THURSDAY, OCTOBER 10, 1889.

THE SCIENCE COLLECTIONS AT SOUTH KENSINGTON.

THE English, like their American cousins, are a remarkable people, and in nothing more remarkable than in their toleration of incongruities.

They are gradually amassing at South Kensington an art collection which has but few rivals in the world, and the approach to it is shadowed by architectural monstrosities which, if regard is had to their position, are absolutely unrivalled. Within, they visit with apparent pleasure a hall decorated by the hand of Sir Frederick Leighton, and filled with the most exquisite masterpieces of Oriental art. Without, they are satisfied with the "Brompton boilers."

They concentrate, during a quarter of a century, collections to illustrate natural history, the physical sciences, and art, on one spot. They make it the head-quarters of State-aided education in science and art. Under the highest auspices a building is growing there, in which the products of the colonies are to be displayed to view. Close to it stands the Central Institute of the City and Guilds of London for Technical Education. Thus, in their own peculiar way, by apparently disconnected steps, and under the management of half-a-dozen independent, authorities, they evolve the noble idea of a great centre in which collections of all that is interesting, beautiful, and useful in the history and present applications of science and art shall illustrate, and be illustrated by, the researches and teaching of men like Profs. Huxley and Flower. They arrange a scheme by which the benefits are not confined to the metropolis only. The collections are circulated through the provinces, and provincial teachers are brought to the collections. Having done all this, they leave it in the power of one of the authorities—a set of irresponsible Commissioners—to cut into the heart of the site thus dedicated to science and art, with rows of stucco "mansions."

They house their natural history collections in a palace, and place its management in hands which have made it a palace of delight. They store invaluable collections to illustrate the progress of science and technology in sheds which are barely water-tight, and liable to burn like a tinder-box. They rely for their arrangement upon the "good-nature" of Professors of whose formal duties it forms no part, and upon the patriotism of men like Mr. E. A. Cowper, who, rather than see a good thing left undone, are willing, at great personal sacrifice, to do it themselves. Lastly, they leave questions as to the success of this haphazard system to be raised, not by some recognized scientific authority, but by the Treasury clerks.

Fortunately, as is so often the case in England, the results are better than the system. The suggestion that a great collection of scientific apparatus should be formed at South Kensington was made by the Duke of Devonshire's Commission. The ideal to be aimed at was defined by Commissioners appointed by the Science and Art Department, under the Chairmanship of Sir Frederick Bramwell. The Treasury, however, alarmed by "complaints of want of space in the galleries," recently determined to reconsider the matter for themselves, and appointed another Commission, which was, as it was doubtless intended to be, a very strong one. Had the Report, signed by the Treasurer of the Royal Society—Dr. Evans—as Chairman, by Lords Rayleigh and Francis Hervey, by Mr. Bernhard Samuelson, by Sir Douglas Galton and Sir Henry Roscoe, been adverse to the practical results attained by the collections, apart from the system under which they have been achieved, their opinion would doubtless have been regarded as conclusive. As it is, the Treasury will now have to bear in mind the following, which it will perhaps regard as inconvenient, facts.

The Commissioners were asked "whether there are any duplicates or other objects no longer essential to the value and representative character of the collections, which might be removed in order to provide additional accommodation for new objects of greater importance." They reply that "little, if any, space can be gained by weeding the existing collections."

They were requested to "investigate the existing practice of circulating scientific objects on loan to museums and schools so far as it affects the question of accommodation for storage or exhibition purposes at South Kensington." They reply that the space used for such purposes "has no practical bearing as to the housing of the collections."

They were not explicitly asked as to whether the existing museum accommodation is or is not adequate, but they assert that the question cannot be separated from those which were referred to them, and they recommend that, instead of the 60,000 square feet at present occupied, "an exhibition space of about 90,000 square feet should be provided without delay" in order to secure "a creditable Science Museum."

They also assume that this space will be covered with buildings "well arranged, well lighted, and of a durable character," a series of conditions which, as is evident to the casual visitor no less than to the Commissioners, is not fulfilled by some of those at present in use.

In short, the South Kensington Museum contains only objects which ought to be exhibited, in buildings *not* suited for their preservation and exhibition, and in space so cramped that it ought *without delay* to be increased by 50 per cent.

The Report was very well received by the daily press, and is certainly justified by the facts. It now only remains for public opinion to urge the Treasury to carry out the recommendations of its own Commission.

The Museum appears to be appreciated by the general public, and even to compete with the Natural History Museum on more equal terms than could have been expected. During the last four years there has been a steady increase in the number of visitors. In 1888 the Science Museum was inspected by 259,588, and the Natural History Museum by 372,802 persons.

Teachers under the Department are allowed, with certain reasonable restrictions, to bring their classes to the galleries, and to have the apparatus taken out of the cases for their inspection. The number of visitors who have thus had the cases opened for them, or in other ways have received special assistance in the galleries, has risen from 174 in 1880 to 1687 in 1888. The number of classes which have had the advantage of instruction illustrated by the apparatus which their teachers have been allowed to handle and to demonstrate, has increased from 7 in 1880 to 81 last year.

Every year 600 science teachers apply to be allowed to attend the summer courses held in the Normal School of Science. From these 200 are selected, and not only are the lectures which they attend illustrated by means of objects contained in the Museum, but they are able to inspect typical collections of apparatus, ready set up and arranged for the performance of the experiments which they are recommended in the Directory of the Science and Art Department to show to their classes.

It appears, then, that the full use of the galleries is not hampered by unnecessary restrictions. Permissicn to handle the apparatus is a privilege which does not suggest red-tape. Every effort is made to enable provincial teachers to share the advantages which may be reaped from the Museum, and it is casting no discredit upon the admirable provincial colleges which are springing up in our large towns to say that even a country like England could not gather more than one such collection as that which is being formed at South Kensington. Let us hope that the Report of the Commission has made it certain that, to the benefit of both town and country, the development of the collection will be promoted, and that before long it will be properly housed.

Although, however, the management of the Museum seems to have been satisfactory in practice, the Commissioners again travel outside the exact terms of reference to them, to express an opinion that the organization of the staff in charge of it requires revision. One of the Professors was specially examined on this point, and his opinion appears to be closely in accordance with the terms of the Report.

At present the responsibility for the collections lies primarily with the Lord President, and next to him with the Vice-President and the Secretary to the Science and Art Department. It is but one of the many proofs—which are often overlooked or ignored—of the ability with which the Department has been administered, that the Commissioners find no fault with the present state or futuie aims of the Museum. It is, however, impossible that collections so varied should be controlled without the help of experts, and, as matters stand, this assistance is sought in a more or less informal way from consultative Committees who have no real authority and no official responsibility. The results attained in some sections have been, for the most part, due to volunteers like Mr. Cowper, who has acted on several Committees of advice, and done the lion's share of the work, so that he "has been"—to quote his own words—"familiar with every one of the machines in the Department." The collections of scientific instruments are supervised by the Professors of the Normal School, whose advice is given subject to the limitations just described.

It would be premature to discuss the details of a scheme by which these arrangements might with advantage be superseded. A hearty admiration for the work which has been accomplished and for the ability of the officers of the Department under whose authority it has been carried out is, however, compatible with a no less hearty assent to the opinion of the Commissioners that "the system is defective in principle, apart from the personal qualities of those working under it," and that "the responsibility for the formation and supervision of these collections should certainly be of a more definite kind."

DARWINISM.

Darwinism. By Alfred Russel Wallace, LL.D., F.R.S. (London: Macmillan and Co., 1889.)

THE object of Mr. Wallace in writing the admirable work which he has published with the title of "Darwinism" has been "to give such an account of the theory of natural selection as may enable any intelligent reader to obtain a clear conception of Darwin's work, and to understand something of the power and range of his great principle." No one has so strong a claim as Mr. Wallace to be heard as an exponent of the theory of the origin of species, of which he is—with Darwin—the joint author. He has produced a thoroughly readable book, condensing into an octavo volume much of the speculation and description of important facts which are contained in the numerous volumes published by Darwin himself, and in the essays and occasional contributions of subsequent writers. Besides this, Mr. Wallace's book contains an exposition of highly important and interesting views of his own on subsidiary matters, which have either not been published previously or have appeared in a scattered and more or less inaccessible form. Consequently, the book is one which has interest not only for the general reader, to whom it is primarily addressed, but also for the more special student of natural history. The latter will find in its pages an abundance of new facts and arguments which, whether they prove convincing or not, are of extreme value and full of interest. If we attempt here to point out some of the shortcomings of Mr. Wallace's treatise, it is not from any desire to minimize its value and interest, but rather an acknowledgment of the weight and significance of a work on so important a subject by so specially competent an author.

Mr. Wallace's book necessarily suffers, in comparison with the works of Darwin himself, by the limitation of space. It is in consequence of this compression that we miss in the new statement by Mr. Wallace that extraordinary cogency or power of convincing which so distinguished the writings of Darwin. With Darwin one becomes accustomed to see no speculation put forward, no step of an argument advanced, unless there is an overwhelming weight of testimony in its favour: facts are cited in astonishing abundance, and at the same time the conviction establishes itself that the author has reserves of fact as rich as those of which he makes use, and further that he is so scrupulous and so modest that he will never ask his reader to accept a conclusion, however trivial, without stating fairly the amount of evidence for and against such conclusion. Mr. Wallace is prevented by the scope of his work from such treatment of his subject. As a result, his conclusions often appear to be (when they may not be so) based on very insufficient evidence, and his statement meagre. "Darwinism" can never take the place of the "Origin of Species," but may well serve as an introduction to the study of that and the other works of Darwin—the value of which, not only as storehouses of fact and suggestion, but as classical models of scientific discussion, cannot be over-estimated, and will probably never be surpassed.

In his preface, Mr. Wallace, through a misconception which is perhaps explained by the retired life which he enjoys—makes an attack upon what he calls "the modern school of laboratory naturalists." He states that these persons seek to minimize the agency of natural selection and to subordinate it to laws of variation, of use and disuse, of intelligence and heredity. He commends, as leading to truer views, the study of the external and vital relations of species to species in a state of nature—a study which Semper has called "the physiology of organisms," and I have proposed in the article "Zoology" in the "Encyclopædia Britannica" to call "bionomics." Now though there is no doubt an increasing number of younger students who have little or no interest in natural history beyond what is derived from the contemplation of ribbons of sections dyed like Joseph's coat, yet it is going too far to say that they have in any sense formed a school. And further, if we endeavour to estimate the influence on naturalists of a considerable devotion of time to the study in the laboratory of histology and embryology, physiology and morphology, we shall be led to the conclusion that this study has been associated with exactly opposite results from those attributed to it by Mr. Wallace. Who are they who seek to minimize natural selection and to set up the false gods of variation, use and disuse, &c.? Certainly not laboratory men. Is the Duke of Argyll a laboratory naturalist? Is Dr. George Romanes? Is Prof. Cope? Are Mr. Herbert Spencer and Prof. Patrick Geddes? I venture to say they are not; yet they are the authors with whom Mr. Wallace has subsequently to contend when he maintains that the selection of congenital variations by natural selection is an adequate theory of the origin of species, and requires no aid from Lamarckism, Copism, or other interlopers. Who are they who agree with Mr. Wallace in this contention? Precisely "laboratory men," who are, however, by no means *only* laboratory men, but, like Darwin himself, search for their material in the garden, the field, the sea-shore, or the sea-bottom; and as a part—but only a part—of their study of it eventually bring it to the laboratory. Such a "laboratory naturalist" is Weismann, whose essays and memoirs in favour of the identical view maintained by Mr. Wallace, appear to have escaped his attention until very recently. I presume also that I may claim to be a laboratory naturalist; and yet four years ago I found it necessary, in lectures delivered at the London Institution, to discard even that tincture of Lamarckism which Darwin had admitted, and to advocate "pure Darwinism," on the ground that the Lamarckian hypothesis is still devoid of experimental basis, and in view of the logical principal *Entia non sunt multiplicanda præter necessitatem.* It is true, as I have elsewhere insisted, that there are not at present such facilities for the study of bionomics as are provided in our laboratories for the study of histology, embryology, morphography, and the physics and chemistry of living bodies. But it is not right to identify the class of speculations, to which Mr. Wallace is opposed, with laboratory training. This, indeed, in virtue of its tending to bring speculation to the test of fact, is favourable, and often directly conducive, to the study of "the external and vital relations of species to species in a state of nature," or in one word "bionomics." I will only cite as instances Bateson's researches in Tartary, Caldwell's in Australia, Poulton's experiments on insects, and Moseley's "Notes of a Naturalist on the *Challenger*."

Mr. Wallace's plan of treatment of his subject is an excellent one. After a brief statement of what naturalists have understood by the word "species," and a lucid exposition of the views of the earlier transmutationists, he enunciates Darwin's theory. He then proceeds to show, by citing a wide and comprehensive array of facts, that the foundations of the theory are secure. In one chapter he describes the rapid multiplication of organisms and the consequent struggle for existence; in further chapters the fact of variability is shown, by an appeal to instances, to be one of the widest and most general character; in another chapter the facts of heredity and selection are brought forward. Then follow discussions of "difficulties and objections," hybridity, the origin and use of colour in animals and in plants, geographical distribution, the geological evidences of evolution, the fundamental problems of variation and heredity, and, lastly, Darwinism applied to man.

The chapter on "Variability of Species in a State of Nature" is one on which considerable pains has been expended. It presents some of the facts of variation in a very striking manner, and provides us with a number of well-studied instances which have not before been accessible to naturalists. A method followed by Mr. Wallace is to take any large collection of a single species and to measure various parts, such as length of head, tail, limb, &c. As he observes, it is very important to convince ourselves that variation does occur in a state of nature, so that natural selection has the material to act upon. He considers that the instances which he brings forward show that the range of variation is larger and more general in a state of nature than is usually assumed, and that "it is clear that Mr. Darwin himself did not fully recognize the enormous amount of variability that actually exists." Whilst admitting the interest of Mr. Wallace's present contribution to this subject, I think it is clear that he has failed to make a distinction which is desirable and important, viz. that between *variations* exhibited by adult specimens and the *variability* presented by the young of any given species. After all, the specimens of lizards and birds, of which the measurements are given to us by Mr. Wallace, only comprise such individuals as were *not too widely divergent* from the parent form to survive to maturity under conditions which select more or less closely a given specific set of characters. What one would like to know is the actual range of variability as shown by the artificial rearing of *all* the offspring of a single pair. With plants such a study of variation is practicable, but less so with animals. Variation includes those extreme cases which are called "monstrosities," and it is by no means certain that natural selection would *always* exclude these extreme cases from survival. The facts of variation under domestication are more to the point, in so far as the range of congenital variability is concerned, since in regard to a limited number of animals and plants we have removed the primary sifting of young forms. This sifting must occur under natural conditions, so as to allow only a limited range of variations to reach the collector in his museum. Clearly enough, this primary sifting, and all later operations of the same kind due to natural conditions, may under new circumstances be vastly modified in their nature, and variations may be allowed to pass the sieve which at another time are excluded. The range of variation, therefore, in even a very large museum series of a wild species, can afford but an inadequate notion of the variability of animals. We may, however, justly conclude that, if the former is so large as Mr. Wallace shows it to be, the congenital variations which occur, but never in given conditions reach maturity, must comprise instances which are very much more marked, and would furnish abundant material for natural selection were the natural conditions of the species to change. An attempt to determine by experimental rearing, the range of congenital variation (that is, of *possible* adult variation) in such animals and plants as are fitted for the inquiry, seems to be well worth making.

Mr. Wallace, who must have watched the early criticism of Darwin's theory with special keenness, makes a good point when he insists that the objection that it is difficult "to imagine a reason why variations tending in an infinitesimal degree in any special direction should be preserved" is a quibble. Darwin never used the word "infinitesimal," but spoke of variations being "slight" or of "small amount," and we agree with Mr. Wallace that even those terms are open to the objection that they may seem to imply that congenital variation is of less range and frequency than it really is.

Naturally enough, Mr. Wallace is not equally thorough in his treatment of each of the various "difficulties and objections" which he discusses, but the chapter thus headed gives an interesting summary of the present state of opinion. Among the matters discussed are the supposed smallness of variations, the doubt as to the right variations occurring when required, the beginnings of important organs, useless or non-adaptive characters, the instability of non-adaptive characters, the swamping effects of intercrossing, and the effects of isolation. In some of these instances Mr. Wallace's reasoning is very clear and forcible; in other cases it is much less so. Mr. Cunningham has already pointed out, in a letter to NATURE (July 25, p. 297) a curious slip on Mr. Wallace's part in his explanation of the gradual development of the twisted condition of the head and eyes of flat-fish. Mr. Wallace declines to admit the transmission of acquired characters as a cause of variation and progressive development; yet, apparently without being conscious of it, he attributes the movement of the eye of flat-fish from one side of the head to the other, to the transmission of a series of slight shiftings of the eye acquired in successive generations by the muscular effort of the ancestors of our present flat-fish, which is (to use an expression already known to the readers of NATURE) "flat Lamarckism." In relation to this, I may mention that the asymmetry of the Gastropod Mollusca, the forward position of the anus, and the twisted condition of the nerve-loop in the Streptoneurous division of that class, had been similarly attributed by myself to the cumulative effect of a mechanical cause—the one-sided lopping of the shell—operating in successive generations. Like Mr. Wallace, I had failed to notice that the explanation adopted was an admission of Lamarckism. It seems to me possible to explain the position of the flat-fish's eye by the selection of congenital variations, since there is no doubt of the advantage to the animal of having its two eyes on the one side of the body. But I confess that the Gastropods at present have not been satisfactorily explained. I have not been able at present (and I say at present advisedly) to find any evidence of advantage to the Gastropod in the torsion of its visceral hump, such as would justify the supposition that a monstrosity presenting this condition in full development was favoured by natural selection; still less does it appear how the steps of a gradual torsion—that is, a series of approximations to complete torsion—could be advantageous. It does not follow that we must admit Lamarckism; but merely that we must further examine Gastropod habits, structure, and development with this problem in mind.

Mr. Wallace does not, in my judgment, give sufficient grounds for rejecting the proposition which he indicates as the main point of Mr. Gulick's valuable essay on "Divergent Evolution through Cumulative Segregation." By the bye, Mr. Gulick is one of the heretics who attribute some part in the production of species to other causes than natural selection, yet he is not a laboratory naturalist, but one who, substituting land-shells for butterflies, has precisely the same foundations and training as Mr. Wallace himself. Mr. Gulick's idea is that there is an inherent tendency to variation in certain divergent lines, and that when one portion of a species is isolated, even though under identical conditions, that tendency sets up a divergence, which carries that portion further and further away from the original species; or, in other words, no two portions of a species possess exactly the same average character, and the initial differences will, if the individuals of the two groups are kept from intercrossing, assert themselves continuously by heredity in such a way as to insure an increasing divergence of the forms belonging to the two groups, amounting to what is recognized as specific distinction. Mr. Gulick's idea is simply the recognition of a permanence or persistency in heredity, which, *cæteris paribus*, gives a twist or direction to the variations of the descendants of one individual as compared with the descendants of another. Ireland is cited by Mr Wallace as an evidence that isolation has not been effective in modifying specific character of plants and animals. If, however, unlike Mr. Wallace, we may look upon mankind as subject to the same developmental causes, and only to the same causes, as animals, then Ireland would seem to be a very interesting case of the production of divergent character by isolation. All parties are agreed that, whatever value is to be assigned to the fact, the human inhabitants of Ireland, whether of Celtic or Teutonic ancestry, exhibit characters which are "divergent" from those of the inhabitants of Great Britain, and, without going into details, we may say that the isolation and persistence of an original tendency seem to be the only explanation of the divergence.

The subject of "correlated variations" is but lightly touched on by Mr. Wallace, and its immense importance in relation to the whole question of "useless organs" and useless characters of growth and structure is not sufficiently put forward, as it was by Mr. Darwin. It is true that we know little about the physical basis of correlated variation, and are therefore open to hostile criticism when we take refuge in an appeal to it as an explanation of phenomena. The truth is that correlated variation is as important a property of living matter as heredity and variability themselves. It may be formulated thus : "Every departure from the parental form of any given part of an animal or plant is accompanied by a definitely correlated and often a commensurate departure in other parts remote from it." The possibilities thus introduced are simply gigantic—a new factor is brought in which extends the results of simple variation and selection indefinitely. In the future the laws and limitations of correlated variation will no doubt be determined. At present our knowledge of them rests where Mr. Darwin himself placed it. Both Mr. Gulick's doctrine of persistent hereditary tendency, and that of the immense capacities of correlation in variation, commend themselves to the mind of a laboratory naturalist who is accustomed to conceive of vital phenomena as mechanico-physical affections of a living substance, viz. protoplasm. They are, on the other hand, less valued—perhaps insufficiently—by Mr. Wallace.

In his chapter on the infertility of crosses, Mr. Wallace treats at length and with admirable effect a very important subject, as to which he is full of ingenious novel suggestions and apposite facts. His criticism of Mr. Romanes's essay, entitled "Physiological Selection," appears to me to be entirely destructive of what was novel in that laborious attack upon Darwin's theory of the origin of species.

The chapter on the origin and uses of colour in animals is that which will be most interesting to the general reader, and is indeed a charming essay, illustrated by numerous woodcuts. Here Mr. Wallace sets forth at length his convincing argument as to the use of colour as a means of recognition among animals, giving many examples—amongst others, that of the white patch on the rabbit's tail. In conjunction with his theory of the importance of the principle "like to like" in the segregation of varieties and the consequent development of new species, great significance must be attached both to the nervous organization, which makes recognition possible, and to the markings or other characters which are recognized. A very interesting discussion of Mr. Darwin's theory of sexual selection occurs in a subsequent chapter. Mr. Wallace, whilst admitting some of the effects of sexual selection recognized by Darwin, is not able to follow him in attributing to it the brilliant colours of birds and butterflies. Mr. Wallace attributes the deeper or more intense colouring of the male, which often occurs, to his "greater vigour and excitability." The female in many groups retains the primitive and more sober colours of the group for purposes of protection. The occurrence of colour itself in patches and lines is attributed by Mr. Wallace (following the late Mr. Alfred Tylor) to the distribution of subjacent nerves and blood-vessels, which follow, like the colour-patches, in the main, certain lines determined by the general structure. Mr. Wallace seems scarcely to have succeeded in showing that Darwin's theory of sexual selection is inapplicable to the explanation of special developments of colour and ornament, although he has suggested additional causes which influence the primary distribution and development of colour.

We have not space to speak of subsequent chapters on colour in plants and on geographical distribution, concerning the latter of which subjects Mr. Wallace speaks with every title to respect, and suggests some novel views. On the "Geological Evidences of Evolution" as well as on the "Fundamental Problems" of variation and heredity, he is less satisfactory. In regard to the latter, one chapter is altogether an inadequate space in which to deal with such an array of antagonists as Mr. Herbert Spencer, Dr. Cope, Dr. Karl Semper, and Mr. Patrick Geddes. Mr. Wallace has barely space to do more than state his opponents' views, and to give a rapid summary of reasons for his dissent, without sufficiently establishing those reasons. This will be especially regretted by those who, like myself, agree with Mr. Wallace in his rejection of Spencer's and Semper's Lamarckism, and are unable to attach any serious value to the speculations put forward on this matter by Dr. Cope and Mr. Geddes. The translation of Weismann's "Essays," which appeared coincidently with Mr. Wallace's book—although many of the essays have been for some years familiar to readers of German—supplies that more solid treatment of the subject which is desirable. It is satisfactory to find that justice is done by Mr. Wallace to Mr. Francis Galton, whose views on heredity, arrived at by a special method of inquiry, are closely similar to those arrived at on other grounds by Weismann.

Prof. Semper's work "On the Natural Conditions of Existence as they affect Animal Life" is duly mentioned by Mr. Wallace, and he does not fail to notice the striking fact that in this interesting volume the author entirely fails—as I pointed out in NATURE when it appeared—to adduce a single fact in proof of the Lamarckian theory which he sets out to champion.

Of the American evolutionists Mr. Wallace justly says: "In place of the well-established and admitted laws to which Mr. Darwin appeals, they have introduced theoretical conceptions which have not yet been tested by experiments or facts, as well as metaphysical conceptions which are incapable of proof." They have, in fact, conspicuously abandoned the "scientific method."

The words which Mr. Wallace has applied to the American evolutionists are, in the opinion of many, strangely applicable to portions of his own concluding chapter on "Darwinism applied to Man." He here introduces us to a "spiritual world" and to "different degrees of spiritual influx." Mr. Wallace is in the peculiar position of one who believes that he has experimental evidence of the remarkable theoretical and metaphysical conceptions which he introduces. He boldly takes up this position, and we may be sure that he would not wish attention to be diverted from it. It remains an interesting problem for the future student of human faculty to reconcile Mr. Wallace's wonderful ingenuity and skill as a reasoner and observer concerning animal life, with his views as to the so-called "manifestations" of spiritualists.

Mr. Wallace's contention that the mathematical, musical, and artistic faculties of man have not been developed under the law of natural selection must in large part be conceded. Whilst the earlier development of these faculties may be explained as due to natural selection, since some amount of each may well have been an advantage to the primitive man in his struggle for existence, it is yet true that their sudden and rapid development to a very much higher level in civilized communities cannot be traced to the struggle between man and man. It does not, however, follow that, because natural selection will not account for these extraordinary developments of the human brain, therefore we must have recourse to the assumption of supernatural agencies. Mr. Wallace seems so much convinced of the importance and capability of the principle of natural selection, that when it breaks down as an explanation he loses faith in all natural cause, and has recourse to metaphysical assumption. On the other hand, it must be contended that we know very little of the development, either in the individual or in various races, of these and other faculties of the mind. The formation of civilized communities has had the result of withdrawing the individual man almost entirely from the operation of natural selection. Such selection as still obtains operates by the struggle of communities rather than by that of individuals. Accordingly there is a possibility of the most useless "sports" making their appearance, and even establishing themselves in human communities as hereditary qualities. Mr. Gulick's notion that an initial tendency due to accidental variation can increase and develop in succeeding generations, without reference to the advantage or disadvantage of the species, would assuredly be applicable, if anywhere, to the human mind in communities where individuals are no longer subject to natural selection, or only to a minimal extent, and in relation to a few points of structure. Does the luxuriant development of some Professor's mathematical faculty, as compared with the poor numerical conceptions of an Australian black, offer really any greater difficulty of transition than do the 9-feet-long tail feathers of some Japanese barn-door fowls, as compared with the shorter feathers of other varieties? That is a question which can only be answered by a more elaborate analysis of the nature of the qualities compared than has, so far as I know, been hitherto accomplished.

E. RAY LANKESTER.

GALTON'S AFRICAN TRAVELS.

Narrative of an Explorer in Tropical South Africa; being an Account of a Visit to Damara Land in 1851. By F. Galton. With *Vacation Tours in* 1860 *and* 1861, by Sir George Grove, F. Galton, and W. G. Clark. (London: Ward, Lock, and Co., 1889)

THE editor of the Minerva Library has added Mr. Francis Galton's "Narrative of an Explorer in Tropical South Africa" to a library consisting only of "works of the most widely-spread and lasting popularity, which have proved themselves worthy of a permanent place in literature." Had the stamp of popularity not been so precisely insisted upon as a title to admission into this particular series, it might have been his gracious task, now and then, to have rescued from oblivion a stray work which had failed to obtain recognition from causes in no way affecting its intrinsic merits. Books of travel, for instance, have often suffered from inopportune publication, and, being out of season, have fallen flat and unheeded; whereas, if introduced a quarter of a century later, they would have been welcomed by an expectant public, educated for the occasion. But the condition, as it now stands, relieves the editor from a difficult and somewhat delicate responsibility, the adequate discharge of which might well need the exercise of more than one man's judgment and experience. Fortunately for the book under notice, it fulfils all the professed requirements of Mr. Bettany's prospectus. It presents quite a model in respect of style, and is essentially the production of a master mind. The writer lucidly recounts his experiences for the instruction as well as entertainment of his readers, and the outcome of his labour is a success to himself and a benefit to his fellows. His narrative is interesting and well sustained; his descriptive powers are manifestly considerable; his appreciation of men and things, the animate and inanimate, is admirable; and he is realistic to the legitimate extent of insuring credence for a record of personal adventure not untinged with romance.

Independently of these considerations, however, there is another reason why the reproduction of Mr. Galton's book at the present hour is practically useful. He treats of a particular section of Africa which has hardly received its fair amount of attention from the politicians and geographers of Europe, when discussing the great partition question of the day. The Congo has had the lion's share of solicitude as regards the western coast-line; while the spirited and intelligent action of the British East African Company, and movements of rival or contemporary Companies, have resulted in throwing, as it were, a broad bright light upon the whole length of eastern sea-board from Guardafui to the Zambesi. Names such as Mombasa and Vitu are becoming familiar as Zanzibar and Mozambique, and the practical effect of Messrs. Johnston and Thomson's successful journeys has been to introduce into our school teaching the true stories of Kilimanjaro and the Masai Land. Below the Congo there has been little demand for information, and consequently little supply. The knowledge that Portugal is owner of the coast-line from the much-coveted mouth of that river to that of the Kunene, has appeared sufficient to render Benguela and Mossamedes, with their inland territories and contiguous tracts, matters of secondary interest; and below Mossamedes nothing but a political embroglio has caused Walfisch Bay and Angra Pequena to emerge from out the haziness of quasi-mythical places. Mr. Galton's narrative will not only be found instructive in itself, but it cannot fail to lead the uninitiated reader into references and inquiries also full of enlightenment; and even those who are well up in African geography, and may have read the book on its first appearance, will most probably derive advantage in reverting to its pages. The part of the Dark Continent comprehended in its treatment is from somewhere near the mouth of the Kunene to the mouth of the Orange River, and may be roughly indicated by a figure to which the parallels 17° and 27° S. latitude give a northern and southern boundary, and the meridians 12° and 20° longitude western and eastern limits. In the appendix much of the exploration of later years is reviewed, and the names of Messrs. Anderson and Coates Palgrave, Lord Mayo, and Père Duparquet of the Huilla Mission are honourably mentioned. Those of Drs. Höpfner and Stapff might have been added, bringing up to May 1886 the roll of authorities whose labours need only some new political complication to become of palpable value. Moreover, it may here be parenthetically stated that the West African Telegraph Company have recently prolonged their cable south of St. Paul de Loanda, opened as a station in 1886—a proceeding which should facilitate the acquisition of much new geographical information in connection with the region specified. Already has a contribution from one of its officers appeared in the Proceedings of the Royal Geographical Society, from which we learn that the harbour of Mossamedes is only second to that of Loanda among the harbours of Western Africa, and that the latter, possessing less depth of water, and being smaller, suffers in the comparison. Benguela is described as the more important commercial port, the total amount of its annual imports and exports (£268,398) being nearly four times more than those of Mossamedes.

Now that thirty-nine years have passed since this exploration was undertaken it may be well to recall its extent and character. As apparent from the map illustrative of his labours, Mr. Galton moved in a north-easterly direction from Walfisch Bay to Barmen, journeying a distance of nearly 200 miles. After an excursion from that place—called "the head seat of intelligence as regards Damara and Hottentot movements"—he struck off to Ovampo Land in the north by a route involving a slight detour to the east. But before making a fair start from Schmelen's Hope (a point somewhat further inland than Barmen), he was led by circumstances to Eikhams and Rehoboth in the south; and part of the return road from Ondongo, the *ultima Thule* of his northerly journey, was by a track other than that he had at first pursued. From the date that his waggons left Schmelen's Hope to that of their return, a period of exactly five months elapsed. Of these, we are told that "ninety days were employed in journeying onwards, independently of such excursions as were made from time to time to look out for roads." The return distance is reckoned at about 462 miles, the average rate of travelling being nine and a half miles a day—fair indeed with a more or less improvised carriage, barbarous retinue, and tired or hungry and thirsty bullocks.

Some notion of the privations endured may be gathered from our traveller's statement that, on his leaving Schmelen's Hope for Ovampoland, his "biscuit and every kind of vegetable food had been eaten up"; or, as later expressed, he had "no biscuit, no flour, or anything of the sort." He and his party had to live on the oxen and sheep they possessed, and the game they contrived to shoot on the way, so that grain-food was a special luxury. Milk, though used in profusion by the Damaras, could rarely be purchased from them, owing to some impeditive superstition. Their ordinary *pabulum*, pig-nuts, was "worthless and indigestible," inasmuch as it should be "eaten in excessive quantities to afford enough nourishment to sustain life." As to water, this was a more serious matter still. On some days it utterly failed; on others it was brackish, or barely drinkable—as may be inferred from the following description of a *vley*, or watering-place:—

"Fancy a shallow pool, from 10 to 20 yards across, and from 6 to 12 inches deep, in which a herd of wild animals, say fifty zebras, have been splashing and rolling themselves all night, and which they have left in every respect like the water pumped out of a farmyard; and, where wild animals are wanting, the oxen, in spite of every precaution, will do the same."

The sporting adventures of the book are especially remarkable. It is not improbable that more than one tender-minded reader will accord sympathy to a wounded giraffe hunted to the death, or a dog slain for the sake of its skin—both occasions illustrating an act perpetrated to satisfy the pressing wants of man. Few, however, can fail to appreciate the manly qualities of a traveller whose career in Africa brings him into frequent and willing contact with the lion, rhinoceros, hippopotamus, elephant, and those larger and more formidable animals contention with which is risk of limb and life. The same stout-heartedness is exhibited in his intercourse with the savage inhabitants of the tracts through which he moves, and in the fearless but efficient diplomacy which characterizes his dealings with Bushman and Hottentot, Namaqua, Damara, Ovampo, or whatever the designation of those with whom he has to do.

Not the least exciting part of Mr. Galton's narrative will be found in the penultimate chapter. At this time he had retraced his steps from Ovampo Land to Barmen. Instead, however, of continuing his return journey to the coast, he resolved to devote three and a half out of the four months which would necessarily intervene before the arrival of an expected schooner in Walfisch Bay, to an expedition to the eastward, for the double purpose of seeing something of the Hottentot inhabitants, and ascertaining how far correct was the statement that the Karri-karri Desert was "interposed as an impracticable barrier between the sea-coast countries and Lake 'Ngami." The following extract from this particular chapter (pp. 168–69) furnishes a good specimen of the writer's descriptive powers. He and his associates had just repaired some circular walls of loose stones intended to serve as shooting-screens:—

"It is one of the most strangely exciting positions that a sportsman can find himself in, to lie behind one of these screens or holes by the side of a path leading to a watering-place so thronged with game as 'Tounobis. Herds of gnus glide along the neighbouring paths in almost endless files; here standing out in bold relief against the sky, there a moving line just visible in the deep shades; and all as noiseless as a dream. Now and then a slight pattering over the stones makes you start; it jars painfully on the strained ear, and a troop of zebras pass frolicking by. All at once you observe, 20 or 30 yards off, two huge ears pricked up high above the brushwood; another few seconds, and a sharp solid horn indicates the cautious and noiseless approach of the great rhinoceros. Then the rifle or gun is poked slowly over the wall, which has before been covered with a plaid, or something soft, to muffle all grating sounds; and you keep a sharp and anxious look-out through some cranny in your screen. The beast moves nearer and nearer; you crouch close up under the wall, lest he should see over it and perceive you. Nearer, nearer still; yet somehow his shape is indistinct, and perhaps his position unfavourable to warrant a shot. Another moment, and he is within 10 yards, and walking steadily on. There lies a stone, on which you had laid your caross and other things, when making ready to enter your shooting-screen; the beast has come to it, he sniffs the taint of them, tosses his head up wind, and turns his huge bulk full broadside on to you. Not a second is to be lost. Bang! and the bullet lies well home under his shoulder. Then follows a plunge and a rush, and the animal charges madly about, making wide sweeps to right and left with his huge horn, as you crouch down still and almost breathless, and with every nerve on the stretch.

"He is off; you hear his deep blowing in the calm night; now his gallop ceases. The occasional rattling of a stone alone indicates that he is yet a-foot; for a moment all is still, and then a scarcely audible 'sough' informs you that the great beast has sunk to the ground, and that his pains of death are over."

The author has long since been so well known as a gold medallist and leading member of the Royal Geographical Society, and for his important contributions to anthropology and other departments of science, that it would be superfluous at this time to dwell on the value of his explorations, compared with those of the ordinary and less-gifted traveller. His exceptional aptitude for what may be called professional travel is well exemplified at pp. 180-82 of the volume now republished, in which, among other useful hints, he gives plain and practical instructions for selecting the best sort of travelling compass and checking distances and directions. His manual on the "Art of Travel" has for many years been a standard work of reference; while no one who reads, in the "Narrative of an Explorer," his amusing record of "a series of observations" taken by sextant upon the figure of a Hottentot lady—with results worked out "by trigonometry and logarithms"—can affirm that his sense of humour has been blunted by scientific pursuits.

A republication of "Vacation Tours in 1860 and 1861" —papers by Sir George Grove and the late Mr. W. G. Clark, added to one by Mr. Galton—enhances the value of this new accession to the Minerva Library.

OUR BOOK SHELF.

Practical Photometry: a Guide to the Study of the Measurement of Light. By W. J. Dibdin. (London: Walter King, 1889.)

THIS work forms a good practical text-book on the art of photometry, which, both scientifically and commercially, is becoming more and more important. It contains a comprehensive account of the various methods in daily use, so that the student, when he finds that he is dealing with instruments and methods unfamiliar to him, may turn to this book as a guide to the many precautions necessary to insure accurate results. The first few chapters deal with the history and principles of photometry, together with horizontal, radial, and jet photometers, and diagrams are given of the determinations of the quantity of light afforded in all directions horizontally by three classes of flames tested at every 10°, and also of Dr. Pole's method of expressing the illuminating power and rates of consumption per hour of fifteen-candle gas. In chapters vi. and vii. we have a discussion on the various standards of light which have been and are still in use, followed by the numerous proposed substitutes, such as Harcourt's pentane, Sugg's sixteen-candle argand, Methuen's screen, &c. The apparatus necessary to check and measure the flow of gas to the standard burner is given in chapter viii., with detailed descriptions. Chapters ix. and x. treat of "The Examination and Adjustment of a Gas-testing Photometer" and "Colour Photometry," the latter dealing with methods of estimating the colour and intensity of the illumination of fabrics, &c. Lastly, in chapter xi., on "Stellar Photometry," the author gives an account of the methods employed by Sir John Herschel, Zöllner, and others, concluding with a description of a method proposed by himself.

The appendix contains some useful pieces of miscellaneous information, and tables of illuminating power of sperm candles, candle corrections, &c. The work is well illustrated with numerous woodcuts of the various instruments employed.

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 Testing of Colour-Blindness.

THE important article in NATURE of September 5 (p. 438) will have been read by all friends of education with deep and melancholy interest.

I desire to point out that the real remedy ought not to be dealt with by an Act of Parliament, partly because this is a makeshift, and partly because the sudden dismissal of a trained seaman for a constitutional defect is so cruel that human nature would get very much in the way of such an Act of Parliament.

What is wanted is that the colour sight and other measurable faculties of youths should be tested before they go out into life. It is a detail of practical anthropometry. Just as no parent would think of encouraging his son to become a barrister when he knew that he stammered or was deaf, so no parent would waste money in training his son to be an engine-driver or a sailor when he knew—as he ought to know—that the lad was colour-blind.

I protest against a defect of our educational system being treated as a defect of legislation or of administration, and I trust that the friends who seek that scientific education should have its due place will move in this direction, and thus prevent the cruelty to seamen and their families as well as the deaths of their fellow-creatures which are at present possible from the causes indicated in your article.

I desire to ask if some of your readers will kindly furnish information, through your columns, as to where the colour-tests can be obtained, their cost, and the literature respecting them.

9 King Street, Oxford, September 19. J. F. HEYES.

Mites.

IN the grounds of the Leicester Museum there are some half-dozen young lime trees, about fifteen years old, of which the trunks and under sides of the main branches are covered with an extremely thin glistening film. It seems to consist of a similar material to that of which spider's web is made, but spread out into a film instead of being spun into thread. When rubbed up it has all the appearance of spider's web similarly treated. But there are no spiders visible, except here and there an Epeira, who has fixed his geometric web to some protuberances on the trunk.

The bark, however, is thickly dotted over with small yellowish mites, very similar to cheese mites in size, form, and colour. Some of these mites are creeping on the outside of the film also, and a few of them are captured in the webs of the Epeira. No other trees appear to be affected except the limes, and only one particular row of these. I have found a few of the mites, but no film, on other limes in the grounds, but none on horse-chestnuts or laburnums which stand nearer to the affected trees.

How has this film been produced? and are the mites connected with it as cause or effect? F. T. MOTT.

Leicester, September 23.

Sailing Flight of Large Birds over Land.

THE explanation of this, in NATURE (p. 518), seems inadequate.

If a large body of air be moving uniformly both in respect of direction and of velocity, no matter at what rate, it might as well be perfectly motionless, in respect of its ability to aid the flight of a bird that is simply floating in it. But in fact the air never is motionless: it moves with the earth, from west to east, at the rate, let us assume, of 500 miles an hour. To a bird floating in the air, whether the earth beneath it moves exactly as the air moves, or not, must be a matter of perfect indifference. The earth's relative motion does not affect it. I must myself adhere to the explanation which I gave in a former number of NATURE (vol. xxviii. p. 28), that the birds avail themselves of differences of the movement of the air, in respect of velocity, or of direction, or of both. Mr. S. E. Peal has noticed that "flocks which drift over the hills recover their position on the plains by descending to windward." This is simple enough. The wind is flowing from the plains towards the hills. It rises then as it flows, and has many inequalities in its direction and rate. On entering a gorge a narrow current of air would be thrown upwards with very rapid ascent. Of all the inequalities the birds know how to avail themselves.

R. COURTENAY.

Tean Vicarage, September 28.

A Remarkable Meteor.

ON Sunday, September 29, at 7.30 p.m., I observed a very brilliant meteor falling nearly perpendicular a little to the west of north. Its progress towards the earth appeared to be much slower than is usually the case with such bodies, the heavens being illuminated for several seconds. The meteor was of a bright sapphire hue; preceding it were a few drops of bright fiery red, whilst following it came a brilliant trail of light. It seems to have been pretty generally observed throughout Ireland, and letters to the Press from counties Roscommon, Galway, Kilkenny, and Kildare, testify to the interest it has awakened in the country.

RICHARD CLARK.

113 Upper Leeson Street, Dublin, October 7.

THE METHOD OF QUARTER SQUARES.[1]

THE method of quarter squares consists in the use of the formula

$$ab = \tfrac{1}{4}(a + b)^2 - \tfrac{1}{4}(a - b)^2$$

to effect the multiplication of two numbers, a and b. If we are provided with a table giving the values of $\frac{1}{4}n^2$ up to a given value of n, we may obtain, by the aid of this formula, without performing any multiplication, the product of any two numbers whose sum does not exceed the limit of the table.

The method is specially interesting on account of the great simplicity of the formula, by means of which a table of double entry may be replaced by one of single entry. How great a transformation is effected by such a change is evident, if we consider that the largest existing multiplication table of double entry reaches only to 1000 × 1000, and forms a closely-printed folio of 900 pages, but that a table of quarter squares of the same extent (*i.e.* of $\frac{1}{4}n^2$ up to $n = 2000$) need only occupy 4 octavo pages. The disparity becomes even more conspicuous as the limit of the table is extended, for a table of double entry extending to 10,000 × 10,000, would require nearly 100 folio volumes; and one extending to 100,000 × 100,000, would require nearly 10,000 volumes; whereas the corresponding quarter-square tables need only occupy 40 and 400 octavo pages respectively.

The use of a table of squares in effecting multiplications was recognized as far back as 1690, when Ludolff published his large table of squares, extending to 100,000. In the introduction to the table Ludolff explained how it could be employed in multiplications. In order to multiply a and b the table is to be entered with $a + b$ and $a - b$ as arguments, and the difference of the corresponding squares divided by 4. If a and b are both even, or both uneven, their sum and difference will both be even numbers, so that $\frac{1}{2}(a + b)$ and $\frac{1}{2}(a - b)$ will be integers. In either of these two cases we may therefore enter the table with the semi-sum and semi-difference of the numbers as arguments, the product being the simple difference of the corresponding squares.

It was not, however, till 1817 that a table of *quarter* squares (*i.e.* of $\frac{1}{4}n^2$ for argument n) was published, for the purpose of facilitating multiplications. If n be uneven, $\frac{1}{4}n^2$ consists of an integer and the fraction $\frac{1}{4}$. This fraction $\frac{1}{4}$ may be ignored in the use of the table, for if either $a + b$ or $a - b$ is uneven, the other is so too; the fraction $\frac{1}{4}$ therefore occurs in both squares, and disappears from their difference. It may therefore be omitted from the table.

The table of 1817, which contained the first practical application of the method, was published by Antoine Voisin, at Paris, under the title "Tables des Multiplications; ou, Logarithmes des Nombres entiers depuis 1 jusqu'à 20,000." It is curious that Voisin should have called a quarter square a logarithm: he called a the root, and $\frac{1}{4}a^2$ its logarithm. His table extended to 20,000, and was thus available for multiplications up to 10,000 × 10,000. On the title-page Voisin described it as effecting multiplications up to 20,000 by 20,000. This statement is justified by the formula

$$ab = 2\{\tfrac{1}{4}a^2 + \tfrac{1}{4}b^2 - \tfrac{1}{4}(a - b)^2\},$$

by which the product was to be obtained if the sum of the numbers exceeded 20,000, the method of quarter squares being then no longer available. It is to be observed, however, that this formula requires three entries besides the final duplication.

Almost simultaneously (1817) a similar table, of the same extent, was published independently by A. P. Bürger at Carlsruhe. The method was rediscovered by J. J. Centnerschwer, who published a table of the same extent in 1825 at Berlin. In 1832, J. M. Merpaut published, at Vannes, a table of quarter squares extending to 40,000. In 1852, Kulik (well known for his large table of squares and cubes to 100,000), who had again rediscovered the method, published a table extending to 30,000. In 1856, Mr. S. L. Laundy published, at London, the largest table of quarter squares which had appeared previous to the publication of the present table. Laundy's table extends to 100,000. It was intended that the multiplications should be effected by means of quarter squares if the sum of the numbers did not exceed 100,000, but other five-figure numbers were to be multiplied by means of Voisin's three-entry formula referred to above.

It is this change of method that has detracted so greatly from the value of Laundy's fine table. It is evident that the table should have been carried to double its actual extent, *i.e.* to 200,000, so that any two five-figure numbers could be multiplied together by means of the two-entry formula. The late General Shortrede constructed such a table, but it was never printed. In the work under notice Mr. Blater carries the table as far as 200,000; so that, more than sixty years after the publication of the first table effecting the multiplication of two four-figure numbers, the extension to five figures has at last been completed.

The method of quarter squares has had no opportunity of a fair trial in the absence of a table extending to 200,000. Considering the many purposes to which Crelle's tables (which give the product of any two three-figure numbers by a single entry) are continually applied, it is perhaps surprising that no general use should ever have been made of a table which in a very small compass gives, by only two entries, the product of two four-figure numbers. Still it is clear that the full power of the method

[1] "Table of Quarter Squares of all Whole Numbers from 1 to 200,000 for simplifying Multiplication, Squaring, and Extraction of the Square Root, and to render the Results of these Operations more certain." Calculated and published by Joseph Blater. (London: Trübner and Co., 1888.)

is not felt till we are provided with such a table giving the product of two five-figure numbers. As already stated, the fact that the limit of Laundy's table was only 100,000 has deprived it of most of its value, for it is obvious that, unless all five-figure numbers can be treated by a uniform method, the table could not be conveniently employed in practice.

Mr. Blater's work consists of the principal table (giving quarter squares up to 200,000), which occupies 200 pages; a small table of four pages, called the index, to facilitate the use of the table in the extraction of square roots; and an introduction, &c., of fourteen pages.

The arrangement of the table (in which the author has followed the plan adopted by Kulik in his table of 1852, already referred to) is somewhat peculiar. The table is first entered (*i.e.* the required page of the table is found) by means of the *last* three figures of the number: the table is then entered on this page (or, more correctly, double page), by means of the preceding figures. For example, the quarter square corresponding to 126,993 is found by turning to the double page headed 990. In one of the four columns headed 993 we enter the table at the line 126: from this line we obtain, in the first column, the first four figures of the result, 4031; in the column under 993, the next three, 805; from the bottom of the column we take the last three figures, 512. The result is therefore given in three parts A, B, C; A being common to ten numbers (in the same line) beginning with 126, C being common to fifty numbers (in the same column) ending with 993, and B being special to the combination 126,993.

The table is beautifully printed in large antique figures on thick and excellent paper, and is a handsome piece of typography. The author mentions that it was entirely set up by a single compositor at the printing-office of Mr. Falk, at Mayence, and that it occupied his whole time for eleven months. Besides being admirably printed, the table is no doubt very correct, as a triple calculation was made, and no pains seem to have been spared by Mr. Blater for insuring accuracy.

The book is dedicated to Mr. Anthony Steinhauser, of Vienna, who has contributed a short historical preface. Mr. Steinhauser, who is himself the author of several logarithmic tables, encouraged Mr. Blater in his work, and rendered him great assistance throughout. The actual calculation occupied eighteen months.

With respect to the general employment of Mr. Blater's table for the performance of multiplications, it is to be feared that its utility has been jeopardized by the size of page adopted. Anyone who has had occasion to make constant use of tables knows the enormous advantage of the octavo form over the quarto. The book is placed to the left of the computer, and the effort of carrying by the eye a series of figures from the left-hand page of a quarto table to the paper—a distance of 18 inches to 2 feet—is but ill compensated for by larger figures or fewer pages. Handsome as the book is to look at, it seems to us that the table would have had much more chance of bringing the method into general use if it had been of octavo form. It is greatly to be regretted that it was not printed on 400 octavo instead of 200 quarto pages, which would have been quite possible with the existing arrangement of the table. If this had been done, and if the type had been somewhat smaller, a neat and handy volume might have been produced.

The mode of entering the table is very insufficiently explained in the introduction. This is unfortunate, as the mode of entry (by the last figures) is so unusual in tables that it should have been explicitly mentioned. Also the translation into English is so very unsatisfactory as to be obscure in places. These, however, are minor blemishes which would have but slight influence on the general utility of the table, if only the form were convenient.

The question of how far the method of quarter squares is likely to come into use is of some interest. Hitherto the method has been very little known, and, so far as we know, it has never been used in practice on any extended scale. The mere fact that it has been so constantly discovered anew is sufficient evidence of the slight attention that it has received. Still, there ought to be room for a table that gives, to the last figure, the products of any two five-figure numbers with only two entries. A seven-figure table of logarithms is inadequate for this purpose, for, besides requiring three entries, it only gives the first seven figures of the result. On the other hand, it may be said that in ordinary calculations seven figures are as many as are required, and that logarithms possess the advantage of being equally convenient for divisions and multiplications. It must be admitted that a five-figure quarter-square table is appropriate to only a limited class of calculations: it applies only to multiplications, and the number of figures in each of the two numbers must not be greater than five. These conditions are of a somewhat special kind. In recent years when heavy multiplications have been required it has become the custom to make use of Thomas de Colmar's arithmometer; and probably, at the present time, nearly all systematic work of this character is carried out either by Crelle's tables or by the arithmometer.

Passing now to the general question of multiplication by means of a table of single entry, we have the two methods of quarter squares and logarithms, each possessing its special advantages. There is also an older method which passed out of notice with the invention of logarithms. This method was called "prosthaphæresis," and depended on the formula

$$\sin a \sin b = \tfrac{1}{2}[\sin\{90^\circ - (a - b)\} - \sin\{90^\circ - (a + b)\}].$$

A table of natural sines could therefore be used as a multiplication table, four entries being required. This method is due to Wittich, of Breslau, who was assistant for a short time to Tycho Brahé, and it was used by them in their calculations in 1582. It is referred to by Raymarus Ursus, Clavius, and Longomontanus; and it seems to have been used for performing multiplications not only when the numbers occurred as sines but also in the case of ordinary numbers.

The method of quarter squares depends upon so simple a formula, that it is strange that the first table should not have appeared until 1817. There seems no reason why it should not have been employed before the invention of logarithms, when it would have been a most valuable aid to calculation. The geometrical theorem, which is equivalent to the algebraical identity $(a + b)^2 - (a - b)^2 = 4ab$, on which the method depends, forms Prop. viii. of the second book of Euclid; and one would think that the application of the geometrical or algebraical theorem to arithmetic might have been noticed at any time. The actual history of mathematical tables is, however, entirely different from what we might expect it to have been, owing to the wonderfully early invention of logarithms: and it was, in fact, only just about that time that the importance of tables as an aid to general calculation was beginning to be felt. The date of Herwart ab Hohenburg's great double-entry multiplication table, extending to 1000 × 1000 (the same limit as Crelle's table, and which has never been exceeded) is only four years earlier (1610) than that of Napier's "Canon Mirificus" (1614).

It is interesting to notice that the method of quarter squares is more closely connected mathematically with the method of prosthaphæresis than with that of logarithms; in fact, from the formula

$$\sin a \sin b = \tfrac{1}{2}\{\cos(a - b) - \cos(a + b)\},$$

we readily deduce

$$ab = \tfrac{1}{4}\{(a + b)^2 - (a - b)^2\},$$

by expanding the sines and cosines in ascending powers of their arguments and equating the terms of two dimensions.

The method of quarter squares enables us to multiply together two numbers of n figures each if we have a table extending to 2×10^n. If the latter only extends to 10^n three entries are required, and the final result has to be doubled whenever the sum of the numbers exceeds 10_n (as in Laundy's table). If we consider the question of the multiplication of two numbers of n figures each by means of a table extending only to 10^n, the same process being employed in all cases, it appears that three entries are necessary, and that it would be better to tabulate half squares, using the formula

$$ab = \tfrac{1}{2}a^2 + \tfrac{1}{2}b^2 - \tfrac{1}{2}(a-b)^2,$$

In tabulating the half squares the fraction $\frac{1}{2}$ would be thrown off, so that if a and b were both uneven, unity would have to be added to the result.

It would, however, we think, if the table is not to go beyond 10^n, be more convenient to employ a table of triangular numbers. The nth triangular number is $\frac{1}{2}n(n \times 1)$, and if we are provided with a table extending to 10^n we may multiply any two numbers not exceeding 10^n by means of the formula

$$ab = \tfrac{1}{2}(a-1)a + \tfrac{1}{2}b(b+1) - \tfrac{1}{2}(a-b-1)(a-b);$$

or, as we may write it

$$ab = \mathrm{T}(a-1) + \mathrm{T}(b) - \mathrm{T}(a-1-b),$$

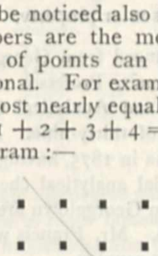
T(n) denoting the nth triangular number.[1]

Thus, to multiply two numbers we subtract unity from the larger number, and enter the table with the larger number so diminished, with the smaller number, and with the difference of these two numbers. For example, to multiply 5289 and 2156, we add the tabular results corresponding to 5288 and 2156, and subtract from this sum the tabular result corresponding to 3132.

The mode of construction of a table of triangular numbers is almost the simplest possible, the numbers being formed by adding to zero the natural numbers 1, 2, 3, ... *e.g.*,

$$0+1=1,\ 1+2=3,\ 3+3=6,\ 6+4=10,\ 10+5=15,$$

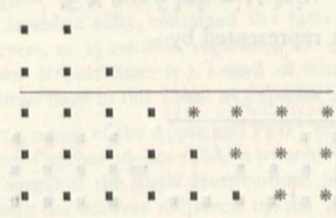
and so on. It may be noticed also that any two consecutive triangular numbers are the most nearly equal parts into which a square of points can be divided by a line parallel to the diagonal. For example, in the square of 16 points, the two most nearly equal triangular parts are, $1+2+3=6$, and $1+2+3+4=10$. This is shown in the following diagram :—



Whether the square contains an even or an uneven number of points, the diagonal, which is in the middle, has to be given to one of the two parts, which therefore necessarily differ by the number of points it contains. In the square n^2, the two consecutive triangular numbers which form it are $\frac{1}{2}n(n-1)$ and $\frac{1}{2}n(n+1)$, differing, as they should, by n, the number of points in the diagonal. Viewing the same matter from a slightly different point of view, we see that any two consecutive triangular numbers always make a square, *e.g.*,

$$1+3=4,\quad 3+6=9,\quad 6+10=16, \text{ \&c.}$$

It is interesting to exhibit by means of a diagram the manner in which the rectangle representing the product ab is derived from the three triangular numbers corresponding to $a-1, b, a-1-b$. As an example, the mode of formation of the product 8×4 is shown below, the triangular number corresponding to 7 being represented by dots and the triangular number corresponding to 4 by stars :—



The dots above the line form the triangular number corresponding to $7-4=3$.[1]

It is not suggested that the method just described by means of triangular numbers is comparable to that of quarter squares. It is certainly better to double the extent of the table and have but two entries. Still, it is interesting to note how readily a table of triangular numbers extending only to 10^n is available for the performance of multiplications of two n-figure numbers. So far as we know, only one extended table of triangular numbers has ever been published. This table, which gives the value of $\frac{1}{2}n(n+1)$ from $n=1$ to $n=20{,}000$, was published at the Hague, by E. de Joncourt, in 1762, under the title "De Natura et Præclaro Usu Simplicissimæ Speciei Numerorum Trigonalium." The book is a small and handsomely printed volume of 267 pages, 224 of which are occupied by the table.

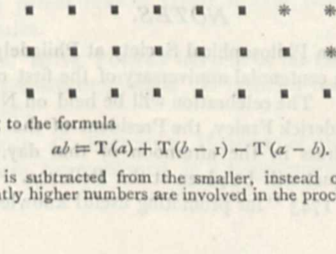
In tabulating quarter squares, the fraction $\frac{1}{4}$ which occurs when the square is uneven is omitted. If we denote by qsq n the tabulated quarter square of n, we have, therefore—

$$\text{qsq}\,(2n) = n^2,$$
$$\text{qsq}\,(2n+1) = n^2+n.$$

A table of quarter squares may be formed by adding to zero the numbers 1, 1, 2, 2, 3, 3, ... *e.g.* $0+1=1$, $1+1=2$, $2+2=4$, $4+2=6$, $6+3=9$, $9+3=12$, and so on. Its construction, therefore, is

[1] It is interesting to compare the two formulæ which involve half squares and triangular numbers respectively. In the former case we tabulate a discontinuous function, and in the use of the formula a unit has sometimes to be arbitrarily added. In the latter case we tabulate a continuous function, and the formula always holds good (the larger of the arguments being always reduced by unity). One formula depends on squares, n^2; the other on factorials of the second order, $n(n-1)$.

[1] We might of course also perform the multiplication thus :—



corresponding to the formula

$$ab = \mathrm{T}(a) + \mathrm{T}(b-1) - \mathrm{T}(a-b).$$

But if unity is subtracted from the smaller, instead of from the larger, number, slightly higher numbers are involved in the process.

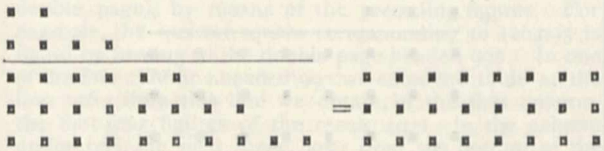
very similar to that of a table of triangular numbers, the only difference being that the added numbers 1, 2, 3, ... are each twice repeated. We may also regard the tabulated quarter squares as defined by this rule: The quarter square of n is equal, if n be even, to the sum of all the uneven numbers less than n, and, if n be uneven, to the sum of all the even numbers less than n. For evidently the series $1 + 3 + 5 + \ldots + (2n - 1) = n^2$, and the series $2 + 4 + 6 + \ldots + 2n = n^2 + n$.

By means of this definition of a quarter square we may exhibit the method of quarter squares diagrammatically as follows.

Taking as examples the products 8×3 and 7×4, we have

$$\text{qsq } 11 - \text{qsq } 5 = 8 \times 3,$$

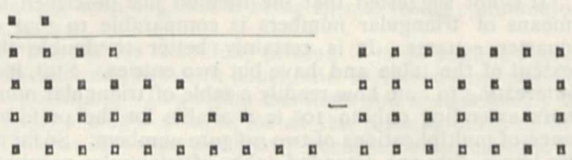
which may be represented by—



and

$$\text{qsq } 11 - \text{qsq } 3 = 7 \times 4,$$

which may be represented by—



The number of points in the extreme left-hand column of the difference of the quarter squares is always equal to the smaller of the numbers to be multiplied. If this number is uneven, there will be one middle line containing a number of points equal to the greater of the two numbers; the points in excess of this number are to be transferred from the line below the middle one to the line next above it, the excess from two lines below is to be transferred to make up the deficiency two lines above, and so on. If the smaller number is even, as in the second diagram, there are two middle lines differing from each other by two points; one point from the lower of these lines is to be transferred to the upper, three points from the line below the lower middle line to the line above the upper middle line, and so on.

It will be noticed that the tabulated quarter squares are, as it were, a species of triangular number in which the succeeding lines of points differ by two, instead of by one, as in ordinary triangular numbers (*i.e.* viewing the matter arithmetically, the quarter squares are derived alternately from the series $1 + 3 + 5 + \ldots$ and $2 + 4 + 6 + \ldots$, and the triangular numbers from the series $1 + 2 + 3 + \ldots$). It is the fact of the lines differing by two which enables us in all cases to adjust the points in the difference of two quarter squares so as to form a rectangle in the manner indicated above.

J. W. L. GLAISHER.

NOTES.

THE American Philosophical Society at Philadelphia is about to celebrate the centennial anniversary of the first occupation of its present hall. The celebration will be held on November 21. The Hon. Frederick Fraley, the President of the Society, will deliver an address in the afternoon of that day, and in the evening a banquet will be given at the Bellevue. The Society was founded in 1743 "for promoting useful knowledge."

ON November 16, the Naturwissenschaftlicher Verein of Bremen will celebrate the twenty-fifth anniversary of its foundation by an evening gathering and a banquet.

THE International Medical Congress will meet next year in Berlin, from August 4 to 10. Inquiries by intending visitors should be addressed to the General Secretary, Dr. Lassar, Karl Strasse, Berlin. The Congress will be divided into eighteen Sections, and the official languages will be German, English, and French.

THE Congress of the International Geodesic Association was opened in Paris, at the Foreign Office, on October 3. M. Spuller welcomed the delegates, who represented Austria, Belgium, Denmark, Spain, France, Greece, Hamburg, Hesse-Darmstadt, Italy, Holland, Prussia, Roumania, Servia, and Switzerland. M. Faye presided.

THE Ethnographic Congress, which held meetings of its various Sections every day last week in Paris, brought its proceedings to a close on Monday afternoon in one of the large halls of the College of France. It was decided that the Congress should hold its next meeting at Bucharest in the autumn of 1890.

THE next annual meeting of the Mineralogical Society will be held in the apartments of the Geological Society, Burlington House, London, on Tuesday, November 5, at 8 p.m.

THE Committee of the Sunday Lecture Society have decided that twenty-one lectures shall be given, during the winter, in St. George's Hall, Langham Place, on Sunday afternoons at 4 p.m., as in former years. The first lecture, on "The Origin and Uses of the Colours of Animals," with oxy-hydrogen lantern illustrations, will be delivered by Dr. Alfred Russel Wallace on October 20. This will be followed by lectures by Mr. John M. Robertson, Mr. Arthur Nicols, Mr. Chas. Cassal, Dr. Andrew Wilson, Prof. Percy Frankland, and Sir R. S. Ball.

ON Monday afternoon, about a quarter to 2 o'clock, a shock of earthquake was felt in Cornwall. It was accompanied by a loud underground noise like thunder, and was felt distinctly at Doubleborough, Boscastle, and Camelford. The earthquake is said to have been severe enough to shake houses, but no harm was done either to person or property.

THE death is announced from Georgetown, British Guiana, of Mr. E. E. H. Francis, Professor of Chemistry in Queen's College, Georgetown, and Analytical Chemist to the Government, at the early age of thirty-nine. Mr. Francis entered the service of British Guiana in 1875, having formerly been Professor of Chemistry and official analytical chemist in Trinidad. The two posts now vacant in Georgetown are said to be worth more than £700 per annum. Mr. Francis was a member of various learned Societies in this country.

AT the recent meeting of the Iron and Steel Institute, Mr. Frederick Siemens made some remarks during the discussion of Sir Lowthian Bell's paper on gaseous fuel, of which we gave an abstract last week. We draw attention to these remarks on account of the important influence Mr. Siemens's experiments and inventions have had on the application of gas for the heating of furnaces. Mr. Samson Fox had stated that he did not propose to use water gas alone for furnace purposes, but mixed with producer gas or other carburetted gas. Mr. Siemens said that a gas would thus be produced similar to that formed in the Siemens gas producer, which he had applied for a considerable time to furnace purposes. He had lately been experimenting with various gases for furnace use, and had come to the conclusion to divide gases generally into two classes, luminous and non-luminous. The latter, the class to which water gas belonged, could be advantageously used for heating by contact; as, for instance, in incandescent gas-lamps, for heating up refractory material to the temperature of incandescence, as the flame struck upon the material which had to give the light. But it would never do to heat high-temperature furnaces with a non-luminous gas, because it radiated very little light or heat, and, as he had proved some years ago, large furnaces must be heated by radiation to work economically and efficiently. Although it was proposed to mix water gas and producer gas, he thought that this would not be very easily effected, the one being supplied at a pressure, and the other without pressure. He had no doubt, however, that, if so mixed, they could be advantageously employed.

THE Report of the Manchester Technical School, presented at the annual meeting of the life members last week, is a very satisfactory document, and as a record of the year's work it is highly encouraging to all who are interested in the extension of technical instruction. The number of students has increased during the year from 2371 to 3328, and the amount paid in students' fees from £2970 to £3711, and the financial position is otherwise hopeful. The details of the work carried on in the school and in the recently-established spinning and weaving department, afford abundant evidence that the Council take a very comprehensive view of the sphere of technical education. The opening of this new department was the most important extension made during last year, and though at present the number of day students enrolled is small, there can be no doubt that, as the advantages it offers become better known, it will be more and more resorted to by those who wish to gain practical instruction in the chief textile industries in combination with the study of the scientific laws which regulate them. A considerable number of students are already attending the evening classes in the department. The Report refers to the proposal to establish a technical school in connection with the Whitworth Institute, and the prospects are considered promising by the Council. The negotiations between the Council and the Governors of the new institution will probably result in the foundation of a technical college on the scale, and with all the appliances, of the best schools of the kind in Germany and Switzerland. The growth of the school during the past six years is well shown in the following short table:—

Year ending July 31.	Individual students.	Tickets issued.	Fees. £ s. d.
1884	1429	2046	1259 12 5
1885	1995	2783	2010 10 5
1886	2299	3091	2241 11 8
1887	2529	3406	2931 10 4
1888	2871	3918	2970 1 8
1889	3328	4383	3711 15 0

THE grotto lately discovered near the caves of Adelsberg was said to be superior to these famous caverns. According to the Burgomaster of Adelsberg, who has written on the subject to the Vienna Correspondent of the *Times*, this is a mistake. The new grotto, he says, is very far from equalling the Adelsberg caves. It is a little more than a kilometre in length, and is believed to be a continuation of the Adelsberg caves. Its entrance is half an hour's walk from the latter, between the villages of Gressotak and Zagon. It is rich in stalactites, but most of these have the appearance of being covered with white chalk.

THE Boston Society of Natural History is at present much interested in a scheme for the establishment of natural history gardens in that city, and it has authorized its Council to proceed with the work as soon as the sum of 200,000 dollars shall have been raised for the purpose. Mr. A. Hyatt, Curator of the Society's Museum, is sanguine enough to write, in his latest Annual Report, that "it is perfectly feasible to establish a series of natural history gardens which shall co-operate with the Museum and other public work of the Society, and to form, perhaps, the most effective apparatus for public culture in natural history that has ever been planned before for any city in the world."

LAST week we noticed (p. 549) the work done, under the supervision of Prof. Giglioli, at the stations established in Italy for ornithological observations. Similar stations were created in Saxony, in accordance with the decisions of the Ornithological Congress held at Vienna in 1884, and we have now received the fourth annual Report relating to the results achieved by the observers. The Report, which is accompanied by a map, is by Dr. A. B. Meyer and Dr. F. Helm, and presents a great mass of information, clearly arranged. The first Report, issued in 1885, contained the facts noted in Saxony by 43 observers, at 35 stations, regarding 180 species of birds. In the present Report there is a record of what 122 observers at 111 stations have to tell about 213 species of birds.

A FULL report of the Apple and Pear Conference held at Chiswick from October 16-20, 1888, is presented in the tenth volume to the Journal of the Royal Horticultural Society. The report opens with the address delivered by the President, Sir Trevor Lawrence, after which come the following papers: apples for profit, by Mr. George Bunyard; fruit culture for profit in the open air, by Mr. W. Paul; dessert pears, by Mr. W. Wildsmith; on pruning, by Mr. Shirley Hibberd; canker in fruit trees, by Mr. Edmund Tonks; canker, its cause and cure, by Mr. James Douglas; enemies of the apple and pear, by Mr. J. Fraser; apples for Sussex, by Mr. J. Cheal; orchards in the West Midlands, by Mr. W. Coleman; apples and pears for Scotland, by Mr. Malcolm Dunn; cultivation in Jersey, by Mr. C. B. Saunders; production and distribution, by Mr. F. J. Baillie; compensation for orchard planting, by Mr. W. F. Bear; the railway difficulty, by Mr. D. Tallerman. The volume includes statistical and other information, compiled by Mr. A. F. Barron, as to the cultivation of apples in Great Britain and Ireland; and a descriptive catalogue, also by Mr. Barron, of apples exhibited in 1883 and 1888.

A SECOND edition of Prof. Lloyd Morgan's "Animal Biology" (Rivingtons) has been issued. The author has revised the text, substituted in several cases improved woodcuts, and added a brief classification of the types, and a glossary.

THE Royal Geographical Society has issued the sixth edition of the well-known "Hints to Travellers," edited for the Society by Mr. Douglas W. Freshfield and Captain W. J. L. Wharton, R.N. The work has been revised and enlarged.

M. A. VAYSSIÈRE has just completed the publication of his "Atlas d'Anatomie comparée des Invertébrés," in 60 quarto plates. This atlas, accompanied by concise and clear explanations, has been received with much favour in the zoological laboratories.

M. R. BLONDEL has published a pamphlet of 150 pages, with plates, on the odorous properties of the rose, and the methods used in industry for the extraction of its perfume.

MR. WILLIAM JORDAN has issued a pamphlet entitled "Instructions to Inventors as to obtaining Letters Patent and registering Trade Marks and Designs." Reference is made to the latest patent laws of Great Britain, our colonies, and foreign countries.

MR. R. H. PORTER has sent us his October catalogue of new and secondhand books. It includes many works of scientific interest.

PROF. G. POUCHET, Director of the Laboratory of Concarneau, has published, in a large quarto volume of eighty-one pages, with plates, an account of experiments made by him under the auspices of the Municipal Council of Paris, on the currents of the North Atlantic. The work deals more particularly with the Gulf Stream, and with the details of the experiments made on board the *Hirondelle* with the assistance of the Prince of Monaco, but also takes into consideration the results of investigators from the earliest times. The experiments refute the idea that the French coast is warmed by the Gulf Stream; M. Pouchet states that they show clearly that, at least in summer, no surface-current reaches France from the south-west, but that, on the contrary, there is a current from the west and north-west.

THE following details about the Tomsk University have been published. The buildings constitute the finest edifice in the town, and are situated in the middle of a magnificent park. When the staff is complete there will be twenty-five Chairs—thirteen ordinary, eleven extraordinary, and one for orthodox theology. There will also be librarians, teachers of music, and surgeons and assistant-surgeons. There are already seventy-two students, who pay about 12 or 13 roubles a month for lodging, books, and attendance, and it is expected that this number will be largely increased before the end of the year.

THE Pilot Chart of the North Atlantic Ocean for the month of September shows that the most important storm during the month of August was one first reported about San Domingo on the 19th, whence it moved north-westward over the Bahamas and afterwards recurved and followed the course of the Gulf Stream. During the first half of the month the pressure along the Atlantic States was persistently high, and the tracks of all storms from the continent lay well to the northward of this area of high barometer, moving mostly beyond the region of observation. No storm can be traced all the way from the American continent to the British Isles, although several originated in mid-ocean and moved in an east-north-east direction. A severe tornado was reported off Cienfuegos, Cuba, on August 4. Ice was reported in great quantities about the Straits of Belle Isle, but very little off the Grand Banks.

METEOROLOGICAL science will be much enriched by the recent contributions of the Danish observers in Greenland during 1882–83 ("Expédition Danoise," vol. ii., part 2, Copenhagen, 1889). The principal station was at Godthaab, on the west coast of Greenland, in latitude about 64°, where the observations were made under the direction of M. Adam Paulsen. A large series of observations of temperature was made and the results are given in tables, as well as represented by curves. The temperature was taken every hour during twelve months, and the mean temperatures at each hour for each month are given in the tables. As might be expected, the greatest variation occurs in August—namely, from + 3°·5 C. at 3 a.m. to + 7°·2 at 2 p.m. The minimum variation is in February, from − 15°·4 to − 15°·7. During the summer months the maximum occurs about 1 p.m., and in winter about 2 p.m. Similar results are recorded for the stations at Reykjavik, in lat. 64° and Stykkisholm, in lat. 65°. The maximum monthly mean temperature occurs in July, and the minimum in February. Observations were also made of the direction, force, and velocity of the wind, form and direction of clouds, temperature of the soil, &c., full details of which are given in the report. An appendix contains the results of various meteorological observations in the Kara Sea in 1882-83, and at Nanortalik and D'Angmagsalik. Observations of auroræ at the two latter stations and at Godthaab also form part of the appendix. It is greatly to be regretted that the spectroscope was not employed in the auroral observations.

THE additions to the Zoological Society's Gardens during the past week include a Mealy Amazon (*Chrysotis farinosa*) from South America, presented by the Hon. and Rev. F. G. Dutton; two Cape Crowned Cranes (*Balearica chrysopelargus*) from South Africa, presented by the Hon. Mrs. Barker; seven Common Slowworms (*Anguis fragilis*), British, presented by Miss Alice Leonora Selly; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. J. Watkins; a Long-nosed Crocodile (*Crocodilus cataphractus*) from West Africa, presented by Mr. John R. Holmes; a Royal Python (*Python regius*) from West Africa, deposited; four Common Rheas (*Rhea americana*) from Holland, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *OCTOBER* 13–19.

(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 October 13

Sun rises, 6h. 23m.; souths, 11h. 46m. 11·5s.; daily decrease of southing, 14·0s.; sets, 17h. 10m.: right asc. on meridian, 13h. 15·1m.; decl. 7° 57′ S. Sidereal Time at Sunset, 18h. 40m.

Moon (at Last Quarter October 17, 1h.) rises, 19h. 7m.*; souths, 2h. 57m.; sets, 11h. 0m.: right asc. on meridian, 4h. 24·4m.; decl. 19° 11′ N.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° ′
Mercury..	7 2	...	12 4	...	17 6	...	13 32·8	...	12 4 S.
Venus.....	3 25	...	9 56	...	16 27	...	11 24·3	...	5 19 N.
Mars......	2 45	...	9 29	...	16 13	...	10 57·6	...	8 5 N.
Jupiter....	12 47	...	16 39	...	20 31	...	18 9·0	...	23 30 S.
Saturn....	1 36	...	8 44	...	15 52	...	10 12·6	...	12 27 N.
Uranus...	6 32	...	11 54	...	17 16	...	13 23·1	...	8 8 S.
Neptune..	18 54*	...	2 43	..	10 32	..	4 10·3	...	19 21 N.

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

Oct.		h.		
16	...	0	...	Mercury in inferior conjunction with the Sun.
16	...	13	...	Venus at least distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·5	... 81 17 N.	... Oct. 15,	2 45 *m*
λ Tauri...	3 54·5	... 12 11 N.	... ,, 16,	21 36 *m*
ζ Geminorum ...	6 57·5	... 20 44 N.	... ,, 13,	23 0 *M*
			,, 19,	2 0 *m*
U Coronæ	15 13·7	... 32 3 N.	... ,, 17,	23 5 *m*
β Lyræ...	18 46·0	... 33 14 N.	... ,, 16,	22 30 *M*
U Aquilæ	19 23·4	... 7 16 S.	... ,, 19,	19 0 *M*
η Aquilæ	19 46·8	... 0 43 N.	... ,, 13,	22 0 *M*
T Vulpeculæ ...	20 46·8	... 27 1 N.	... ,, 19,	19 0 *M*
δ Cephei	22 25·1	... 57 51 N.	... ,, 16,	21 0 *m*
S Aquarii	22 51·2	... 20 56 S.	... ,, 14,	*M*

M signifies maximum: *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Andromedæ ...	26	... 43 N.	... Slow; faint.
,, ξ Ceti	31	... 9 N.	... Slow; trained.
,, ν Arietis	41	... 20 N.	... Swift.
,, κ Cephei	307	... 77 N.	... Slow; faint.

THE LIFE-WORK OF A CHEMIST.[1]

IN asking myself what subject I could bring before you on the present occasion, I thought I could not do better than point out by one example what a chemist may do for mankind. And in choosing this theme for my discourse I found myself in no want of material, for amongst the various aspects of scientific activity there is surely none which, whether in its most recondite forms or in those most easily understood, have done more to benefit humanity than those which have their origin in my own special study of chemistry. I desired to show what one chemist may accomplish, a man devoted heart and soul to the investigation of Nature, a type of the ideal man of science—whose example

[1] An Address delivered to the members of the Birmingham and Midland Institute, in the Town Hall, Birmingham, on October 7, 1889, by Sir Henry E. Roscoe, M.P., D.C.L., LL.D., F.R.S., President.

may stimulate even the feeblest amongst us to walk in his footsteps if only for a short distance, whose life is a consistent endeavour to seek after truth if haply he may find it, whose watchwords are simplicity, faithfulness, and industry, and whose sole ambition is to succeed in widening the pathway of knowledge so that following generations of wayfarers may find their journeys lightened and their dangers lessened.

Such men are not uncommon amongst the ranks of distinguished chemists. I might have chosen as an example the life and labours of your sometime townsman, Joseph Priestley, had not this theme been already treated by Prof. Huxley, in a manner I cannot approach, on the occasion of the inauguration of the statue which stands hard by. To-day, however, I will select another name, that of a man still living, the great French chemist—Pasteur.

As a chemist Pasteur began life, as a chemist he is ending it. For although, as I shall hope to point out, his most important researches have entered upon fields hitherto tilled, with but scanty success, by the biologist, yet in his hands, by the application of chemical methods, they have yielded a most bountiful harvest of new facts of essential service to the well-being and progress of the human race.

And after all the first and obvious endeavour of every cultivator of science ought to be to render service of this kind. For although it is foolish and shortsighted to decry the pursuit of any form of scientific study because it may be as yet far removed from practical application to the wants of man, and although such studies may be of great value as an incentive to intellectual activity, yet the statement is so evident as to almost amount to a truism, that discoveries which give us the power of rescuing a population from starvation, or which tend to diminish the ills that flesh, whether of man or beast, is heir to, must deservedly attract more attention and create a more general interest than others having so far no direct bearing on the welfare of the race.

"There is no greater charm," says Pasteur himself, "for the investigator than to make new discoveries, but his pleasure is more than doubled when he sees that they find direct application in practical life." To make discoveries capable of such an application has been the good fortune—by which I mean the just reward—of Pasteur. How he made them is the lesson which I desire this evening to teach. I wish to show that these discoveries, culminating as the latest and perhaps the most remarkable of all, in that of a cure for the dreaded and most fearful of all fearful maladies, hydrophobia, have not been, in the words of Priestley, "lucky haphazardings," but the outcome of patient and long-continued investigation. This latest result is, as I shall prove to you, not an isolated case of a happy chance, but simply the last link in a long chain of discoveries, each one of which has followed the other in logical sequence, each one bound to the other by ties which exhibit the life-work of the discoverer as one consequent whole. In order, however, to understand the end we must begin at the beginning, and ask ourselves what was the nature of the training of hand, eye, and brain which enabled Pasteur to wrest from Nature secret processes of disease the discovery of which had hitherto baffled all the efforts of biologists. What was the power by virtue of which he succeeded when all others had failed, how was he able to trace the causes and point out remedies for the hitherto unaccountable changes and sicknesses which beer and wine undergo? What means did he adopt to cure the fatal silkworm disease, the existence of which in the south of France in one year cost that country more than one hundred millions of francs? Or how did he arrive at a method for exterminating a plague known as fowl cholera, or that of the deadly cattle disease, anthrax, or splenic fever, which has killed millions of cattle, and is the fatal woolsorters' disease in man? And last but not least how did he gain an insight into the working of that most mysterious of all poisons, the virus of hydrophobia?

To do more than point out the spirit which has guided Pasteur in all his work, and to give an idea of the nature of that work in a few examples, I cannot attempt in the time at my disposal. Of the magnitude and far-reaching character of that work we may form a notion, when we remember that it is to Pasteur that we owe the foundation of the science of bacteriology, a science treating of the ways and means of those minute organisms called microbes, upon whose behaviour the very life, not only of the animal, but perhaps also of the vegetable, world depends—a science which bids fair to revolutionize both the theory and practice of medicine, a science which has already, in the hands of Sir Joseph Lister, given rise to a new and beneficent application in the discovery of antiseptic surgery

The whole secret of Pasteur's success may be summed up in a few words. It consisted in the application of the exact methods of physical and chemical research to problems which had hitherto been attacked by other less precise and less systematic methods. His early researches were of a purely chemical nature. It is now nearly forty years ago since he published his first investigation. But this pointed out the character of the man, and indicated the lines upon which all his subsequent work was laid.

Of all the marvellous and far-reaching discoveries of modern chemistry, perhaps the most interesting and important is that of the existence of compounds which, whilst possessing an identical composition—that is, made up of the same elements in the same proportions—are absolutely different substances judged of by their properties. The first instance made known to us of such isomeric bodies, as they are termed by the chemist, was that pointed out by the great Swedish chemist Berzelius. He showed that the tartaric acid of wine-lees possesses precisely the same composition as a rare acid having quite different properties and occasionally found in the tartar deposited from wine grown in certain districts in the Vosges. Berzelius simply noted this singular fact, and did not attempt to explain it. Later on, Biot observed that not only do these two acids differ in their chemical behaviour, but likewise in their physical properties, inasmuch as the one (the common acid) possessed the power of deviating the plane of a polarized ray of light to the right, whereas the rare acid has no such rotatory power. It was reserved, however, for Pasteur to give the explanation of this singular and at that time unique phenomenon, for he proved that the optically inactive acid is made up of two compounds, each possessing the same composition, but differing in optical properties. The one turned out to be the ordinary dextro-rotatory tartaric acid; the other a new acid which rotates the plane of polarization to the left to an equal degree. As indicating the germ of his subsequent researches, it is interesting here to note that Pasteur proved that these two acids can be separated from one another by a process of fermentation, started by a mere trace of a special form of mould. The common acid is thus first decomposed, so that if the process be carried on for a certain time only the rarer lævo-rotatory acid remains.

Investigations on the connection between crystalline form, chemical composition, and optical properties, occupied Pasteur for the next seven years, and their results—which seem simple enough when viewed from the vantage-ground of accomplished fact—were attainable solely by dint of self-sacrificing labours such as only perhaps those who have themselves walked in these enticing and yet often bewildering paths can fully appreciate, and by attention to minute detail as well as to broad principles to an extent which none can surpass and few can equal. A knowledge of the action of the mould in the changes it effects on tartaric acid led Pasteur to investigate that *bête noire* of chemists, the process of fermentation. The researches thus inaugurated in 1857 not only threw a new and vivid light on these most complicated of chemical changes, and pointed the way to scientific improvements in brewing and wine-making of the greatest possible value, but were the stepping-stones to those higher generalizations which lie at the foundation of the science of bacteriology, carrying in their train the revolutions in modern medicine and surgery to which I have referred.

The history of the various theories from early times until our own day which have been proposed to account for the fact of the change of sugar into alcohol, or that of alcohol into vinegar, under certain conditions, a fact known to the oldest and even the most uncivilized of races, is one of the most interesting chapters in the whole range of chemical literature, but, however enticing, is one into which I cannot now enter. Suffice it here to say that it was Pasteur who brought light out of darkness by explaining conflicting facts and by overturning false hypotheses. And this was done by careful experiment, and by bringing to bear on the subject an intelligence trained in exact methods and in unerring observation, coupled with the employment of the microscope and the other aids of modern research.

What now did Pasteur accomplish? In the first place he proved that the changes occurring in each of the various processes of fermentation are due to the presence and growth of a minute organism called the ferment. Exclude all traces of these ferments, and no change occurs. Brewers' wort thus preserved remains for years unaltered. Milk and other complex liquids do not turn sour even on exposure to pure air, provided these infinitely small organisms are excluded. But introduce even the smallest trace of these microscopic beings, and the peculiar changes which they alone can bring about at once begin. A few cells of the yeast plant set up the vinous fermentation in a sugar solution. This is clearly stated by Pasteur as follows:—"My decided opinion," he says, "on the nature of alcoholic fermentation is the following. The chemical act of fermentation is essentially a correlative phenomenon of a vital act beginning and ending with it. I think that there is never any alcoholic fermentation without there being at the same time organization, development, multiplication of globules, or the continued consecutive life of globules already formed."

Add on a needle's point a trace of the peculiar growth which accompanies the acetous fermentation, and the sound beer or wine in a short time becomes vinegar. Place ever so small a quantity of the organism of the lactic fermentation in your sweet milk, which may have been preserved fresh for years in absence of such organisms, and your milk turns sour. But still more, the organism (yeast) which brings about the alcoholic fermentation will not give rise to the acetous, and *vice versâ*, so that each peculiar chemical change is brought about by the vital action of a peculiar organism. In its absence the change cannot occur; in its presence only that change can take place.

Here again we may ask, as Pasteur did, Why does beer or wine become sour when exposed to ordinary air? And the answer to this question was given by him in no uncertain tone in one of the most remarkable and most important of modern experimental researches. Milk and beer which have become sour on standing in the air contain living micro-organisms which did not exist in the original sound fluids. Where did these organisms originate? Are they or their germs contained in the air, or are these minute beings formed by a process of spontaneous generation from material not endowed with life?

A controversy as to the truth or falsity of the theory of spontaneous generation was waged with spirit on both sides, but in the end Pasteur came off victorious, for by a series of the most delicate and convincing of experiments he proved the existence of micro-organic forms and their spores—or seeds—in the air, and showed that whilst unpurified air was capable of setting up fermentative changes of various kinds, the same air freed from germs could not give rise to these changes. Keep away the special germ which is the incentive to the pathological change, and that change cannot occur. In the interior of the grape, in the healthy blood, no such organisms, no such germs exist; puncture the grape or wound the animal body, and the germs floating in the air settle on the grape-juice or on the wounded tissue, and the processes of change, whether fermentative or putrefactive, set in with all their attendant symptoms. But crush the grape or wound the animal under conditions which either preclude the presence or destroy the life of the floating germ, and again no such change occurs; the grape-juice remains sweet, the wound clean.

I have said that every peculiar fermentative change is accompanied by the presence of a special ferment. This most important conclusion has only been arrived at as the result of careful experimental inquiry. How was this effected? By the artificial cultivation of these organisms. Just as the botanist or gardener picks out from a multitude of wild plants the special one which he wishes to propagate, and planting it in ground favourable to its growth, obtains fresh crops of the special plant he has chosen, so the bacteriologist can by a careful process of selection obtain what is termed a pure cultivation of any desired organism. Having obtained such a pure cultivation, the next step is to ascertain what are the distinctive properties of that special organism; what characteristic changes does it bring about in material suitable for its growth. This having been determined, and a foundation for the science having thus been laid, it is not difficult to apply these principles to practice, and the first application made by Pasteur was to the study of the diseases of beer and wine.

In September 1871, Pasteur visited one of the large London breweries, in which the use of the microscope was then unknown. A single glance at the condition of the yeast instantly told its tale, and enabled him to explain to the brewers the cause of the serious state of things by which frequently as much as 20 per cent. of their product was returned on their hands as unsaleable—this being that this yeast contained foreign or unhealthy organisms. And just as pure yeast is the cause of the necessary conversion of wort into beer, so these strange forms which differ morphologically from yeast, and whose presence can therefore be distinctly ascertained, are the cause of acidity, ropiness, turbidity, and other diseases which render the beer undrinkable. It is no exaggeration to say that, whereas before Pasteur's researches the microscope was practically unknown in the brewhouse, it has now become as common as the thermometer or the saccharimeter, and by its help and by the interpretations we can place upon its revelations through Pasteur's teaching, yeast—of all brewers' materials the least open to rough and ready practical discernment—becomes easy of valuation as to its purity or impurity, its vigour or weakness, and, therefore, its behaviour during fermentation. Thus, while in former days the most costly materials were ever liable to be ruined by disease organisms unconsciously introduced into them with the yeast, at the present day the possibilities of any such vast pecuniary disasters become easily avertable.

Of all industries, brewing is perhaps the one which demands the most stringent care in regard to complete and absolute cleanliness. The brewers' materials, products, and by-products, are so putrescible, there is always so vast an abundance of disease-organisms in the brewery air, that the minutest amounts of these waste products lying about in vessels or pipes transform these places into perfect nests for the propagation of these micro-organisms, whence, transferred into the brewings, they inevitably ruin them, however carefully and scientifically prepared in other respects. Without the microscope, any breach of discipline in the way of the supreme cleanliness necessary is impossible of detection; with it we can track down the micro-organisms to their source, whether it be in uncleanly plant, in impurity of materials, or in carelessness of manipulation.

Among the more direct applications of Pasteur's researches, the so-called Pasteurization of beer claims a place. Pasteur showed that temperatures well below the boiling-point sufficed for destroying the disease organisms in alcoholic fluids, and, based on these results, enormous quantities of low-fermentation beers are annually submitted to these temperatures, and thus escape the changes otherwise incident to the micro-organisms which have succumbed to the treatment. This process is, however, for several intricate reasons, not suited for English beers, but if we cannot keep our beers by submitting them to high temperatures, we can foretell to a nicety how they will keep by artificially forcing on those changes which would occur more slowly during storage. The application of a suitable temperature, the exclusion of outside contamination, a microscopic examination of the "forced" beer, and the knowledge which we owe to Pasteur of what the microscopic aspect means, suffice to make each brewing foretell its own future history, and thus suffice to avert the otherwise inevitable risks incident to the storage and export of beer, the stability of which is unknown.

Brewing has thus become a series of precise and definite operations, capable of control at every point. Instead of depending—as it had to depend—on intuition and experience handed down in secrecy from father to son, it now depends upon care, forethought, and the soundness of the brewer's scientific training. This change in the nature of the brewer's operations, and in the persons who govern them, is primarily due to Pasteur. Other men have done much to carry on his work, but it is to his example of ceaseless patience, and to his example of freely publishing to the world all the results of his work, that the brewers of all countries are indebted for the connection of each phenomenon with a controllable cause, and for thus emancipating their industry from empiricism and quackery.

Much the same story has to be told about Pasteur's investigation of wine and its diseases. As with the brewer, so with the wine-grower Pasteur has pointed out the causes of his troubles, and, the causes having been ascertained, the remedies soon followed, and the practical value of these researches to the trade of France and other wine-producing countries has been enormous.

The next labour of our scientific Hercules was of a different kind, but of a no less interesting or important character. The south of France is a great silk-producing district. In 1853 the value of the raw silk was represented by a sum of some five millions sterling, and up to that date the revenue from this source had been greatly augmenting. Suddenly this tide of prosperity turned, a terrible plague broke out amongst the silkworms, and in 1865 so general had the disease become that the total production of French silk did not reach one million, and the consequent poverty and suffering endured in these provinces became appalling. Every conceivable means was tried to overcome the disease, but all in vain. The population and the Government of France—for the evil was a national one—were at their wits' end, and a complete collapse of one of the most important French industries seemed inevitable. Under these circumstances the great chemist Dumas, who was born at Alais, in the centre of one of the districts most seriously affected, urged his friend Pasteur to undertake an investigation of the subject. Pasteur, who at this time had never seen a silkworm, naturally felt diffident about attempting so difficult a task, but at last, at Dumas' renewed entreaty, he consented, and in June 1865 betook himself to the south for the purpose of studying the disease on the spot. His previous training here again stood him in good stead, and in September 1865 he was able to communicate to the Academy of Sciences results of observation and experiment which, striking at the root of the evil, pointed the way to the means of securing immunity from the dreaded plague. This paper was freely criticized. Here, it was said, was a chemist who, quitting his proper sphere, had the hardihood to lay down rules for the guidance of the physician and biologist in fields specially their own. Why should his proposals be more successful than all the other nostrums which had already so egregiously failed?

In order to appreciate the difficulties which met Pasteur in this inquiry, and to understand how wonderfully he overcame them, I must very shortly describe the nature of this disease, which is termed *pébrine*, from the black spots which cover the silkworm. It declares itself by the stunted and unequal growth of the worms, by their torpidity, and by their fastidiousness as to food, and by their premature death.

Before Pasteur went to Alais the presence of certain microscopic corpuscles had been noticed in the blood and in all the tissues of the diseased caterpillar, and even in the eggs from which such worms were hatched. These micro-organisms often fill the whole of the silk organs of the insect, which in a healthy condition contain the clear viscous liquid from which the silk is made. Such worms are of course valueless. Still this knowledge did not suffice, for eggs apparently healthy gave rise to stricken worms incapable of producing silk, whilst again other worms distinctly diseased yielded normal cocoons. These difficulties, which had proved too much for previous observers, were fully explained by Pasteur. "The germs of these organisms," said he, "which are so minute, may be present in the egg and even in the young worms, and yet baffle the most careful search. They develop with the growth of the worm, and in the chrysalis they are more easily seen. The moth derived from a diseased worm invariably contains these corpuscles, and is incapable of breeding healthy progeny."

This moth-test is the one adopted by Pasteur, and it is an infallible one. If the female moth is stricken, then her eggs—even though they show no visible sign of disease—will produce sick worms. If in the moth no micrococci are seen, then her immediate progeny at any rate will be sound and free from inherited taint, and will always produce the normal quantity of silk. But this is not all. Pasteur found that healthy worms can be readily infected by contact with diseased ones, or through germs contained in the dust of the rooms in which the worms are fed. Worms thus infected, but free from inherited taint, can, however, as stated, spin normal cocoons, but—and this is the important point—the moths which such chrysalides yield invariably produce diseased eggs. This explains the anomalies previously noticed. The silkworms which die without spinning are those in which the disease is hereditary, viz. those born from a diseased mother. Worms from sound eggs which contract the disease during their life-time always spin their silk, but they give rise to a stricken moth, the worms from which do not reach maturity and furnish no silk.

As I have said, these results were but coldly received. It was hard to make those engaged in rearing the worms believe in the efficacy of the proposed cure. Then, seeing this state of things, Pasteur determined to take upon himself the *rôle* of a prophet. Having in 1866 carefully examined a considerable number of the moths which had laid eggs intended for incubation, he wrote down a prediction of what would happen in the following year with respect to the worms hatched from these eggs. In due course, after the worms from a mixed batch of healthy and unhealthy eggs had spun, the sealed letter was opened and read, and the prediction compared with the actual result, when it was found that in twelve out of fourteen cases there was absolute conformity between the prediction and the observation, for twelve hatchings were predicted to turn out diseased, and this proved to be the case. Now all these "educations" were believed to be healthy by the cultivators, but Pasteur foretold that they would turn out to be diseased by the application of the moth-test in the previous year. The other parcels of eggs were pronounced by Pasteur to be sound, because they were laid by healthy moths containing none of the micrococci, and both these yielded a healthy crop. So successful a prophecy could not but gain the belief of the most obtuse of cultivators, and we are not surprised to learn that Pasteur's test was soon generally applied, and that the consequence has been a return of prosperity to districts in which thousands of homes had been desolated by a terrible scourge.

I must now ask you to accompany me to another and a new field of Pasteur's labours, which, perhaps more than his others, claims your sympathy and will enlist your admiration, because they have opened out to us the confident hope of at least obtaining an insight into some of the hidden causes and therefore to the possible prevention of disease.

In the first place, I must recall to your remembrance that most infectious diseases seldom if ever recur, and that even a slight attack renders the subject of it proof against a second one. Hence inoculation from a mild case of small-pox was for a time practiced, but this too often brought about a serious if not fatal attack of the malady, and the step taken by Jenner of vaccinating, that is of replacing for the serious disease a slight one which nevertheless is sufficient protection against small-pox infection, was one of the highest importance. But Jenner's great discovery has up to recent years remained an isolated one, for it led to no general method for the preventive treatment of other maladies, nor had any explanation been offered of its mode of action. It is to Pasteur that science is indebted for the generalization of Jenner's method, and for an explanation which bids fair to render possible the preventive treatment of many—if not of all—infectious diseases. It was his experience, based upon his researches on fermentation, that led to a knowledge of the nature of the poison of such diseases, and showed the possibility of so attenuating or weakening the virus as to furnish a general method of protective or preventive inoculation.

I have already pointed out how a pure cultivation of a microbe can be effected. Just as the production of pure alcohol depends on the presence of the pure yeast, so special diseases are dependent on the presence of certain definite organisms which can be artificially cultivated, and which give rise to the special malady. Can we now by any system of artificial cultivation so modify or weaken the virus of a given microbe as to render it possible to inoculate a modified virus which, whilst it is without danger to life, is still capable of acting as a preventive to further attack? This is the question which Pasteur set himself to solve, nor was the task by any means an apparently hopeless one. He had not only the case of Jennerian vaccination before him, but also the well-known modifications which cultivation can bring about in plants. The first instance in which Pasteur succeeded in effecting this weakening of the poison was in that of a fatal disease to which poultry in France are very liable, called chicken cholera. Like many other maladies, this is caused by the presence of a micro-organism found in the blood and tissues of the stricken fowl. One drop of this blood brought under the skin of a healthy chicken kills it, and the same microbe is found throughout its body. And if a pure culture of these microbes be made, that culture—even after a series of generations—is as deadly a poison as the original blood. Now comes the discovery. If these cultures be kept at a suitable temperature for some weeks exposed to pure air, and the poisonous properties tested from time to time, the poison is found gradually to become less powerful, so that after the lapse of two months a dose which had formerly proved fatal now does not disturb in the slightest the apparent health of the fowl. But now let us inoculate a chicken with this weakened virus. It suffers a slight illness, but soon recovers. Next let us give it a dose of the undiluted poison, and, as a control, let us try the action of the same on an unprotected bird. What is the result? Why, that the first chicken remains unaffected, whilst the second bird dies. The inoculation has rendered it exempt from the disease, and this has been proved by Pasteur to be true in thousands of cases, so that, whereas the death-rate in certain districts amongst fowls before the adoption of Pasteur's inoculation method was 10 per cent., after its general adoption it has diminished to less than 1 per cent.

We can scarcely value too highly this discovery, for it proves that the poisonous nature of the microbe is not unalterable, but that it can be artificially modified and reduced, and thus an explanation is given of the fact that in an epidemic the virus may either be preserved or become exhausted according to the conditions to which it is subjected. We have here to do with a case similar to that of Jenner's vaccine, except that here the relation between the weak and the strong poison has become known to us, whilst in Jenner's case it has lain concealed. This, then, is the first triumph of experimental inquiry into the cause and prevention of microbic disease, and this method of attenuation is of great importance, because, as we shall see, it is not confined to the case of chicken cholera, but is applicable to other diseases.

And next I will speak of one which is a fatal scourge to cattle, and is not unfrequently transmitted to man. It is called anthrax, splenic fever, or woolsorters' disease. This plague, which has proved fatal to millions of cattle, is also due to a microbe, which can be cultivated like the rest, and the virus of which can also be weakened or attenuated by a distinct treatment which I will not here further specify. Now, what is the effect of inoculating cattle or sheep with this weakened poison? Does it act as a preventive? That the answer is in the affirmative was proved by Pasteur by a convincing experiment. Five-and-twenty sheep, chosen promiscuously out of a flock of fifty, were thus inoculated with the weak virus, then after a time all the fifty were treated with the strong poison. The first half remained healthy, all the others died of anthrax. Since the discovery of this method, no fewer than 1,700,000 sheep and about 90,000 oxen have thus been inoculated, and last year 269,599 sheep and 34,464 oxen were treated. The mortality which, before the introduction of the preventive treatment, was in the case of sheep 10 per cent., was, after the adoption of the method, reduced to less than 1 per cent. So that now the farmers in the stricken districts have all adopted the process, and agricultural insurance societies make the preventive inoculation a *sine quâ non* for insuring cattle in those districts. This is, however, not the end of this part of my story, for Pasteur can not only thus render the anthrax poison harmless, but he has taught us how to bring the highly virulent poison back again from the harmless form. This may go to explain the varying strength of an attack of infectious disease, one case being severe and another but slight, due to the weakening or otherwise of the virus of the active microbe.

Last, but not least, I must refer to the most remarkable of all Pasteur's researches, that on rabies and hydrophobia. Previous to the year 1880, when Pasteur began his study of this disease, next to nothing was known about its nature. It was invested with the mysterious horror which often accompanies the working of secret poisons, and the horror was rendered greater owing to the fact that the development of the poison brought in by the bite or by the lick of a mad dog might be deferred for months, and that, if after that length of time the symptoms once make their appearance, a painful death was inevitable. We knew indeed that the virus was contained in the dog's saliva, but experiments made upon the inoculation of the saliva had led to no definite results, and we were entirely in the dark as to the action of the poison until Pasteur's investigation. To begin with, he came to the conclusion that the disease was one localized in the nerve-centres, and to the nerve-centres he therefore looked as the seat of the virus or of the microbe. And he proved by experiment that this is the case, for a portion of the matter of the spinal column of a rabid dog, when injected into a healthy one, causes rabies with a much greater degree of certainty and rapidity than does the injection of the saliva. Here, then, we have one step in advance. The disease is one of the nerve-centres, and, therefore, it only exhibits itself when the nerve-centres are attacked. And this goes to explain the varying times of incubation which the attack exhibits. The virus has to travel up the spinal cord before the symptoms can manifest themselves, and the length of time taken over that journey depends on many circumstances. If this be so, the period of incubation must be lessened if the virus is at once introduced into the nerve-centres. This was also proved to be the case, for dogs inoculated under the *dura mater* invariably became rabid within a period rarely exceeding eighteen days.

Next came the question, Can this virus be weakened, as has been proved possible with the former poisons? The difficulty in this case was greater, inasmuch as all attempts to isolate or to cultivate the special microbe of rabies outside the animal body had failed. But Pasteur's energy and foresight overcame this difficulty, and a method was discovered by which this terrible poison can so far be weakened as to lose its virulent character, but yet remain potent enough, like the cases already quoted, to act as a preventive; and dogs which had thus been inoculated were proved to be so perfectly protected, that they might be bitten with impunity by mad dogs, or inoculated harmlessly with the most powerful rabic virus.

But yet another step. Would the preventive action of the weakened virus hold good when it is inoculated even after the bite? If so, it might be thus possible to save the lives of persons bitten by mad dogs. Well, experiment has also proved this to be true, for a number of dogs were bitten by mad ones, or were inoculated under the skin with rabic virus; of these some were subjected to the preventive cure and others not thus treated. Of the first or protected series not one became mad, of the other, or unprotected dogs, a large number died with all the characteristic symptoms of the disease. But it was one thing to thus experiment upon dogs, and quite another thing, as you may well imagine, to subject human beings to so novel and perhaps dangerous a treatment. Nevertheless, Pasteur was bold enough to take this necessary step, and by so doing has earned the gratitude of the human race.

In front of the Pasteur Institute in Paris stands a statue worked with consummate skill in bronze. It represents a French shepherd boy engaged in a death struggle with a mad dog which had been worrying his sheep. With his bare hands, and with no weapon save his wooden *sabot*, the boy was successful in the combat. He killed the dog, but was horribly bitten in the fight. The group represents no mythical struggle; the actual event took place in October 1885; and this boy, Jupille, was the second person to undergo the anti-rabic treatment, which proved perfectly successful, for he remained perfectly healthy, and his heroic deed and its consequences have become historic. "*C'est le premier pas qui coute*," and as soon as the first man had been successfully treated, others similarly situated gladly availed themselves of Pasteur's generous offer to treat them gratuitously. And as soon as this cure became generally known, crowds of persons of all ages, stations, and countries, all bitten by rabid animals, visited every day Pasteur's laboratory in the Rue d'Ulm, which, from being one in which quiet scientific researches were carried on, came to resemble the out-patient department of a great hospital. There I saw the French peasant, the Russian *moujik* (suffering from the terrible bites of rabid wolves), the swarthy Arab, the English policeman, with women too and children of every age, in all perhaps a hundred patients. All were there undergoing the careful and kindly treatment, which was to insure them against a horrible death. Such a sight will not be easily forgotten. By degrees this wonderful cure for so deadly a disease attracted the attention of men of science throughout the civilized world. The French nation raised a monument to the discoverer better than any statue, in the shape of the "Pasteur Institute"—an institution devoted to carrying out in practice this anti-rabic treatment, with laboratories and every other convenience for extending by research our knowledge of the preventive treatment of infectious disease.

For, be it remembered, we are only at the beginning of these things, and what has been done is only an inkling of what is to come. Since 1885, twenty anti-rabic institutions have been established in various parts of the world, including Naples, Palermo, Odessa, St. Petersburg, Constantinople, Rio Janeiro, Buenos Ayres, and Havannah.

We in England have also taken our share, though a small one, in this work. In 1885 I moved in the House of Commons for a Committee to investigate and report on Pasteur's anti-rabic method of treatment. This Committee consisted of trusted and well-known English men of science and physicians—Sir James Paget, Sir Joseph Lister, Drs. Burdon Sanderson, Lauder Brunton, Quain, Fleming, and myself, with Prof. Victor Horsley as secretary. We examined the whole subject, investigated the details of a number of cases, repeated Pasteur's experiments on animals, discussed the published statistics, and arrived unanimously at the opinion that Pasteur was justified in his conclusions, and that his anti-rabic treatment had conferred a great and lasting benefit on mankind. Since then His Royal Highness the Prince of Wales, who always takes a vivid interest in questions affecting the well-being of the people, has visited the Pasteur Institute, and has expressed himself strongly in favour of a movement, started by the present Lord Mayor of London, for showing to Pasteur, by a substantial grant to his Institute, our gratitude for what he has done to relieve upwards of 250 of our countrymen who have undergone treatment at his hands, and likewise to enable poor persons who have been bitten, to undertake the journey to Paris, and the sojourn there necessary for their treatment. This lasts about a fortnight, it is nearly painless, and no single case of illness, much less of hydrophobia—due to the preventive treatment—has occurred amongst the 7000 persons who have so far undergone the cure.

Now let me put before you the answer to the question, Is this treatment a real cure? For this has been doubted by persons, some of whom will I fear still doubt or profess to doubt, and still abuse Pasteur whatever is said or done! From all that can be learnt about the matter, it appears pretty certain that about from fifteen to twenty persons out of every hundred bitten by mad dogs or cats, and not treated by Pasteur's method, develop the disease, for I need scarcely add that all other methods of treatment have proved fallacious; but bites on the face are much more dangerous, the proportion of fatal cases reaching 80 per cent. Now of 2164 persons treated in the Pasteur Institute, from November 1885 to January 1887, only thirty-two died, showing a mortality of 1·4 per cent. instead of fifteen to twenty, and amongst these upwards of 2000 persons, 214 had been bitten on the face, a class of wounds in which, as I have said, when untreated, the mortality is very high; so that the reduction in the death-rate seems more remarkable, especially when we learn that in all these cases the animal inflicting the wound had been proved to be rabid. The same thing occurs even in a more marked degree in 1887 and 1888. In 1887, 1778 cases were treated, with a mortality of 1·3 per cent., whilst last year 1626 cases were treated, with a mortality of 1·16 per cent.[1]

Statistics of the anti-rabic treatment in other countries show similar results, proving beyond a doubt that the death-rate from hydrophobia is greatly reduced. Indeed, it may truly be said that in no case of dangerous disease, treated either by medicine or surgery, is a cure so probable. Moreover, in spite of assertions to the contrary, no proof can be given that in any single case did death arise from the treatment itself. And as showing the safety of the inoculation, I may add that all Pasteur's assistants and laboratory workers have undergone the treatment, and no case of hydrophobia has occurred amongst them.

You are no doubt aware that Pasteur's anti-rabic treatment has been strongly opposed by certain persons, some of whom have not scrupled to descend to personal abuse of a virulent character of those who in any way encouraged or supported Pasteur's views, and all of whom persistently deny that anything good has come or can come from investigations of the kind. Such persons we need neither fear nor hate. Their opposition is as powerless to arrest the march of science as was King Canute's order to stop the rising tide. Only let us rest upon the sure basis of exactly ascertained fact, and we may safely defy alike the vapourings of the sentimentalist and the wrath of the opponent of scientific progress. But opposition of a much fairer character has likewise to be met, and it has with propriety been asked—How comes it that Pasteur is not uniformly successful? Why, if what you tell us is true, do any deaths at all follow the anti-rabic treatment? The answer is not far to seek. In the first place, just as it is not every vaccination which protects against small pox, so Pasteur's vaccination against rabies occasionally fails. Then, again, Pasteur's treatment is really a race between a strong and an attenuated virus. In cases in which the bite occurs near a nerve-centre, the fatal malady may outstrip the treatment in this race between life and death. If the weakened virus can act in time, it means life. If the strong virus acts first, prevention comes too late—it means death. So that the treatment is not doubtful in all cases, but only doubtful in those which are under well-known unfavourable conditions. This, it seems to me, is a complete reply to those who ignorantly fancy that, because Pasteur's treatment has not cured every case, it must be unreliable and worthless.

One word more. I have said that Pasteur is still—as he has always been—a chemist. How does this fit in with the fact that his recent researches seem to be entirely of a biological character? This is true. They seem, but they really are not. Let me in a few sentences explain what I mean. You know that yeast produces a peculiar chemical substance—alcohol. How it does so we cannot yet explain, but the fact remains. Gradually, through Pasteur's researches, we are coming to understand that this is not an isolated case, but that the growth of every micro-organism is productive of some special chemical substance, and that the true pathogenic virus—or the poison causing the disease—is not the microbe itself, but the chemical compound which its growth creates. Here once more "to the solid ground of nature trusts the man that builds for aye," and it is only by experiment that these things can be learnt.

[1] For further details, see Dr. Ruffer, *Brit. Med. Journ.*, Sept. 21, 1889.

Let me illustrate this by the most recent and perhaps the most striking example we know of. The disease of diphtheria is accompanied by a peculiar microbe, which, however, only grows outside, as it were, of the body, but death often takes place with frightful rapidity. This takes place not by any action of the microbe itself, but by simple poisoning due to the products of the growing organism, which penetrate into the system, although the microbe does not. This diphtheritic *Bacillus* can be cultivated, and the chemical poison which it produces can be completely separated by filtration from the microbe itself, just as alcohol can be separated from the yeast granules. If this be done, and one drop of this pellucid liquid given to an animal, that animal dies with all the well-known symptoms of the disease. This, and similar experiments made with the microbes of other diseases, lead to the conclusion that in infectious maladies the cause of death is poisoning by a distinct chemical compound, the microbe being not only the means of spreading the infection, but also the manufacturer of the poison. But more than this, it has lately been proved that a small dose of these soluble chemical poisons confers immunity. If the poison be administered in such a manner as to avoid speedy poisoning, but so as gradually to accustom the animal to its presence, the creature becomes not only refractory to toxic doses of the poison, but also even to the microbe itself. So that instead of introducing the micro-organism itself into the body, it may now only be necessary to vaccinate with a chemical substance which in large doses brings about the disease, but in small ones confers immunity from it, reminding one of Hahnemann's dictum of "Similia similibus curantur."

Here then we are once more on chemical ground. True, on ground which is full of unexplained wonders, which, however, depend on laws we are at least in part acquainted with, so that we may in good heart undertake their investigation, and look forward to the time when knowledge will take the place of wonder.

In conclusion, I feel that some sort of apology is needed in thus bringing a rather serious piece of business before you on this occasion. Still I hope for your forgiveness, as my motive has been to explain to you as clearly as I could the life-work of a chemist who has in my opinion conferred benefits as yet untold and perhaps unexampled on mankind, and I may be allowed to close my discourse with the noble words of our hero spoken at the opening of the Pasteur Institute in the presence of the President of the French Republic:—

"Two adverse laws seem to me now in contest. One law of blood and death, opening out each day new modes of destruction, forces nations to be always ready for the battlefield. The other a law of peace, of work, of safety, whose only study is to deliver man from the calamities which beset him.

"The one seeks only violent conquests. The other only the relief of humanity. The one places a single life above all victories. The other sacrifices the lives of hundreds of thousands to the ambition of a single individual. The law of which we are the instruments, strives even through the carnage to cure the bloody wounds caused by this law of war. Treatment by our antiseptic methods may preserve thousands of soldiers.

"Which of these two laws will prevail over the other? God only knows. But of this we may be sure, that science in obeying this law of humanity will always labour to enlarge the frontiers of life."

THE PHYSICAL PAPERS AT THE BRITISH ASSOCIATION.

PROF. A. W. RÜCKER, F.R.S., read a paper on cometic nebulæ. Prof. Lockyer has suggested that comet-like nebulæ may be caused by the passage of a very dense swarm through a stream of meteorites, the relative velocity of the two being very considerable. The author has, therefore, attempted to calculate the increase in the number of collisions which takes place in the rear of an attracting mass which passes through a swarm of meteorites so sparsely scattered through space that the main effects of the attraction are produced in a distance which is small compared with the length of the mean free path. Assuming, with Clausius, that the particles have equal velocities equally distributed in all directions, and which are small compared with the relative velocity of approach, the collisions will be most numerous within a cone the apex of which is the attracting body or nucleus, and which contains the lines which are parallel to the relative velocities of the individual meteorites and the nucleus when at an infinite distance apart. The collisions will increase enormously when the nebulæ come nearer the sun, and will take place about every second at points, in round numbers, 100 miles apart.

Prof. C. Piazzi Smyth, late Astronomer-Royal for Scotland, read a paper entitled "Re-examination of the Spectra of Twenty-three Gas-vacuum, End-on Tubes, after six to ten years of existence and use." This inquiry began in an attempt to ascertain, by refined mensurations, whether there was any sensible difference of spectral place for hydrogen lines, when they appeared adventitiously and scantily in tubes of other and very different gases, or in tubes of nothing but pure and abundant hydrogen by original intention. But, after having obtained a negative in every case, the inquirer became more taken up with the *changes* that had occurred in certain of the tubes subsequent to 1880, when he published upon them in the Transactions of the Royal Society of Edinburgh. Thus, a chlorine tube, of which it was printed in 1880 that it was then still showing its chlorine lines, though fainter, after two years' use; while carbon bands and hydrogen lines had begun to appear; yet now, in 1889, it has nothing but hydrogen lines, and in great brilliance, to show. Again, an iodine tube which had a comparatively large quantity of solid iodine granules introduced into, and sealed up in, its interior eleven years ago; and showed then a splendid spectrum of 148 measured iodine lines, extending discontinously from red to violet, and had nothing else, *save* three very faint puny images of the three principal lines of hydrogen—this tube, in 1889, has not a single iodine line now left; but its spectrum, which is brighter than ever, is composed of nothing but hydrogen lines, so that the once solid iodine granules would seem to be partly changed into hydrogen, and partly deposited on the inside of the tube as yellow haze, besides leaving a trifle of loose dust. The author also mentions with much satisfaction that a London maker, Mr. Charles Casella, transcended all others by supplying him with one tube of CO, two of N, and two of O, which have, through six years of existence and work, shown their respective spectra without a trace of hydrogen.

Lord Rayleigh, Sec. R.S., in a paper on the tones of bells, said that observations were made on a considerable number of bells of the usual church pattern. The pitch of the various tones, usually five for each bell, was fixed by comparison with a harmonium and with the aid of resonators. The nominal pitch of the bells appears to depend on the *highest* of the tones recorded. From a musical point of view it would seem that all the bells are far removed from perfection; while the differences of relative pitch in the various cases recorded indicates that it may be possible to effect an improvement even to the extent of bringing all the five tones into harmonious relations. The quality of the sound is, however, very difficult to estimate, and even the imperfect octaves, of which several examples occur, give much less of a dissonant effect than might have been expected.

Captain Abney, F.R.S., read a paper on the quantity of deposit of silver produced by the development on a photographic plate in terms of the intensity of light acting. The author concludes from his experiments that the deposit of silver made by different intensities of light varies directly as the intensity of light acting—this, of course, within such limits that reversal of the image is not commenced, and that the film is not at any part exhausted of the silver salt which can be reduced.

Lord Rayleigh, Sec. R.S., read a paper on pin-hole photography. It was shown, in the *Philosophical Magazine* for 1880, that a simple aperture was as effective as the best possible lens in forming an image, provided only that the focal length (f) was sufficiently great. Conversely, if f be given, the aperture may be made so small that the use of a lens will give no advantage; but if the focal length was such as was usually afforded by a camera, the admissible aperture, being very much less than that of the pupil of the eye, is insufficient for reasonably good definition. In some recent experiments the focal length was about 9 feet, and the aperture $\frac{1}{16}$ inch. The specimens exhibited were taken upon gelatine plates, and represented a weathercock seen against the sky. The amount of detail was not materially less than that observable by direct vision in the case of ordinary eyes, but modern plates are so sensitive that there would be no difficulty in working with an aperture equal to that of the pupil, other than that incurred in providing a focal length of 66 feet, with the necessary exclusion of foreign light.

Prof. O. J. Lodge, F.R.S., and Mr. R. T. Glazebrook, F.R.S., read a paper on the determination of v by means of electric oscillations. The authors have recently made a determination of v, using the oscillatory discharge of a condenser. The period of the discharge, which passed between two terminals connected through a circuit of measured self-induction to a condenser of known capacity, was determined by forming an image of the spark on the edge of a rapidly revolving circular photographic plate, the rate of revolution being accurately ascertained.

Prof. A. W. Rücker, F.R.S, read a paper on the instruments used in the recent magnetic survey of France. A magnetic survey of France has recently been completed by M. Mourcaux, who has determined the magnetic elements at some seventy stations. Prof. Rücker exhibited a set of instruments recently made under the supervision of M. Moureaux for the Science Museum at South Kensington. The point aimed at in their construction was to secure accuracy combined with dimensions and weight much less than those of the Kew pattern instruments. The main points in the construction are (1) that the needles used are much smaller than those used in the Kew pattern instruments; (2) the end of the declination needle forms a concave mirror, and a reading is taken when the image of a linear mark formed by the mirror is in the prolongation of another line which is exactly opposite to the first on a thin piece of metal; (3) the geographical meridian is determined by a theodolite, which forms part of the apparatus, instead of by using—as in the Kew—a plane mirror to reflect the image of the sun into a horizontal telescope; (4) extremely fine silk threads are able to support the small magnets used; (5) in the dip circle the graduated arc rotates in its own plane about a horizontal axis, and a reading is taken when the end of the needle and its image, formed by a concave mirror attached to the graduated circle, coincide when viewed through a microscope.

Prof. J. A. Ewing, F.R.S., in a paper on the magnetic viscosity of iron, described experiments showing that in certain circumstances the process is gradual by which iron assumes magnetization after the imposition of magnetizing force. The experiments are given in detail in Proc. Roy. Soc., June 20, 1889.

Prof. Everett, F.R.S., read a paper on the relation between brachistochrones and ray-paths, in which he pointed out an application of well-known laws of optics to the comparison of the law of force for a free path with the law of force for a path of least time, the path itself being supposed given; and to the closely allied problems of deducing the form of the path of least time from the form of the free path when the law of force is given. Most of his results, he stated, have previously been obtained by means of the principle of least action, but the optical method will be available for many students who have not mastered that principle.

Prof. Cayley, F.R.S., addressed the Section on curves in space.

Prof. J. A. Ewing, F.R.S., read a paper on hysteresis in the relation of strain to stress. It is now well known that when an iron wire is subjected to the alternate application and removal of tensile stress, many times repeated, certain of its qualities which are affected by the changes of stress exhibit hysteresis with regard to the changes of stress. If the load is cyclically varied between definite limits, these qualities do not have the same values at corresponding intermediate points during the application and removal of load; there is hysteresis or lagging in the change of quality, and in some cases this appears to be of a static character—that is to say, independent of the time-rate of variation of stress. Conspicuous instances of this action are seen in the change of magnetic and thermo-electric qualities under change of stress, some of which have been described by the author in former papers. It is natural to look for an effect of the same kind in the extension and retraction which the wire undergoes. We should expect that, after the change of loads has been frequently repeated so that a cyclic *régime* is established, the wire will, for any value of load intermediate between the two extremes, be longer during unloading than during loading. Evidently, if such an effect exist, it must be small, as it is well known that the proportionality of strain to stress which is expressed by Hooke's law is at least approximately exact. Sir W. Thomson's experiments on the damping of torsional vibrators have long ago shown that an action of the kind spoken of occurs in quickly-performed cycles of torsional strain. But it does not appear to have been looked for in slow cycles of longitudinal pull. The author has, with the assistance of one of his students, Mr. D. Low, looked for the effect in question, and has found it, not only in iron, but in steel, brass, and copper wires. He has not yet examined other metals. The experiment consisted in observing, with much optical magnification, the extension of a very long piece of wire, directly loaded with lead weights. The wire was hung from a rigid support in a testing flue or recess, built in the wall of the laboratory, and extending up through four stories. At a distance of 806 cms. below the top a small clamp was fixed on the wire, and this formed the support of the back foot of a little tripod, the feet of which consisted of three needle points about an inch apart. The two front feet rested in a slot and hole in a fixed shelf which stood in front of the long wire. The tripod carried a plane mirror which became tilted forward or backward as the wire extended or retracted. Readings were taken by a telescope of the reflected scale of a levelling staff placed vertically at a distance of some 5 metres from the mirror. The staff was graduated to 1/100 of a foot, and it was easy to read by estimation to 1/1000 of a foot, which corresponded to 0·000000102 of the length of the wire. At first a fixed shelf was used to support the two front feet of the mirror, but the effects of temperature were found to be excessive, although the greatest care was taken to shield the wire from draughts; and the plan was resorted to of hanging the shelf from two adjacent wires of the same material, suspended from the same support as the wire which was to be stretched. In this form the arrangement for optical multiplication was nearly the same as one used by Mr. Bottomley in recent experiments on the extension of loaded wires by heat.

Prof. Henry Stroud, in a paper on the E.M.F. produced by an abrupt variation of temperature at the point of contact of two portions of the same metal, gave the results of experiments on the E.M.F. produced when two portions of the same metal are in contact, and one portion is *kept* at a high temperature and the other at a low temperature. The precautions necessary in the performance of such experiments are discussed in the paper, and the results obtained are given in the case of copper and iron. The E.M.F. established in the case of copper is from hot to cold across the junction, while in the case of iron it is from cold to hot, and of far greater magnitude. These experiments are being continued with a view to obtaining the relation between the E.M.F. and the difference of temperature.

Prof. McLeod, F.R.S., in a paper on the black-bulk thermometer *in vacuo*, concludes the black bulbs should be as small as possible, and very little of the stem blackened, and also that the case should be as thin as is consistent with strength.

Sir W. Thomson, F.R.S., communicated a paper, by Magnus Maclean and Makita Goto, on electrification of air by combustion. A series of experiments was made, under the instructions of Sir William Thomson, for the purpose of endeavouring to find: (1) the state of the air inside a room as regards electrification; (2) the relation between electrification of air within a building and the atmospheric potential in its neighbourhood outside; and (3) the causes which produce or change the electrification of any given mass of air. It was found that an inclosed mass of air is electrified negatively by the burning of a paraffin lamp, of coal gas, of sulphur, of magnesium, and of several other substances; and, on the other hand, the burning of charcoal electrified a room positively.

Prof. C. Michie Smith read some notes on atmospheric electricity and the use of Sir W. Thomson's portable electrometer. Recent observations fully confirm the author's previous conclusions that, with a dry west wind, the potential of the air for some distance above the ground is usually negative. This seems always to be associated with dusty air. In using the portable electrometer in hot moist climates, special precautions have to be taken in drying it. As much sulphuric acid must be used as the pumice can absorb, and the pumice should be dried at least once a fortnight. The charging-rod itself must be very carefully dried, and, after charging, should be lifted out with a piece of warm silk.

Mr. A. W. Clayden read a paper on dark flashes of lightning. The author exhibited a negative taken on June 6 last, which shows several reversed images of lightning-flashes. He described a series of experiments which he had carried out with the object of discovering whether the phenomenon could be imitated in the laboratory. The steps in the investigation were illustrated by a series of negatives showing photographs of electric sparks. The conclusions arrived at are, that photographic images of electric sparks can be reversed by the action of diffused light. Reversal is only produced when the exposure to diffused light is subsequent to (or possibly simultaneous with) exposure to the image of the spark. If the plate is first acted upon by diffused light, the sparks give a direct image unless the action has been considerable, in which case they seem to make no impression.

Prof. Cleveland Abbe, in a paper on the determination of the amount of rainfall, gave a *résumé* of the general results of investigations on this subject. The deficit in catch by a gauge due to the eddies of wind is shown, on general reasoning, to be proportional to the velocity of the wind and to the relative percentage of small and slowly-falling drops.

Prof. C. Piazzi Smyth, late Astronomer-Royal for Scotland, read a paper entitled "Hygrometry in the *Meteorological Journal*." After noting the superior value officially attached to determinations of the mean temperature by observations of self-registering thermometers, recording its maximum and minimum every twenty-four hours, the author inquires why the still more difficult problem of ascertaining the mean daily moisture of the atmosphere is thrown over to a different principle of observing, long since condemned for simple temperature. Believing that the want of good self-registering dry- and wet-bulb thermometers for the purpose was the chief obstacle, and having alighted on some recent makes of Sixe's thermometers with several very recommendable qualities, he invested two out of four of them with the peculiar fittings for wet-bulb hygrometers, after having made a table of index corrections for them all, as compared with standard thermometers. But as soon as hygrometric observations began, the depression of the wet, below the dry, bulb always came out at only two-thirds of what a standard but non-registering Glaisher arrangement gave out, for the horizontal form of Sixe, and no more than half for a vertical form. These differences of hygrometric statement, though rather puzzling for a time, were traced up to the wet bulbs of the Sixe's thermometers being in contact on one side with the scale-plate. For when that plate in the horizontal Sixe form was cut entirely away from the bulb, and a new vertical form of Sixe was made (*per* Mr. James Bryson, Edinburgh) with its long thin bulb wholly outside the rest of the instrument, the indications of all three varieties of wet-bulb hygrometers became practically the same; and hygrometry was relieved of the old drawback on its registrations of maximum and minimum quantities of moisture in every twenty-four hours.

Mr. F. T. Trouton read a paper on some experiments on radiation with Prof. Hertz's mirrors. These experiments were described in NATURE, vol. xxxix. p. 391, and vol. xl. p. 398. Certain of them were repeated by Mr. Trouton in the Physical Laboratory of the Durham College of Science.

Profs. A. W. Rücker, F.R.S., and T. E. Thorpe, F.R.S., read a paper, on the relation between the geological constitution and the magnetic state of the United Kingdom, before a joint meeting of Sections A and C. The authors have, during the last five summers, determined the magnetic elements at 200 stations, distributed over the whole of the United Kingdom, and have employed the results of their observations in a study of the disturbing magnetic forces in play in various districts. If all disturbing causes were removed, an observer travelling due west from London would find that the declination increased by about half a degree for each degree of longitude. In fact, the rate of increase, though equal on an average to this amount, is irregular. Between London and Windsor it is considerably larger, and between Windsor and Reading smaller than the mean. The forces which produce these abnormal variations depend upon the geological character of the district. Two principal theories have been proposed to account for these disturbances: (1) the theory of direct action of magnetic rocks; (2) the other explanation associates the deflection of the needle with disturbances of the earth currents of electricity produced by irregularities in the geological constitution of the country, and especially with geological faults. On the whole, the authors think that the theory of the direct action of magnetic rocks agrees best with the observed facts, and they have shown that the kingdom can be divided into a small number of magnetic districts in which the directions of the disturbing forces are evidently closely connected with the geological constitution.

In a paper on the passage of electricity through gases, Prof. Arthur Schuster, F.R.S., gave an acccount of his investigations during the last two years on the distribution of potential in the neighbourhood of the negative pole of discharge of electricity through rarefied gases. Knowing the rate of fall of potential, it can be determined whether there is any bodily electrification in any part of the negative glow. It was found that the kathode is surrounded by an atmosphere of positively electrified gaseous particles extending to the outer edge of the so-called dark space. According to the author's views, this atmosphere corresponds to the polarized layer adjoining the negative electrode in an electrolyte. The cause of the sudden difference in luminosity between the dark space and the negative glow has also been investigated, and it has been found that negative particles projected from the kathode pass unhindered through the dark space, while their velocity is quickly reduced in the glow proper, the translatory energy being thus changed into energy of vibration.

Prof. Oliver J. Lodge, F.R.S., read a paper on the failure of metal sheets to screen off the electrostatic effect of *moving* or *varying* charges. Experiments have been made on the screening effect of a very thin film of silver chemically deposited, the thickness of the deposit being different in the different experiments. The silver screens are only found to protect so long as they are opaque; they no longer do so when the deposit is so thin as to be transparent.

Prof. James Blyth described a new form of current-weigher. In the construction of balances for the measurement of electric currents, a greater or less difficulty has always been experienced in leading the current into the movable parts of the instrument without seriously interfering with its sensibility. Several methods have been adopted to overcome this difficulty. In some balances the current is led in by mercury cups; in others, flexible leads, made of thin wire spirals or thin metal strips, are employed; while, in the recent balances by Sir William Thomson, the difficulty has been overcome by means of his well-known ligature suspension. Some time ago it occurred to the author that still another form of balance might be employed for this purpose, and the present paper is a short description of one which he has made. The balance referred to is of the ordinary Roberval type, with the pivot connections all replaced by tightly-stretched torsion wires. It is constructed as follows:—On a flat base-board are placed two vertical uprights of wood or other insulating material, about 6 inches apart. Between these are stretched two parallel wires in the same vertical plane about 3 inches apart. To the middle points of these wires are soldered the two horizontal metal bars of the Roberval. These are about 9 inches long. Both horizontal bars terminate at each end either in forks or rings, placed in a horizontal plane, and wires are tightly stretched between the prongs of the forks, or across a diameter of the rings. To the middle points of these last wires are attached, also by soldering, the vertical bars of the balance, thus completing what takes the place of the ordinary jointed parallelogram of the Roberval. The vertical bars have metal terminals, insulated from each other, and carry the circular disks, on the rims of which the movable coils of wire are wound. The bars pass at right angles through the centres of the disks, and are fixed to them at their middle points. The middle coils, which are of exactly the same diameter as the movable ones, are supported from the base-board, and are placed so that one is about half an inch above one movable coil, while the other is as much below the other movable coil. From this it will be seen that, when the balance is in equilibrium, the fixed and movable coils are all horizontal, with a space of about half an inch between each pair. The stretched wires may be either of steel or phosphor-bronze, and before being finally soldered are placed under considerable tension. The current flows through the instrument thus:—Entering, say, by the upper wire connecting the fixed supports, it passes to the upper horizontal bar. There it splits into two, one half (supposing the resistances equal) passing to each end of the bar, and, by means of the corresponding fork-wires, passing through the movable coils. From the movable coils each half returns along the lower horizontal bar, and together pass out by the lower wire connecting the two main supports. From this the whole current passes first through the one fixed coil, and then through the other, and in such a direction as to produce a repulsion between each pair of coils. In the constructing, care is taken that the suspended coils are both made of equal weight, and that when the balance is in equilibrium no torsion is in any of the wires. Small scale-pans are attached to each vertical bar, and a bob for raising or lowering the centre of gravity of the whole is placed on a rod springing at right angles from the middle of one of the horizontal bars. The current strength is estimated by the weight needed to restore the balance to equilibrium when disturbed by the passage of the current. A sliding weight may also be used, as in a steelyard. It will be readily seen that, as in all forms of current-weigher, the weights are proportional to the square of the current strength.

Prof. S. P. Thompson described a phenomenon in the electro-chemical solution of metals—originally discovered by Planté—which occurs when a current is passed between two copper wires immersed in dilute sulphuric acid in a cell. The solution takes place in two stages, the metal first oxidizing, and then the oxide dissolving in the acid. This was projected on the screen, the audience being able to observe that the bubbles evolved when the current was turned on ceased as the current died down, choked by the formation of oxide, but almost immediately recommenced when the oxide began to dissolve. The author has tried other metals and other acids—silver, zinc, and iron showing similar effects in sulphuric acid, but not in nitric, acetic, or hydrochloric acids.

Mr. J. Wilson Swan read a paper on chromic acid as a depolarizer in Bunsen's battery. When chromic acid became an article of commerce at a moderate price, it occurred to the author to see if he could not obtain a substitute for the nitric acid of the Bunsen battery. As the results of his experiments he finds that a solution, of the composition nitric acid 1 part, chromic acid 2 parts, sulphuric acid 5 parts, and water 5 parts, gives results equal to that obtained with nitric acid.

Prof. Perry, F.R.S., described a variable standard of self-induction. An instrument like that hitherto used as a variable standard by Prof. Ayrton and the author, was used by Prof. Hughes. It is a fixed coil of wire, inside which a movable coil is placed; the coils are in series one with another. When their planes are parallel there is either a minimum or a maximum coefficient. When the movable coil is rotated so that its plane makes various angles with the plane of the fixed coil, a pointer shows on a scale the coefficient for that particular position.

In another paper on a hot twisted strip voltmeter, Prof. Perry described the behaviour of twisted strips subjected to axial pull. A small elongation is accompanied by a great rotation, so that these strips may be employed in measuring instruments such as weighing-machines, aneroid barometers, testing-machines, &c. For the Ayrton and Perry voltmeter a double twisted strip of platinum silver with its ends insulated is initially in tension inside a tube or frame two-thirds brass and one-third iron, a pointer at the middle of the strip being visible above a dial and capable of a motion of nearly 360°. When the current passes the pointer rotates because of the heating of the strip. By continuous making and breaking of a large current through the strip during seventy-two hours all zero and other errors seem to be eliminated. The highest reading of the exhibited instrument was 2½ volts, so that it is particularly useful in accumulator-work. The author gave the law of transformers generally, and found that a small transformer on the base of the instrument converted it into a voltmeter of any range whatsoever for alternating currents, the readings being independent of the frequency of alternation. The author exhibited twisted strips of carbon made by Messrs. Woodhouse and Rawson, which he intends to use for voltmeters of higher range.

Mr. W. H. Preece, F.R.S., read a paper on the relative effects of steady and alternate currents on different conductors. Sir W. Thomson, at the Bath meeting of the British Association, stated that alternating currents entered a distance of only about 3 millimetres into the heart of a thick round copper conductor, when the frequency was 150. This "diffusion law," as he called it, is dependent on the coefficient of self-induction and on the frequency, *i.e.* on the number of complete alternations of positive and negative currents transmitted per second. As this law has a most important bearing on the commercial value of systems of distribution dependent on alternating currents, it becomes most desirable to study the question practically. The verification of this law is almost beyond the reach of experiment. It occurred to the author, however, that if conductors of different materials are taken, such as iron, copper, lead, and platinoid, of easily measurable lengths and of convenient sectional areas, and if measurable and variable currents, both alternating and direct, approximately similar to those used in practice, are transmitted through them, the question could be studied by observing any difference, if such existed, in the total expenditure of energy in the conductors under these different circumstances. The general conclusion to be drawn from the experiments is that practically no serious error has been made in the form of conductors so much used for alternating current systems, and that nothing cheaper or better has been devised than a simple stranded conductor coated with a suitable insulating coating. The experiments do not solve the question of the distribution of current density through the section of the conductor, but they do show that within the range of practice the total flow of energy is the same in copper conductors, whether it be urged by alternate or by steady currents.

Prof. G. Forbes, F.R.S., and Mr. W. H. Preece, F.R.S., in a paper on a new thermometric scale, suggest the joule as the thermal unit instead of the therm or the calorie. 4·2 joules will raise the temperature of 1 gramme of water at 4° one degree, hence to raise 1 gramme of water from freezing-point to boiling-point requires 420 joules; and it is suggested that the difference of temperature between freezing- and boiling-points should be represented by 420 units of temperature.

Mr. J. T. Bottomley, F.R.S., exhibited some large Leyden jars, broken during the testing of a large Wimshurst machine which was being constructed by Sir Archibald Campbell, Bart., of Blythswood. They were excellent examples of multiple fracture.

Prof. S. P. Thompson, in a paper on sparkless electro-magnets, discussed the various means of suppressing the sparks in the circuits of electro-magnets. The most effective means, he finds, is to surround the core of the magnet with a substantial shield of copper. The author illustrated his method by experiments before the Section.

Mr. W. W. Haldane Gee and Dr. Arthur Harden read a paper on stereometry. This communication relates to the methods used for the determination of the volumes of bodies to which the hydrostatic method is not applicable. The authors have devised a convenient form of instrument, first proposed by Say, and afterwards modified by Leslie, Kopp, Regnault, and Miller. They have also shown that the following method for ascertaining volumes is very generally applicable, and likely to be of considerable service in physico-chemical researches. The body whose volume is desired is inclosed within a vessel of known volume, and then carbon dioxide (or other dry soluble gas) is passed into it for some time. The gas is then displaced by dry air (or other gas), and the volume of the carbon dioxide driven out is ascertained *gravimetrically* by absorption in bulbs containing caustic potash solution. By first filling the vessel with dry air and then driving it out with carbon dioxide, the volume of the air, and hence that of the body, may also be ascertained *volumetrically*, but with less accuracy. The gravimetric method is especially applicable for accurately ascertaining the density of soluble gases. For this purpose it is far more convenient than the process of direct weighing as used by Regnault.

In a paper on the specific heat of caoutchouc, Mr. W. W. Haldane Gee and Mr. Hubert L. Terry described a number of determinations which had been made of the specific heat of Para india-rubber which had been masticated, hydraulically compressed, and then cut into sheets from 0·22 millimetre to 1·40 millimetre thick. The rubber was alternated with sheets of tin-foil, and heated for two hours in a steam-jacket at 100° C. A modification of Regnault's method of mixtures was employed to find the specific heat of the hot rubber. Owing to the non-conducting nature of the substance the time of the calorimeter attaining its maximum temperature is as long as ten minutes; hence it has been necessary to apply special formulæ for the correction due to cooling. Those proposed by Pfaundler, Pape, and Schuster have been used, and the results calculated in accordance with them. The mean of the best-conducted experiments gives for the Para rubber the number 0·486. The investigation is being extended to allied bodies, especially the different forms of vulcanized rubber and gutta-percha.

Mr. F. T. Trouton read a paper on the temporary thermo-currents in iron. If a portion of an iron wire connected up to a galvanometer be heated red hot, and the heated portion be caused to travel along the wire by moving the flame, a current is produced, which is due to the fact that when iron changes its molecular state at the temperature of recalescence this change does not take place at the same temperature on heating as on cooling, so that when a flame is moved along an iron wire so as to continuously heat it above the temperature of recalescence the junction of the altered with the unaltered metal in front is at a higher temperature than of the junction behind the flame.

Prof. Barrett read a paper on recalescence of iron.

THE CHEMICAL PAPERS AT THE BRITISH ASSOCIATION.

OWING, doubtless, to the numerous chemical industries of the district, many of the papers read in Section B at Newcastle were of special interest to the technical student.

The President's Address on "The Metallurgy of Iron" was followed by the Report of the Committee for Investigating the Influence of Silicon on the Properties of Steel. From the results of Mr. Hadfield's experiments, it appears that silicon by itself does not produce a steel that will harden by water-quenching. The brittleness noticed in ordinary so-called silicon steel is due, not to silicon alone, but to the combined presence of the two hardeners, silicon and carbon. Silicon, up to 6 per cent., does not destroy the malleability of iron, nor are the magnetic properties greatly affected by increasing percentages of silicon, as is the case with manganese steel.

The Committee for Establishing an International Standard for the Analysis of Iron and Steel reported that they had made good progress with the work, and hoped before long to have the necessary analyses completed.

Some curious experiments were described by Mr. J. W. Hogg, on the volatilisation of lead oxide and its action upon glass at low temperatures. If some writing is placed upon a glass plate or platinum-foil, using lead oxide as a pigment, if a polished plate of glass be placed over this as closely as possible and prevented from actual contact by suitable means, upon now heating up to scarcely visible redness, a distinct reverse of the design will appear upon the upper glass. The quantity of lead oxide which will produce this effect is not shown by the most delicate balance.

Photographers were interested in Prof. Liveing's account of a new developer, "Eikonogen," which appears to give greater detail than most of those now in use. The tone of the negative is also excellent. Eikonogen is the sodium salt of amido-β-naphthol-β-sulphonic acid.

Mr. C. T. Heycock and Mr. F. H. Neville read a paper on Raoult's method applied to alloys. Alloys of a number of metals with sodium and tin were studied. The "atomic fall" of the freezing-point for aluminium was about half that of most other metals—pointing to twice the present number as the atomic weight of aluminium. Antimony produces a rise in the freezing-point of the alloy, for some unexplained reason. Contrary to Raoult's so-called second law, it was found that the nature of the solvent is of influence; 1 atomic weight of metal in 100 atomic weights of sodium not giving the same fall as 1 in 100 of tin. The method would seem to give a means for correcting and verifying atomic weights; though it does not give much information as to the molecular weight. It was shown that, in accordance with Van 't Hoff's theories on the nature of solution, each metal produces its own lowering of the freezing-point in presence of others.

On the Friday morning, Prof. Dunstan read the Report on the present methods of teaching chemistry.

Prof. Bedson afterwards gave an interesting description of Dr. Netto's process, at work on the Tyne, for the manufacture of aluminium from cryolite. The cryolite is first fused with salt in a reverberatory furnace; then run out into converters in which sodium is gradually added—about five pounds at a time. Sodium fluoride and metallic aluminium are formed. The sodium is obtained by allowing molten caustic soda to flow gradually onto charcoal contained in a cast-iron retort heated to dull redness. The sodium carbonate formed in the reaction sinks to the bottom of the retort. The greater concentration of the caustic soda thus produced enables the temperature to be kept lower than in the Castner process.

An account was afterwards given, by Mr. J. H. J. Dagger, of the Cowles method for manufacturing aluminium alloys.

On Monday, numerous Reports of Committees were handed in, the most interesting being that read by Dr. Richardson on the action of light on the hydracids. Dr. Richardson has found that, if white light is allowed to act upon water in presence of oxygen, a considerable quantity of hydrogen peroxide is produced. This accounts for the fact mentioned in previous Reports, that a mixture of dry oxygen with dry hydrogen chloride, or bromide, is unaffected by light.

Dr. Richardson also exhibited and described a new self-registering actinometer, based on the fact, discovered by Budde, that chlorine expands in the actinic rays, contracting again in the dark.

Prof. H. B. Dixon gave an account of experiments made by himself and Mr. J. A. Harker, on the explosion of a mixture of hydrogen, chlorine, and oxygen. It was found, in contradiction to previous statements, that steam is produced by the explosion even when chlorine is in excess. It was noted that hydrogen and chlorine, when exploded alone, give a sensible contraction.

In another paper the same workers show that hydrogen and chlorine do not explode when dry, unless exposed to very intense light.

Dr. A. P. Laurie gave some results of his researches on artists' colours. He is comparing the recipes given in the manuscripts of the old masters with those in modern use.

A new ferment was described on Tuesday, in a paper by Prof. P. F. Frankland, Miss Grace F. Frankland, and Mr. J. J. Fox. From the products of its activity, ethyl alcohol and acetic acid, it is termed *Bacillus ethaceticus*. It will cause a solution of mannite to ferment, while dulcite is unaffected by it.

In his paper on the Constitution of the Aromatic Nucleus, Mr. S. A. Sworn gave preference to the octahedral formula of Thomsen. A further development of Thomsen's formula, he believes, affords a full explanation of the laws of para- and meta-substitution.

Dr. Isaac Ashe read a paper entitled "Dimidium: an Attempt to represent the Chemical Elements by Physical Forms." He put forward the view that the primordial basis is to be found in an element having half the combining weight of hydrogen. This hypothetical element is named dimidium. The relations of attraction and repulsion under the influence of polar force suggest a linear form for such a body. A series of vortex-rings, superposed one on the other, would yield a form elongated in one direction and limited in the other two. Having shown that the primordial element may have a bar-like form of definite length, the author proceeds to construct models of the different elements, conforming in each case to the combining weight, valency, crystalline form, &c.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 30.—M. Des Cloizeaux, President, in the chair.—Presentation of the fourth sheet of the Bulletin of the International Committee for preparation of a map of the heavens; meeting of Committee at Paris Observatory, by M. E. Mouchez. Five other Observatories (Vienna, Catania, Mexico, Manilla, and the Vatican) have been added to the original sixteen. Each Observatory will have to take about 700 photographs in the zone alloted to it, and it is hoped to finish the work in three or four years. A central office for utilizing the results will be necessary.—Addition to the theory of thin weirs extending throughout the breadth of the bed of a water-course; calculation of variations in the contraction of the outflowing sheet at its lower face, by M. J. Boussinesq.—On the last communication of Halphen to the Academy, by M. F. Brioschi.—On the denomination of the industrial unit of work, by M. H. Resal. He advocates the unit of 100 kilogrammetres, to be called the *quintalmetre*.—On the application of high temperatures in observing the spectrum of hydrogen, by MM. L. Thomas and Ch. Trépied. The electric arc is found a sure and comparatively easy way of making hydrogen sufficiently luminous for spectroscopic observation, even with large dispersions; (four jets of the gas were made to converge conically towards an axis coinciding with that of the carbons).—On concatenation (*enchainement*) of the atomic weights of the elements, by M. Delauney. He shows that the atomic weights may be joined together by addition in each case of the square root of a whole number, which is variable, but always *harmonic* (not containing any other prime factors than 1, 2, 3, and 5).—Combinations of cupric oxide with amylaceous matters, sugars, and mannites; new reagents for proximate analysis, by M. Ch.-Er. Guignet. Solutions of cellulose, also dry starch, or inuline, give well-defined combinations with oxide of copper, when put in contact with its solution in ammonia. Some sugars (pure glucose from honey, galactose, &c.) quickly precipitate copper ammonio-sulphate (but not the oxide); and while inverted sugar does not precipitate the sulphate, a previous addition of glucose produces a deposit of the glucosic combination (which does not retain ammonia). Mannite and dulcite, &c., yield at once blue precipitates in an ammoniacal sulphate of copper solution, which reagent is useful with decoctions of vegetable matters, as most substances in these are not precipitated by it.—On the number and calibre of nerve-fibres in the common oculomotor nerve, in the new-born and in the adult cat, by M. H. Schiller. The number does not increase during life (or increases very little); average 2942 in the kitten, 3035 in the cat. The calibre is increased six or eight times.—On the preceding investigation, by M. Aug. Forel. Various researches point to the stability of the nervous elements during life, and this he regards as very important for explanation of the phenomena of memory.—On the vitality of trichinæ, by M. Paul Gibier. He submitted small pieces of fresh pork with numerous trichinæ (which were much more lively when brought out of their cysts into a water-heated vessel than those of the salt meat) to a temperature of 20° to 25° below zero, for about two hours, and found the animals, on reheating, as lively as before.—The innervation of the osphradium of mollusks, by M. Paul Pelseneer. Like the other sensorial organs of mollusks, the osphradium proves to be innervated by the cerebral ganglion.—On the *Spongeliomorpha Saportai*, a new Parisian species, by M. S. Menier.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

On the Causes, Treatment, and Cure of Stammering: A. G. Bernard (Churchill).—A Text-book of Physiology, 5th Edition, Part 2: M. Foster (Macmillan).—A Contribution to the Flora of Derbyshire: Rev. W. H. Painter (Bell).—Notes on the Pinks of Western Europe: F. N. Williams (West).—Watts' Dictionary of Chemistry, vol. ii.: M. M. P. Muir and H. F. Morley (Longmans).—The Microscope in the Brewery and Malthouse: C. G. Matthews and F. E. Lott (Bemrose).—An Epitome of the Synthetic Philosophy: F. H. Collins; Preface by H. Spencer (Williams and Norgate).—Watts' Manual of Chemistry, vol. i. 2nd edition: W. A. Tilden (Churchill)—Nature Stories, Myths, and Phantasies (Hamilton).—Prodromus of the Zoology of Victoria, Decade xviii.: F McCoy (Trübner).—Service Chemistry: V. B. Lewes (Whittingham).—Chemical Technology; vol. i. Fuel and its Applications: E. J. Mills and F. J. Rowan (Churchill).—The Cradle of the Aryans: G. H. Rendall (Macmillan).—Mount Vesuvius: J. L. Lobley (Roper and Drowley).—Thermodynamics of the Steam-Engine: C. H. Peabody (Macmillan)—A Manual of Forestry, vol. i. The Utility of Forests and Fundamental Principles of Sylviculture: W. Schlich (Bradbury).—Sixth Annual Report of the Bureau of Ethnology, 1884–85: J. W. Powell (Washington).—Hydrostatics for Beginners: F. W. Sanderson (Macmillan). Differential Equations: W. W. Johnson (Macmillan).—Manures and their Uses: A. B. Griffiths (Bell).—A Text-book of Physiology; vol. ii. Special Physiology: J. G. M'Kendrick (Glasgow, Maclehose)—Problems of the Future, and Essays: S. Laing (Chapman and Hall).—Geological Record for 1880–84. vol. ii.: Edited by Topley and Sherborn (Taylor and Francis).—The Brook and its Banks: Rev. J. G. Wood (Religious Tract Society).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 4th series, vol. ii. (Manchester).—Notes Biographiques sur J. C. Houzeau: A. Lancaster (Bruxelles).—Notes on Indian Insect Pests, vol. i. No. 1 (Calcutta).—Das Australische Florenelement in Europe; Dr. C. Ettingshausen (Gray)—Onderhouden Trillingen van Gespannen Draden: H. J. Oosting (Helder De Boer).—Records of the Geological Survey of New South Wales, vol. i. Part 2 (Sydney, Potter).—Internationales Archiv für Ethnographie, Band ii. Heft 4 (Trübner).—Jahrbuch der k.k. Geologischen Reichsanstalt, Jahrg. 1889, xxxix. Band, 1 and 2 Heft (Wien. Hölder).—The Photographic Quarterly, No. 1 (Hazell).—Bulletin of the United States National Museum, No. 37: W. H. Dall (Washington).—Aus dem Archiv der Deutschen Seewarte, xi. Jahrg., 1888 (Hamburg).—Mind, September (Williams and Norgate).—Bulletin of the United States National Museum, No. 35: H. Edwards (Washington).

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