decided that the pollution of underground watercourses and of rivers by the residue of factories should in principle be forbidden, and that water from factories should not flow into a stream till it had been proved to be absolutely free from all injurious substances. The Congress was strongly of opinion that the most perfect method of purification was by irrigation. This, of course, must, in certain cases, be preceded by such mechanical and chemical processes as would render the water fit for agricultural purposes. It was related that many manufacturers had benefited by the application of the law, as in their efforts to prevent the pollution of watercourses they had made discoveries enabling them to utilize waste products. The difficulty was with the smaller manufacturers, who were not rich enough to take the necessary measures. The Congress decided that where persistent resistance was displayed the authorities should themselves execute the works prescribed for the purification of the water and compel the persons interested to pay the cost.

Arrangements are being made by the local committee of the American Association at Toronto for an excursion, starting September 3 or 4, to the Huronian district. Particulars will be given in a circular, which is to be issued by the American Geological Society. There will also be an excursion to the Pacific Coast.

Some time ago the Berlin Academy of Sciences received from Count Loubat, of New York, about £1150, with a request that a prize might be founded for the encouragement of North American studies. At the same time he sent £120, which was to be offered as a special prize. It has now been decided that a prize of £150 shall be awarded in July 1891, to the author of the best printed work on the settlement of civilized colonies in North America and their later history. The works to be submitted must have appeared between July 1, 1884, and July 1, 1889; and the authors must communicate to the Berlin Academy before July 1, 1890, their intention to compete. The language of the books may be German, English, French, or Dutch. In 1896 there will be another prize of similar amount for a work on the North American aborigines. Every five years a prize of £150 will be offered, the subjects being aboriginal and civilized history, chosen alternately. The money for the first of this series of prizes will be obtained by the addition of the special sum of £120 to the interest on the larger and permanent fund.

The Royal Danish Academy of Sciences invites research on the following among other subjects:—Compounds of alcohol radicals with copper, silver, or gold, and compounds of polyvalent alcohol radicals with metals (all unknown at present). Prize a gold medal. The fatty acids in the fat of butter; to be isolated and determined, and relations indicated especially between the quantities of oleic acid and those of palmitic acid and their higher homologues. Prize about £32. The Mycorhizæ of the beech. Are they different in different kinds of humus? Does the structure of the mycelium give a basis for classification? Is there a reciprocal symbiosis, the fungus preparing food for the plant, &c., &c. Prize about £32. Memoirs to be sent to Prof. Zeuthen, of Copenhagen, before October 31, 1890, except in the last case, for which the date is October 31, 1891.

The twelfth annual meeting of the Midland Union of Natural History Societies will be held at Oxford on the afternoon of Monday, September 23. An inaugural address, on heredity, will be delivered by the President, Mr. E. B. Poulton, F.R.S. This will be followed by a discussion, after which the meeting will transact all necessary business. In the evening there will be a *conversazione* in the Museum, and the Oxfordshire

Natural History Society and Field Club has arranged for exhibitions of objects of great interest. On the following day members will drive to Shotover Hill, and a lecture will be delivered by the President on "the geology of the district." Afterwards there will be a lunch in Christ Church dining hall. Special arrangements will be made for members to visit, on this and also on Monday afternoon, the following places:—Museum, Ashmolean Museum, Radcliffe Observatory, Botanic Gardens, Bodleian Library, and Clarendon Printing Press. The Oxford Society cordially invites members of natural history societies, and their friends, to the meetings of the Union; and no doubt so pleasant a programme will attract a very large number of visitors. Further information may be obtained from the Secretary, Mr. H. M. J. Underhill, 7 High Street, Oxford.

A USEFUL little volume relating to the approaching meeting of the British Association has been issued at Newcastle. It contains, in addition to the programme and other information, memoirs (reprinted from the *Newcastle Daily Chronicle*) of the President, the Presidents of Sections, and the lecturers. The volume costs threepence, and will, of course, be widely circulated. There has also been issued, for the use of those who propose to attend the meeting, a list of the hotels, apartments, and furnished houses, with a plan of Newcastle.

MR. GEORGE PHILLIPS BEVAN, F.G.S., died on the 3rd inst. at Yaldhurst, Lymington, in his sixtieth year. He was the author of many popular hand-books and guide-books. He also wrote industrial geographies of Great Britain, France, and the United States, and edited a series of works on "British Manufacturing Industries."

THE American *Botanical Gazette* announces the death of Mrs. Lydia S. Bennett, a well-known botanist at Fisk University, Tennessee.

ON August 12, about 3 o'clock a.m., a rather severe shock of earthquake was felt at Poitiers. Clocks were stopped, and furniture was displaced. No one was injured.

THE German edition of the Report of the International Meteorological Committee meeting at Zürich, in September last, contains a preface by Dr. Neumayer, relating to the development of meteorological research in Germany, which, prior to the Congress of Vienna (1873), was almost at a standstill, very little attention having been paid to what was passing in other countries. But the impulse given by that Congress and various meetings of the Committee, together with the establishment of the Deutsche Seewarte at Hamburg and the Meteorological Institute at Berlin, have made the German-speaking countries rank among the foremost and most active promoters of meteorology, all now working together on one uniform plan. The Report contains also a very useful index to all the German editions of the publications of the International Meteorological Committee since the Congress of Rome (1879). A mere glance at this index shows the generality and usefulness of the labours of the Committee, exclusive of the meetings in some way connected with it, among which may be specially mentioned those of the Polar Committee (Hamburg, 1879), and the Conference for Agricultural Meteorology (Vienna, 1880). The preparation of meteorological bibliographies, the establishment of stations in remote parts of the world and upon high mountains, the preparation of elaborate meteorological conversion tables (not yet published) for use in all countries, and uniformity of methods, are but a few of the principal results arrived at in the last sixteen years.

ONE of the latest novelties in the application of electricity consists of an electric reading-lamp, which is being fitted to the carriages on the main line of the South-Eastern Railway. It is on the principle of the "put a penny in the slot" automatic machines. The apparatus is situated immediately over the passenger's head, and under the rack, and is contained in a small box 5 inches by 3. The light is of five-candle power, and is obtained by the introduction of a penny at the top of the box, and by a subsequent pressure of a knob, and will last for half an hour, extinguishing itself at the end of that time automatically. If the light be required for an indefinite period, a penny every half an hour will suffice. The light can be extinguished at any moment by means of a second button provided for the purpose. One of the special features of the invention is that, if the instrument is out of order, the penny is not lost, as it is in the present machines. It drops right through, and comes out at the bottom of the box, so that it can be recovered, and the same result happens in the case of any coin other than a penny. Each carriage is fitted with an accumulator which supplies the electricity. This invention will add greatly to the comfort of passengers during night journeys.

SOME new light on the subject of indirect vision (*i.e.* vision with the lateral parts of the retina), is thrown by recent experiments made by Kirschmann. The common idea, that the sensitiveness of the retina diminishes outwards to the periphery, appears to be incorrect. There is an objective diminution of light-action, when a source of light is moved away laterally from the middle of the field of vision; for the mass of penetrating light gets less. Hence, were the diminishing sensitiveness a fact, a luminous surface should seem to lose brightness when moved to the side; but it does not (though it appears less distinct in outline and modified in colour). Kirschmann placed two rotatory disks made up of moveable black and white sectors (giving any degree of brightness), before the observer; who shut one eye, and looked at the middle of one disk, about a metre and a half from him, while he gave his attention to comparing the brightness of the second disk, seen at different angles, by indirect vision. The figures from numerous experiments prove that, in the horizontal meridian, the sensibility to brightness has a maximum at 22° to 25° from the centre, while in the vertical direction the maximum is at 12° to 15°. The growth of sensibility is much greater in the horizontal than in the vertical direction, and the upper part of the retina is superior in this respect to the lower. This corresponds to the needs of vision. Indirect vision with lateral parts of the retina is more important than that with the upper and lower regions; and the upper half is more important than the lower.

MESSRS. MACMILLAN have issued a new volume of the "Nature Series"—"Timber and some of its Diseases," by H. Marshall Ward, F.R.S. Until the author's articles appeared in this journal, the subject, as he says in his preface, was almost unknown in England.

A SUGGESTIVE paper, on "The Ta Ki, the Svastika, and the Cross in America," by Dr. Daniel G. Brinton, is printed in the new number of the Proceedings of the American Philosophical Society. He holds that all these symbols are graphic representations of the movements of the sun with reference to the figure of the earth, as understood by primitive man everywhere, and hence that these symbols are found in various parts of the globe without necessarily implying any historic connections of the peoples using them.

THE Geological and Natural History Survey of Minnesota has issued its sixteenth Annual Report, dealing with such results of its work as are capable of being put into a shape fit for publication. Mr. Winchell, State Geologist, says in an introductory statement that investigations are being carried forward in the lithology of the crystalline rocks and in the palæontology of the fossiliferous ores, which are not yet sufficiently far advanced to be dealt with in a Report.

IN a valuable paper, included in the Annual Report of the United States Department of Agriculture for 1888, on the plum

Curculio, Messrs. C. V. Riley and L. O. Howard state that this insect has brought about an almost entire abandonment of plum culture in many parts of America within the last twenty years; but it is by no means confined to this fruit. It breeds in great numbers in cherries, peaches, apricots, nectarines, and other stone fruits, including the Persimmon, and also infests many varieties of apples, crabs, and haws. It prefers, however, smooth-skined fruits. It is also a common inhabitant of the fungus growth of plum and cherry known as "Black Knot" (*Plowrightia morbosa*), from which it was first reared by Peck in 1818. Under the headings, "Habits and Natural History," "Natural Enemies," and "Remedies," the authors of the paper (which has just been reissued separately) give full information as to the pest to which they have devoted so much attention.

THE "Catalogue of the Moths of India," on which the compilers, Colonel C. Swinhoe and Mr. E. C. Cotes, have been engaged for three years, is completed; and an elaborate index has now been published. The compilers claim that the "Catalogue" comprises all the known moths of the Indian region, including Burma and Ceylon.

THE *Madras Journal of Literature and Science*, for the session 1888-89, contains, besides various other papers, the second part of an elaborate treatise, by Gustav Oppert, on the original inhabitants of India. In the first he treated of the Dravidians; here he deals with the other aboriginal tribes, whom he classes together under the name of the Gaudians. In the third part he proposes to set forth various conclusions to be drawn from this inquiry, supported by as much trustworthy evidence as he may be able to collect.

THE University Correspondence College, Cambridge, has sent us a copy of its latest "Matriculation Directory." The volume is full of information that will be useful to persons who propose to pass the matriculation examination of the London University. In the parts relating to text-books, much excellent advice is offered to candidates.

THE Mason Science College, Birmingham, has issued its syllabus of day classes for the session 1889-90.

THE Agricultural Society of the Gironde, as quoted in a recent British Consular Report, has published a statement showing the average costs incurred last year by proprietors in this department in employing the best-known remedies, viz. (1) against the *Phylloxera*, sulphuretted carbon; (2) against mildew, the so-called *Bouillie Bordelaise*, a mixture of three pounds of sulphate of copper with one pound of slaked lime and twenty-two gallons of water; (3) against *Oïdium*, sulphur; and (4) against *Antrachnosis*, a mixture of eighty pounds of sulphate of iron and ten pounds of sulphate of copper. The total cost of using all these remedies is said to have amounted on an average to about 31*s*. per acre, an expense which cannot be called excessive, especially when it is added that their application served at the same time as a preventive against snails and slugs, which also often do much damage to vines.

Globus contains an abstract of a paper read lately before the German Scientific Association of Santiago, on the inhabitants of Tierra del Fuego, by the Rev. C. Aspinall, an English missionary who has laboured long among them. The particular tribe amongst which the Ooshonia mission was established has received from Mr. Bridges the name of Jahgan, from a place to the south of Beagle Channel frequented by the tribe. The people usually go naked, save for a small skin thrown over the shoulders, but they smear their bodies with a mixture of train-oil and red earth as a protection against the cold. They support themselves by hunting, and at the worst feed on shell-fish. Certain disorders of the digestion, arising from the latter, they cure by a fungus diet. For the most part they move about from place to place, without any fixed abode, in bark canoes, in the centre of which a fire always burns. Each canoe contains a family, the wife rowing while the husband is always on the watch with his javelin. He always carries three kinds of spears with him, one for birds, the second for fish, and the third for crabs. On landing, the woman has first of all to carry her husband ashore, he holding the fire carefully above water, and then she begins the erection of their primitive hut. The men are rarely able to swim, but the women invariably, and this, together with their constant work at rowing, gives them extraordinary muscular power. To maintain their position as lords of creation the men have recourse to mysterious rites, from which the women are excluded. The men have usually two wives, an older and a younger one. Without writing of any kind, they yet preserve many rules and customs, mainly relating to the chase. They are good-natured and helpful, not addicted to lying or theft, but tenacious in the defence of their rights. They have many amiable traits of character. They love long stories and conversations, and in these a good part of their time is spent. One of their tales, of an extraordinary strong man who was made of stone, and ultimately was killed by a thorn entering a vulnerable spot in his heel, recalls the story of Achilles. Devoid of all religious ideas and duties, they have a vague idea of the spirits of the departed wandering about in the world, and greatly to be feared.

THE additions to the Zoological Society's Gardens during the past week include a Gazelle (*Gazella dorcus* ♂) from Egypt, presented by Mr. Umberto Arbib; a Cinereous Vulture (*Vultur monachus*) from Central Spain, presented by Lord Lilford; two Vinaceous Turtle Doves (*Turtur vinaceus*) from West Africa, presented by Mrs. Ffoulkes; two Alligators (*Alligator mississippiensis*) from Florida, presented by Mr. J. W. Bannehr; twenty-two Gold Fish (*Carassius auratus*), four Carp (*Cyprinus carpio*), British fresh waters, presented by Mr. A. H. Hastie; a Cinereous Vulture (*Vultur monachus*) from Central Spain, a Grey Parrot (*Psittacus erithacus*) from West Africa, twenty-four Teydean Chaffinches (*Fringilla teydea*) from the Canary Islands, deposited; a Globose Curassow (*Crax globicera*), two Triangular Spotted Pigeons (*Columba guinea*), two Cambayan Turtle Doves (*Turtur senegalensis*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 AUGUST 18-24.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 18

Sun rises, 4h. 52m.; souths, 12h. 3m. 35·1s.; daily decrease of southing, 13·3s.; sets, 19h. 15m.: right asc. on meridian, 9h. 51·8m.; decl. 12° 58′ N. Sidereal Time at Sunset, 17h. 4m.

Moon (at Last Quarter August 18, 11h.) rises, 22h. 18m.*; souths, 5h. 39m.; sets, 13h. 13m.: right asc. on meridian, 3h. 26·4m.; decl. 14° 48′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 48 ...	12 46 ...	19 44 ...	10 34·0 ...	10 33 N.
Venus.....	1 10 ...	9 11 ...	17 12 ...	6 58·5 ...	21 9 N.
Mars......	3 2 ...	10 52 ...	18 42 ...	8 39·9 ...	19 31 N.
Jupiter....	16 11 ...	20 4 ...	23 57 ...	17 53·5 ...	23 25 S.
Saturn....	4 38 ...	11 59 ...	19 20 ...	9 47·1 ...	14 36 N
Uranus...	9 55 ...	15 23 ...	20 51 ...	13 11·4 ...	6 56 S.
Neptune..	22 34* ...	6 24 ..	14 14 ..	4 11·2 ...	19 26 N.

* Indicates that the rising is that of the preceding evening.

Aug.	h.	
22 ...	22 ...	Venus in conjunction with and 1° 59′ south of the Moon.
24 ...	18 ...	Mars in conjunction with and 1° 29′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
Algol	3 1·0	40 32 N.	Aug. 22,	3 32	*m*
δ Libræ	14 55·1	8 5 S.	,, 22,	1 14	*m*
U Ophiuchi	17 10·9	1 20 N.	,, 23,	3 10	*m*
			,, 23,	23 18	*m*
X Sagittarii	17 40·6	27 47 S.	,, 19,	0 0	*M*
W Sagittarii	17 57·9	29 35 S.	,, 23,	4 0	*m*
T Herculis	18 4·9	31 0 N.	,, 20,		*m*
U Sagittarii	18 25·6	19 12 S.	,, 22,	0 0	*m*
			,, 24,	23 0	*M*
β Lyræ	18 46·0	33 14 N.	,, 23,	1 0	m_2
R Lyræ	18 52·0	43 48 N.	,, 21,		*m*
U Aquilæ	19 23·4	7 16 S.	,, 22,	0 0	*m*
S Sagittæ	19 51·0	16 20 N.	,, 20,	21 0	*m*
			,, 23,	21 0	*M*
T Vulpeculæ	20 46·8	27 50 N.	,, 22,	1 0	*m*
			,, 22,	3 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor Showers.

	R.A. °	Decl. °	
Near γ Andromedæ	26	42 N.	Swift; streaks.
,, γ Camelopardalis	55	71 N.	,, ,,
,, π Draconis	290	62 N.	Slow.

THE CONGRESS OF GERMAN NATURALISTS AND PHYSICIANS.

THE sixty-second Congress of German Naturalists and Physicians will be held, as we have already announced, at Heidelberg, from September 18 to 23; and the meeting promises to be one of great interest. Dr. G. Quincke and Dr. W. Kühne, by whom the various necessary arrangements are being made, have issued a programme, and take the opportunity to say that all naturalists, physicians, and students of the natural sciences, who may choose to attend the meeting, will be cordially welcomed. Although by its statutes the Congress consists only of Germans, foreign investigators, by being present and taking part in the proceedings, give much pleasure to their German colleagues.

The three general meetings will be held in the great hall of the Heidelberg Museum on September 18, 20, and 23. To the second of these general meetings, on September 20, a scheme embodying new statutes will be submitted for consideration. This scheme has been worked out by a committee in consequence of resolutions passed at the Congress held last year at Cologne.

According to the old statutes of 1822, still in force, the Congress consists of members and associates, but only the former have the right of voting. Everyone who has written a book on natural science or on medicine is regarded as a member. No one, however, who has merely written an inaugural dissertation can be considered as an author. Admission as associates is open to all who have occupied themselves scientifically with natural science and medicine. The ordinary ticket costs 12 marks; and the same ticket may be obtained for ladies at half the price. In return for an annual subscription of 5 marks, paid in accordance with the decisions of the Cologne Congress last year, the members receive a special member's ticket. The tickets will often have to be shown, so that members and associates ought to have them always ready.

Resolutions can be passed only at a general sitting; and everything is decided by the votes of a majority of the members. Resolutions with regard to scientific theses are not adopted either at the general sittings or in the Sections. The Sections elect their own Presidents, and may, in addition to the Secretary previously appointed, nominate, if necessary, a second or a third Secretary.

In connection with the meeting, there will be an exhibition of scientific apparatus, instruments, and preparations. It will be held in the town gymnastic hall (*Turnhalle*), Grabengasse, 22. Arrangements are being made for this exhibition by a special committee, the president of which is Herr Stadtrath Leimbach, 59 Gaisbergstrasse, Heidelberg, to whom all communications relating to the exhibition should be addressed. The ordinary ticket secures admission to the exhibition, which will be open daily, to members and associates only, from 8 to 11 o'clock a.m. During these hours, explanations will, if desired, be given by exhibitors or their representatives. After 11 o'clock the exhibition will be open to the public, who will have to pay for admission. A catalogue will be provided for members and associates.

The Directors of the Museum and of the Harmonie Society have placed their rooms at the disposal of the Congress; and through the kindness of the civic authorities, and of the Society to which the Museum belongs, it has been arranged that a concert shall be given in the town garden on September 18, and a festival in the Castle grounds on September 20; that the Castle shall be illuminated on the evening of September 23; and that a ball shall be given in the Museum on September 21. Tickets for the dinner in the great hall of the Museum, on September 19, will be issued at the Reception Room on Tuesday and Wednesday, September 17 and 18. The Reception Room and Inquiry Office will be on the ground floor of the Bayrischer Hof, 2 Rohrbacher Strasse, near the railway station.

Applications for lodgings will be received by Herr Rathschreiber Webel (Rathhaus, Heidelberg), Secretary of the Lodgings Committee. A representative of this Committee will be present in the Reception Room.

A daily Bulletin will be issued during the sitting of the Congress. Every morning it will be found in the Reception Room, with a list of members and associates; also with the orders of the day in the Sections, &c. Reports of papers can only be printed afterwards in the scientific part of the Bulletin. Papers which are intended for publication must be written plainly, and handed in not later than October 8.

A post and telegraph office will be open in the ground floor of the University from 8 a.m. to 8 p.m., and a room will be set apart for the writing of letters. Dr. Quincke and Dr. Kühne have not been able to induce the railway companies to issue tickets at a reduced price.

On Sunday, September 22, the following excursions will be made:—

(1) Through the Valley of the Neckar to Neckarsteinach, Hirschhorn, Eberbach, Ernstthal.

(2) By the Bergstrasse to Weinheim, Bensheim, Auerbach, Zwingenberg, Jugenheim.

(3) In the Palatinate, to Speyer, Neustadt, Annweiler, Dürkheim.

(4) To Mannheim, for the examination of the collections there, and for a visit to the opera.

All members and associates (even those who may already have their tickets) are requested to write their names on the list in the Reception Room, and at the same time to give their cards, with name, titles, and place of residence.

The following is the general order of the day:—

Tuesday, September 17: 9 a.m., opening of the Exhibition; 8 p.m., friendly meeting in the Museum.

Wednesday, the 18th: 9 a.m., first general meeting in the great hall of the Museum. (1) Opening of the Congress: Speeches. (2) Lecture by Geh. Rath von Meyer (Göttingen-Heidelberg), on "Chemical Problems of the Present Day." (3) Lecture by Dr. G. H. Otto Volger (Frankfort), on the "Life and Achievements of Dr. K. Schimper."

Midday: Assembling and formation of the Sections.

Afternoon: Sittings of the Sections.

7 p.m.: Concert in the Town Garden.

Thursday, the 19th: Sittings of the Sections; 5 p.m., dinner in the great hall of the Museum.

Friday, the 20th: 9 a.m., second general meeting in the great hall of the Museum. (1) Lecture by Prof. Hertz (Bonn), on "The Relations between Light and Electricity." (2) Consideration of the scheme of new statutes, under the presidency of Dr. Virchow, as Chairman of the Committee by which the scheme was drawn up. (3) Election (*a*) of the new Committee, (*b*) of the next place of meeting, (*c*) of the business managers of the next meeting.

Afternoon: Sittings of the Sections.

6.30 p.m.: Festival at the Castle.

Saturday, the 21st: Sittings of the Sections. 7.30 p.m., ball in the Museum.

Sunday, the 22nd: Excursions in the neighbourhood.

Monday the 23rd: 9 a.m., third general meeting in the great hall of the Museum. (1) Lecture by Prof. Th. Puschmann (Vienna), on "The Significance of History for Medicine and the Natural Sciences." (2) Lecture by Prof. Brieger (Berlin), on "Bacteria."

Afternoon: Sittings of the Sections.

7.30 p.m.: Illumination of the Castle.

We append a list of the Sections:—(1) Mathematics and Astronomy; (2) Physics; (3) Chemistry; (4) Botany; (5) Zoology; (6) Entomology; (7) Mineralogy and Geology; (8) Ethnology and Anthropology; (9) Anatomy; (10) Physiology; (11) General Pathology and Pathological Anatomy; (12) Pharmacology; (13) Pharmacy and Pharmacognosis; (14) Medicine; (15) Surgery; (16) Gynæcology; (17) Children's Diseases; (18) Neurology and Pyschiatry; (19) Diseases of the Eye; (20) Diseases of the Ear; (21) Laryngology and Rhinology; (22) Dermatology and Syphilis; (23) Hygiene; (24) Medical Jurisprudence; (25) Medical Geography; (26) Military Sanitation; (27) Dentistry; (28) Veterinary Medicine; (29) Agricultural Chemistry; (30) Mathematics and the Natural Sciences in Relation to Education; (31) Geography; (32) Philosophical Instruments.

THE PROGRESS OF SCIENCE AS EXEMPLIFIED IN THE ART OF WEIGHING AND MEASURING.[1]

TWO centuries ago the world was just beginning to awaken from an intellectual lethargy which had lasted a thousand years. During all that time the children had lived as their parents before them, the mechanical arts had been at a standstill, and the dicta of Aristotle had been the highest authority in science. But now the night of mediævalism was approaching its end, and the dawn of modern progress was at hand. Galileo had laid the foundation for accurate clocks by discovering the isochronism of the simple pendulum; had proved that under the action of gravity light bodies fall as rapidly as heavy ones; had invented the telescope, and with it discovered the spots on the sun, the mountains on the moon, the satellites of Jupiter, and the so-called triple character of Saturn; and, after rendering himself immortal by his advocacy of the Copernican system, had gone to his grave aged, blind, and full of sorrows. His contemporary, Kepler, had discovered the laws which, while history endures, will associate his name with the theory of planetary motion, and he also had passed away. The first Cassini was still a young man, his son was a little child, and his grandson and great-grandson, all of whom were destined to be directors of the Paris Observatory, were yet unborn. The illustrious Huyghens, the discoverer of Saturn's rings and the father of the undulatory theory of light, was in the zenith of his powers. The ingenious Hooke was a little younger; and Newton, towering above them all, had recently invented fluxions, and on April 28, 1686, had presented his "Principia" to the Royal Society of London, and given the theory of gravitation to the world. Bradley, who discovered nutation and the aberration of light; Franklin, the statesman and philosopher, who first drew the lightning from the clouds; Dollond, the inventor of the achromatic telescope; Euler, the mathematician who was destined to accomplish so much in perfecting algebra, the calculus, and the lunar theory; Laplace, the author of the "Mécanique Céleste"; Rumford, who laid the foundation of the mechanical theory of heat; Dalton, the author of the atomic theory, upon which all chemistry rests; and Bessel, the greatest of modern astronomers—these and others almost as illustrious, whom we cannot even name to-night, were still in the womb of time.

Pure science first felt the effects of the new intellectual life, and it was more than a century later before the arts yielded to its influence. Then came Hargreaves, the inventor of the spinning-jenny; Arkwright, the inventor of the cotton-spinning frame; Watt, who gave us the condensing steam-engine; Jacquard, the inventor of the loom for weaving figured stuffs; Murdock, the originator of gas lighting; Evans, the inventor of the high-pressure steam-engine; Fulton, the father of steam navigation; Trevithick, who ranks very near Watt and Evans in perfecting the steam-engine; and Stephenson, the father of railroads. If now we add the names of those who have given us the telegraph, to wit: Gauss, the eminent physicist and the greatest mathematician of the present century; Weber, Wheatstone, and Henry—all famous physicists—and Morse, the inventor and engineer; we have before us the demi-gods who have transformed the ancient into the modern world, given us machinery which has multiplied the productive power of the human race many-fold, annihilated time and space, and bestowed upon toiling millions a degree of comfort and luxury which was unknown to kings and emperors of old.

The discoveries and inventions of the last two centuries have so far exceeded all others within historic times that we are amply justified in calling this an age of phenomenal progress, and under the circumstances a little self-glorification is pardonable—perhaps even natural. The weekly and monthly records of scientific events which appear in so many newspapers and magazines are the immediate result of this, and the great increase of ephemeral scientific literature has led multitudes of educated people to believe that such records represent actual progress. The multiplication of bricks facilitates the building of houses, but does not necessarily improve architecture. Similarly, the multiplication of minor investigations improves our knowledge of details, but rarely affects the great philosophic theories upon which science is founded. The importance of human actions is measured by the degree in which they affect human thought, and the only way of permanently affecting scientific thought is by modifying or extending scientific theories. The men who do that are neither numerous nor do they require weekly paragraphs to record their deeds; but their names are honoured by posterity. Even in this golden age the advance of science is not steady, but is made by spasmodic leaps and bounds. Mere scientific brick-making, commonly called progress, is always the order of the day until some genius startles the world by a discovery affecting accepted theories. Then every effort is directed in the new line of thought until it is measurably worked out, and after that brick-making again resumes its place. While the progress in two centuries has been immense, the progress in a week or a month is usually almost *nil*. Optimism has its uses in many departments of human affairs, but science should be cool and dispassionate, having regard only for the truth. To make a trustworthy estimate of the actual state of the whole vast realm of science would be a task beyond the powers of any one man; but perhaps it will not be amiss to spend the time at our disposal this evening in briefly reviewing the recent progress and present condition of the fundamental processes upon which the exact sciences rest—I allude to the methods of weighing and measuring.

Physical science deals with many quantities, but they are all so related to each other that almost every one of them can be expressed in terms of three fundamental units. As several systems of such units are possible, it is important to select the most convenient, and the considerations which guide us in that respect are the following:—

(1) The quantities selected should admit of very accurate comparison with other quantities of the same kind.

(2) Such comparisons should be possible at all times, and in all places.

(3) The processes necessary for making such comparisons should be easy and direct.

(4) The fundamental units should be such as to admit of easy definitions and simple dimensions for the various derived units.

Scientific men have long agreed that these requirements are best fulfilled by adopting as the fundamental units a definite length, a definite mass, and a definite interval of time. Length is an element which can be very accurately measured and copied, but it must be defined by reference to some concrete material standard, as, for example, a bar of metal; and as all substances expand and contract with changes of temperature, it is necessary to state the temperature at which the standard is correct. A standard of mass, consisting of a piece of platinum, quartz, or other material not easily affected by atmospheric influences, probably fulfils the conditions set forth above better than any other kind of magnitude. Its comparison with other bodies of approximately equal mass is effected by weighing, and as that is among the most exact of all laboratory operations, very accurate copies of the standard can be made, and they can be carried from place to place with little risk of injury. Time is also an element which can be measured with extreme precision. The immediate instruments of measurement are clocks and chronometers, but their running is checked by astronomical observations, and the ultimate standard is the rotation of the earth itself.

It is important to note that the use of three fundamental units is simply a matter of convenience and not a theoretical necessity, for the unit of mass might be defined as that which at unit distance would generate in a material point unit velocity in unit time; and thus we should have a perfectly general system of measurement based upon only two fundamental units—namely

[1] Annual Address of Dr. William Harkness, President of the Philosophical Society of Washington, delivered on December 10, 1887.

those of space and time. Such a system is quite practicable in astronomy, but cannot yet be applied with accuracy to ordinary terrestrial purposes. According to the law of gravitation,

$$\text{Mass} = \text{Acceleration} \times (\text{Distance})^2;$$

and as in the case of the earth we can measure the quantities on the right-hand side of that equation with considerable accuracy, we can satisfactorily determine the earth's mass in terms of the supposed unit. That suffices for the needs of astronomy, but for other scientific and commercial purposes a standard of mass having a magnitude of about a pound is necessary, and as two such masses can be compared with each other from five to ten thousand times more accurately than either of them can be determined in terms of the supposed unit, three fundamental units are preferable to two.

The Chaldeans, Babylonians, Persians, Greeks, and Romans, all seem to have had systems of weights and measures based upon tolerably definite standards, but after the decline of the Roman Empire these standards seem to have been forgotten, and in the beginning of the sixteenth century the human body had so far become the standard of measurement that the units in common use—as, for example, the foot, palm, &c.,—were frequently taken directly from it. The complete table of measures of length was then as follows: the breadth (not the length) of four barley-corns make a digit, or finger-breadth; four digits make a palm (measured across the middle joints of the fingers); four palms are one foot; a foot and a half is a cubit; ten palms, or two feet and a half, are a step; two steps, or five feet, are a pace; ten feet are a perch; one hundred and twenty-five paces are an Italic stadium; eight stadia, or one thousand paces, are an Italic mile; four Italic miles are a German mile; and five Italic miles are a Swiss mile. It was then the practice to furnish standards of length in books by printing in them lines a foot or a palm long, according to the size of the page, and from these and other data it appears that the foot then used on the continent of Europe had a length of about ten English inches.

In England, the first attempts at scientific accuracy in matters of measurement date from the beginning of the seventeenth century, when John Greaves, who must be considered as the earliest of the scientific metrologists, directed attention to the difference between the Roman and English foot by tolerably accurate determinations of the former, and also attempted the investigation of the Roman weights. He was followed by Dr. Edward Bernard, who wrote a treatise on ancient weights and measures about 1685, and towards the end of the century the measurements of the length of a degree by Picard and J. D. Cassini awakened the attention of the French to the importance of rigorously exact standards. In considering the progress of science with respect to standards of length, we may safely confine our inquiries to the English yard and the French toise and metre, for during the last two hundred years they have been almost the only standards adopted in scientific operations.

The English measures of length have come down from the Saxons, but the oldest standards now existing are the Exchequer yards of Henry VII. (1490) and Elizabeth (1588). These are both brass-end measures, the former being an octagonal rod about half an inch in diameter, very coarsely made, and as rudely divided into inches on the right-hand end and into sixteenths of a yard on the left-hand end; the latter a square rod with sides about half an inch wide, also divided into sixteenths of a yard, and provided with a brass bed having end-pieces between which the yard fits. One end of the bed is divided into inches and half-inches. Francis Baily, who saw this Elizabethan standard in 1836, speaks of it as "this curious instrument, of which it is impossible, at the present day, to speak too much in derision or contempt. A common kitchen poker, filed at the ends in the rudest manner by the most bungling workman, would make as good a standard. It has been broken asunder; and the two pieces have been dove-tailed together: but so badly that the joint is nearly as loose as that of a pair of tongs. The date of this fracture I could not ascertain, it having occurred beyond the memory or knowledge of any of the officers at the Exchequer. And yet, till within the last ten years, to the disgrace of this country, copies of this measure have been circulated all over Europe and America, with a parchment document accompanying them (charged with a stamp that costs £3 10*s.*, exclusive of official fees), certifying that they are true copies of the English *standard.*"

In the year 1742 certain members of the Royal Society of London, and of the Royal Academy of Sciences of Paris, proposed that, in order to facilitate a comparison of the scientific operations carried on in the two countries, accurate standards of the measures and weights of both should be prepared and preserved in the archives of each of these Societies. This proposition having been approved, Mr. George Graham, at the instance of the Royal Society, had two substantial brass rods made, upon which he laid off, with the greatest care, the length of 3 English feet from the standard yard kept at the Tower of London. These two rods, together with a set of troy weights, were then sent over to the Paris Academy, which body, in like manner, had the measure of a French half-toise set off upon the rods, and keeping one, as previously agreed, returned the other, together with a standard weight of two marcs, to the Royal Society. In 1835, Baily declared this copy of the half-toise to be of little value because the original *toise-étalon* was of iron, and the standard temperature in France differed from that in England. In his opinion the French should have sent over an iron half-toise in exchange for the English brass yard; but this criticism loses much of its force when it is remembered that in 1742 neither England nor France had fixed upon a temperature at which their standards were to be regarded as of the true length. On the return of the rod from Paris, Mr. Graham caused Jonathan Sisson to divide the English yard and the French half-toise each into three equal parts, after which the rod was deposited in the archives of the Royal Society, where it still remains. Objection having been made that the original and legal standard yard of England was not the one at the Tower, but the Elizabethan standard at the Exchequer, the Royal Society requested Mr. Graham to compare his newly-made scale with the latter standard, and on Friday, April 22, 1743, he did so in the presence of a Committee of seven members of the Royal Society. In the following week the same gentlemen compared the Royal Society's scale with the standards at Guildhall and the Tower, and also with the standard of the Clockmakers' Company. These comparisons having shown that the copy of the Tower yard upon the Royal Society's scale was about 0·0075 of an inch longer than the standard at the Exchequer, Mr. Graham inscribed upon the Royal Society's scale a copy of the latter standard also, marking it with the letters Exch., to distinguish it from the former, which was marked E. (English), and from the half-toise which was marked F. (French).

In the year 1758 the House of Commons appointed a Committee to inquire into the *original* standards of weights and measures of England; and, under instructions from that Committee, the celebrated instrument-maker, John Bird, prepared two brass rods, respecting which the Committee speak as follows in their Report: "And having those rods, together with that of the Royal Society, laid in the same place, at the receipt of the Exchequer, all night with the standards of length kept there, to prevent the variation which the difference of air might make upon them, they the next morning compared them all, and, by the means of beam compasses brought by Mr. Bird, found them to agree as near as it was possible." One of these rods was arranged as a matrix for testing end-measures, and the other was a line measure which the Committee recommended should be made the legal standard of England, and which has since been known as Bird's standard of 1758. Respecting the statement that after lying together all night the rods were *all* found to agree as near as it was possible, Baily says: "This is somewhat remarkable, and requires further explanation, which unfortunately cannot now be accurately obtained. For it is notorious that the measure of the yard of the Royal Society's scale differs very considerably from the standard yard at the Exchequer. . . . Owing to this singular confusion of the lengths of the measures, which does not appear to have been unravelled by any subsequent Committee, it has happened that the Imperial standard yard . . . has been assumed nearly 1 ÷ 140 of an inch longer than the ancient measure of the kingdom." There is little difficulty in surmising what Bird did. The Exchequer standard consisted of a rod and its matrix. The Royal Society's Committee assumed the rod to be the true standard of 36 inches, and upon that assumption Graham's measurements gave for the length of the matrix 36·0102 inches, and for the length of the Royal Society's yard 36·0075 inches. The Parliamentary Committee of 1758 probably assumed the standard to consist of the rod and matrix together, which seems the better view; and by laying the rod in its matrix, and measuring to the joint between them, Bird would have got a length of about 36·0051 inches. The mean between that and 36·0075 would be 36·0063, which differs very little from the length of Bird's standard resulting from Sir George Shuckburgh's measurements. Thus the Committee's statement is justified, and there has been no falsification of the ancient standards.

On December 1, 1758, Parliament created another Committee on Weights and Measures, which, in April 1759, repeated the recommendation that Bird's standard of 1758 should be legalized, and further recommended that a copy of it should be made and deposited in some public office, to be used only on special occasions. The copy was made by Bird in 1760, but, owing to circumstances entirely unconnected with the subject, no legislation followed for sixty-four years.

The Royal Commission appointed during the reign of George III. to consider the subject of weights and measures, made its first Report on June 24, 1819, and therein recommended the adoption of the standard of length which had been used by General Roy in measuring the base on Hounslow Heath; but in a second Report, made July 13, 1820, they wrote:—"We . . . have examined, since our last Report, the relation of the best authenticated standards of length at present in existence, to the instruments employed for measuring the base on Hounslow Heath, and in the late trigonometrical operations; but we have very unexpectedly discovered that an error has been committed in the construction of some of these instruments. We are therefore obliged to recur to the originals which they were intended to represent, and we have found reason to prefer the Parliamentary standard executed by Bird in 1760, which we had not before received, both as being laid down in the most accurate manner, and as the best agreeing with the most extensive comparisons which have been hitherto executed by various observers, and circulated through Europe; and in particular with the scale employed by the late Sir George Shuckburgh."

Accordingly, when in 1824 Parliament at length took action, Bird's standard of 1760 was adopted instead of that of 1758. The former being a copy of a copy, its selection as a national standard of length seems so singular that the circumstances which brought about that result should scarcely be passed over in silence. Bird had a very accurate brass scale 90 inches long, which he used in all his dividing operations, whether upon circles or straight lines, and which Dr. Maskelyne said was 0·001 of an inch shorter on 3 feet than Graham's Royal Society yard E. In the year 1792, or 1793, the celebrated Edward Troughton made for himself a 5-foot scale, which conformed to Bird's, and which he afterwards used in laying down the divisions of the various instruments that passed through his hands. This was the original of all the standard scales he ever made, and at the beginning of the present century he believed these copies, which were made by the aid of micrometer microscopes, to be so exact that no variations could possibly be detected in them, either from the original or from each other. Among the earliest of the scales so made by Troughton was the one used by Sir George Shuckburgh in 1796–98 in his important scientific operations for the improvement of the standards. Subsequently, the length of the metre was determined by comparison with this scale and with the supposed facsimile of it made by Troughton for Prof. Pictet, of Geneva; and thus it happened that on the continent of Europe all measures were converted into English units by a reference to Sir George Shuckburgh's scale. The Royal Commission of 1819 believed Bird's standard of 1760 to be identical with Shuckburgh's scale, and they legalized it rather than the standard of 1758, in order to avoid disturbing the value of the English yard, which was then generally accepted for scientific purposes.

There are yet four other scales of importance in the history of English standards—namely, the brass 5-foot scale made for Sir George Shuckburgh by Troughton in 1796; two iron standard yards, marked 1A and 2A, made for the English Ordnance Survey Department by Messrs. Troughton and Simms in 1826–27; and the Royal Society's standard yard, constructed by Mr. George Dollond, under the direction of Capt. Henry Kater, in 1831.

Bearing in mind the preceding history, the genesis of the present English standard yard may be thus summarized. In 1742, Graham transferred to a bar made for the Royal Society a length which he intended should be that of the Tower yard, but which was really intermediate between the Exchequer standard yard of Elizabeth and its matrix. That length he marked with the letter E, and, although destitute of legal authority, it was immediately accepted as the scientific standard, and was copied by the famous instrument-makers of the time with all the accuracy then attainable. Thus it is in fact the prototype to which all the accurate scales made in England between 1742 and 1850 can be traced. Bird's standard of 1758 was compared with the Exchequer standard and with the Royal Society's yard E., and was of a length between the two. Bird's standard of 1760, legalized as the Imperial standard, in June 1824, was copied from his standard of 1758. After becoming the Imperial standard, Bird's standard of 1760 was compared with Sir George Shuckburgh's scale by Capt. Kater, in 1830, and by Mr. Francis Baily, in 1834; with the Ordnance yards 1A and 2A, in 1834, by Lieut. Murphy, R.E., Lieut. Johnson, R.N., and Messrs. F. Baily and Donkin; and with Kater's Royal Society yard by Capt. Kater, in 1831. On October 16, 1834, the Imperial standard (Bird's standard of 1760) was destroyed by the burning of the Houses of Parliament, in which it was lodged; and very soon thereafter the Lords of the Treasury took measures to recover its length. Preliminary inquiries were begun on May 11, 1838; and on June 20, 1843, they resulted in the appointment of a Commission to superintend the construction of new Parliamentary standards of length and weight, among whose members the Astronomer-Royal (now Sir George B. Airy), Messrs. F. Baily, R. Sheepshanks, and Prof. W. H. Miller, were prominent. The laborious investigations and experiments carried out by that Commission cannot be described here, but it will suffice to say that for determining the true length of the new standard Mr. Sheepshanks employed a provisional yard, marked upon a new brass bar designated "Brass 2," which he compared as accurately as possible with Sir George Shuckburgh's scale, the two Ordnance yards, and Kater's Royal Society yard. The results in terms of the lost Imperial standard were as follows:—

Brass bar 2 = 36·000084 from comparison with Shuckburgh's scale, 0–36 in.
36·000280 ,, ,, ,, ,, 10–46 in.
36·000303 from comparison with the Ordnance yard, 1A.
36·000275 ,, ,, ,, ,, 2A.
36·000229 from Capt. Kater's Royal Society yard.

Mean = 36·000234

Respecting this mean, Mr. Sheepshanks wrote:—"This should be pretty near the truth; but I prefer 36·00025, if in such a matter such a difference be worth notice. I propose, therefore, in constructing the new standard to assume that—

Brass bar 2 = 36·00025 inches of lost Imperial standard at 62° F."

And upon that bar is the standard now in use was constructed.

Turning now to the French standards of length, it is known that the ancient *toise de maçons* of Paris was probably the toise of Charlemagne (A.D. 742 to 814), or at least of some Emperor Charles, and that its *étalon* was situated in the courtyard of the old Châtelet, on the outside of one of the pillars of the building. It still existed in 1714, but entirely falsified by the bending of the upper part of the pillar. In 1668 the ancient toise of the masons was reformed by shortening it five lines; but whether this reformation was an arbitrary change, or merely a change to remedy the effects of long use and restore the *étalon* to conformity with some more carefully-preserved standard, is not quite clear. These old *étalons* were iron bars having their two ends turned up at right angles so as to form *talons*, and the standardizing of end measures was effected by fitting them between the *talons*. Being placed on the outside of some public building, they were exposed to wear from constant use, to rust, and even to intentional injury by malicious persons. Under such conditions every *étalon* would, sooner or later, become too long and require shortening.

Respecting the ancient toise of the masons there are two contradictory stories. On December 1, 1714, La Hire showed to the French Academy what he characterized as "a very ancient instrument of mathematics, which has been made by one of our most accomplished workmen with very great care, where the foot is marked, and which has served to re-establish the toise of the Châtelet, as I have been informed by our old mathematicians." Forty-four years later, on July 29, 1758, La Condamine stated to the Academy that "We know only by tradition that to adjust the length of the new standard, the width of the arcade or interior gate of the grand pavilion, which served as an entrance to the old Louvre, on the side of the rue Fromenteau, was used. This opening, according to the plan, should have been 12 feet wide. Half of it was taken to fix the length of the new toise, which thus became five lines shorter than the old one." Of these two contradictory statements that of La Hire seems altogether most trustworthy, and the ordinary rules of evidence indicate that it should be accepted to the exclusion of the other.

In 1668 the *étalon* of the new toise, since known as the *toise-étalon du Châtelet*, was fixed against the wall at the foot of the staircase of the *grand Châtelet de Paris*—by whom or at what season of the year is not known. Strange as it now seems, this standard—very roughly made, exposed in a public place for use or abuse by everybody, liable to rust, and certain to be falsified by constant wear—was actually used for adjusting the toise of Picard, that of Cassini, the toise of Peru and of the North, that of La Caille, that of Mairan; in short, all the toises employed by the French in their geodetic operations during the seventeenth and eighteenth centuries. The lack of any other recognized standard made the use of this one imperative, but the French Academicians were well aware of its defects, and took precautions to guard against them.

The first toise copied from the *étalon* of the Châtelet for scientific purposes was that used by Picard in his measurement of a degree of the meridian between Paris and Amiens. It was made about the year 1668, and would doubtless have become the scientific standard of France had it not unfortunately disappeared before the degree measurements of the eighteenth century were begun. The second toise copied from the *étalon* of the Châtelet for scientific purposes was that used by Messrs. Godin, Bouguer, and La Condamine for measuring the base of their arc of the meridian in Peru. This toise, since known as the *toise du Pérou*, was made by the artist Langlois under the immediate direction of Godin in 1735, and is still preserved at the Paris Observatory. It is a rectangular bar of polished wrought iron, having a breadth of 1·58 English inches and a thickness of 0·30 of an inch. All the other toises used by the Academy in the eighteenth century were compared with it, and, ultimately, it was made the legal standard of France by an order of Louis XV., dated May 16, 1766. As the toise of Peru is the oldest authentic copy of the toise of the Châtelet, the effect of this order was simply to perpetuate the earliest known state of that ancient standard.

The metric system originated from a motion made by Talleyrand in the National Assembly of France, in 1790, referring the question of the formation of an improved system of weights and measures, based upon a natural constant, to the French Academy of Sciences; and the preliminary work was intrusted to five of the most eminent members of that Academy—namely, Lagrange, Laplace, Borda, Monge, and Condorcet. On March 19, 1791, these gentlemen, together with Lalande, presented to the Academy a Report containing the complete scheme of the metric system. In pursuance of the recommendations in that Report the law of March 26, 1791, was enacted for the construction of the new system, and the Academy of Sciences was charged with the direction of the necessary operations. Those requisite for the construction of a standard of length were:—

(1) The determination of the difference of latitude between Dunkirk and Barcelona.

(2) The remeasurement of the ancient bases which had served for the measurement of a degree at the latitude of Paris, and for making the map of France.

(3) The verification by new observations of the series of triangles employed for measuring the meridian, and the prolongation of them as far as Barcelona.

This work was intrusted to Méchain and Delambre, who carried it on during the seven years from 1791 to 1798, notwithstanding many great difficulties and dangers. The unit of length adopted in their operations was the toise of Peru, and from the arc of 9° 40′ 45″ actually measured, they inferred the length of an arc of the meridian extending from the equator to the Pole to be 5,130,740 toises. As the metre was to be 1/10,000,000 of that distance, its length was made 0·5130740 of a toise, or, in the language of the Committee, 443·296 lines of the toise of Peru at a temperature of 13° Réaumur (16¼° C. or 61¼° F.).

Before attempting to estimate how accurately the standards we have been considering were intercompared, it will be well to describe briefly the methods by which the comparisons were effected. In 1742, Graham used the only instruments then known for the purpose—namely, very exact beam compasses of various kinds, one having parallel jaws for taking the lengths of the standard rods, another with rounded ends for taking the lengths of the hollow beds, and still another having fine points in the usual manner. The jaws, or points, of all these instruments were movable by micrometer screws having heads divided to show the eight-hundredth part of an inch directly, and the tenth of that quantity by estimation; but Mr. Graham did not consider that the measurements could be depended upon to a greater accuracy than 1/1600 of an inch.

Troughton is generally regarded as the author of the application of micrometer microscopes to the comparison of standards of length, but the earliest record of their use for that purpose is by Sir George Shuckburgh in his work for the improvement of the standards of weight and measure in 1796-98. Since then their use has been general; first, because they are more accurate than beam compasses, and, second, because they avoid the injury to standard scales which necessarily results from placing the points of beam compasses upon their graduations. As the objective of the microscope forms a magnified image of the standard, upon which the micrometer wires are set by the aid of the eye-piece, it is evident that in order to reduce the effect of imperfections in the micrometer, the objective should have the largest practicable magnifying power. To show the progress in that direction, the optical constants of the microscopes, by means of which some of the most important standards have been compared, are given in the accompanying table.

Date.	Observer.	Power of microscope.	Magnifying power of objective.	Equivalent focus of eye-piece.	Value of one revolution of micrometer screw.
				Inches.	Inches.
1797	Sir Geo. Shuckburgh	14	1·7	1·50	0·01000
1817	Capt. Henry Kater	18	(2·5)	—	·00428
1834	Francis Baily	27	(2·0)	¾	·00500
1834	Lieut. Murphy, R.E.	—	(2·0)	—	·00500
1850	R. Sheepshanks	—	(2·8)	—	·00358
1864	Gen. A. R. Clarke, R.E. ...	60	4·0	0·67	·00287
1880	Prof. W. A. Rogers, 1 in. obj.	—	(12·7)	—	·00079
	,, ,, ½-in. obj.	—	(28·6)	—	·00035
	,, ,, ¼-in. obj.	—	(52·7)	—	·00019
1883	International Bureau	90	7·5	0·83	0·00394

NOTE.—The magnifying power of Sir Geo. Shuckburgh's microscope seems to be referred to a distance of 12 inches for distinct vision. The powers inclosed in parentheses are estimated upon the assumption that the respective micrometer screws had 100 threads per inch.

In the Memoirs of the French Academy nothing is said respecting the method adopted by the Academicians for comparing their various toises; but in his "Astronomy," Lalande states that the comparisons were effected partly by beam compasses, and partly by superposing the toises upon each other and examining their ends, both by touch and with magnifying glasses, they being all end standards. For the definitive adjustment of the length of their metres, which were also end standards, the French Metric Commission used a lever comparator by Lenoir.

In 1742, Graham used beam compasses, which he considered trustworthy to 0·00062 of an inch, in comparing standards of length; but at that time the French Academicians made their comparisons of toises only to one-twentieth or one-thirtieth of a line, say 0·00300 of an inch, and it was not until 1758 that La Condamine declared they should be compared to 0·01 of a line, or 0·00089 of an English inch, "if our senses, aided by the most perfect instruments, can attain to that." Half a century later, ten times that accuracy was attained by the lever comparator of Lenoir, which was regarded as trustworthy to 0·00077 of an inch ("Base du Système Métrique," t. iii. pp. 447-62).

The heads of micrometer microscopes are usually divided into 100 equal parts, and if we regard one of these parts as the least reading of a microscope, then in 1797 Sir George Shuckburgh's microscopes read to 1/10,000 of an inch; and the least reading of microscopes made since that date has varied from 1/20,000 to 1/35,000 of an inch. A few investigators, among whom may be mentioned Prof. W. A. Rogers, of Colby University, have made the least reading of their microscopes as small as 1/90,000 of an inch, but it is doubtful if there is any advantage in so doing. At the present day the errors committed in comparing standards arise, not from lack of power in the microscopes, but from the difficulty of determining sufficiently exactly the temperature of the standard bars, and the effect of flexure upon the position of their graduations. In order to ascertain the length of a 3-foot standard with an error not exceeding 0·000020 of an inch, its temperature must be known to 0°·06 F. if it is of brass, or to 0°·09 F. if it is of iron. To get thermometers that will indicate their own temperature to that degree of accuracy is by no means easy, but to determine the temperature of a bar from their readings is far more difficult. Again, we imagine the length of our standards to follow their temperature rigorously, but what proof is there that such is the case? If we determine the freezing point of an old thermometer, then raise it to the temperature of boiling water, and immediately thereafter again determine its freezing-point, we invariably find that the freezing-point has fallen a little; and we explain this by saying that the glass has taken a 'set, from which it requires time to recover. Is it not probable that an effect similar in kind, although less in degree, occurs in all solids when their temperature is varying? When we look at the highly polished terminals of an end-standard, we are apt to regard them as mathematical surfaces, separated by an interval which is perfectly definite, and which could be measured with infinite precision if we only had the necessary instrumental appliances; but is that a correct view? The atomic theory answers emphatically, No. According to it, all matter consists of atoms, or molecules, of a perfectly definite size, and with definite intervals between them; but even if that is denied, the evidence is now overwhelming that matter is not homogeneous, but possesses a grain of some kind, regularly repeated at intervals which cannot be greater than 1/2,000,000 nor less than 1/400,000,000 of an inch. Accordingly we must picture our standard bar as a conglomeration of grains of some kind or other, having magnitudes of the order specified, and all in ceaseless motion, the amplitude of which depends upon the temperature of the bar. To our mental vision the polished terminals are therefore like the surface of a pot of boiling water, and we recognize that there must be a limit to the accuracy with which the interval between them can be measured. As a basis for estimating how near this limit we have approached, it will suffice to say that for fifty years past it has been customary to state comparisons of standards of length to 1/100,000, of an inch. Nevertheless, most authorities agree that, although 1/100,000 of an inch can be distinguished in the comparators, 1/25,000 of an inch is about the limit of accuracy attainable in comparing standards. Possibly such a limit may be reached under the most favourable circumstances, but in the case of the yard and the metre, which are standard at different temperatures, the following values of the metre by observers of the highest repute render it doubtful if anything like that accuracy has yet been attained:—

1818	Capt. Henry Kater	39·37079 inches.
1866	General A. R. Clarke ...	39·37043 ,,
1883	Prof. Wm. A. Rogers ...	39·37027 ,,
1885	General C. B. Comstock ...	39·36985 ,,

The earliest standard of English weight of which we have any very definite knowledge is the Mint pound of the Tower of London. It weighed 5400 troy grains, and the coinage was regulated by it up to the year 1527, when it was abolished in favour of the troy pound of 5760 grains. Contemporaneously with the Tower pound there was also the merchant's pound, whose exact weight is now involved in so much doubt that it is impossible to decide whether it consisted of 6750 or of 7200 grains. The Tower pound and the troy pound were used for weighing only gold, silver, and drugs, while all other commodities were weighed by the merchant's pound until the thirteenth or fourteenth century, and after that by the avoirdupois pound. It is not certainly known when the troy and avoirdupois pounds were introduced into England, and there is no evidence of any relation between them when they first became standards. The present avoirdupois pound can be clearly proved to be of similar weight to the standard avoirdupois pound of Edward III. (A.D. 1327–77), and there is good reason for believing that no substantial change has occurred either in its weight or in that of the troy pound since their respective establishment as standards in England.

The oldest standard weights now existing in the English archives date from the reign of Queen Elizabeth, and consist of a set of bell shaped avoirdupois weights of 56, 28, and 14 pounds, made in 1582, and 7, 4, 2, and 1 pounds made in 1588; a set of flat circular avoirdupois weights of 8, 4, 2, and 1 pounds, and 8, 4, 2, 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, and $\frac{1}{16}$ ounces, made in 1588; and a set of cup-shaped troy weights, fitting one within the other, of 256, 128, 64, 32, 16, 8, 4, 2, 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$ (hollow) and $\frac{1}{8}$ (solid) ounces, also made in 1588. All these standards were constructed by order of Queen Elizabeth, under the direction of a jury composed of eighteen merchants and eleven goldsmiths of London, the avoirdupois weights being adjusted according to an ancient standard of 56 pounds, remaining in the Exchequer from the time of Edward III.; and the troy weight being adjusted according to the ancient standard in Goldsmiths' Hall.

In view of the fact that the weight mentioned in all the old Acts of Parliament from the time of Edward I. (A.D. 1274–1307) is universally admitted to be troy weight, the Parliamentary Committee of 1758, appointed to inquire into the original standards of weights and measures in England, recommended that the troy pound should be made the unit or standard by which the avoirdupois and other weights should be regulated; and by their order three several troy pounds of soft gun-metal were very carefully adjusted under the direction of Mr. Joseph Harris, who was then Assay Master of the Mint. To ascertain the proper mass for these pounds, the Committee caused Messrs. Harris and Gregory, of the Mint, to perform the following operations in their presence:—

(1) In the before-mentioned set of troy weights, made in 1588, which were then the Exchequer standard, each weight, from that of 4 ounces up to that of 256 ounces was compared successively with the sum of all the smaller weights; and by a process for which no valid reason can be assigned, it was concluded from these weighings that the troy pound composed of the 8 and 4 ounce weights was $1\frac{1}{2}$ grains too light.

(2) The aforesaid 8 and 4 ounce weights of the Exchequer were compared with five other authoritative troy pounds, four of which belonged to the Mint and one to Mr. Freeman, who, like his father before him, was scale-maker to the Mint, and from the mean of these weighings it appeared that the sum of the Exchequer 8 and 4 ounce weights was 1 grain too light.

The Committee adopted the mean between the latter result and that which they had deduced from the Exchequer weights alone, and accordingly Mr. Harris made each of his three troy pounds $1\frac{1}{4}$ grains heavier than the sum of the Exchequer 8 and 4 ounce weights; but sixty-six years were destined to elapse before Parliament took action respecting them.

The Commissioners appointed in 1818 to establish a more uniform system of weights and measures repeated the recommendations of the Committee of 1758, and as the avoirdupois pound which had long been used, although not legalized by any Act of the Legislature, was very nearly 7000 troy grains, they recommended that 7000 such troy grains be declared to constitute a pound avoirdupois. These recommendations were embodied in the Act of Parliament of June 17, 1824, and thus one of the troy pounds made in 1758 became the Imperial standard. That standard, like Bird's standard yard, was deposited in the Houses of Parliament, and was burned up with them in October 1834.

The present English standard pound was made in 1844–46 by Prof. W. H. Miller, who was one of the members of the Commission appointed in 1843 to superintend the construction of the new Parliamentary standards of length and weight destined to replace those destroyed in 1834. A number of weights had been very accurately compared with the lost standard; namely, in 1824 or 1825, by Capt. Kater, 5 troy pounds of gun-metal, destined respectively for the use of the Exchequer, the Royal Mint, and the cities of London, Edinburgh, and Dublin; and in 1829, by Capt. von Nehus, two troy pounds of brass and one of platinum, all in the custody of Prof. Schumacher, and a platinum troy pound belonging to the Royal Society. The first step for recovering the mass of the lost standard was manifestly to compare these weights among themselves, and upon so doing it was found that for the brass and gun-metal weights the discrepancies between the weighings made in 1824 and 1844 amounted to 0·0226 of a grain, while for the two platinum weights the discrepancies between the weighings made in 1829 and 1845 was only 0·00019 of a grain. With a single exception, all the *new* brass or gun-metal weights had become heavier since their first comparison with the lost standard, the change being probably due to oxidation of their surfaces, and on that account the new standard was made to depend solely upon the two platinum weights. For convenience of reference these weights were designated respectively S*p* (Schumacher's platinum, and RS (Royal Society). A provisional platinum troy pound, T, intermediate in mass between S*p* and RS was next prepared, and from 286 comparisons made in January, February, July, and August, 1845, it was found that in a vacuum

$$T = Sp + 0{\cdot}00105 \text{ grain},$$

while from 122 comparisons made in January, July, and August, 1845,

$$T = RS - 0{\cdot}00429 \text{ grain.}$$

By combining these values with the results of the weighings made in 1824–29—namely,

$$Sp = U - 0{\cdot}52956 \text{ grain,}$$
$$RS = U - 0{\cdot}52441 \text{ grain,}$$

where U designates the lost standard—the comparisons with S*p* gave

$$T = U - 0{\cdot}52851 \text{ grain,}$$

while those with RS gave

$$T = U - 0{\cdot}52870 \text{ grain.}$$

To the first of these expressions double weight was assigned, because the comparisons of T and U with S*p* were about twice as numerous as those with RS. The resulting mean was therefore

$$T = U - 0{\cdot}52857 \text{ grain} = 5759{\cdot}47143 \text{ grains,}$$

and from that value of T the new standard avoirdupois pound of 7000 grains was constructed.

From some time in the fifteenth century until the adoption of the metric system in August 1793, the system of weights employed in France was the *poids de marc*, having for its ultimate standard the *pile de Charlemagne*, which was then kept in the Mint, and is now deposited in the Conservatoire des Arts et Métiers. The table of this weight was

72 grains	= 1 gros	= 72 grains.
8 gros	= 1 once	= 576 ,,
8 onces	= 1 marc	= 4608 ,,
2 marcs	= 1 livre	= 9216 ,,

The origin of the *pile de Charlemagne* is not certainly known, but it is thought to have been made by direction of King John (A.D. 1350–64). It consists of a set of brass cup-weights, fitting one within the other, and the whole weighing 50 marcs. The nominal and actual weights of the several pieces are as follows:—

	Marcs.		Grains.
Boîte de 20 marcs ...	20	+	1·4
Pièce de 14 ,, ...	14	+	4·5
,, de 8 ,, ...	8	−	0·4
,, de 4 ,, ...	4	−	2·1
,, de 2 ,, ...	2	−	1·0
,, de 1 ,, ...	1	−	0·7
Marc divisé	1	−	1·7
	50	±	0·0

In determining the relation of the *poids de marc* to the metric weights, the Committee for the Construction of the Kilogramme regarded the entire *pile de Charlemagne* as a standard of 50 marcs, and considered the individual pieces as subject to the corrections stated. On that basis they found

$$1 \text{ kilogramme} = 18{,}827{\cdot}15 \text{ French grains;}$$ [1]

and as a kilogramme is equal to 15,432·34874 English troy grains, we have

$$1 \text{ livre, poids de marc} = 7554{\cdot}22 \text{ troy grains.}$$
$$= 489{\cdot}506 \text{ grammes.}$$

The metric standard of weight, called a kilogramme, was constructed under the direction of the French Academy of Sciences simultaneously with the metre, the work being done principally by Lefèvre-Gineau and Borda. It was intended that the kilogramme should have the same mass as a cubic decimetre of pure water at maximum density, and the experimental determination of that mass was made by finding the difference of weight in air and in water of a hollow brass cylinder whose exterior dimensions at a temperature of 17°·6 C. were: height = 2·437672 decimetres, diameter = 2·428368 decimetres, volume = 11·2900054 cubic decimetres. The difference of weight in question was first measured in terms of certain brass weights, by the aid of which the platinum kilogramme of the Archives was subsequently constructed, special care being taken to apply the corrections necessary to reduce all the weighings to what they would have been if made in a vacuum ("Base du Système Métrique," t. iii. pp. 574–75).

[1] "Base du Système Métrique," t. iii p. 638.

The best results hitherto obtained for the weight of a cubic decimetre of water, expressed in terms of the kilogramme of the Archives, are as follows [1]:—

Date.	Country.	Observer.	Weight of a cubic decimetre of water at 4° C.
			Grammes.
1795	France ...	Lefèvre-Gineau	1000·000
1797 } 1821 }	England ...	Shuckburgh and Kater	1000·480
1825	Sweden ...	Berzelius, Svanberg, and Akermann	1000·296
1830	Austria ...	Stampfer	999·653
1841	Russia ...	Kupffer	999·989
		Mean	1000·084

These results show the extreme difficulty of determining the exact mass of a given volume of water. The discordance between the different observers amounts to more than one part in a thousand, while good weighings are exact to one part in eight or ten millions. Without doubt two weights can be compared at least a thousand times more accurately than either of them can be reproduced by weighing a specified volume of water, and for that reason the kilogramme, like the English pound, can now be regarded only as an arbitrary standard of which copies must be taken by direct comparison. As already stated, the kilogramme is equivalent to 15,432·34874 English troy grains, or about 2 pounds 3 ounces avoirdupois.

In consequence of the circumstance that the mass of a body is not affected either by temperature or flexure, weighing is an easier process than measuring; but in order to obtain precise results many precautions are necessary. Imagine a balance with a block of wood tied to its right-hand pan and accurately counterpoised by lead weights in its left-hand pan. If, with things so arranged, the balance were immersed in water, the equilibrium would be instantly destroyed, and to restore it all the weights would have to be removed from the left-hand pan, and some of them would have to be placed in the right-hand pan to overcome the buoyancy of the wood. The atmosphere behaves precisely as the water does, and although its effect is minute enough to be neglected in ordinary business affairs, it must be taken into account when scientific accuracy is desired. To that end the weighing must either be made in a vacuum, or the difference of the buoyant effect of the air upon the substances in the two pans must be computed and allowed for. As very few vacuum balances exist, the latter method is usually employed. The data necessary for the computation are the latitude of the place where the weighing is made and its altitude above the sea-level; the weights, specific gravities, and coefficients of expansion of each of the substances in the two pans; the temperature of the air, its barometric pressure, and the pressure both of the aqueous vapour and of the carbonic anhydride contained in it.

Judging from the adjustment of the *pile de Charlemagne*, and the Exchequer troy weights of Queen Elizabeth, the accuracy attained in weighing gold and silver at the mints during the fourteenth, fifteenth, and sixteenth centuries must have been about 1 part in 10,000. The balance which Mr. Harris, of the London Mint, used in 1743 indicated one-eighth of a grain on a troy pound, or about one part in 50,000; while, according to Sir George Shuckburgh, the balance used by Messrs. Harris and Bird in making their observations upon the Exchequer weights, apparently in 1758 or 1759, turned with 1/230,000 part of its load. In 1798, Sir George Shuckburgh had a balance sensitive enough to indicate 0·01 of a grain when loaded with 16,000 grains, or about one part in 1,600,000. The balance used by Fortin in 1799, in adjusting the kilogramme of the Archives, was not quite so delicate, its sensitiveness being only the 1,000,000th part of its load; but in 1844, for the adjustment of the present English standard pound, Prof. Miller employed a balance whose index moved about 0·01 of an inch for a change of 0·002 of a grain in a load of 7000 grains. He read the index with a microscope, and found the probable error of a single comparison of two avoirdupois pounds to be 1/12,000,000 of either, or about 0·00058 of a grain. At the present time it is claimed that two

[1] This table has been deduced from the data given by Prof. Miller in Phil Trans., 1856, p. 760.

avoirdupois pounds can be compared with an error not exceeding 0·0002 of a grain, and two kilogrammes with an error not exceeding 0·02 of a milligramme.

The mean solar day is the natural unit of time for the human race, and it is universally adopted among all civilized nations. Our ultimate standard of time is therefore the rotation of the earth upon its axis, and from that rotation we determine the errors of our clocks and watches by astronomical observations. For many purposes it suffices to make these observations upon the sun, but when the utmost precision is desired it is better to make them on the stars. Until the close of the seventeenth century, quadrants were employed for that purpose, and so late as 1680, Flamsteed, the first English Astronomer-Royal, thought himself fortunate when he succeeded in constructing one which enabled him to be sure of his observed times within three seconds.[1] About 1690, Roemer invented the transit instrument, which soon superseded the quadrant, and still remains the best appliance for determining time. Most of his observations were destroyed by a fire in 1728, but the few which have come down to us show that as early as 1706 he determined time with an accuracy which has not yet been very greatly surpassed. Probably the corrections found in the least square adjustment of extensive systems of longitude determinations afford the best criterion for estimating the accuracy of first-class modern time observations, and from them it appears that the error of such observations may rise as high as ± 0·05 of a second.

During the intervals between successive observations of the heavenly bodies we necessarily depend upon clocks and chronometers for our knowledge of the time, and very erroneous ideas are frequently entertained respecting the accuracy of their running. The subject is one upon which it is difficult to obtain exact information, but there are few time-pieces which will run for a week without varying more than three-quarters of a second from their predicted error. As the number of seconds in a week is 604,800, this amounts to saying that the best time-pieces can be trusted to measure a week within one part in 756,000. Nevertheless, clocks and chronometers are but adjuncts to our chief time-piece, which is the earth itself, and upon the constancy of its rotation depends the preservation of our present unit of time. Early in this century Laplace and Poisson were believed to have proved that the length of the sidereal day had not changed by so much as the 100th part of a second during the last 2500 years, but later investigations show that they were mistaken, and, so far as we can now see, the friction produced by the tides in the ocean must be steadily reducing the velocity with which the earth rotates about its axis. The change is too slow to become sensible within the lifetime of a human being, but its ultimate consequences will be most momentous.

Ages ago it was remarked that all things run in cycles, and there is enough truth in the saying to make it as applicable now as on the day it was uttered. The Babylonian or Chaldean system of weights and measures seems to be the original from which the Egyptian system was derived, and is probably the most ancient of which we have any knowledge. Its unit of length was the cubit, of which there were two varieties—the natural and the royal. The foot was two-thirds of the natural cubit. Respecting the earliest Chaldean and Egyptian system of weights, no very satisfactory information exists, but the best authorities agree that the weight of water contained in the measure of a cubic foot constituted the talent, or larger unit of weight, and that the sixtieth or fiftieth parts of the talent constituted, respectively, the Chaldean and Egyptian values of the mina, or lesser unit of commercial weight. Doubtless these weights varied considerably at different times and places, just as the modern pound has varied, but the relations stated are believed to have been the original ones. The ancient Chaldeans used not only the decimal system of notation, which is evidently the primitive one, but also a duodecimal system (as shown by the division of the year into twelve months, the equinoctial day and night each into twelve hours, the zodiac into twelve signs, &c.), and a sexagesimal system (by which the hour was divided into sixty minutes, the signs of the zodiac into thirty parts or degrees, and the circle into 360 degrees, with further sexagesimal subdivisions). The duodecimal and sexagesimal systems seem to have originated with the Chaldean astronomers, who, for some reason which is not now evident, preferred them to the decimal system, and by the weight of their scientific authority impressed them upon their system of weights and measures. Now observe how closely the scientific thought of to-day repeats the scientific thought of four thousand years ago. These old Chaldeans took from the human body what they regarded as a suitable unit of length, and for their unit of mass they adopted a cube of water bearing simple relations to their unit of length. Four thousand years later, when these simple relations had been forgotten and impaired, some of the most eminent men of science of the last century again undertook the task of constructing a system of weights and measures. With them the duodecimal and sexagesimal systems were out of favour, while the decimal system was highly fashionable, and for that reason they subdivided their units decimally, instead of duodecimally, sexagesimally, or by powers of two; but they reverted to the old Chaldean device for obtaining simple relations between their units of length and mass, and to that fact alone the French metric system owes its survival. Everyone now knows that the metre is not the 10,000,000th part of a quadrant of the earth's meridian; and in mathematical physics, where the numbers are all so complicated that they can only be dealt with by the aid of logarithms, and the constant π, an utterly irrational quantity, crops up in almost every integral, mere decimal subdivision of the units counts for very little. But in some departments of science, as, for example, chemistry, a simple relation between the unit of length (which determines volume), the unit of mass, and the unit of specific gravity, is of prime importance; and wherever that is the case the metric system will be used. To engineers such relations are of small moment, and consequently among English-speaking engineers the metric system is making no progress, while, on the other hand, the chemists have eagerly adopted it. As the English yard and pound are the direct descendants of the Chaldean-Babylonian natural cubit and mina, it is not surprising that the yard should be only 0·48 of an inch shorter than the double cubit, and the avoirdupois pound only 665 grains lighter than the Babylonian commercial mina; but, considering the origin of the metric system, it is rather curious that the metre is only 1·97 inches shorter than the Chaldean double royal cubit, and the kilogramme only 102 grains heavier than the Babylonian royal mina. Thus, without much exaggeration, we may regard the present English and French fundamental units of length and mass as representing respectively the commercial and royal units of length and mass of the Chaldeans of 4000 years ago.

Science tells us that the energy of the solar system is being slowly dissipated in the form of radiant heat; that ultimately the sun will grow dim; life will die out on the planets; one by one they will tumble into the expiring sun; and at last darkness and the bitter cold of the absolute zero will reign over all. In that far-distant future imagine some wandering human spirit to have penetrated to a part of space immeasurably beyond the range of our most powerful telescopes, and there, upon an orb where the mechanical arts flourish as they do here, let him be asked to reproduce the standards of length, mass, and time, with which we are now familiar. In the presence of such a demand the science of the seventeenth and eighteenth centuries would be powerless. The spin of the earth which measures our days and nights would be irretrievably gone; our yards, our metres, our pounds, our kilogrammes would have tumbled with the earth into the ruins of the sun, and become part of the *débris* of the solar system. Could they be recovered from the dead past and live again? The science of all previous ages mournfully answers, No; but with the science of the nineteenth century it is otherwise. The spectroscope has taught us that throughout the visible universe the constitution of matter is the same. Everywhere the rhythmic motions of the atoms are absolutely identical, and to them, and the light which they emit, our wandering spirit would turn for the recovery of the long-lost standards. By means of a diffraction grating and an accurate goniometer he could recover the yard from the wave-length of sodium light with an error not exceeding one or two thousandths of an inch. Water is everywhere, and with his newly recovered yard he could measure a cubic foot of it, and thus recover the standard of mass which we call a pound. The recovery of our standard of time would be more difficult; but even that could be accomplished with an error not exceeding half a minute in a day. One way would be to perform Michelson's modification of Foucault's experiment for determining the velocity of light. Another way would be to make a Siemens's mercury unit of electrical resistance, and then, either by the British Association method, or by Lord Rayleigh's modification of Lorenz's method, find the velocity which measures its resistance in absolute units. Still another way would be to find the ratio of the electro-static and electro-magnetic units of electricity. Thus all the units now

[1] Account of the Rev. John Flamsteed. By Francis Baily. Pp. 45-9 (London, 1835, 4to.)

used in transacting the world's business could be made to reappear, if not with scientific, at least with commercial accuracy, on the other side of an abyss of time and space before which the human mind shrinks back in dismay. The science of the eighteenth century sought to render itself immortal by basing its standard units upon the solid earth; but the science of the nineteenth century soars far beyond the solar system, and connects its units with the ultimate atoms which constitute the universe itself.

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for July contains an article by Prof. H. A. Hazen on storms and a central ascending current. The author discusses a few of the arguments for and against the theories of storm formation, viz. the theory of an ascensional current in the centre of a storm, and that of the increase of energy, through the liberation of latent heat and consequent production of a partial vacuum. Some of the conclusions arrived at are that the theories are exceedingly unsubstantial, and that, above all things, positive information of the processes going on in upper air strata is necessary; that the dependence of the generation of storms on temperature distribution in a vertical direction appears open to doubt; and that, reasoning from the behaviour of thunderstorms, it seems possible that some electrical action, not thoroughly understood, supplies the force which keeps up their energy.—Mr. A. L. Rotch contributes an article on the organization of the Meteorological Service in Holland. The Institute at Utrecht existed as a private establishment as early as 1849, and its Director, Dr. Buys Ballot, first stated publicly in 1857 the relation between wind and atmospheric pressure in the law which bears his name. Dr. Buys Ballot has for many years endeavoured to combine trustworthy observations all over the world with each other, and has published, in his "Year-book," detailed observations for various distant parts. Branch offices are established at Amsterdam and Rotterdam, and the collection of observations made at sea is actively carried on. At present the work bears upon the South Atlantic in connection with the Deutsche Seewarte, and, independently, upon the Indian Ocean.—Prof. C. F. Marvin continues the discussion between himself and Prof. Hazen as to the cause of differences obtained in anemometer experiments. Prof. Hazen attributes them to the influence of the natural wind blowing at the time, while Prof. Marvin thinks this of little importance and calculates its effect by a formula. He thinks that the momentum theory of the cups explains the discrepancies in a satisfactory manner.—Lieut. J. P. Finley contributes tornado statistics for the State of Iowa for fifty-two years ending 1888.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, August 7.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—Prof. C. V. Riley, of Washington, was elected an Honorary Fellow in place of the late Dr. Signoret, of Paris.—Mr. Walter F. Blandford exhibited a specimen of *Cardiophorus cinereus*, taken at Tenby, and remarked that the species had rarely, if ever previously, been found in the United Kingdom. Mr. C. O. Waterhouse said he believed that there was a specimen in the collection of his late father, and also another specimen in the collection of the British Museum.—Mr. Waterhouse stated that the British Museum had just received from the Rev. A. Elwin, of Hangchow, China, a luminous larva about 1½ inch long, and 3½ lines broad, which he believed to be one of the *Lampyridæ*.—Lord Walsingham exhibited specimens of *Conchylis degreyana*, McLach., bred from seed-heads of *Plantago lanceolata* at Merton, Norfolk; also a specimen of *Tineidæ* allied to the genus *Solenobia*, probably belonging to *Dissoctena*, Staud., but differing somewhat in the structure of the antennæ. He said that the specimen was taken by himself at Merton on July 31 last, and that the species was apparently undescribed.—Herr Meyer Darcis exhibited a collection of Coleoptera, comprising specimens of a species of *Loethrus* from Turkestan; *Julodis globithorax*, Stev., from the Caucasus; a new species of *Julodis* from Kurdistan; *Cardiaspis Mouhotii*, Saunders, from Sikkim; *Carabus smaragdinus*, Fisch., from Siberia; *Julodis ampliata*, Mars., from Aintab, Asia Minor; and *Julodis luteogramma*, Mars., from Syria, and a variety of the same from Kurdistan. —Mr. H. Goss read extracts from letters from Mr. R. W. Fereday, of New Zealand, and Sir John Hall, K.C.M.G., relating to a number of Lepidoptera recently collected at sea, about half-way between the River Plate and Rio, at a distance of over 250 miles from land, in about 30° S. lat. and 46° W. longitude. It was stated that the ship was surrounded by swarms of moths. Mr. J. J. Walker, R.N., observed that he had seen large numbers of insects at sea about 150 miles off the coast of Brazil, and he referred to other records of the capture of insects at sea in Darwin's "Voyage of the *Beagle*," and Dr. Carpenter's "Cruise of the *Alert*." The discussion was continued by Dr. Sharp, Lord Walsingham, Mr. White, and Mr. Kirby.—Mr. E. Meyrick read a paper entitled "On some Lepidoptera from New Guinea," and exhibited the species described in the paper. He stated that the specimens were derived from two sources, viz. (1) a portion of the collection received by the Society from Baron Ferdinand von Müller, F.R.S., and collected by Mr. Sayer when accompanying the Australian Geographical Society's Exploring Expedition; and (2) a number of specimens collected by Mr. Kowald near Port Moresby.—Mr. Blandford read a letter from Mr. Wroughton, of Poona, asking for assistance in working out certain Indian Hymenoptera and Diptera in the collections of the Bombay Natural History Society. Lord Walsingham, Colonel Swinhoe, and Mr. Moore made some remarks on the subject.

PARIS.

Academy of Sciences, July 29.—M. Des Cloizeaux, President, in the chair.—On a means of studying the natural history of the river-eel after its migration from fresh to salt water, by M. Émile Blanchard. It was shown by the author many years ago that, during its sojourn in fresh-water lakes and rivers, the eel remains in an immature state, incapable of reproduction. With a view to completing its life-history, he now suggests that large numbers should be taken on their way seawards, labelled with a little metallic plate, and then returned to the water, in the hope that a few so marked may afterwards be captured at intervals in the sea, and thus enable zoologists to follow their complete evolution. To give effect to this project, he seeks the co-operation both of Government and naturalists.—On the variations of latitude in the solar spots, by M. R. Wolf. Attention is directed to pp. 84–91 of No. 73 of the *Astronomische Mittheilungen*, where the author infers, from his studies of solar physics, that the abrupt change of latitude noticed at the epoch of the minimum does not depend exclusively on the period of 11⅓ years, but also on the longer period of 66⅔ to 88⅓ years. This longer period determines the altitude of the maxima, so that the extent of the change in latitude and the altitude of the ensuing maximum increase or diminish simultaneously. Hence the change of latitude recorded by Spoerer for the second half of the seventeenth century would not appear to have been anomalous, but merely the result of the low maxima reached at that time.—On the transmission of power by alternate currents, by M. Maurice Leblanc. But for the difficulties caused by the phenomena of self-induction, alternate currents might be advantageously employed for transmitting power, as they may easily receive the highest tensions, while they do not alter the insulators, as is the case with continuous currents. The author here describes an apparatus by means of which he hopes that these difficulties may be obviated.—On the conductibility of electrolytes at very high temperatures, by M. Lucien Poincaré. The methods employed by MM. Bouty and Poincaré in their experiments on the electric conductivity of salts in solution cannot be directly applied beyond the melting-point of glass. But, by various dispositions here detailed, M. Poincaré has been enabled to obtain measurements to within about one-fiftieth of absolute accuracy.—On a new method of volumetric analysis for silver, mercury, and thallium, by M. Adolphe Carnot. Chemists possess excellent processes for the volumetric analysis of silver, but very defective ones for that of mercury. M. Carnot here describes a new method, based on the use of potassium iodide, which is about equally applicable to both of these metals, as well as to thallium.—Researches on the sulphites, by M. P. J. Hartog. Despite the difficulty of its preparation, the author has succeeded in obtaining an anhydrous and crystallized sulphite of potassa in large quantities. By the same process, which is described in detail, he has also prepared the sulphite of soda in the same form, but not in a pure state, and the double normal sulphite of potassium and soda. The preparation of other sulphites will be described in a future communication.—Synthesis of some selenium compounds in the aromatic series, by M. C. Chabrié. The syntheses here undertaken are those in which the metalloid is united directly with the carbon of the cyclic nucleus. By making selenium tetrachloride act on benzine, M. Chabrié has obtained compounds corresponding to the sulphides and thiophenols prepared by Friedel and Crafts from sulphur and the chloride of sulphur.—On the oxidizing action of nitroso-camphor under the influence of light, by M. P. Cazeneuve. This substance, recently obtained by the author (*Comptes rendus*, cviii. p. 857), yields Liebermann's blue reaction with phenol and sulphuric acid, and also presents the curious phenomenon of becoming decomposed under the influence of light. The conditions seem somewhat analogous to those attending the formation of chlorophyll and of the colouring-matter in flowers.—On the isocamphols, by M. A. Haller. The paper deals more particularly with the influence of solvents on the rotatory power of the isocamphols.—Respiratory combustion by the nervous system in its relation to the size of the animal, by M. Charles Richet. A large number of experiments on dogs confirm, for animals of the same species, the law established by Regnault and Reiset for animals of different species —namely, that the respiratory combustions, by kilogramme of living weight, increase in inverse ratio to the size of the animal. —On the products of microbes favourable to the development of infections, by M. G. H. Roger. Amongst the substances secreted by Bacteria, some are known to produce intoxicating phenomena, while others possess vaccinating properties. M. Roger's researches lead to the inference that there are others that tend to stimulate the development of certain pathogenic agencies, at least in the case of symptomatic charbon.—On a new Mediterranean species of the genus Phoronis, by M. Louis Roule. This specimen was found in the Zoological Station at Cette, and has been named *Ph. sabatieri*, from the founder of that station. The characteristics of the species are here described by contrast with *Ph. hippocrepis*, Str. W.—On the growth of the oceanic sardine, by M. Georges Pouchet. Observers have failed to determine the region where the sardine is hatched and passes the first phases of its development. Those reaching the fishing-grounds are already several months old, and the observations made at several points present so many discrepancies that no general law can be laid down regarding their growth during the fishing season. The difficulty of determining this point is increased by the fact that the shoals themselves appear to be continually renewed by fresh arrivals throughout the whole season.

August 5.—M. Des Cloizeaux, President, in the chair.—Heat of combination of fluor with hydrogen, by MM. Berthelot and Moissan. After many failures, the authors have at last succeeded in measuring the heat of combination of these bodies. Reserving for a future communication the details of their experiments, they here give the broad results expressed in the formulas :—

$$H + F \text{ gas} = HF \text{ gas} \quad \ldots \quad + 37.6 \text{ calories}$$
$$H + F = HF \text{ dissolved} \quad \ldots \quad + 49.4 \quad ,,$$

—On the relations of atmospheric nitrogen with vegetable soil, by M. Th. Schlœsing. Continuing his researches on this subject with fresh samples of earth taken from various districts and under varying conditions, the author has still failed to discover any soil which being destitute of vegetable germs fixes gaseous nitrogen. Hence he concludes that if any exist they must be regarded as quite exceptional, and not to be depended on by agriculturists. —Observations of Davidson's comet (July 23) made at the Observatory of Algiers, by MM. Trépied, Sy and Renaux. The observations cover the period from July 26 to July 30, when the nucleus of the comet was comparable to a star of the eighth magnitude.—A study of the electric phenomena produced by solar radiations, by M. Albert Nodon. Numerous observations made at the laboratories of the Sorbonne and the Collège de France show that on meeting an insulated metallic or carbon conductor the solar rays communicate to it a positive electric charge ; that the amplitude of this charge increases with the intensity of the rays, and decreases with the hygrometric state of the air, the phenomenon attaining its maximum value in Paris about 1 p.m. in summer, when the atmosphere is pure and dry ; lastly, that the effects cease during the transit of clouds across the face of the sun. If these results can be extended to non-metallic bodies, then solar radiation may be regarded as one of the causes of the electrization of the clouds.—Researches on the sulphites (continued), by M. P. J. Hartog. Here are studied the double normal sulphites of potassium and ammonium, and the bisulphite sulphite of sodium and potassium.—On the heat of combustion of some organic compounds, by M. J. Ossipoff. The bisubstituted succinic acids, presenting certain analogies with the fumaric and malic acids, are here studied thermochemically with a view to determining their heat of combustion.—A chemical and thermic study of the phenolsulphuric acids (continued), by M. S. Allain Le Canu. In the present paper the author confines his researches to orthophenolsulphuric acid, the preparation and properties of which are fully described.—On the distribution of Nemertes on some points of the French seaboard, by M. L. Joubin. A systematic exploration of the Roscoff and Banyuls districts has resulted in the discovery of nearly sixty species of Nemertes in those two localities alone. About ten of these have not yet been described, and will form the subject of a future memoir.—On the mechanism of the photodermatic and photogenic functions in the siphon of *Pholas dactylus*, by M. Raphael Dubois. Although these mollusks possess no eyes, they display extreme sensibility to light, the least change of its intensity sufficing to excite a more or less sudden contraction of the siphon. M. Dubois's already described graphic process has enabled him to verify the existence of two distinct functions, one receptive, the other emissive, thus showing that the mechanism of sight belongs to the category of tactile phenomena in the higher animals gradually differentiated and localized in a special organ. It also appears that the photodermatic (receptive) function is stimulated by luminous vibrations from without, while the photogenic (emissive) has for its final outcome the emission of luminous rays through the circumambient medium.—On some habits of the sea trout, by M. A. Giard. The author's observations in the Wimereux estuary and neighbouring waters, tend to show that many smelts and grilses, and even a number of adults (bull-trout ?), pass a much longer time in the sea than is generally supposed by ichthyologists.—On the colouring matter of the spermoderm in the Angiosperms, by M. Louis Claudel. The results are here given of a series of studies on the pigments of grains made in the botanical laboratory, Marseilles. It appears generally that the solid pigments of grains are scarcely ever presented under the form of leucite, and that they derive directly from the protoplasm. In this respect they differ from the pigments of flowers and pericarps which, according to Flahault and others, derive from pre-existing leucites.—On the recent eruption of the island of Vulcano (Lipari Group), by M. O. Silvestri. The volcanic phenomena presented by the eruption, which began on August 2, 1888, are characteristic of a special phase, which has already been observed by M. Silvestri at Etna, and to which he proposes to give the name of *Vulcanian phase*.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Practical Photometry: W. J. Dibdin (King).—Picture-Making by Photography, 2nd edition: H. P. Robinson (Hazell).—Shut out from Love: J. Nickal (Hughes).—Éléments d'Économie Politique Pure, deuxième édition: L. Walras (Lausanne, Rouge).—Lehrbuch der Vergleichenden Anatomie, Zweite Abtheilung: Dr. A. Lang (Jena, Fischer).—Cours de Minéralogie, deuxième édition: A. de Lapparent (Paris, Savy).—Chemical and Physical Studies in the Metamorphism of Rocks; A. Irving (Longmans).

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