THURSDAY, MARCH 26, 1891.

SCIENTIFIC WORTHIES.

XXVII.—LOUIS PASTEUR.

LOUIS PASTEUR was born on December 27, 1822, at Dôle, where his father, an old soldier who had been decorated on the field of battle, worked hard as a tanner. He was an earnest, industrious, and thoughtful man, fond of reading, and very desirous that his son should be well educated and should gain renown in some branch of learning. Father and mother, alike in their enthusiasm and ambition, devoted themselves to their son—they would "make a man of him," they said.

In 1825 they removed to Arbois, and as soon as he was old enough to be admitted as a day-boy, Pasteur began his studies in the Communal College, and there, after the first year or two, he worked hard and gained distinction. Thence he proceeded to Besançon, and thence, after a year's successful study, to the École Normale in Paris, to which, after gaining a high place in the entrance-examination, he was admitted in 1843. His devotion to chemistry had begun while he was a pupil of Prof. Darlay at Besançon, and now he studied it under Dumas at the Sorbonne, and Balard at the École Normale. His love of science became intense: he spent every day in attendance on the lectures, in reading, and in practical work in both chemistry and physics.

Among the teachers in the École by whom he was most encouraged was M. Delafosse, who was especially studying molecular physics, and it was after a conversation with him that Pasteur was guided to the careful study of crystals, and to the inquiry which led him to his first important discovery.

It was known that the tartrate and the paratartrate of soda and ammonia, though exactly isomeric, similar in their atomic composition, specific gravity, and crystalline form, yet differed, not only in many of their chemical relations, but, as Biot had shown, in the fact that a watery solution of the tartrate deflected the plane of polarized light, and that of the paratartrate did not. Pasteur could not believe that bodies apparently identical in atomic composition and construction could thus differ in their relations to light. He had already observed a dissimilarity between the crystals of the tartaric and of the paratartaric acids, in that the latter were, and the former were not, symmetrical; and he found the same difference between the crystals of their salts. Now, by a laborious study and repeated measurements of the crystals of these and other similar compounds, he showed that in the paratartaric (now called racemic) acid there are two distinct forms of tartaric acid, of which the one dextro-tartaric) is identical with the ordinary tartaric, and like it, in solution, deflects light to the right; the other (lævo-tartaric) deflects it to the left. The two in combination, as they occur in the ordinary paratartaric acid, neutralize one another in their influences on polarized light, and do not deflect it in either direction. Similarly, he next found that the crystals of the two acids and of their respective salts are different in their forms; those of the dextro-tartaric and its salts are similar to those of the ordinary tartaric and its salts in their dissymmetry; those of the lævo-tartaric are also dissymmetrical, but in the opposite direction; those of the two combined in the paratartaric and its salts are symmetrical.

Thus was explained the seeming anomaly of so distinct a difference between bodies exactly isomeric. A great problem was solved; and it was among its results that, gradually, the way was made clearer by which the synthesis of organic alkaloids and of sugars has been achieved.

These researches had occupied six years, and Pasteur had gained by them an early high renown, and he hoped that, by continuing to work on the same lines, he might obtain results yet more important, especially in the contrast of the symmetrical forms of crystals derived from the chemistry of dead substances, and the dissymmetrical of those derived from the chemistry of living bodies. He had invented instruments and methods for experiments. But it chanced that at this period of his work he was, in 1854, appointed Dean of the Faculty of Sciences at Lille, and, though he gave up crystallography with great regret, he determined to investigate and teach a subject of more direct utility. The chief industry of the town was in the manufacture of alcohol from beetroot and corn, and he decided to teach the scientific methods of improving it, and to promote the scientific brewing of beer that might compete with those of Germany and Austria.

Hence, fermentation became his chief study, and in this was the beginning of the researches which led to the most important of his discoveries. But, although it may have seemed like entering on a new subject, the change was an illustration of the regular sequence that may be observed in the order of all Pasteur's work, for, among the facts which had most influence on the course of his investigations, was one which he had observed in his study of the tartaric salts. He had traced the fermentation of the ordinary right tartrate of ammonia in a solution containing albuminous matter, and had observed the coincident appearance of a distinct micro-organism. Then, he had shown the breaking up of the composition of the paratartaric acid by a similar process of fermentation, by putting a minute portion of green mould into water containing phosphate of potassium and ammonia, but no albuminous matter. Here also there was an abundant formation of organisms, and, with this, the gradual disappearance of the dextro-tartaric acid, of which, as other experiments showed, the carbon was taken for the sustenance and growth of the organism of the mould. In both cases the fermentation had seemed due to the action of a living organism.

Pasteur's belief that these and all processes of fermentation were primarily and essentially due to the presence and action of minute living organisms in the variously fermentescible fluids led him to study them with the combined chemical and microscopical research which no one before him had ever used with the same constancy or the same skill. It was, indeed, a combination of methods of research which had hardly ever before been used. There were excellent chemists and there were excellent microscopists; but they usually worked apart, and hardly helped one another. Pasteur—well instructed in exact methods of research, and both chemist and microscopist—attained results far beyond what any before him had reached. It was no easy thing for him to justify the study of fermentation on the lines suggested by what was called the vitalistic or germ-theory, when the hypothesis of "spontaneous generation" was held by many, and when the whole process of fermentation seemed so well explained by the chemical theory of Berzelius, or by that of communicated molecular motion held by Liebig and others who agreed with him in thinking that the organisms in fermenting liquids might be considered as accidental, except in so far as they might supply organic matter to the liquid. But Pasteur did more than justify the germ theory; for he proved its truth by a multitude of facts previously unobserved, and from which he constructed general principles of which both the scientific value and the practical utility are now beyond estimate.

It is not possible to describe in detail or in chronological order the course and methods of Pasteur's researches on the various fermentations and on processes closely allied to them. Only their chief results can here be told; only the things which he discovered, or which, since he studied them, have been deemed not merely probable, but sure. Thus, he proved the constant presence of living micro-organisms, not only in yeast, in which Cagniard-Latour, and Schwann, especially, had studied them, but in all the fermenting substances that he examined; he proved the certain and complete prevention of fermentation, putrefaction, and other similar processes in many substances, however naturally subject to them, by the exclusion of all micro-organisms and their germs, or by their destruction if present; and he proved the constant presence of various micro-organisms and their germs in the air and water, in the earth, in dust and dirt of every kind—their abundance "everywhere." By the proof of these things the evidence became complete, that fermentations and similar processes are primarily and essentially due to the action of organisms living in the fermenting substance. But, more than this, it was ascertained that each method of fermentation—vinous, lactic, acetic, putrefactive, or whatever it may be—is due to a distinct specific micro-organism appropriate to that method, and by its own vital processes initiating the changes which lead to the formation of the "specific products" of the fermentation, the alcohol, the vinegar, the virus, or whatever else. For each specific micro-organism has to live on the constituents of an appropriate fermentescible fluid which it decomposes, assimilating to itself such elementary substances as it needs for its own maintenance and increase, and leaving the rest to combine in the forming of the "specific products." Further, and most importantly, it was made certain by Pasteur's investigations that, by various appropriate methods of "cultivation" which he invented, the individuals of each species of ferment may be separated from others with which they are mingled, and that, thus separated, the individuals of each species may, by means of a "pure cultivation" be multiplied indefinitely, as, especially, in the production of "pure yeast." And in certain instances it was shown that the active power of micro-organisms may, by cultivation, be so "attenuated" that they will produce only comparatively slight changes in substances which, in their natural state, they potently affect.

The most direct applications of the results of Pasteur's studies of fermentation were, naturally, made in the manufacture of wine and vinegar, and, at a later time, of beer. The "diseases," as they were fairly called, of these fermented liquors, the "thickness," "ropiness," "sourness," and others, could be traced to the disturbing influences of various other micro-organisms mingled with those of pure yeast, the true alcoholic ferment, and each producing an injurious method of fermentation. This being made sure, the prevention of the diseases became possible, by the exclusion or the destruction of every organism other than the true one; and of this one the proper cultivation could insure the sufficient production. The methods of thus preventing or arresting the diseases have become too various to be described here. Among them is that of heating wines and beers sufficiently to destroy all the germs remaining in them; and the uses of this, which has been specially called Pasteurization, and of many others, have made brewing and wine-making nearly secure against the great losses to which, before Pasteur's time, they were always liable from the disturbances of their fermentations.

The utility of the proof that fermentations, including the putrefactive, are absolutely dependent on the action of living micro-organisms was speedily shown in the prevention or remedy of diseases far more important than those of wine and beer. The discovery of the causes and processes of what were justly called the "diseases" of fermented liquids enlarged the whole range of a great section of pathology, showing, as it soon did, so intimate relations between contagious diseases and disordered fermentations that it became safe to apply the facts found in the more simple to the study and treatment of the most complex. The study of the diseases of fermented liquids led straightway to the practice of antiseptic surgery. Its first practical application in medicine was in 1862, when Pasteur, who had proved the existence of a living organism in fermenting ammoniacal urine from a diseased bladder, recommended the washing of such bladders with a solution of boracic acid, now a well-known "germicide." This was done successfully by M. Guyon, and the practice is still commonly followed.

For the treatment of wounds and of parts exposed by wounds, the whole subject of the complete exclusion of micro-organisms, and of their destruction if present, was, in and after 1865, studied with especial care by Sir Joseph Lister, and the results obtained by him and others following his example were so convincing that the antiseptic practice, which many justly call Listerism, is followed everywhere by every surgeon of repute. The methods of excluding micro-organisms from all wounds, the antiseptic substances employed, the modes of applying them are more than need be told, and they increase every year; but they have all the same design—the complete exclusion or destruction of all living germs and micro-organisms capable of exciting putrefaction or any other fermentation in parts exposed by wounds; their exclusion from air, water, sponges, surgical instruments, dressings, and everything that can come in contact with such parts. And the system is not nearly limited to the treatment of wounds. In various methods and degrees it is applied in the construction of hospitals and infirmaries, in the prevention of the spread of infection and of every form of blood-poisoning. It is impossible to estimate the number of the thousands of lives that are thus annually saved by practices which are the direct consequences of Pasteur's observations on the action of living ferments, and of Lister's application of them. In the practice of surgery alone, they are by far the most important of the means by which the risks of death or serious illness after wounds are reduced to less than half of what they were thirty years ago; and of the means by which a large number of operations, such as, at that time, would have been so dangerous that no prudent surgeon would have performed them, are now safely done.

In antiseptic practice, and in the manufactures connected with fermentation, the design is to exclude or destroy all micro-organisms. Many kinds might be harmless, but at present there are no ready and sure means of excluding only those from which mischief might arise, and therefore, in the schemes for complete exclusion, no account is taken of the great general principle established by Pasteur, that each method of fermentation is due to the action of one special living organism. This in relation to contagious diseases, would now have become one of the chief subjects of his study. But again, he was obliged to change the subject of his work; for in 1865 he was urged by Dumas, and was forced to consent, to undertake the investigation of a disease of silkworms in the south of France. Since 1849 this disease, called *pébrine*, had been ruinously prevalent. In 1865 the loss due to it was estimated at four millions sterling, and it had spread to many other countries from which silkworms' eggs had been brought to France. M. Quatrefages had reported to the Academy that, among the results of many previous injuries, especially by Italian naturalists, minute "corpuscles" had been found in the bodies of diseased silkworms and in the moths and their eggs. Pasteur, of course, suspected that these were disease-giving organisms, and directed his chief studies to them, though not without making a careful investigation of the whole disease—a task in which he proved himself to be, not only a chemist and microscopist, but an excellent clinical observer.

He soon found that, among several diseases of silkworms, the two most important were *pébrine*, which was specially called *the* silkworm disease, and was the most destructive, and another, often very prevalent, called *flacherie*. He found "corpuscles" in both, and he confirmed and widely extended the observations of those who had already studied them. But the most important facts, both for the prevention of the *pébrine* and for general pathology, were those by which he showed that it was not only contagious but hereditary. The proof of its being contagious was chiefly seen, as by others, in the inoculations of silkworms through accidental wounds, and in the consequences of their feeding on mulberry-leaves on which the disease-germs had been deposited. Many of the worms thus, or in any other way, infected soon died; in many the material for their silk was spoiled; in others, the disease continued, after, their spinning, in their chrysalides and in the moths developed from them. These moths laid diseased eggs and of these were born diseased worms, which died young, or, at the most, were spoiled for the production of silk, and so long as they lived were sources of contagion. Thus the disease passed on by inheritance from year to year. The germs in eggs laid by diseased moths survived; but those left on leaves, or in the dust, or in the bodies of dead moths, soon perished; only in the diseased and living eggs was the contagion maintained.

These things were proved by repeated experiments, and by observations by Pasteur in his own breeding-chamber, year after year; and they made him believe that the disease might be put an end to by the destruction of all diseased eggs. To this end he invented the plan which has been universally adopted, and has restored a source of wealth to the silk-districts. Each female moth, when ready to lay eggs, is placed on a separate piece of linen on which it may lay them all. After it has laid them and has died it is dried and then pounded in water, and the water is examined microscopically. If "corpuscles" are found in it, the whole of the eggs of this moth, and the linen on which they were laid, are burnt; if no corpuscles are found, the eggs are kept, to be, in due time, hatched, and they yield healthy silkworms.

Pasteur continued these studies for four years, going every year for several months to a little house near Alais, in which he watched every step in the life of the silkworms bred and fed by himself and others. His other investigations had thus been in no small degree interrupted; but worse interruption came in October 1868, when, as it seemed from overwork, he had a paralytic stroke. For a time his life was in great peril; but, happily, he recovered, and without mental impairment, though with permanent partial loss of power on his left side. For nearly two years he could do very little beyond directing experiments for repeating and testing his researches at Alais in 1869, and in Austria in 1870. Then came the French-German war, the misery of which, added to that of his paralysis, made him utterly unfit for work. At the end of the war he returned to work, and, after careful researches on the diseases of beer, similar to those by which he had studied the diseases of wines, he gave himself especially to the study of the "virulent diseases" of animals—the diseases which might reasonably be suspected to be due to different kinds of "virus" derived from different species of micro-organisms. The suspicion was already justified by what he had observed in the diseases of wine and beer, and by some more direct facts: for, in 1850, Rayer and Davaine had found organisms in the blood of animals with anthrax; and in 1865, Davaine, stirred by Pasteur's recent demonstrations of organisms much like these as agents in the butyric fermentation, had gathered evidence of the absolute dependence of anthrax on the presence of these organisms in the blood. But his observations were disputed, and his conclusions not accepted, till Pasteur proved that they were correct, and then extended his researches over a far wider range of subjects. The objects of research in this wider range included, indeed only a small proportion of the diseases connected with micro-organisms; but in a few years it was made certain that some, and probable that all, of the diseases usually classed as virulent, contagious, or specific, are due to distinct living micro-organisms, and to the products of the changes which are initiated by them in the blood or other fluids or substances in which they live. Thus, that which had often been an ingenious hypothesis of a *contagium animatum* became an accepted general law; diseases that had been called virulent were now more often called parasitic; and it may justly be said that Pasteur's researches were the efficient beginning of the vast science of bacteriology—vast alike in natural history and in pathology and in its intimate relations with organic chemistry.

Besides ascertaining the micro-organisms appropriate to several diseases, Pasteur, still working on the lines which he had followed in his studies of fermentation and of the diseases of beer and wine, found various means of "cultivating" the germs, separating them, multiplying them, and then testing their different influences on different animals, or on the same animals in different conditions, or after various changes induced in themselves. Among these changes, the most important and most fruitful in its further study were the various means of "attenuation" by which the virulence of disease-producing micro-organisms can gradually be so diminished that at last they can, without harm, be inoculated or injected into an animal which they would have rapidly killed if similarly inserted in their natural state. And some of these injections were shown to be better than harmless, for, by conveying the disease in a very mild form, they rendered the animal, for some considerable time, insusceptible of that same disease in a more severe form; they conferred an immunity similar to that given by mild attacks of the contagious fevers which, as it is commonly and often truly said, "can be had only once." Or, as Pasteur held, the inoculation with the attenuated virus was similar to vaccination, which gives protection from small-pox by producing similar disease in a milder form. Hence began the practice of "protective inoculation" for many diseases besides small-pox.

In studying the methods of attenuation, Pasteur found many facts which are not only valuable in bacteriology, but are likely to help to the knowledge of important principles in general pathology. To cite only a few examples—he found marked differences among the micro-organisms of different ferments in their degrees of dependence on air. The great majority need oxygen for the maintenance of life; but unlike these, which he named aërobic, were some anaërobic, the first examples ever known of organisms capable of living without oxygen. He showed that the bacilli of anthrax, being aërobic, soon perish and disappear in the blood of the animals that have died of the diseases due to them, and that in the same blood the anaërobic septic bacilli needing no oxygen now appear and multiply. In anthrax, also, he showed that the attenuation may best be attained by keeping the cultivated bacilli at a high temperature (about 42° C.) for a certain number of days, regulated according to daily tests of the reduction of their virulence. In the end they become incapable of killing even mice, and are protective for sheep and cattle, and other animals, which in their natural intensity they would rarely fail to kill. In chicken-cholera, the disease for which the first experiments in protective inoculation were made, he showed that the due attenuation can be obtained by a series of successive cultivations of the micro-organisms in pure air, provided that intervals of several days or weeks are allowed between each two of the cultivations in the series. In experiments on the transmission of the virus of a disease of one species through a succession of animals of another species, he showed that the virulence of the bacilli of swine-erysipelas was increased by transmission through pigeons, but diminished by transmission through rabbits. And, as to the varying susceptibility of the same animal under different conditions, a fact so commonly observed in man, he showed that chickens, which are ordinarily insusceptible of anthrax, could be made susceptible by lowering their temperature. They became again susceptible when their natural temperature was restored; and when apparently dying of anthrax in the cold, they recovered if warmed.

It was a step far beyond what had been obtained by protective inoculations when Pasteur invented and proved the utility of his treatment of rabies. Here he proved that when a virus has been inoculated or in any way so inserted that it may justly be deemed sure to destroy life, this result may, at least in the case of rabies, be prevented by a daily or otherwise gradual series of inoculations, beginning with the same virus very attenuated, and diminishing the degree of attenuation till it is used in such intensity as, without the previous graduated inoculations, would certainly have been fatal. The results of the treatment of rabies on this principle are well known; its success is certain, and is enough to justify the hope that by similar treatment, whether with virus simply attenuated, or with some "lymph" derived from a cultivated virus, or from the chemical products of its action on the liquids in which it has grown, other specific diseases may be similarly controlled. This is especially probable for those in which, as in rabies, there is a clear interval between the entrance of the virus and the first outbreak of the disease; and it is becoming very probable that tuberculosis will be one of these. But it would be useless to imagine the probabilities of what will now follow from the researches that have already followed the discoveries of Pasteur.

It hardly need be said that this summary of Pasteur's life and works, and of the chief results to which they have led, can give no fair estimate of the number and the variety of his experiments and observations. Only a complete personal study of his published works, and especially of those in the *Comptes rendus de l'Académie des Sciences*, can give this. Yet even a mere summary may indicate the most notable points that may be studied in his scientific character: of his charming personal character there is no need to speak here. Clearly, he had a native fitness and love for the study of natural science, and these were well educated, and have been manifest in his whole life. But with this loving devotion to science, he has shown not only a very rare power both of thinking and of observing, but that spirit of enterprise which stirs to constant activity in the search after truth, especially by way of experiment. With the power of accurately thinking what is likely to be true, he shows a happily adjusted ingenuity in the invention of experiments for tests of thoughts, and the habit of doubting the value of any scientific thought, even of his own, which does not bear experimental tests. Especially, the thoughts of what may be true in biology seem to have been always submitted, if possible, to tests as strict as those that may be used in chemistry and physics; and they appear to have been repeated and varied with admirable patience and perseverance whenever any doubt of previous conclusions was felt by himself or reasonably expressed by others. He has practised what he urged on his younger colleagues at the opening of the Pasteur Institute: "N'avancez rien qui ne puisse être prouvé d'une façon simple et décisive." Besides, with all his mental power and caution, we can see, in the course and results of Pasteur's work, the evidence of rare courage and strong will, and of singular skill in the use of the best means of scientific investigation. He has been chemist, microscopist, and naturalist, and has applied all the knowledge thus gained to the practical study of pathology. It is not strange that he has attained the results of which the best, and only the best, have here been told.

The honours that have been bestowed on Pasteur need not be mentioned. His chief reward may be in the happiness of seeing some of the results of his life-long work; and, indeed, very few scientific men have lived to see their work bear such good and abundant fruit. No field of biological study has in the last twenty years been so effectually studied as that which he opened, and in which he showed the right methods of research. Now, wherever biology is largely taught, the bacteriological laboratory has its place with the chemical and the physiological; and, for a memorial of the gratitude not only of France, but of many other nations, there is in Paris the Pasteur Institute, which was constructed at a cost of more than £100,000, and was opened in 1889. Here, he may not only see the daily use of his treatment for the prevention of rabies, but may observe and still take his part in the extension of the vast range of knowledge in which there has been constant increase ever since the first sure steps were made by his discoveries.

JAMES PAGET.

THE PAST HISTORY OF THE GREAT SALT LAKE (UTAH).

Lake Bonneville. By Grove Karl Gilbert. "Monographs of the United States Geological Survey," Vol. I. (Washington, 1890.)

WEST of the Rocky Mountains, inclosed by regions which drain to the Pacific, is the extensive area which bears the name of the Great Basin, for from it there is no outflow. This basin in form is rudely triangular, the most acute angle pointing southward, and its greatest length is about 880 miles. At the broader end the general elevation of the wide valleys or plains, which intervene between a series of parallel ridges, is about 5000 feet above the sea; at the narrower end the ground descends gradually till it is about on, or even below, the sea-level. Streams empty themselves into inland lakes in different parts of the Basin, the most important of these being familiar to everyone as the Great Salt Lake of Utah. This, however, is only the shrunken representative of a grander predecessor, a mere brine-pan compared with its fresh and far-spread waters. To a height of about 1000 feet above the present surface, the evidence of lacustrine wave-work and lacustrine sedimentation can still be traced, and to the lake thus indicated the American geologists have given the name of Lake Bonneville. This, in general outline, was rudely pear-shaped, but its shore-line was very irregular, a succession of jutting headlands and deep bays; its surface also was broken with islands. Its area measured about 19,750 miles, not much less than that of Lake Huron. This is now a region of arid deserts, spotted here and there with a salt marsh or a lagoon, and diversified by the Great Salt Lake and two others of smaller size. The greatest depth was originally 1050 feet, for the Great Salt Lake does not exceed 50 feet in any part. Then the waters of Lake Bonneville found an outlet at the northern end, not far away from the mouth of the Bear River, which is now the principal affluent of the Salt Lake. The rainfall then in the northern part of the Great Basin must have been much heavier than it is now; as it diminished, Lake Bonneville contracted in size and increased in saltness. The annual rainfall in this district is now only about 7 inches, while over the region between the Appalachians and the Mississippi it is 43 inches. In the latter the average moisture in the air is about 69 per cent. of saturation, in the former it is only 45; while the evaporation from the surface of Lake Michigan is only 22 inches per annum, for the Great Salt Lake it amounts to 80 inches. The level of the water in the latter is subject to oscillations, dependent partly on variations in the rainfall, partly on the results of extended cultivation, and appears likely in the future to fall somewhat below its present height.

In the disappearance of the ancient lake, epochs—sometimes perhaps rather long—of stability appear to have alternated with eras of change; at any rate, shore-cliffs, terraces, spits, and bars of detritus are very distinctly grouped at intervals above the present water-level. Owing to the scanty rainfall, and the absence, until late years, of any attempt at cultivation, these natural features are preserved with unusual distinctness. In the admirable plates by which the memoir is illustrated, we can see the enclosing hills, bare and arid, but furrowed into a thousand gulleys by the transient storms of myriad years; the wave-worn cliffs which overlooked the margin of the vanished lake; the long shelving slopes which formed its bed; the water-worn *débris*, here accumulated in a long spit, and indicating the general set of the waves; there piled up in a bar, which now runs, like a railway embankment, from headland to headland across the opening of a bay. The plates in themselves are an object-lesson in physical geography.

The origin of the basin of Lake Bonneville, as of all other large lake basins, is undoubtedly, as Mr. Gilbert points out, deformation of the earth's crust, or *diastrophism*, as he proposes to call it. But there is evidence to show—and this is a point of much interest—that during the process of desiccation, this crust has not remained absolutely at rest; these "bench marks" afforded by the lake margins have undergone movements which are not uniform in amount. They are found, on examination, to exhibit variations amounting in some cases to about 350 feet in altitude. Faults, also, may be traced for considerable distances which are later in date than the desiccation of Lake Bonneville. These in places are made very distinct by scarps, crossing lines of terraces or alluvial fans, and facing outwards towards the lower ground. These faults, however, do not indicate any great displacement. The maximum throw does not exceed about 60 feet, and it is often less. Certain localities also have been disturbed by volcanic eruptions. These have occurred before, during, and since, the epoch of the greatest extension of Lake Bonneville. Craters of scoria, as well as flows of lava, remain as monuments—the former occasionally three or four hundred yards in diameter, the latter sometimes a little more than three miles long; the materials are all basaltic. Rhyolitic lavas also occur in the region, but these are long anterior in date to the epoch of the lake. Organic remains are not common in the marls and other deposits of the old lake-bed. This is not surprising in the later period of its history, but they might have been expected in greater abundance in that when the waters were still fresh. The earliest deposits do not carry us back beyond the Pleistocene age, so that, geologically speaking, both the formation and desiccation of the lake are modern events.

This is a bare outline of the last pages of the story of a remarkable district in America, the like of which can be found in more than one other locality on the earth, though, perhaps, in none of them is the record so clearly preserved. Mr. Gilbert's memoir is not only a most careful description and full discussion of the various phenomena presented by this singular dried-up region, but also he turns, not seldom, to questions of wider import, on which, however, want of space forbids us to touch. Moreover, the second chapter of the volume is occupied by a very full discussion of the various topographical features of lake shores. Perhaps in this the author errs occasionally on the side of prolixity, but he brings together so much valuable information that the book will be indispensable to all who wish to study the history and phenomena of lakes and inland seas. We lay it down with a deep sense of gratitude to him for the loving labour which he has evidently bestowed upon this memoir, and will only add that, high as the standard already attained by the publications of the American Geological Survey may be, this monograph, especially in the work of the printer, and in the number, interest, and excellence of its illustrations, more than attains to it. T. G. Bonney.

ON DUCKS AND AUKS.

On the Morphology of the Duck and Auk Tribes. By W. Kitchen Parker, F.R.S. With Nine Plates. (London: Williams and Norgate, 1890.)

When the grave, a few months ago, closed over the remains of W. Kitchen Parker, there still remained with those who knew him the memory of an excellent man and brilliant anatomist. None save one devoted to the science could have worked on as he did, often amid many cares and troubles, feeling a high delight in his work, and considering the attainment of knowledge to be its own reward. From a very early period of his life all his spare moments were devoted to anatomical research, and the hour of death overtook him while as yet, though full of years, he was labouring still.

One of his latest, if not his last work, lies before us. It treats of the morphology of the Anatidæ and the Alcidæ, and has been published by the Royal Irish Academy, as one of its "Cunningham Memoirs." It may be necessary to add that these memoirs are published from the resources of a fund left to the Irish Academy by a Mr. Cunningham, and that great care is taken that the memoirs published therein shall be of a high order of merit.

Most of the materials for this memoir had been in the late Dr. W. K. Parker's hands for many years, but those which he needed to complete his investigations had been only obtained during the last few years from several friends. In a brief introduction he states that for many years after the publication of his first two papers on the osteology of birds (1850-60), his attention was directed solely to the skull. The burden of the anatomy of the skull was placed upon his willing shoulders by Prof. Huxley, who then by degrees tempted him into the investigation of the organs of support, which have proved to be of as great interest and profit as the anatomy of the skull itself.

The two families of birds whose morphology is treated of in this memoir are very distantly related, and the true position and genealogy of the duck tribe present as tough a problem as those of the auk tribe; indeed, herein is enough, Mr. Parker writes, to task the ingenuity and strength of two or three generations of biologists. The cranium in Cygnus and its vertebral column are described from an early stage; the wings of Cygnus and Anas and the hip-girdle of Anas in various stages of development are noticed. Among the auks, *Uria troile* has been selected for description.

In a summary of nearly seven pages, it is pointed out that the Anatidæ manifestly converge towards the Gallinaceous group; that they have the Struthious division of the Ratitæ obliquely below them; whilst the Alcidæ are related to a large and varied group of existing families, but in their ancestry belong somewhere between those two extremely dissimilar extinct families, the Hesperornithidæ and the Ichthyornithidæ. The revelations made by the precious remains of those two toothed types throw a bright light on one side of these questions of origin and relationship, but intensify the darkness of the other side.

Though both groups are adapted for an aquatic life, they are very sharply defined from each other. The ducks are more or less terrestrial, but are also swimmers; while the auks are not adapted for a land life, but are at home and at ease in the denser or rarer medium—they can dive and fly.

From the ontological standpoint, it will be conceded that that which has dominated the whole bird form is the wing, and embryology shows that this is merely the modified fore-paddle of a low gill-breathing amphibian—a nailless fore-paw. But the nails or claws do appear; yet, in the wing, they are out of place; and this reptilian stage is only transient. If the bird is, indeed, the child of the reptile, it must forget its father's house; it must proceed beyond its progenitor. But if we are willing to see the bird's wing grow, not out of a perfect and typical cheiropterygium, but out of an ichthyopterygium in an unsettled state, ready for transformation into the higher type of limb, then the difficulty is solved. It was a fish paddle; it was not to become a fore-foot; it did change into the framework of a bird's wing; in that respect it is a perfect thing; as a paw, it is an abortion. But an organism moves together in all its parts, if it moves at all; and thus we see that, in correlation to the profoundly modified fore-limb, every other part of this feathered creature has suffered changes.

The whole memoir is devoted to a detailed account of the changes which are thus brought about during this beautiful metamorphosis, the interest of which is increased by the peculiarly fascinating manner of their description, and to which in a brief notice it would be impossible to do proper justice.

OUR BOOK SHELF.

A Dictionary of Metric and other Useful Measures. By Latimer Clark. (London: E. and F. N. Spon, 1891.)

THIS dictionary will be found to be a most valuable and useful *vade-mecum* by all those who have occasion to employ metric and other physical measures. The arrangement of the tables in this form is the most convenient that could have been adopted, and for uniformity and facility of reference could hardly be excelled. One great feature, which is generally lacking in ordinary sets of tables, is the setting forth of the relations of the different metric units to each other: thus, for instance, on looking under the heading gramme-centimetre, we find its equivalent in kilogramme-metres, foot-grains, foot-pounds, joules, ergs, &c., while the latter are indexed under their respective titles. Not only have the French measures with their factors for conversion into British measures been given, but physical, electrical, and other modern units which are so numerous and indispensable.

With regard to some of the fundamental units we may mention that the value of the cubic inch of water, adopted here, is that which "was recently determined with great care by the Standards Department of the Board of Trade"; and in consequence of its being recently legalized, the values of the cubic foot, gallon, &c., have been revised. Throughout the work the logarithms of all the chief factors have been inserted, and at the end there is a short table of logarithms and anti-logarithms adapted for use with any number of figures up to five.

A Text-book of Geometrical Deduction. Book I., corresponding to Euclid, Book I. By James Blaikie and W. Thomson. (Longmans, Green, and Co., 1891.)

THIS work forms an excellent supplement to the first book of Euclid, and by its means a systematic course of training in the art of solving geometrical deductions can be obtained. The arrangement adopted is good, and of a very progressive character. The propositions are divided into sections, and each section is subdivided into three parts: in the first a deduction is worked out in full to serve as a guide to the student; deductions similar to the one already mentioned then follow, in which the figures are in each case given and such notes as are deemed necessary for a beginner. In the last part no figures or notes are added, but occasionally references are given to the propositions on which the proofs depend. The deductions in the last two parts should be written out by the student, and the proofs made to depend on the preceding propositions of Euclid. Additional parts, corresponding to the remaining books of Euclid, are in preparation, and if they are up to the standard of the present one, the series will be found generally useful.

Elementary Algebra. By W. W. Rouse Ball. "Pitt Press Mathematical Series." (Cambridge: University Press, 1890.)

IN this book all those parts of the subject which are usually termed "elementary" are dealt with. It is a sound and well-written treatise. No deviation of importance has been made in the general order of arrangement that has been lately adopted, but many articles and examples which might profitably be left for a second reading have been marked with an asterisk. Permutations and combinations, the binomial theorem and the exponential theorem—subjects which are sometimes included in an elementary treatise, and sometimes excluded—have here only been lightly touched upon, and will serve as an introduction to the more detailed discussions contained in more advanced text-books. Numerous examples are interspersed in the text of each chapter, and here and there are papers and questions that have been set in various examinations. The table of contents is fuller than usual, and will enable the student to find readily any particular article to which he may wish to refer.

A Ride through Asia Minor and Armenia. By Henry C. Barkley. (London: John Murray, 1891.)

THE "ride" described in this book came off in 1878, but the author writes so brightly that only very exacting readers will complain of any lack of freshness in his narrative. His journey from Constantinople occupied ninety-six days, of which fifty-three were spent in the saddle. He rode fourteen hundred miles, the average distance done each day being about twenty-two and a half miles; and, says Mr. Barkley, "if the miserable mountain roads are taken into consideration, I think this was very fair work for a lot of ponies." Apart from the personal incidents of the journey, Mr. Barkley was interested chiefly in the character, manners, and customs of the inhabitants of the districts through which he passed; and on these subjects he records a good many acute observations. It is worth noting that he speaks in high terms of the spirit of hospitality displayed in the parts of the Turkish dominions he has visited. Of course, the Turk is most hospitable to the Turk, and the Christian to the Christian; but "it often happens that the Turk receives the Christian as his guest, and the Christian the Turk." If a respectable traveller finds a want of hospitality on the part of either Turk or Christian, Mr. Barkley cannot but think it is the traveller's own fault.

LETTERS TO THE EDITOR.

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Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

WITH regard to Mr. Bottomley's criticism, I should like to add to what Prof. Rücker has said that Prof. Van der Waals's book is not in any sense a *treatise* on the continuity of the liquid and gaseous states, but a *thesis* wherein is put forward the author's own work on which he claims a doctor's degree.

The preface explains that, in the attempt to determine the value of one of Laplace's capillary constants, the author was forced to proceed by theory, and that the course of these theoretical investigations led him to see that there must be continuity between the gaseous and liquid states. He was, in fact, led to his well-known characteristic equation for a substance in a *fluid* state, an equation in no way depending on the character of the fluidity.

This characteristic and its application are for this thesis the important things, and Prof. Van der Waals proceeds therefore to show that the results deducible from it are in complete agreement with Dr. Andrews' experiments and Prof. James Thomson's suggestions. It is not a point with him to discuss the question of continuity except as bearing on his characteristic; but this continuity is doubtless taken for the title of the thesis as being the most important deduction from his theory.

There is no question of priority: the author gives full information as to where the experiments bearing on the subject are recorded, and only claims to have shown this continuity as a consequence of known laws.

Prof. Van der Waals has been unfortunate in that the English dress in which his thesis appears is a translation from a translation. A literal rendering would have shown that he took his descriptions and diagrams from Maxwell's "Theory of Heat" because this is a "little book which is certainly in the hands of every physicist": it would have prevented the insertion of that footnote on p. 416 alluded to by Mr. Bottomley, since the text runs, "That Maxwell joins the points C and G by a straight line I do not think happy. It is apt to lead off the track and not on to it." The first of Mr. Bottomley's quotations—and with this, I might add, the scientific part of the preface of the original concludes—should read: "These considerations have led me to perceive continuity between the gaseous and liquid states, the existence of which, as I saw later, had been already surmised by others." *Vermoed* (surmised) certainly seems a weak word in the light of Dr. Andrews' experiments, but it may possibly point to an earlier date for Prof. Van der Waals's theoretical conclusion than that of his thesis.

Christ Church, Oxford. ROBERT E. BAYNES.

The Flying to Pieces of a Whirling Ring.

DR. LODGE having set the ball of paradox rolling, perhaps I may be allowed to point out some of the paradoxes of his critics on the subject of revolving disks, of the well-known "grindstone problem." Prof. Ewing refers to two treatments of this problem, which, however, stand upon quite different footings. Prof. Grossmann's discussion reduces the problem to one in *two-dimensions*, and leaves an unequilibrated surface stress over both faces of the disk. Even if the disk be moderately thin, the solution cannot be considered satisfactory till the *degree of approximation* has been measured by comparison with the accurate solution of the problem. But Grossmann's method is precisely that of Hopkinson (*Messenger of Mathematics*, vol. ii., 1873, p. 53), except that the latter has dropped by mischance an r in his equation (1) [or Grossmann's (6)]. This slip I pointed out in 1886; and Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations. Between Hopkinson and Grossmann this theory has several times been reproduced in technical books and newspapers without comment on its want of correctness. Such first-class technical authorities as Ritter and Winkler have also given quite erroneous solutions of the "grindstone problem."

Prof. Boys refers to Clerk Maxwell's solution. Unfortunately the editor of his scientific papers has given no word of warning about the difficulties of that solution. It involves the paradox of an equilibrated shearing stress on the faces of the disk, and this stress is comparable with the stress which Maxwell supposes to burst the stone (see "History of Elasticity," vol. i. p. 827). Thus both the solutions suggested by Profs. Ewing and Boys suffer from the same defect of unequilibrated stress on the faces. Their difference leads to the fact that Maxwell's causes a hollow disk to burst first at the outer rim, and Grossmann's at the hole.

The solution by Mr. Chree, to which Prof. Ewing refers, seems to me to lie on a higher plane than the other two, and to have been better worth reproducing than Grossmann's, although it cannot be considered as final. Mr. Chree recognizes that for his form of solution normal stresses over the faces of the disk would be necessary, and he proceeds to find their values. Grossmann failed to notice this paradox of his supposed solution, and therefore gives *no measure of the amount of its error*. Some years ago Mr. Chree kindly provided me for lecture purposes with a solution of the disk problem in which the stress on the disk face was zero over a circle of given radius. This was a closer approximation to the facts of the case, but as the stress was still unequilibrated at other points of the face the solution was not of course final.

If all these solutions are therefore paradoxical, where is the correct one to be sought? I fear it has yet to be worked out. Some progress can easily be made with it. It involves four series of Bessel's functions, two of either type, but the surface conditions lead to equations so complex that they will, I think, puzzle the ingenuity of our best Cambridge analysts. When solved, the work to be of practical value must be reduced to numerical tables and not left in the form of infinite series—a type of solution of elastic problem which is so common and yet so technically useless. An Italian has recently solved, by a finite number of definite integrals, the problem of the elastic spherical shell under given surface forces: possibly something might be done for the grindstone problem in the same direction. At any rate, my object in writing to NATURE is to point out that the solutions referred to by Profs. Ewing and Boys are incorrect, and to express a hope that no competent analytical elastician will, owing to these paradoxical solutions, hesitate to try his hand at a very important problem. I am quite certain that no real solution (the paradoxical are myriad) exists prior to 1860, and pretty nearly certain that none has been achieved since, although my bibliography of papers on the strength of materials for the last twenty years is not so complete as I could wish.

University College, March 20. KARL PEARSON.

Deductions from the Gaseous Theory of Solution.

FROM the gaseous theory of solution, Prof. Orme Masson has concluded (see NATURE of February 12, p. 345) that there must be some temperature above which two mutually soluble bodies will be infinitely soluble in each other. This, no doubt, is a fact, and it may be interesting to show that precisely the same conclusion can be drawn from the hydrate theory of solution.

Take first the case of a solution from which a solid separates on cooling. The body which separates, say solid water, does so owing to the tendency of its molecules to coalesce and form solid aggregates; and their tendency to do so is, we know, increased by lowering the temperature: on introducing any substance which possesses an attraction for the water molecules, the attraction of these for their fellows will be in part counterbalanced, and to get them to coalesce a lower temperature will be necessary, and the lower will this temperature be, the more foreign substance there is present; thus the freezing-point of the water will fall as the amount of, say, any salt present in it is increased, as in ADC, Fig. 1. Similarly, if

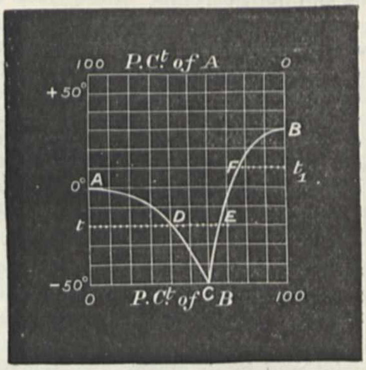


FIG. 1.

we start with the pure salt at B, its freezing-point will be lowered by the addition of water, giving us a curve such as BFEC, which meets or cuts the first curve at some point C—the miscalled cryohydric point. This is precisely what does occur; the woodcut in fact represents the crystallization of water and the hexhydrate of calcium chloride from solutions of this salt, and may be taken as a typical example of the figures obtained in all cases. A solution of the composition D will be the one containing the most water of any which can exist at the temperature t, while E is the one containing the most salt at this temperature, all solutions of intermediate composition being capable of stable existence at t. At t_1 any solution weaker than F will be able to exist, since there is no inferior (*i.e.* for weak solutions) limit of stability, while above B there is neither superior nor inferior limit, and the two substances will be infinitely soluble in each other.

The same general results will obtain when the substances separate on cooling in the liquid instead of the solid condition, but they may be expressed in another form. From Fig. 1 we see that the maximum amount of B which the liquid A can hold at different temperatures is represented by CB, and that this maximum increases with the temperature; it may be represented by AC, Fig. 2; similarly, the maximum amount of A which B can contain is represented by CA, Fig. 1, or BC, Fig. 2, and we thus get in Fig. 2 a double curve which shows that at any temperature below C, such as t, the two substances on being mixed will form two solutions of the composition D and E respectively, whereas at and above C they will form but one homogeneous liquid, their mutual solubility being infinite. This figure is similar to that for aniline and water reproduced by Prof. Orme Masson in Fig. 1, p. 347.

In the above I have purposely avoided using the terms solvent and dissolved substance, since there is much confusion as to their meaning, and, indeed, it is perhaps impossible to differentiate them: when talking of the freezing-points, that substance which crystallizes from the liquid is generally termed the solvent, whereas, when talking of solubilities, the crystallizing substance is termed the dissolved substance.

The want of sound logic displayed in the arguments of the advocates of the gaseous theory of solution, must, I think, be a matter of surprise to many. Their chief argument is this: so-called osmotic pressure is (roughly and with many palpable exceptions) numerically equivalent to the gaseous pressure which the dissolved substance might be expected to exert if it could be gasified; *therefore*, the dissolved substance is a gas. They forget that the osmotic passage of water through a membrane in order to arrive at a solution on the other side, though it might be caused by the dissolved substance bombarding the membrane which it cannot penetrate, might also be caused by other means, such as an attraction of the solution for more water, or by the effective pressure of the solution being less than that of pure water. Surely before building up such a vast superstructure on the foundation of the gaseous nature of the dissolved substance, it would be well to see whether that foundation is real or imaginary. If the dissolved substance is truly gaseous, and if the

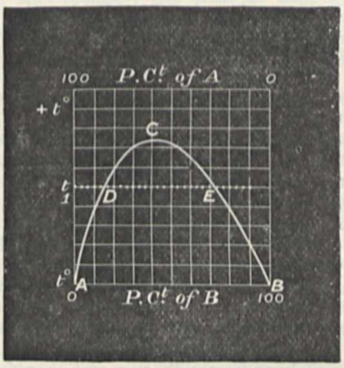


FIG. 2.

solvent, as Prof. Orme Masson says, only "plays the part of so much space," then the dissolution of a gas at constant pressure in a liquid should neither evolve nor absorb heat, but this, I can confidently assert, is not the case: if, again, the so-called osmotic pressure is due to gaseous bombardment, it will be $\frac{1}{2}mv^2$, and we should be able to deduce it from independent measurements of m and v, whereas preliminary experiments of my own lead me to feel fairly certain that $\frac{1}{2}mv^2$ does not give the osmotic pressure. Surely these are fundamental points, the investigation of which should have been the first duty of the advocates of the theory. The flimsy nature of the foundation will account for the number of props which the building requires. The osmotic pressure is not a constant independent of the nature either of the solvent or of the dissolved substance, it is not a rectilineal function of the concentration, and the solvent most palpably does not act as "so much space." The theory has, consequently, to be bolstered up on the side of weak solutions by a supplementary theory of dissociation into ions (with all its inconsistencies), and on the side of strong solutions by the never-failing resource of the destitute—the disturbing influence of hydrates and molecular aggregations.

Van't Hoff started his theory by talking of osmotic phenomena as due to the attraction of the solution for more water. The proximate numerical equivalence of this attraction to gaseous pressure soon caused the substitution of "pressure" for "attraction." The latter has now been entirely lost sight of, and "pressure" has become a catch-word which has blinded his followers to the most patent facts, and has led them to press the supposed analogies of gaseous and osmotic pressure to the most absurd consequences. Prof. Orme Masson's address contains the latest development in this direction. He says, "Imagine, then, a soluble solid in contact with water at a fixed temperature. The substance exercises a certain pressure, in right of which it proceeds to dissolve. This pressure is analogous to the vapour pressure of a volatile body in space, the space being represented by the solvent; and the process of solution is analogous to that of vaporization." Now what can be the meaning of these sentences? What sort of pressure is it that a stable solid exercises? It is not ordinary vapour pressure, for that, where it does exist, does not render a solid soluble, and, indeed, we are told that it is only "analogous to vapour pressure," nor can it be osmotic pressure, for that is a property confined exclusively to (supposed) gases in solution; it can only be some novel property of solids which has yet to be revealed to an expectant world. This, I believe, is the only attempt which has been made to explain on the physical theory *why* a substance dissolves at all, why the solvent which, it is said, has no attraction for it and acts only as "so much space" should not only perform the mighty work required to liquefy and gasify a solid, but should also be able to retain it as a gas under enormous pressure; and if the physical theory is capable of giving no more satisfactory explanation of *the* fundamental fact of dissolution than the above, the sooner that theory is abandoned the better.

Harpenden, March 2. SPENCER U. PICKERING.

Co-adaptation.

THERE is one point in Prof. Meldola's review of Mr. Pascoe's book on the origin of species touching which it seems desirable that I should say a few words. The matter is introduced by the following passage:—

"Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—'On the converting "an ordinary hoofed quadruped" into a giraffe, Mr. Romanes observes: "Thousands and thousands of changes will be necessary." . . . "The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters." The chances of such changes are "infinity to one" against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident.' I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it."

The "dissected" sentences here referred to have been taken from an article on Mr. Wallace's "Darwinism," which I published in the *Contemporary Review* for August 1889. It is, perhaps, needless to say that the "pleonasm" does not occur in the original, and that I do not there hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, but likewise entertained such aid to natural selection as of "importance" in other cases where the phenomena of "co-adaptation" are concerned. Whether or not he underrated the power of natural selection in regard to such cases, it is in my opinion too early to dogmatize. But I am quite sure that "the well-known difficulty" in question cannot be met by the "Neo-Darwinians" with any appeal—explicitly or implicitly—to what is here the false analogy supplied by artificial selection. For example, suppose that there are n different parts which are required to vary, each in one particular way, but all to vary together in the same individual, if any of the variations is to confer an advantage in the struggle for existence. Suppose, further, that there is nothing but "chance" to lead to the simultaneous variation of all these parts in the same individual. Upon these data it is sufficiently evident that the happy combination would not occur with sufficient frequency to admit of being perpetuated in progeny—even if n be only equal to 4 or 5. Now I say that this "difficulty," be it great or small, cannot be met by what Mr. Wallace has called "the best answer"—namely, "the very thing said to be impossible by variation and natural selection has been again and again effected by artificial selection." For there is no "difficulty" at all in understanding how artificial selection is able to choose the separate congenital variations A, B, C, D, &c., as they severally occur in different individuals, and, by suitable mating, to blend them all in a single individual. Here the "selection" is *intentional;* and therefore the whole ground on which the "difficulty" stands is absent. This ground is the supposition of *fortuity,* with regard (*a*) to *all* the variations A, B, C, D, &c., happening to occur in any one individual to begin with, or (*b*) being afterwards preserved (by suitable mating) from obliteration by free intercrossing. Therefore, thus to appeal explicitly from natural selection to the analogy of artificial selection is to be cheated by a metaphor.

How, then, does it fare if the appeal be made implicitly, as in Prof. Meldola's review, by supplying *utility* in the one case as corresponding to *intelligence* in the other? Obviously, here again, the element of *fortuity* is ignored, and therefore, as previously, the "difficulty" is not met, but evaded. For no one who believes in natural selection could deny, that if *each* of the variations, A, B, C, D, &c., is of advantage *per se*, they would all be preserved as they severally happened to arise in this, that, and the other individual, till, by general intercrossing, they would eventually coalesce in single individuals—as in the case of artificial selection. But all this is quite wide of the mark. Indeed, intercrossing is here a necessary condition to, instead of a fatal impediment against, the blending of co-operative modifications; and therefore Mr. Spencer would have been a fool had he brought his "difficulty" to bear upon this case. This case, however, is not that which is meant by "co-*adaptation*": it is the case of a confluence of *adaptations.* Or, otherwise stated, it is not the case where adaptation is *first initiated in spite of intercrossing,* by means of a fortuitous concurrence of variations, each in itself being without any adaptive value; it is the case where adaptation is *afterwards increased by means of intercrossing,* on account of the blending of variations each of which has always been of adaptive value in itself.

The "difficulty," therefore, remains just where it was before; and the only way of meeting it is to show that the phenomenon of co-adaptation does not occur in nature. In other words, it must be shown that the difficulty is fictitious, by showing that, as a matter of fact, there are no cases to be found where *n* modifications, each being useless in itself, become useful in association. Whether or not the difficulty does admit of this the only rational solution, I will not occupy space by discussing; but I have thought it desirable to state what I have always understood to be the real nature of Mr. Spencer's "well-known objection."

Oxford, March 10. GEORGE J. ROMANES.

Neo-Lamarckism and Darwinism.

IT has been sometimes said that it is difficult to tell the difference between the supposed effects of the environment upon an organism, and the accumulation of favourable variations. There can be no difference; for they are but two explanations or theories to account for the same thing. A species is characterized by certain features; it is *these* which have to be accounted for; and any number of theories may be propounded as to the cause. It is simply a question as to which can be "proved" to be either the most probable or actually true.

At one time it was thought satisfactory to account for everything by a direct creative act. A man is exactly the same, whether he was created as he is, or evolved from animals; and if evolved, whether by the direct action of the environment or by natural selection or any other way. We may say with Burns, "A man's a man for a' that."

It is also said that the value of a theory depends upon the number of phenomena it can satisfactorily explain or account for. This is not altogether the case. The theory of creation accounted for everything; but we have abandoned it, nevertheless. The value of a theory really depends, not so much on what it can explain, as upon *the number of facts* on which it is based.

Now, are not many theorists forgetting the importance of this? I have just read Mr. Cockerell's paper on the "Alpine Flora" (NATURE, January 1), which will illustrate my contention. He has studied the flora of the mountains of Colorado, and finds that, as a whole, the plants are characterized by certain features. These are the same as are noticeable, not only on European and the Rocky Mountains, but in Arctic and Antarctic regions as well. He comes to the conclusion that "If this [lack of nourishment] were the only cause of dwarfing, the Alpine flora would present clear evidence for the transmission of acquired characters, as the character has undoubtedly become a *specific* one in several mountain plants." He here alludes to *one*, viz. a dwarf habit. The cause, however, which he gives is not the only one, nor is it in this case probably always the right one. If it were, then all mountain and all Arctic and Antarctic regions must have poor soils, for which there is no evidence. All these regions, however, have a relatively lower temperature.

Here, then, we have *two coincidences* of *universal application*—a dwarf habit and a low isotherm. Now we all know from experience how suddenly cold weather instantly checks growth in spring, &c.; therefore, we can infer, or draw the deduction, that the constantly low temperature of the Alps and Lapland perpetually check growth in those regions.

This alone would be a perfectly legitimate conclusion; as the probabilities of there being a distinct cause and effect underlying these coincidences are so great as to amount to a "moral conviction" of the truth.

Though this is logically sufficient, the deduction has been "verified by experiments." When seed is gathered from Alpine plants and sown at low altitudes,[1] and *vice versâ*, the plants raised after a few years begin to assume the characters, respectively, of the same species which are natives of the places.

Now the argument is complete.

The preceding facts, therefore, warrant one in stating the theory thus: "That Alpine plants have acquired their special characteristics, by the responsive power of their protoplasm under the influence of their environment."

Having lived generation after generation under that same influence their characters have become relatively fixed, hereditary and "specific" as Mr. Cockerell believes. Such plants, however, probably never lose the power of changing again, as experiment shows.

To this scientific explanation Mr. Cockerell superadds the theory of natural selection. He endeavours to explain how natural selection "may" come into play as well. He says:—

"(1) They may escape the violence of high winds which prevail at those altitudes; taller plants being broken off before the seed matures."

Instead of appealing to facts, as he did before, he now begins with an hypothesis. Has he ever seen a taller plant broken off (as often occurs at lower altitudes)?

A mere suggestion is scientifically of little value, unless it be founded upon something which actually occurs.

"(2) They may obtain some additional warmth from their close proximity to the ground and partial shelter."

Here is a remark which ought to have been tested experimentally before being given to the world. Why should not a close proximity to the ground give a chill as well as, or instead of, warmth? As a fact, radiation at night begins on the ground, as the presence of hoar-frost tells us; and therefore we might ask, Are not dwarf plants just as likely (*a priori*), if not more so than tall ones, to suffer as well as to be benefited?

To what *facts* does "partial shelter" refer? Alpine plants are particularly exposed.

"(3) The short summer of the mountain tops necessitates very rapid development; and requires every energy to be thrown into the essential function of producing flowers and seed, leaving nothing to spare for the production of branched stems and diffuse foliage."

This seems like putting the cart before the horse; for how can a seedling plant know that the summer is going to be very short, and that it must, therefore, put forth all its energies? If it understood its own functions, it would know that the flowering depends entirely on the foliage; and, since M. Bonnier, for example, has shown[2] that the chlorophyllous tissue is increased in Alpine plants, this justifies us in looking to it as probably a sufficient cause of Alpine plants having fine flowers.

Finally, has Mr. Cockerell observed any plants which have "failed in the race, and so have been ruthlessly cut off by the autumn storms?" If so, will he give examples? If not, I would refer him to the paragraph italicised above.

To refer once more to the difficulty mentioned above. It must not be forgotten by those who feel that difficulty, that, while the action of the environment on plants is a thing which can be tested, and in many cases admits of easy proof by experiment, the accumulation of many useful variations which mark any living species must ever remain an *a priori* assumption, which is absolutely incapable of verification.

Cairo, February. GEORGE HENSLOW.

[1] As by M. Bonnier, see ref. *infra.*
[2] *Bull. Soc. Bot. de Fr.*, 1886, p. 467.

Formation of Language.

SEVERAL years ago, being interested in speculations on the deve opment of language, and having a son a few months old, I instituted a series of minute observations on the part of the entire family as to his utterances. The result, curious at the time, has received a new interest from a later observation. The nursery maid who had charge of the boy did not understand a word of English, Italian being the language spoken with the domestics exclusively. The first articulations of the child were evidently meaningless mimicry of what he heard from us, and it had so much the character of English speech that the maid supposed he was speaking English. There was no attempt to catch or repeat any word—only a gabble, a gibberish, in which we were not able to detect any resemblance to any word of any language. This continued for several weeks, when we perceived that he began to repeat certain sounds to which we found that he attached definite meaning, and as this progressed he left off his incoherent imitation of our language, and he soon had coined a small vocabulary for himself, comprising words for bread, water, milk, &c. The first word we distinguished was as nearly as I can render it "bhumbhoo," meaning water. This phase continued some weeks also, when he began to couple our words for his objects with his own—as "bumbhoo-aqua," when he wanted water. Little by little he dropped his own words and began speaking only Italian. The three stages of the development of language were perfectly distinguishable, but I supposed that the words the child contrived were purely arbitrary, and am inclined to think so still; but during a late visit to Greece I went over to Crete, and visiting in the family of an old Cretan friend, I was interested in a little boy—his young son—who was in the state of development of speech which I have noted in ours as the second. He had only got two or three words, but that for water was precisely the same as that which my own little boy had invented. Have any of your readers who have the watching of child-talk made any analogous observations?

Rome, March 15. W. J. STILLMAN.

Force and Determinism.

IN case any philosophers who do not happen to be physicists feel a doubt about the orthodoxy of what I understand to be one of the main doctrines in Dr. James Croll's recent book, reviewed in your issue of March 12 (p. 435), viz. that although expenditure of energy is needed to increase the speed of matter, none is needed to alter its direction (and the doubt has been already expressed to me); perhaps it will not be regarded as intrusive if I say that this statement is perfectly correct.

Determining the direction of motion involves no expenditure of energy or performance of work. Energy may be guided along desired channels without altering its quantity in the least—just as can matter. The rails which guide a train do not propel it, nor do they necessarily retard it; they have no essential effect upon its energy except a guiding effect. A force at right angles to motion does no work.

It is a function of living organisms thus to direct the path of transference of energy, but they add nothing to its quantity. There is no more energy in a live animal than in a dead one—in a lighted fire than in one ready to be lit. There is activity of transference and transformation in the one case, and stagnation in the other; but the law of conservation has nothing whatever to say against a live animal, or a mind, controlling the motion of molecules; although it would have everything to say against motion being produced *de novo* by an act of will. Life is not energy, it is a determiner of the paths of energy. That is its natural and principal function: it is a director, not a worker. Food and fuel work: life directs. It has control over triggers and sluice-gates. It is not the main-spring of the clock, it is the touch which sets it going. Its best analogue is flame: life is the spark which ignites a conflagration.

The distinction between generating motion and directing motion is evidently one useful to remember. If anyone has thought that an arbitrary alteration of, say, the weather, would necessarily involve a contradiction of the principle of conservation of energy, I think I am right in saying that he has been mistaken.

OLIVER J. LODGE.

Modern Views of Electricity.

MR. BURBURY asks for an explanation of the permanence of the atomic charges in air films, but this carries the question further than I can follow it.

My suggestion is simply that the chemical attraction of zinc for oxygen is necessarily accompanied by + electricity on the zinc and − on the oxygen. The permanence of the charge is, on this view, bound up with the permanence of chemical affinity. It is perhaps only completely to be explained by a knowledge of the mechanism of the latter; and that is one of those "ultimate problems" which I was careful to avoid in my letter.

I fancy, however, that Mr. Burbury has not quite followed me in one point. There is to be no actual combination of zinc with oxygen—only a tendency thereto; and it is to this tendency that the polarized condition of the molecular chain is due.

University College, Bristol. A. P. CHATTOCK.

CHEMICAL SOCIETY'S JUBILEE.

WE have already given the address of the President, Dr. W. J. Russell, at the afternoon meeting. Sir Lyon Playfair and Sir William Grove, two of the five remaining original Fellows of the Society, at the same meeting recited their recollections of the state of chemistry at the time of the foundation of the Society, and we now reproduce their speeches, as they forcibly serve—that of Sir Lyon Playfair in particular—to bring home to us the great changes which have taken place during the fifty years.

At the dinner at the Hôtel Métropole on the Wednesday evening, the Marquis of Salisbury delivered a speech remarkable for the emphasis which he laid on the importance of the work done by the Society in cultivating the higher study of chemistry rather than its industrial applications, and it is noteworthy that Sir William Grove had on the previous day expressed his preference for the abstract rather than the applied side of the science. Such a consensus of opinion is most significant and hopeful. Sir Lyon Playfair, in responding to the Marquis of Salisbury's speech, showed that he was fully aware of the latest discoveries, and able to appreciate their high theoretic import.

At the afternoon meeting Sir Lyon Playfair said:—

It is a sad feeling that there are now living among us only five of the original founders of the Chemical Society. I am one of those five, and have therefore been selected to address a few words to you to-day. You have learned from the excellent discourse of our President that before 1841 chemistry was being both rapidly developed and rapidly evolved. New methods of research were being created; organic chemistry had almost been created. There were many luminaries in the chemical firmament all over the world at that time, and if I mention a few names they will appear to many of you as milestones representing mere discoveries and progress, though they are names well known to the older members of the Society and the few founders who are left as strong personalities with whom we connect much kindness, hospitality, and encouragement. Liebig was then *facile princeps* chemist of the world. He formed a school, and showed how to advance chemistry by original research. At that time, in 1841, the year of our foundation, his brilliant pupil Hofmann had scarcely risen above the horizon. Kopp and Bunsen had made researches, but were still young. There were in Germany names of the highest importance in our science: at Göttingen there was Wöhler, the dear friend of Liebig, and associated with him in his work; in Berlin there was Mitscherlich, the aristocrat of chemistry; there was Rose, the most lovable of our fraternity, who had raised analysis to a high platform by improving methods of research; there was Dove, the jolliest of companions, who had joined physics to chemistry; and lastly, there was Rammelsberg, who took mineralogy out of the domain of physics, and made it part of the domain of chemistry. In France, at that time—I speak only of those whom I personally knew, and whose friendship has ever been valuable to me—there was a man who died only the other day, but who was a veteran then, and famous for his researches on the fatty bodies, Chevreul; there was Balard, the discoverer of bromine; there was

Baron Thenard, the king among lecturers; there was Dumas the eloquent, who established the doctrine of substitutions; and there were other good workers who had not yet acquired the reputations which they afterwards gained —men like Pelouze, Fremy, and Regnault. These were the great luminaries on the Continent; but whom had we at home? There was my old teacher, and to all old chemists devoted friend, Graham, who founded one of the first laboratories of research which existed in this country; who by his profound philosophical views did so much to promote the advancement of chemistry. There was, at Manchester, Dalton, who did as much for chemistry as Kepler did for astronomy. There was Faraday, that prince of electricians; and my dear friend Grove, who now sits beside me, who formulated the correlation of forces; and Joule, who discovered the mechanical equivalent of heat. These names show that the great science of chemistry was active in our country. But it required association to bring the chemists together; it required association to encourage young men in research, and to give them that support which united science always adds to the promotion of investigation. Fifty years, gentlemen, is a long time in the history of an individual, but it is a mere mathematical point in the history of a science. We are sometimes told that chemistry is a modern science: that is not true. The moment that men's minds began to experiment on the constitution of matter, there was a science of chemistry. Tubal Cain was a chemist because he was skilled in brass and iron. Thales was a chemist when he declared that everything was made of water. Anaximines was equally a chemist when he said that everything was made of air. Aristotle was a very advanced chemist when he got out four elements—fire, air, earth, and water. So chemistry has progressed from those days to the present time by the investigation of the laws which govern the combination of the elements, and by examining into the constitution of matter. Now, chemists and microscopists have often been taunted with the fact that they are content to rely on those small particles of matter which we call atoms, and that they are narrow as men of science compared with astronomers, who sweep the skies and examine the motions of large masses of matter. But the astronomers have been obliged to take us into partnership. We have helped them to know the constitution of the stars, and we are now helping them to discover how new worlds are formed. It is unnecessary for me to detain you longer upon the subject of the progress of chemistry; for that has been ably done by the President. But I would like to hold out some encouragement with regard to the future of chemistry. There are periods of great activity in the progress of every science, and that has been manifested during the period terminating in our jubilee. When this Society meets to celebrate its centenary, what a different chemistry it is likely to be from the chemistry of to-day! Already analysis has led to synthesis, yet we know very little with regard to the processes that go on in organic bodies. With regard to the elements, we are beginning to doubt what they are, and even to hope for their resolution. When we find such an important law as the one that the properties of the elements are periodic functions of their atomic weights, what a field is thrown open for investigation! It is a field of discovery the borders of which we have scarcely yet crossed. The motions of the elements may ultimately be known to us, and even the ultimate elements themselves. We call them elements still, because they have a certain fixity, and we are at present unable to decompose them. But recollect that sometimes there comes a man who changes the whole features of a science. What did Newton do for astronomy? With one fell swoop he cleared away the vortices of Descartes, and the tremendous system of "monads," "sufficient reason," and "pre-established harmony" of Leibnitz, by his philosophy; and we may hope that during the next fifty years there will arise a chemical Newton, who will enable us to know far more than we now know, who may bring under one general law the motions of atoms, and even the rupture of those which we now call elements simply because they have acquired a fixity in the order of things and are able to resist changes in the struggle for existence. Let us have hope in the future. Veterans like myself and my friend Grove will not live to see these great discoveries, but some of our younger men will participate in the chemistry of the future, and will look back with interest to the chemistry of the fifty years we are now celebrating. There is no heart here so cold as to doubt the rapid and continuous progress of our science. I express my own thought, and I believe that I express the conviction of each person present, when I conclude in the words of Tennyson:—

> "And men through novel spheres of thought
> Still moving after truth long sought,
> Will learn new things when I am not.
>
> Thou hast not gained a real height,
> Nor art thou nearer to the light,
> Because the scale is infinite."

Sir W. R. Grove spoke as follows:—

My qualification for being here this afternoon is not one of great distinction. It is that of old age, and the privileges of old age are such as nobody envies. With old age come impaired faculties, and one of its effects is loss of memory. When I promised to take part in this celebration I thought that I should have some reminiscences of my early connection with it to bring before you; but when I came to look up the subject I found that my recollections of it were but slender. So that although, as I have said, my main qualification is that of old age—a sort of survival of the unfittest—I am afraid that I shall not be able to assist you very much. Still I do remember some few incidents of the early formation of the Society. I do not remember who was the actual initiator of it; but the most active man in its formation was undoubtedly Prof. Graham. There was a good deal of discussion as to who should be the first President of the Society. We were anxious to get a man of considerable distinction; and I spoke to Faraday. But he thought that he could do more good in research than in assisting in the construction of such a body; and so he declined the honour. Then the matter gradually advanced, until we got the names which appear in the charter of the Society as its original members. Among those names the only ones that I can now recognize are those of my old and good friend Sir Lyon Playfair, Prof. Graham, and my own. After considerable discussion it was agreed that Prof. Graham should be invited to become President, and he accepted I think that Mr. Phillips's name was previously suggested, but he declined, and proposed Prof. Graham. However, among the names I do not recognize more than those I have mentioned. I am surprised not to see one name among them. Perhaps he was then too young; but he afterwards took an active part in the Society. I refer to Jacob Bell, a very able, gentlemanly, and agreeable man, and also a good chemist. He was the means of introducing into this country the system of selling pure drugs. I could wish that my memory enabled me to tell you more about the origin of the Society; but I do not know that I can give you much information. There were of course discussions among the best chemists of the day. The name of Dalton has been mentioned by our President. I was present at the lecture which Dalton gave at the Royal Institution upon the atomic theory. He was then somewhat aged, of great simplicity of character, and thoroughly devoted to his subject. I well recollect the paper, and his drawings of atoms—little circles to represent atoms as minute spheres grouped together to show their action in uniting to form a molecule of a body. Illustrations were given of the combinations of nitrogen and oxygen, the spheres being arranged in symmetrical little groups around one central sphere. The most compact forms consisted of six spheres around one, for in that case they all touch, and thus pressed together give us a hexagon, as shown in the honeycomb, to the explanation of which a great deal of mathematics has been devoted. I have no doubt that it is caused by the pressure of the bees in crowding into the honeycomb; for each bee with closed wings being cylindrical or nearly so, and somewhat elastic, they convert the spherical cylinders which they make into hexagons by mutual pressure. Conversely you will find that pressure from without acts in the same way, as by winding a band round a bundle of soft clay tubes and gradually tightening it the tubes become hexagons. I think the name atomic theory was an unfortunate one. We talk fluently about atoms as the smallest particles that exist, and chemists regard them as indivisible in the sense of being so hard as to be incapable of further division. To my mind the infinitely small is as incomprehensible as the infinitely great. I use the word incomprehensible advisedly. I do not say that you may not believe in the infinity of the universe; but we cannot comprehend it, we cannot take it in. And so with the atom. Therefore I think that it would have been better to have taken a different word—say minim—which would have been a safer term than atom. As it is, different people think differently as to what an atom really is. However, that was Dalton's theory, deduced from the definite proportions of combining bodies, which is now universally regarded as the keystone of the constitution of matter enabling us to comprehend its combination into definite masses. After the elaborate survey you have just heard from our President, I will not attempt anything approaching to a summary of the chemical discoveries made during the lifetime of the Society; the more so as you can get them in the *Standard* of this morning, or at any rate a very large number of them. There are two ways of regarding science: first, as seeking natural revelations; secondly, practically, as applied to the arts and industries. For my own part, I must say that science to me generally ceases to be interesting as it becomes useful. Englishmen have a great liking for the practical power of science. I like it as a means of extending our knowledge beyond its ordinary grasp, leading us to know more of the mysteries of the universe. The little we can see of it even telescopically is a mere nothing, while what we call an atom is gigantic if its divisibility is infinite.

The spectroscope has been discovered during the lifetime of our Society; and I ought to have been its discoverer. I had observed that there were different lines exhibited in the spectra of different metals when ignited in the voltaic arc; and if I had had any reasonable amount of wit I ought to have seen the converse, viz., that by ignition different bodies show in their spectral lines the materials of which they are formed. If that thought had occured to my mind, I should have discovered the spectroscope before Kirchoff; but it didn't. I cannot recall to my mind any further points sufficiently interesting to speak to you about. Alphonse of Castile is reported to have said that if he had had the making of the universe he would have done it much better. And I think so too. Instead of making a man go through the degradation of faculties and death, he should continually improve with age, and then be translated from this world to a superior planet, where he should begin life with the knowledge gained here, and so on. That would be to my mind, as an old man, a more satisfactory way of conducting affairs. However, it is not so, and we must put up with things as they are. I have been sometimes reproached for having to a great extent given up science for my profession. I need not say that I should have preferred the former. But the necessities of a then large family gradually forced me to follow a more lucrative pursuit. I have said that I prefer contemplative science to science applied to the arts; we are overdone with artificial wants, and life becomes in consequence a constant embarrassment. But there is one practical problem which I would venture to urge upon the attention of the members of this Society: and that is that they should endeavour to prevent the existence of London fogs, even under a constitutional and representative Government.

At the dinner, the Marquis of Salisbury, in proposing "Prosperity to the Chemical Society," coupled with the name of the Right Hon. Sir Lyon Playfair, said:—

I have been, though most unworthy, selected to propose this toast. In vain I pleaded that it would be better in the hands of somebody who knew something about the subject, for those to whom I pleaded were hard-hearted and would hear no excuse. I must therefore proceed, hoping that my distinguished friend who sits next me [Sir Lyon Playfair] will supply that element of knowledge which, perhaps, you will find missing on the present occasion. What naturally strikes me is the importance —the enormous importance—of the science which you cultivate, to the community as a whole. Some hundred years ago, the President of a celebrated tribunal, who was a man of rather advanced opinions, informed Lavoisier that a Republic had no need of chemists. But though a man of advanced opinions he was behind his age. It was the beginning of a time when chemistry more and more as each decade rolled by asserted its vital importance to every class and every interest of every community in the world. I thought—if it is possible to pass any criticism upon the learned and able and most interesting discourse to which we have just listened—I thought that our President was a little too apologetic for chemistry in the early part of the century. Annals which contain the names of Davy and Faraday have no reason to be ashamed. But from my point of view—from the social point of view—chemistry undoubtedly has this claim, that it is one of the most powerful agents that has moved the world. But that is common-place. There is no need for me to tell you what Roger Bacon and Volta have done in the history of the world. But it seems to me that as an educational instrument upon the minds of the community it is one of the most valuable that we possess, because more than any other science it is brought into close communion with pure, real fact. Science is a word that is elastic; and in our days we hear many definitions of it. We hear something of the scientific imagination, a most valuable quality, which I would be the last to depreciate; only I think that, like many valuable concentrated essences, it ought to be indulged in only in small quantities. When there is a proportion in its admixture similar to that which Falstaff observed in his mixture of bread and sack, you feel a desire for more of the solid nutriment and less of the stimulating spirit. But chemistry has an enormous deal of bread and very little sack; it has a large amount of solid fact, and comparatively little of scientific imagination. For the chemist can always be certain of his discoveries; all he has to do is to repeat the experiment, and there is no doubt of his discovery. But when a man discovers what happened fifty millions of years ago, it is not easy to ascertain the exact accuracy of his discovery; and when he discovers all that is going on fifty billions of miles from us, although there may be much probability in what he teaches, still its certainty is not the same in character with the certainty of the man who can go back to his laboratory and repeat the experiment which he has made. I should say that astronomy is largely composed of the science of things as they probably are; geology consists mainly of the science of things as they probably were a long time ago; but chemistry is the science of things as they actually are at the present time. Now the application of a science of that kind to the national mind by constant familiarity with its teachings, by constant knowledge of its achievements, is of the highest human value. It teaches the mind the immortal difference between guessing and knowing. And the farther chemistry goes on, and the more it asserts the superiority of its ways and canons in all departments of human thought, so far shall we drive guessing to a distance and be satisfied with nothing but what we can know. But my task is to say something about the Chemical Society, and perhaps the most suitable course I can pursue, following the Chairman, is to take the other side from what he took, because that will at least give variety to our proceedings, and will also give you an opportunity of testing the superior value of his remarks. Now he dilated much, and most fitly and justly, upon the enormous value from a material point of view which chemistry has been to society in the rapid development which has marked the present reign. I am far from disputing its splendid services to the people of all Europe during that period. But I do not think that it is for the purpose of securing those services that this Society exists. My Right Honourable friend Sir Lyon Playfair did quite right to go to Manchester and stir them up there and teach them their business, and he was a benefactor of mankind in doing so. But when that impulse had been given, you may trust the self-interest of mankind to be sure that the material interests of chemistry will not suffer in the race. But there are other aspects of chemistry, higher aspects, which it is the function of a Society such as this to protect. It is your duty to keep up its intellectual spirit, to teach that not only those things which are demanded by the interests and industries of this country shall be cultivated, but those things also which carry us nearer to the essences of truth. I am not going to carry that pretension too high. We are beings of a mixed character, and our pursuits must bear a trace of the mixture which we give to them. I am not going to imitate the Oxford Professor of my youth, who said that the one thing he valued in the system of quaternions was the certainty that it could never be defiled by any utilitarian application. But still you will observe that the industrial part of chemistry has been that which has received the highest development. Our distinguished President gave us a touching and pathetic history of what I may call the loves and the vicissitudes of benzene. But why is benzene so famous? Why is she lifted up among so many of her compeers who appear in the chemical lists with formulas as imposing and with histories quite as difficult to follow? It is because the products drawn from benzene, or at least from coal-tar, have had the good fortune to produce colours which catch the female eye. Therefore, it is that benzene is famous. But I plead for her humbler sisters who have produced no colours, but the study of whom may yet be steps to the discovery of mighty laws and phenomena which may interest the world. And this, in my humble judgment, is one of the advantages of this Society, that it tends, by bringing men of different researches and pursuits and different intellectual qualifications together, to prevent the science from becoming the mere "handmaid of industry," and ensures that its higher claims shall secure recognition from its votaries. And now I must say a word about the future. Our President has prophesied great things, and is imbued with a just confidence as to the future that awaits us. I believe that there is plenty of room for discovery in the future, and that our forefathers have by no means monopolized the glory that our descendants may win. I rather feel as an outsider—looking at what science is and has achieved—that it is like an Alpine prospect in the early morning, when you see here and there a few peaks bathed in light, but separated from each other by depths and chasms of the unknown. And that is what we all of us feel who look with very little skill or very superficially at the history of science in our own days. It seems evident to me that chemistry is entering upon a new stage, in which it may win splendid victories and learn things of which our forefathers never dreamed. Perhaps it will be best to describe the difference between chemistry as it is now and as it was when I was a young man. In those days the atom reigned supreme; but now the atom has been dethroned, and the bacillus reigns in its stead. But that means that you are approaching, with more and more chance of solving, it the vast problem that separates organic and inorganic nature. Your President has claimed that Nature has no longer the monopoly of creating organic substances. That is true; but Nature does still a great many things that you cannot do. And still less can you tell me the reason of the vast difference between organic and inorganic nature. You are all of you familiar with the tremendous vegetable poisons which produce the most fearful and astounding effects upon the human frame; but if I asked you to explain their effects you would show me formulæ showing that they consisted of the most vulgar and commonplace elements, but giving no explanation of the tremendous powers they assume. I am an agriculturist, and a disciple of Dr. Gilbert and others. I compass sea and land in order to get manure to make our products grow. And what is manure? It is an impure form of the carbon and nitrogen in which those products are bathed in the circumambient air every day of their lives. I trust that the chemistry of the future may tell us why we have to go to Chili, and why we cannot take the nitrogen from the air around us. I believe that these and other problems are now approaching nearer to their solution than ever they were before, because we have seen chemistry grapple more closely with the mysteries which separate organic and inorganic life. I believe that in the future—some fifty years hence it may be—in this or some other room, the President of the Chemical Society of that day will congratulate the Associates of that Society on victories and on achievements of which we cannot now dream the nature. And I am quite sure that when he does so he will attribute no small share of that progress to the existence and labours of the Chemical Society.

Sir Lyon Playfair, in responding, said:—

I quite understand that the reason for selecting me for the honour of acknowledging the toast of "The Chemical Society," is the privilege of old age, and of my having been one of its first members. But I am sure that you will agree with me, that we owe a debt of gratitude to the noble Marquis. He has, as Prime Minister, to bear the weight and responsibility of this great Empire, and it is a proud fact that he has recognized so much the influence and the benefits of chemistry as to honour us by appearing here this evening to propose this toast. If Lord Salisbury had not unfortunately become a great statesman, and had followed the inclination of his own mind, he would have been a great chemist. The education of the upper classes in this country has for a long time been too restricted. Science has not formed that element in education which is so necessary for its progress, and I trust now that the Universities, and the various institutions throughout the country which are doing so much for the advancement of science, will produce great results in the future. But we cannot but regret that the education of the past has not given to us that amount of hereditary talent which our old families possess, and which they have generally given to the benefit of the State. It must be recollected that we have had in the past several instances of descendants of noble families becoming great men of science. We all remember that the famous Boyle was called "the father of modern chemistry, and brother of the Earl of Cork"; and he showed us in his work that we must not trust to authority, but must use acumen as a means of demonstration in all questions brought before us. It was a great delight to me to see, in the exhibition at the Goldsmiths' Hall yesterday, those interesting instruments which Boyle used in his researches. There was another member of a noble family whom we are always glad to claim as a master among chemists: I mean Cavendish, who discovered the composition of water. He did much more than that, however, for he taught us that all experiments should be made with absolute accuracy as regards weight and measure. But what am I to say in answer to this toast? It is a large and important subject. I recollect it fifty years years ago. I am glad to say that not many of you have such an antique recollection of our science as that. The changes that have taken place in the science during that time have been vast indeed. Of course, our main object is to study chemical affinity, to understand the relations of the elements, and the families into which they group. One of the results of fifty years' advance in chemistry is that you have introduced a great deal of profligacy into the elements. When I was young we always taught that oxygen was the universal lover, and joined freely with almost every body, while nitrogen was a confirmed bachelor, and could only be put into union under great difficulty. But now, how completely this is all changed. Oxygen is now a respectable bigamist, while nitrogen, which acts so meekly in the atmosphere, when it gets out of it becomes a terrible polygamist, for it takes three and sometimes even five conjugates at a time, and produces bodies of a remarkable character. I have two friends, one of whom, Hofmann, is not here, but the other, Dr. Perkin, is, and they have done very much to corrupt the morality of the nitrogen of my youth. They have not only taught us what it can do in the way of conjugates, but have shown it to be a most fickle body, from whom you may take one conjugate and readily replace it by another, and thus produce most remarkable compounds. Sometimes they carried their efforts so far that nitrogen became apparently ashamed of itself, and blushed as rosanilin or became scarlet as magenta, and even, when moved by strong emotion, became purple as mauve. Occasionally chemists have tried to get nitrogen back to good habits, to be content with more simple conjugates, and be content with fewer elements in combination. But see how it revenges itself. Curtius and Radenhausen have lately described a most extraordinary compound—azoimide—in which three atoms of nitrogen unite with one atom of hydrogen. This was most unfair, for three atoms of nitrogen ought to have at least nine atoms of hydrogen. But they compelled it to do with one, and what is the consequence? They had to make it take the form of a liquid, and when in that condition it exploded with such violence as to break every glass vessel in the laboratory, and, I am sorry to say, injure one of the persons who tried to force it into this unnatural union. I have therefore some right to complain that the respectable nitrogen of my youth has become a most profligate element under your tuition. And what shall I say of carbon? How different was the carbon of 1841 from the carbon which we now know. At that time we knew, of course, that it was combined in most organic bodies, and Liebig had determined the constitution of the bodies into which it entered, but then we did not require to puzzle ourselves with those fearful complications of diagrams and graphic methods by which we now represent the tenacity of carbon for various substances. These methods are very difficult for the pupil to follow, and I am sure that if Cullen, who invented the system of chemical diagrams, could come to life again, and see the wonderful methods by which chemical combinations are now represented, he would ask to go back to his grave again and rest. Chemical substances now have such astounding properties. If there are two bodies which I thought I knew most thoroughly they are the quiet and respectable compounds called in my old professional days carbonic oxide and carbonic acid. But the respectable quiet carbonic oxide of 1841 was shown the other day by Mond to run away with nickel in the state of a gas—a quiet stable element like nickel. And then when it was followed in hot pursuit, by raising the temperature a few degrees, it dropped the nickel like a hot potato. Well, I am speaking of the changes which strike a man looking backwards, and comparing the chemistry of his day with that of the present time. But though I have been chaffing in an after-dinner speech, do not think that I do not appreciate the vast progress that has been made in the discoveries relating to the valency of the elements. That has been the great distinguishing feature of modern chemistry. There is a great future before the chemistry of this country; and when the centenary of this Society takes place, the members will look back not without respect to the efforts we made in the first fifty years of the Society's existence. In conclusion, I must again thank Lord Salisbury for having honoured us on this occasion in the midst of his great and incessant duties, to show his appreciation of a science in which he has often laboured, and the value and importance of which he has recognized in the excellent speech before us.

THE SCIENCE MUSEUM.

THE question of the Science Museum which has been on the *tapis* for the last eighteen years, has moved —backwards—during the past week. The following question and answer will show how:—

"Sir Henry Roscoe asked the Chancellor of the Exchequer whether it was the case that an unknown donor had offered £80,000 to build an art gallery on a site at South Kensington, the erection of which would materially interfere with the purposes for which land was recently bought by Government for housing the science collections, and for the necessary erection of suitable chemical and physical laboratories in connection with the Royal School of Science; whether another site at South Kensington had been offered which would not interfere with the object for which Parliament granted the purchase-money for the land; and whether the Government would give an assurance that those objects would be maintained.

"The Chancellor of the Exchequer—It is true that a public-spirited gentleman has offered £80,000 to build an art gallery on a site at South Kensington; but with regard to the further points raised by the hon. member, I may say at once that the offer only affects about one-tenth of the land recently bought by Government, and the remainder would still be left available, if required, for science collections. No pledge was given that the whole of the land would be appropriated to science collections. On the contrary, the Treasury, in accepting the offer of the Commissioners of the 1851 Exhibition, stated that the land was in excess of even future requirements of the science collections. It would be possible to make adequate provision for chemical and physical laboratories on the land between the Imperial Institute Road and the Technical Institute. This site adjoins the east galleries, and it is in these galleries, together with the west and southern galleries, and a proposed cross gallery joining the east and west galleries, that the science collections may ultimately be housed. The interests of the Royal School of Science, and of the science collections, are being carefully kept in view, and the hon. member will understand that the acceptance of this generous offer will enable us to provide adequate space for exhibition purposes more rapidly than would have been possible under the old scheme."

It thus appears that the ground which was bought to house collections illustrating science is to be used for some other purpose, since the present collections are to be permanently located in those galleries which, rightly or wrongly, are not considered by "the unknown donor" to be good enough for his pictures.

Further, since the new Art Gallery takes up nearly all the frontage of the Royal College of Science, the extension of that building, instead of being opposite, is to be built about 100 yards away on the opposite side of Exhibition Road, exactly over the projected railway. It is true the railway is postponed for this year, but that is no guarantee that it will not be proposed again.

Mr. Goschen appears to have given in to a caprice of an unknown donor without having any notion of the effect of his action. But if this be so, why does not Mr. Goschen abolish the Royal College of Science and the Science and Art Department altogether? This would be more statesmanlike than omitting to ask the opinions of people who are paid to advise on such matters. Can Mr. Goschen be of the same opinion as another great official who maintained that, even if it were conceded that there should be national collections of physical-science objects, as there are of pictures, books, beasts, birds, and the like, still little space would be required, "because there was no instrument a man of science used which could not be put into a hat"?

Certainly, if the absurd scheme sketched in Mr. Goschen's answer is carried out, the intelligent foreigner will have a good time. He will have to determine whether English statesmanship has succeeded best in putting a physical laboratory over a railway which will prevent nine-tenths of the instruments being used, or in sandwiching a building devoted to art between the two halves of a science school.

NOTES.

THE third session of the Australasian Association for the Advancement of Science was held in Christchurch, New Zealand, and began on January 15, 1891. Sir James Hector presided. The meeting was a successful one, the attendance being about 470, and the number of papers read 74. Prof. Goodale, of Harvard University, represented the American Association, but no member of the British Association attended from England. A revised code of laws was adopted for confirmation at the next session. The evening lectures were: (1) "The Glaciers of the Tasman Valley," by G. E. Mannering; (2) "Oysters and Oyster-culture in Australasia," by W. Saville Kent; and (3) "A Short History of Vocal Music," by G. F. Tendall. Ten Research Committees were appointed to report on different subjects to the next meeting, and a grant of £25 was made towards measuring the rate of motion of the New Zealand glaciers. As great inconvenience is often felt from the want of a special name for the sea between New Zealand and Australia, a recommendation was adopted that the Lords of the Admiralty be requested to name this sea the Tasman Sea. The Committee also recommended the appointment, by the British and American Associations, of a conjoint Committee to define the terms of general importance in biology; and that the Little Barrier Island north of New Zealand, and Resolution Island in Dusky Sound, be set apart as reserves, where the native fauna and flora of New Zealand may be preserved from destruction. The next session will be held at Hobart, Tasmania, with Sir Robert Hamilton, Governor of the Colony, as President.

THE Vatican Observatory, which, according to a circular letter from Father Denza, the Director, "now revives under the protection of His Holiness the Pope Leo XIII.," is bestirring itself to come into closer communication with other scientific establishments, especially by way of the exchange of publications.

THE half-yearly general meeting of the Scottish Meteorological Society was held in Edinburgh yesterday. The Report from the Council of the Society was presented, and the following papers were read: on the winter of 1890-91, by Dr. Buchan; silver thaw at the Ben Nevis Observatory, by R. C. Mossman.

THE Shaen Wing of Bedford College for Women, York Place, was opened by the Empress Frederick on Tuesday. On entering the College, the Empress was received by the visitor, Mr. N. Story Maskelyne, and the Chairman of Council, Dr. W. J. Russell. The ceremony took place in the large lecture-room, where there were present, among many others, Mrs. Shaen, Miss E. A. Shaen, Miss E. Shaen, Sir Henry Roscoe, Sir Lyon Playfair, General Donnelly, and Dr. Gladstone. Her Majesty having ascended the daïs, Mr. Maskelyne read an address, in the course of which he said:—"The literary and the art divisions of the College having been sufficiently provided with the necessary space and appointments, an addition to the buildings of the College for the teaching of science became imperative. This has been achieved, and laboratories for complete instruction in chemistry and the several branches of physics have been built, and in part equipped. This addition has been called the Shaen wing, in order to connect permanently with the College the name of one who was always interested in its welfare, and who for 20 years had served on the Council, being elected Chairman of that body in 1880, an office which he held until his death. The sciences, with their vast influence on the one hand as intellectual triumphs, and on the other as mighty means for the material prosperity of the human race, are, and will be for ever, among the great monuments of the Victorian era. It is an encouragement for all interested in the higher training of women throughout the Empire and that of our kinsfolk in Germany to feel that the heart of the Royal race of England is with them in the effort to make this higher training a heritage of our women as well as of our men." The Empress spent some time in the building after the ceremony was over, and expressed herself as greatly pleased with the College and the work it is doing.

LAST week, at the monthly general meeting of the Zoological Society, it was announced that in recognition of the effective protection accorded for sixty years to the Great Skua (*Stercorarius catarrhactes*) at two of its three British breeding stations—namely, in the island of Unst, by the late Dr. Laurence Edmondston, and other members of the same family, and in the island of Foula, by the late Dr. Scott, of Melby, and his son, Mr. Robert Scott—the silver medal of the Society had been awarded to Mrs. Edmondston, of Buness House, as representative of that family, and to Mr. Robert Scott, of Melby. The medals will be delivered to the medallists or their representatives after the close of the anniversary meeting on April 29 next.

THE following are the lecture arrangements at the Royal Institution after Easter:—Mr. J. Scott Keltie, three lectures on the geography of Africa, with special reference to the exploration, commercial development, and political partition of the continent; Dr. E. E. Klein, three lectures on Bacteria, their nature and functions (the Tyndall Lectures); Mr. William Archer, four lectures on four stages of stage history (the Betterton, the Cibber, the Garrick, and the Kemble periods); Prof. Dewar, six lectures on recent spectroscopic investigations; Dr. A. C. Mackenzie, four lectures on the orchestra considered in connection with the development of the overture; Prof. Silvanus P. Thompson, four lectures on the dynamo; Mr. H. Graham Harris, three lectures on the artificial production of cold; Prof. A. H. Church, three lectures on the scientific study of decorative colour. The Friday evening meetings will be resumed on April 10, when a discourse will be given by Sir William Thomson, on electric and magnetic screening; succeeding discourses will probably be given by Prof. A. W. Rücker, the Rev. Canon Ainger, Mr. J. E. Harting, Prof. W. Ramsay, Prof. G. D. Liveing, Prof. J. A. Ewing, Dr. David Gill, Prof. Harold Dixon, and other gentlemen.

THE fourth annual exhibition of the Photographic Society of Philadelphia is to be opened on May 25. One of the three medals is to be for scientific or technical photography.

AT the request of the Minister of Education, Mr. C. Todd, the Government Astronomer for South Australia, has published the results of his weather forecasts for the year 1890, showing that, out of 305 forecasts issued, 250, or 82 per cent., were verified, and 13 per cent. were partially verified; an amount of success which must be considered highly satisfactory. Mr. Wragge has also sent forecasts from Brisbane with a percentage of success amounting to 55, which, considering that his information cannot be so good as that of the local office in South Australia, may be regarded as a creditable success. But, as pointed out, such a double system of forecasting can only lead to occasional conflicting reports, in addition to which, it is contrary to the principle laid down by the Meteorological Conference held in Melbourne in 1888, that forecasts should be issued for a particular colony only by the local authorities.

THE United States National Museum lately received from Dr. W. L. Abbott a large zoological collection from the vicinity of Mount Kilimanjaro, East Africa. The collection includes about ninety skins of mammals, and an equal number of skulls, representing about thirty-eight species. A description of two of the species, an antelope and a tree-coney, which had apparently not been described before, is given by Mr. F. W. True in the Proceeedings of the United States National Museum, and has been separately printed.

MR. F. W. TRUE was requested in 1886, by Prof. Baird, to investigate and report upon the porpoise fishery carried on at Hatteras, North Carolina. With the industrial aspects of the subject he has dealt in the *Bulletin* of the United States Fish Commission; and now, in the Proceedings of the United States National Museum, he has brought together some interesting notes regarding the habits and structure of porpoises. The species captured at Hatteras is *Tursiops tursio* (Bonnaterre). The fishermen informed Mr. True that the young porpoises remained near their mothers when the latter were entangled in the nets, as sometimes happens. He himself saw this in the case of one female which became entangled near the beach. He did not, however, find the young porpoise among those captured. It probably escaped by diving under the net, as the adult porpoises often do. Mr. True was informed that the mothers helped their young in their efforts to breathe, by bearing them up to the surface of the water on their "flippers," or otherwise. The spiracle, or blow-hole, appears to be a sensitive part of the head. When Mr. True touched it with his hand, the porpoises invariably showed signs of discomfort by lashing the tail violently.

THE Smithsonian Institution has reprinted from the Report of the U.S. National Museum for 1887–88 an excellent account of the coast Indians of Southern Alaska and Northern British Columbia, by Mr. Albert P. Niblack. Among the subjects dealt with in this treatise are regulative organization, mutilations, food, land-works, arts and industries, mortuary customs, feasts, dances, and ceremonies.

M. LIÉVIN COPPIN, director of the Brussels *Économiste*, describes (in an article quoted in the *Board of Trade Journal*) the new commercial museum at Rome. The museum will include a permanent exhibition of national products and of such products of other countries as are capable of being used in Italy. The exhibition also aims at making known in every place in Italy the useful products of foreign countries, and in foreign countries Italian products, in order to bring about a more active exchange in the commercial movement of the country. A bulletin-catalogue of the museum will be exchanged with the similar publications of other countries, and sent to all the national and international exhibitors, as well as to all chambers of commerce and geographical societies; it will be put on board the vessels of the chief navigation companies, &c. A library will be attached to the museum, and this will contain all the commercial journals of the world, and every variety of information concerning customs tariffs, treaties of commerce, &c.

IN his recent presidential address to the Engineers' Club of St. Louis, Mr. F. E. Nipher mentioned that there were in St. Louis about 50 isolated electric lighting plants, having in all about 27,000 lights, and representing about 2700 horse-power. The cost of these plants was on the average 9 dollars per lamp.

A "CATALOGUE of the Library of the Royal Meteorological Society," compiled by Mr. J. S. Harding, Jun., has been published by Mr. E. Stanford. It is complete to September 1, 1890. The work has been prepared with great care, and will be of essential service to students of meteorology. A preface is contributed by Mr. Symons and Dr. Tripe, the Secretaries of the Society. The Council, they say, are glad to show the Fellows how extensive and valuable the library has become, and how worthy it is of the better accommodation which, it is hoped, will shortly be provided for it. The Council trust that those Fellows who possess, or may be able to procure, meteorological works not yet in the library will do what they can towards its augmentation.

THE first edition of Lord Lilford's "Coloured Figures of the Birds of the British Islands," with the exception of a few of the earlier parts, has all been subscribed for. He is therefore making preparations for the issue of a second edition in every respect equal to the first.

MR. R. H. PORTER has nearly ready "The Birds of Sussex," by Mr. William Borrer. The author claims that the volume will contain an account of all the birds now to be found in the county, with mention and careful verification of the occurrences of the rarer species during the last fifty years.

WE have received from Mr. R. H. Porter Part I. of "Aves Hawaiienses: the Birds of the Sandwich Islands," by Scott B. Wilson, assisted by A. H. Evans. The work promises to be one of great value. It is finely illustrated.

MR. H. LING ROTH has just completed a translation of "Crozet's Voyage to Tasmania, New Zealand, the Ladrone Islands, and the Philippines in the years 1771–72." It will be published shortly by Truslove and Shirley.

MESSRS. WHITTAKER AND CO. have published the sixth edition of Sir David Salomon's practical hand-book on "Electric Light Installations, and the Management of Accumulators." Few changes have been made in the text, but a new chapter, containing a considerable amount of fresh matter, has been added. The same publishers have issued the third edition of "Electric Transmission of Energy," by Mr. Gisbert Kapp. The work has been revised and slightly enlarged.

A NEW edition of Mr. V. T. Murché's "Elementary Text-book of Physiology" has been issued by Messrs. Blackie and Son in their series of science text-books. It is intended for classes studying the first stage of the Science and Art course in physiology. A supplement has been added, dealing with those subjects which are included in the curriculum of the Science Syllabus, but are either not discussed at all in the earlier parts of the book, or are not treated there with sufficient fulness.

WE have received from the publishers, Messrs. Percival and Co., both the elementary and advanced stages of their examination sheets on practical plane and solid geometry, by A. Godfrey Day, and edited by E. J. Cox. The sheets are similar in form and style to the Government Science and Art papers. The questions embrace a complete course on the subject in an intelligent and compact manner, those on each sheet treating of different portions of the syllabus. For students who are reading the subject a second time, they will be especially useful. Teachers will also find them a great help, forming as they do an excellent series of examination papers.

Messrs. G. W. Bacon and Co. have published a "New Geological Map of England and Wales." It is "compiled from the best authorities."

The Royal University of Ireland has published the Examination Papers, 1890, as a supplement to the University Calendar for the year 1891.

An important communication upon the colour and absorption spectrum of liquefied oxygen is made by M. Olszewski to the January number of the *Anzeiger der Akademie der Wissenschaften in Krakau*, and a brief abstract is published in the current number of the *Chemiker Zeitung*. Liquid oxygen has hitherto been described as a colourless liquid. In thin layers it certainly appears to be colourless; but M. Olszewski in the course of his investigation of the absorption spectrum, has obtained a sufficient quantity of the liquid to form a layer thirty millimetres thick, and makes the somewhat unexpected and very important discovery that it possesses a bright blue colour resembling that of the sky. Great precautions were taken to ensure the purity of the oxygen employed, the absence of ozone, which in the liquid state possesses a deep blue colour, being especially ascertained. Carbon dioxide, chlorine, and water vapour were also completely eliminated, the oxygen having been left in contact under pressure with solid caustic potash for a week. In view of this fact that oxygen in the liquid state transmits a preponderating quantity of blue light, M. Olszewski's latest experiments upon its absorption spectrum are specially interesting. In a former paper to the *Monatshefte*, an account of which was given in Nature, vol. xxxvi. p. 42, the absorption spectrum of a layer 7 mm. thick was shown to exhibit two strong dark bands, one in the orange, extending from wave-length 634 to wave-length 622, distinguished for its breadth, and one in the yellow, wave-length 581–573, distinguished for its intensity. When the thickness of the layer was increased to 12 millimetres, two further bands appeared, a very faint one in the green, about wave-length 535, and a somewhat stronger one in the blue, extending between wave-lengths 481 and 478. M. Olszewski now finds that his layer 30 millimetres thick, which possesses the blue colour, exhibits a fifth band in the red, corresponding with Fraunhofer's A. This band is rendered still more apparent when a plate of red glass is held between the source of light and the slit of the spectroscope. It is stronger in intensity than the band of wave-length 535, but fainter than the other three bands. This observation of the coincidence of an oxygen band with the telluric band A of the solar spectrum is of considerable interest. For Ångström, in 1864, expressed the opinion that this band A was not due to the aqueous vapour of the atmosphere; and Egoroff and Janssen, who examined the spectrum of long layers of compressed gaseous oxygen, were of opinion that it was due to oxygen. In conclusion, M. Olszewski remarks that the colour exhibited by his 30-millimetre layer is exactly what one would expect from the nature of its absorption spectrum. He also suggests that the blue colour of the sky may be simply due to the atmospheric oxygen, which in gaseous layers of such extent may exhibit the same colour as when compressed into a few centimetres of liquid. Apart from the discussion of this debatable subject, the fact is certainly of interest to chemists, that ordinary oxygen and its condensation allotrope ozone, when compressed into the liquid state, are thus related as regards colour, the former possessing a bright blue and the latter a deep blue tint.

The additions to the Zoological Society's Gardens during the past week include a Common Otter (*Lutra vulgaris*) from Suffolk, presented by Mr. G. C. Edwardes-Ker; a Common Rhea (*Rhea americana*) from South America, presented by Mrs. Hatfield; a Brazilian Caracara (*Polyborus brasiliensis*) from Brazil, presented by Mr. J. D. Spooner; a Green-cheeked Amazon (*Chrysotis viridigenalis*) from Columbia, presented by Miss Julia Crooke; two Leopard Tortoises (*Testudo pardalis*), five Angulated Tortoises (*Chersina angulata*), a Tuberculated Tortoise (*Homopus femoralis*), four Areolated Tortoises (*Homopus areolatus*), a Hygian Snake (*Elaps hygiæ*), four Smooth-clawed Frogs (*Xenopus lævis*) from South Africa, presented by theRev. G. H. R. Fisk, C.M.Z.S.; a L'huy's Impeyan Pheasant (*Lophophorus l'huysi* ♂) from Western China, deposited; two White-throated Capuchins (*Cebus hypoleucus* ♂ ♂) from Central America, a Coqueral's Lemur (*Cheirogalus coquerali* ♂) from Madagascar, a Small-clawed Otter (*Lutra leptonyx*) from India, a Collared Peccary (*Dicotyles tajaçu* ♂) from South America, two Griffon Vultures (*Gyps fulvus*), a Ruddy Sheldrake (*Tadorna casarca*), European, six Amherst's Pheasants (*Thaumalea amherstiæ* 6 ♂) from Szechuan, China, purchased.

OUR ASTRONOMICAL COLUMN.

Determination of the Constant of Aberration.—*Comptes rendus* for March 16 contains an abstract of a memoir by MM. Lœwy and Puiseux, on determinations of the aberration constant, in which some of the results obtained by M. Lœwy's method are given. Up to 1828, astronomers accepted, as the constant of aberration, values which were comprised between 20″·255 and 20″·708, these being respectively due to Delambre and Bessel. Richardson then obtained the value 20″·446 from a discussion of 4000 observations made with the Greenwich mural circle by his predecessors. In 1843, W. Struve proposed a value almost identical with this, viz. 20″·445, as the result of a discussion of his careful observations made in the prime vertical. He estimated the probable error as 0″·011, and remarked that he did not think any astronomical element had been determined with so great an accuracy. Struve's work was received with much favour, and appeared to render unnecessary, for a number of years, all researches on the same subject. However, in 1844, Baily deduced 20″·419 as the most probable value, and in after years, Peters, Lundhal, and Lindhagen subjected to a minute discussion all the meridian observations, made at Dorpat and Pulkova, of circumpolar stars. From their researches, a value a little greater than that of Struve was found. Still, when these results were taken in conjunction with the determinations the most worthy of confidence the values 20″·45 and 20″·46 were obtained, thus supporting Struve's work. Nyren, from a discussion of observations made by Struve in the prime vertical as material for the study of nutation, derived the value 20″·43. In 1853, Struve himself proposed to increase his number to 20″·463 with a probable error of 0″·017, but the reasons given to justify the change do not appear to be sufficient. Gylden, Wagner, and Nyren's ulterior observations at Pulkova of circumpolar stars gave the higher value 20″·49. Later, in 1879–82, Nyren made another determination by Struve's method, and used a large number of stars for the investigation. He then found 20″·540 or 20″·517, according to the method of grouping adopted. More recently, in 1885, Küstner, of Berlin Observatory, found 20″·313 by Horrebow and Talcott's method. Between these two last numbers, both of which represent a large amount of work executed with much care, the difference is somewhat greater than 0″·2—that is, about twenty times the probable error estimated by Struve in 1843. This seems to indicate that the astronomers have taken a step backwards. MM. Lœwy and Puiseux do not enumerate the work done on the same subject at Greenwich, the Cape, Washington, and other Observatories, but point out that similar sources of error exist in all the methods employed. Lœwy's method, as is now well known, consists in placing before the object-glass of an equatorial a double plane mirror formed by silvering the sides of a prism of glass. This acts as a sort of compass of strictly constant opening, and brings to the eyes rays which make a constant angle with each other. Pairs of stars separated by a wide angle on the celestial sphere, but which together appear in the field of view, can easily be found, and the variations in relative position due to refraction or aberration can be measured micrometrically with great precision. The adoption of this method leads the authors to the following tentative conclusions :—(1) The number 20″·445, proposed by Struve, is very near to the truth. It would be premature, in our opinion, to wish to modify it. (2) As M. Fizeau supposed, reflected rays behave in the same manner as direct rays from an aberration point of view. (3) The new method for the investigation of aberration may be regarded as proved and definite.

In a future communication the authors will give some details of the method, the observations made on four couples of stars, and the numerical value they find for the aberration constant.

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON Thursday and Friday of last week, the spring meeting of the Institution of Mechanical Engineers was held in the theatre of the Institution of Civil Engineers, by permission of the Council of the latter Society, the President, Mr. Joseph Tomlinson, being in the chair. There were but two items in the programme—namely, the fourth Report of the Research Committee on Friction, and a paper on rock drills, contributed by Messrs. Carbutt and Davey. The meeting suffered a good deal, especially on the second evening, from the fact that the Institution of Naval Architects was in session at the same time. On both evenings very interesting papers on engineering subjects were being read before the latter Society, where the attraction appeared to be greater, for, whilst the Mechanical Engineers meeting was very thinly attended, the Naval Architects had, we hear, an overflowing house on both evenings. It is a pity the secretaries of two Societies having objects so nearly akin, cannot arrange for their meetings not to clash. There is this to be said in favour of the Naval Architects, however, that they were adhering to a time-honoured fixture.

FRICTION OF A PIVOT BEARING.

The Friction Committee's report was taken charge of by Mr. Beauchamp Tower, who was practically the author. The experiments were carried on last year at Simpson and Co.'s engine works, Pimlico, &c. The thanks of the Institution, and of the engineering world at large, are due to this firm for the assistance they have lent, and perhaps the name of Mr. Mair-Rumley should be especially mentioned in this connection.

The pivot bearing operated upon was 3 inches in diameter, and flat ended. The vertical shaft carrying the footstep was geared to a horizontal shaft, which was driven by a belt from the works shafting. Variations of speed were obtained by varying the size of pulley. The bearing was pressed upwards against the footstep by an oil press with a 6-inch diameter plunger. This plunger was made a good but perfectly free fit in its cylinder for a length of 9 inches, a number of grooves being turned in the cylinder throughout its whole length at close intervals. The pressure was applied by means of a small hand-pump, provided with an air-vessel, pumping oil out of a tank into the press. It was found that the leakage of the oil past the plunger, even with the highest pressures, was exceedingly slow, requiring only an occasional stroke of the pump to keep the pressure constant ; and at the same time the friction was practically *nil*. Into the top of the plunger was let a piece of hard steel, having a conical depression, wherein rested a hard steel conical centre, which was formed on the bottom of the plate L that carried the bearing. This plate was circular, and had a groove turned in its periphery ; a small chain was fastened to the plate and lay in the groove round a portion of the circumference, from whence it led off to a spring-balance attached to the fixed frame of the apparatus ; so that the rotation of the plate stretched the spring-balance, and the force tending to turn the plate was thereby indicated. The upper end of the vertical shaft that carried the footstep had a piston fixed on it, which revolved in a cylinder 6 inches diameter. This upper cylinder was connected by a pipe with the cylinder of the lower press, so that, whatever oil-pressure there was in the lower cylinder pressing the bearing upwards, there was the same in the upper cylinder pressing the footstep downwards. This was a convenient way of providing for taking the upward thrust upon the experimental bearing. The footstep having been set running at the desired speed, the hand pump was worked until the pressure gauge on the oil press indicated the desired pressure ; and the friction was then read off the spring balance connected with the bearing plate. The load could be quickly removed from the bearing by opening a cock for discharging the oil from the air-vessel of the pump. This method of applying the load was found to be exceedingly convenient. Efficient automatic means of lubrication were provided, which are well worth following, but which we have not space to describe. In the results the coefficient of friction was obtained by dividing the friction in inch-pounds by the product of the load multiplied by the area of the bearing.

The results of the experiments were given in the report by means of a table and in a graphic form. From these we extract the following outline particulars ; and must refer our readers to the report itself, which will be published in the Proceedings of the Institution, for fuller details upon this important and interesting subject.

Experiments on the Friction of a Pivot Bearing. Steel Footstep on Manganese Bronze Bearing.

Revolutions per min.	Load : lbs. per sq. in.	Oil drops per min.	Friction. Total.	Friction. Coefficient.
			In. lbs.	
50	20	20	2·77	0·0196
	120	56	18·72	0·0221
128	20	79	1·13	0·0080
	160	84	12·82	0·0113
194	20	196	1·44	0·0102
	160	168	7·69	0·0068
290	20	Continuous stream	2·51	0·0178
	140	,, ,,	4·51	0·0046
	160	200	5·03	0·0044
353	20	Continuous stream	2·36	0·0167
	160	,, ,,	6·15	0·0054

The friction given is that of one face of the flat circular bearing surfaces, at the effective radius of the face, viz. 1 inch.

A white metal bearing surface was next substituted for the manganese bronze. The coefficient of friction was a little larger, but the difference was so small that the results may be looked upon as practically identical.

That the coefficient of friction is less at the higher speeds is doubtless due to the more perfect action of the lubricating device. After the completion of these experiments, the endurance of the manganese bronze and white metal bearings were tested. The former heated and seized at 260 pounds per square inch load on one occasion, and 300 pounds on another, running at 128 revolutions per minute without lubrication. The white metal bearing heated and seized in a load of 240 pounds per square inch at 128 revolutions per minute, without lubrication.

These experiments should be studied with those on the same subject which have preceded them.[1] A short but interesting discussion followed the reading of the paper.

ROCK DRILLS.

The paper on rock drills does not call for an extended notice at our hands. It grew out of some trials made last year at the Crystal Palace, in connection with the Mining Exhibition there held.

One cannot help comparing the carefully thought-out trials last described with those now before us. The only point upon which we can commend those responsible for the present competition is that they awarded no prize. Perhaps one of the most difficult subjects to decide by competition would be the superiority of any one rock drill over its fellows, and the conditions of trial would require careful planning and elaborate preparation. We were not present at the trials, but, to judge by the description, they seem to have been organized by persons having a very elementary knowledge of the conditions under which these machines are called upon to work. One of the judges stated that his qualification to act arose from the fact that he had been in the steam-hammer business, and the

[1] For previous reports see Proceedings of the Institution, 1883, p. 632 ; 1885, p. 58 ; and 1888, p. 173.

blow of the rock drill is similar to that of the steam-hammer. *Ex pede Herculem!* It appeared, however, that the makers of the machines framed the conditions of trial, so that, presumably, every one concerned was satisfied.

THE INSTITUTION OF NAVAL ARCHITECTS.

THE annual meeting of the Institution of Naval Architects was held last week, on Wednesday, Thursday, and Friday, at the rooms of the Society of Arts, lent by the latter Society for the purpose. The meeting in question was one of the most successful held for many years; the merit of the papers and the large attendance of members speaking volumes as to the flourishing state of this excellent Society. As there were just a dozen items in the programme, including the President's Address, it will be evident that we can do no more than mention some of the papers read.

The one fault we have had to find in the management of this Institution is that it gives us too many good things at once. It holds but one meeting a year, and that is divided into five sittings. In this way matters that would supply a whole season's programme for many kindred institutions have been crowded into the sole meeting of the year, which has to be rushed through in three days. We have dwelt on this subject before, and know for a fact that our remarks have met with the approval of a considerable number of members. We are glad, therefore, to learn that it is proposed in future to hold two meetings every year. If an effort is made by the Council to improve the quality of the discussions—which can only be done by giving them more time—rather than by adding to the number of papers, the new departure will, we feel sure, be additionally welcome.

The following is a list of the papers read and discussed:— 1. "Future Policy of War-ship Building," by Lord Brassey. 2. "On some recent American War-ship Designs for the American Navy," by J. H. Biles. 3. "On Boiler Deposits," by Prof. Vivian B. Lewes. 4. "Study of Certain Phenomena of Compression," by M. Marchal. 5. "Boiler Construction suitable for withstanding the Strains of Forced Draught," by A. F. Yarrow. 6. "Recent Improvements in Armour for Vessels," by M. Barba, Chief Engineer of Schneider and Co., Creusot. 7. "On the Alteration in form of Steel Vessels due to Different Conditions of Loading," by Thomas Phillips. 8. "The Internal Stresses in Steel Plating," by J. A. Yates. 9. "Certain Details of Marine Engineering," by Thomas Mudd. 10. "On Combined Crank, Crank and Intermediate Shafts, for Marine Engines, and on their liability to Fracture," by C. H. Haswell. 11. "An Assistant Cylinder for Marine Engines," by David Joy. The President, Lord Ravensworth, occupied the chair throughout.

The two great features of the meeting were undoubtedly Mr. Yarrow's paper on boiler construction, and Lord Brassey's contribution on war-ship policy. The respective values attached to these memoirs naturally depended on the walk in life of those appraising them; the Admirals mustering in unusual force to hear Lord Brassey, whilst there was a tremendous gathering of engineers to listen to Mr. Yarrow; indeed, we have seldom seen the theatre in John Street more crowded than it was last Thursday. Each of these papers had an addendum, Lord Brassey's in Mr. Biles's contribution, and Mr. Yarrow's in Mr. Mudd's paper, which gave some very valuable practical additions to our knowledge of the science of boiler construction.

We have used the term "science of boiler construction" advisedly. Last week we should have hesitated to apply it, as being a subject almost non-existent. Steam engineers have woefully neglected the source of their power in time past. The engine has been like a favourite child, no trouble too great to expend upon it; but the boiler has been, figuratively speaking, left out in the cold. Such improvements as have been made in its construction have been due to inventive ability of the ingenious mechanic order. Hardly anyone has thought of treating the boiler philosophically; at least hardly any one before Mr. Yarrow. The boiler has had its revenge. It has been the uncertain factor, and, in marine engineering, the prime source of trouble. We wish we could give all the beautiful experiments by which Mr. Yarrow illustrated the reason of the ills to which boilers are subject when they are pressed to a high rate of duty. Everyone has heard of the difficulties that have arisen in our own and foreign navies from the endeavour to apply forced draught to war-vessels. The curious fact has remained that whilst time after time the larger vessels of the navy came back from abortive trials with boilers leaking at every tube, Mr. Yarrow could run the trials of his torpedo boats, having a high forced draught pressure, with almost unvarying success. The prime reason for all which was made apparent by the paper of Thursday evening last. It may be explained in a few words: Mr. Yarrow has treated his subject in the true spirit of scientific research. He has taken each difficulty as it arose, and investigated it to the bottom, dealing with material he had to use, and the method of construction, upon a basis of scientific reasoning. A good example of this was shown in the manner in which he explained the ovalling of tube plate holes, one of the most fruitful sources of trouble to those who run marine boilers with forced draught. Mr. Yarrow first gathered together all the known facts on the subject. He took the two metals of which tube plates are composed—namely, copper and steel—and tabulated their rates of expansion under various temperatures, and their ratio of conductivity of heat. By the facts so ascertained, and the analogy of a well-known blacksmith's operation—that of reducing the size of a tire by repeated heatings and coolings on one side only—he formulated certain hypotheses, which he proved by experiment to be well founded. His reasoning was clearly set out in his paper, and his experiments were successfully repeated before the meeting. The conclusions involve some interesting problems of molecular physics, and we regret we cannot give the matter the space it deserves; but a satisfactory explanation would involve the reproduction of Mr. Yarrow's diagrams and illustrations of his apparatus. We have dwelt somewhat at length on this paper, partly because it is likely to be of especial interest to our readers, but more especially because it affords a most welcome precedent which we hope many other principals of engineering factories will follow.

Turning to the other papers, we find them all at least of moderate merit, and many of them excellent. Mr. Phillips's contribution on the alteration in form of steel vessels was a praiseworthy effort to put an important branch of ship construction on a more satisfactory basis. From his exceptional position he was able to carry out a series of practical investigations as to the alteration of form of ships under certain conditions of stress, which are so far satisfactory that they go to prove the existing regulations in force on this subject are sufficient. The paper did not pass without criticism, and indeed gave rise to one of the best discussions of the meeting. The paper of Mr. Yates was a more philosophical effort on a cognate subject. A consideration of the internal stresses in steel plating due to water pressure involves some very debatable matter, and the author's mathematics did not pass without criticism. It is characteristic of the time that Mr. Bryan, whose admirable paper on the buckling of a thin steel plate will be remembered, journeyed up from Cambridge purposely to speak on this paper. His mathematical analysis of the subject will form a valuable page in the Transactions.

Prof. Lewes's paper on boiler deposits was eminently practical, and a most welcome addition to a too little studied subject. The Institution and the engineering world in general are fortunate in getting a competent chemist to turn his attention to these matters. M. M. Marchal's paper was taken as read. The paper by M. Barba was somewhat disappointing, and the discussion which followed it was decidedly "shoppy." The two remaining papers which were read, those of Mr. Mudd and Mr. Joy, were of a practical engineering interest; more especially Mr. Mudd's, which was full of instruction for working marine engineers. Mr. Haswell's contribution was not read.

SCIENTIFIC SERIALS.

American Journal of Science, March.—On gold-coloured allotropic silver, by Mr. M. Carey Lea. The present paper is in continuation of one published in this *Journal* in June 1889, and has for its object the description of the reactions of gold-coloured allotropic silver. It is shown that there exists a well characterized form of silver, intermediate between the allotropic silver previously described and ordinary silver, differing in a marked manner from both. All forms of energy act upon allotropic silver, converting it either into ordinary silver or into the intermediate form. Mechanical force (shearing stress) and high tension electricity convert it directly into ordinary silver. Heat and chemical action convert it first into the intermediate form, then into ordinary silver. The action of light is to produce the intermediate form only, and even the most prolonged action at ordinary temperatures does not carry it beyond this. A remarkable parallelism appears to exist between the action of these forms of force on allotropic silver and their action on the silver haloids, indicating that it is not improbable that in these haloids silver may exist in the allotropic condition. Three coloured plates accompanying the paper illustrate the changes described.—The flora of the Great Falls coal-field, Montana, by J. S. Newberry.—High-level shores in the region of the Great Lakes, and their deformation, by J. W. Spencer.—On the composition of pollucite and its occurrence at Hebron, Maine, by G. H. L. Wells.—The volumetric composition of water, by Edward W. Morley. A description is given of the apparatus used to obtain results which will be published in the next number.—On the intensity of sound: a reply to a critic, by Charles K. Wead.—The fire-ball in Raphael's "Madonna di Foligno," by Prof. H. A. Newton. The fire-ball painted by Raphael in his picture, the "Madonna di Foligno," is most probably representative of one that fell at Crema in September 1511. Some political events of importance to Italy and the Pope, which transpired in 1512, were supposed to be connected with the fall of stones that occurred. It appears natural, therefore, that Raphael should introduce the Crema fire-ball into the altar-piece he was painting at the time.

Reale Istituto Lombardo, December 4, 1890.—Observations on the results of mechanical and chemical analyses of some soils in the neighbourhood of Pavia, by Prof. T. Taramelli.—The mean linear coefficient of expansion by heat, between the limits of temperature 0° and t°, of a homogeneous and isotropic solid body, is inversely proportional to the difference which exists between the temperature of fusion T and the temperature t, by Prof. A. Sayno.—On the dynamics of storms, by Prof. L. de Marchi.—On the classification of rational transformations of space, and in particular on transformations of the zero class, by Signor G. Loria.—Note on the calcite of some localities in the Grand-Duchy of Baden, by Prof. F. Sansoni.—On a theorem of differential geometry, by Prof. G. Padova.

December 18, 1890.—The coefficient of elastic expansion of a homogeneous and isotropic solid body at a temperature t, between two given limits, is inversely proportional to the difference which exists between the temperature of fusion T and the temperature t, by Prof. A. Sayno.—On some ancient and modern lavas from Stromboli, by Prof. G. Mercalli.—General formulæ for the representation of a field of force by means of elastic tension, by Dr. C. Somigliana.

January 15.—On the theory of the potential function of surfaces, by S. C. G. A. Maggi.—Results of observations made at the Royal Observatory at Brera during the years 1889-90.—On the diurnal variation in magnetic declination, by M. E. G. V. Schiaparelli. It is shown that the magnitude of the diurnal variation in magnetic declination is connected with the amount of spotted surface on the sun as exhibited by Wolf's relative numbers.—On calculations of condensation and on some applications of them, by E. Cesaro.

Notes from the Leyden Museum, vol. xii., No. 3, July 1890, contains:—P. C. T. Snellen, note on *Tyana superba*, Moore.—C. Ritsem, on *Cyriocrates zonator*, Thoms.—Ed. Lefevre, new Coleoptera belonging to the Eumolpidæ.—Dr. W. van Lidth de Jeude, on a large specimen of Orthragoriscus washed ashore at Ameland. The specimen is figured, and attention is called to discrepancies in the published descriptions and figures.—M. Schepman, on *Oliva semmelinki*, n.sp.—J. Büttikofer, zoological researches in Liberia; birds collected in the district of Grand Cape Mount. *Zosterops demeryi* and *Z. obsoleta* described as new, and eighty-six species enumerated.—W. Roelofs, *Ectatorhinus alatus*, n.sp., described.

No. 4, October 1890, contains:—J. D. Pasteur, on telegraph poles pierced by woodpeckers (*Picus analis*).—Dr. F. A. Jentink, on *Strepsiceros kudu* and *S. imberbis*, rectifies several mistakes made by various authorities about these species.—On two very rare, nearly forgotten, and often misunderstood Mammals from the Malayan Archipelago, *Pithecia melanurus*, S. Muller, the history of which species is quite a romance; but as facts it may be stated that it lives in Sumatra and West Java, that Duvaucel's drawing (reproduced in F. Cuvier's "Histoire naturelle des Mammifères") represents the animal of its natural size, that it has been accurately and satisfactorily drawn, and, finally, that the animal is a true mouse. Two specimens are in the Leyden Museum. *Tupaja dorsalis*, Schlegel. Dr. Jentink justifies Schlegel in keeping this as a species distinct from *T. tana*, contrary to the opinion he expressed in 1888.—On *Rhinoceros simus*, Burch. The Leyden Museum possesses a fine stuffed adult female, and an unstuffed skin of this species. Quotations are given from modern writings, which suggest that the species is not so rare as Dr. P. L. Sclater seemed to think (NATURE, vol. xlii. p. 520). The question is asked, Is the Quagga extinct? If so, it would be well to take stock of the existing specimens.—Dr. R. Horst, on *Perichæta vordermanni* and *P. sluiteri*, new species from the Island of Billiton.—W. Roelofs, on two new species of Poteriophorus, *P. van de polli*, and *P. sellatus*.—E. Candeze, *Melanoxanthus nigrosignatus*, n.sp., Java.—C. Ritsema, *Coloborhombus auricomus*, Java; *Thermonotus pasteuri*, Sumatra; and *Atossa bipartita*, Borneo, all described as new species.—Dr. de Jeude, on some reptiles from Nias.

Vol. xiii., No. 1, January 1891, contains:—Dr. J. G. de Man, carcinological studies in the Leyden Museum (plates i. to iv.). This is No. 5 of a series of studies. Several new species are described, a conspectus of fifteen Indo-Pacific species of the genus Gelasimus is given.—M. Schepman, *Fusus sieboldi*, n.sp., from Japan.—A. B. Meyer, *Cercopithecus wolfi*, n.sp., a beautiful species from Central West Africa, living in the Dresden Zoological Gardens.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 5.—"Some Points in the Structure and Development of Dentine." By J. Howard Mummery. Communicated by C. S. Tomes, F.R.S.

The purpose of the present paper is to show that there are appearances in dentine which suggest that it is formed by a connective tissue calcification, and that the process is more closely analogous to the formation of bone than has usually been supposed.

Processes or bundles of fibres are seen, incorporated on the one side with the dentine, and on the other with the connective tissue stroma of the pulp; some of the bundles give evidence of partial calcification, reminding one of similar appearances in the calcification of membrane bone. Cells are seen included in the bundles and lying parallel to their course; these cells, it is concluded, forming with the odontoblasts the formative cells of the dentine, the calcification of which should be looked upon as in part, at least, a secretion rather than a conversion process, the cells secreting a material which calcifies along the lines of and among the connective tissue fibres, the cells themselves not being converted into dentine matrix. These appearances are seen in the rapidly forming dentine of a growing tooth, as well as in more fully developed specimens. An examination of other Mammalian teeth reveals similar appearances. The dentine of the incisor of the rat (*Mus decumanus*) shows with great distinctness the incorporation of the connective tissue fibres with the dentine, and a marked striation of the dentine near the pulp cavity, parallel with these fibres. The ivory of the elephant's tusk shows the same relation of connective tissue to formed dentine. Vaso-dentine exhibits a very well defined connective tissue layer surrounding the pulp. This layer has hitherto been looked upon as consisting of odontoblasts, but this tissue shows no nuclei, and has the characters of a layer of flattened connective tissue fibres—a layer of nucleated cells in close apposition to the dentine, probably being the odontoblasts of vaso-dentine.

Physical Society, March 6.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Mr. James Swinburne read a note on Electrostatic wattmeters. After referring to the history of the electrometer method of measuring power developed by alternating currents, the author pointed out that the necessity for taking two readings from which to determine the watts might be obviated by having the quadrants separated instead of connected in pairs, as in the ordinary method. Non-inductive resistances are connected to the transformer, motor, or other apparatus in which the power is to be measured, so as to be in series with the apparatus, and on opposite sides of it, and the four ends of these two resistances are connected with the four quadrants. Under these circumstances the deflection of the needle is a measure of the watts. To increase the maximum deflection obtainable, so as to make an instrument capable of being read by a pointer, the needle is made in two halves, and fixed one below the other on the same stem, and, instead of quadrants, semicircular boxes are employed. In this way a range of about 130° is obtainable. Prof. Perry inquired as to what kind of *law* the instrument had, and Mr. Blakesley asked whether it was convenient to use. Mr. E. W. Smith pointed out that there was no necessity to take two observations in the ordinary electrometer method, for, by using a false zero, one deflection gave the watts. Further, the use of the false zero rendered it unnecessary to employ any other voltmeter when experimenting at constant pressure. The President said the historical part of the paper was not quite correct, and recalled attention to the fact that, when high pressures were used, a single reading obtained with the ordinary method of connecting up gave the power. For ordinary low voltages, however, the false zero method described by Mr. Smith was very convenient. Mr. Swinburne, in reply to Mr. Smith, said the observation of the false zero really meant another reading. As to the law of his wattmeter, referred to by Prof. Perry and the President, he said he never calculated a law, but calibrated the instruments directly.—Prof. S. P. Thompson now took the chair, and a paper on Interference with alternating currents, by Prof. W. E. Ayrton, F.R.S., and Dr. Sumpner, was read by the latter. The paper relates to the phenomena which occur when alternating electric pressures are impressed on circuits made up of various combinations of resistances, condensers, arcs, and inductive coils; to the characteristics of alternators; to the properties of transformers; and to the peculiarities exhibited by the Ferranti mains. In one of the experiments, an inductive coil and a condenser were connected in series, and a pressure of 25 volts, as measured by a Cardew voltmeter, impressed on its terminals; the pressures on the two parts, measured in the same way, were 110 and 104 volts respectively, thus showing that each of the two parts was much greater than the whole. On joining a condenser and inductive coil in parallel, an ammeter in the main circuit indicated 5·5 amperes, whilst those in the branches showed 6·4 amperes passing through the condenser, and 10 amperes through the coil. Other experiments of a similar nature were described, and it was pointed out that the ratio of the sum of the two parts to the measured total may be large, being about 8 in the case first mentioned. Theoretically this ratio might be anything, depending as it does on the phases of the pressures in the two parts, and these phases are determined by the ratio of the impedance of the coil to its resistance; practically, however, it was not easy to get a coil of large self-induction and very small resistance. Alternate current arcs and condensers gave results of the same general character as those above described, as also did arcs and inductive coils, such as the regulating coils of lamps; this may cause considerable error in estimating the power supplied to such lamps. The magnitude of the error was found to depend greatly on the quality of the carbons and the character of the arc. With bad carbons and a hissing arc the error was very great, but with good cored carbons burning steadily it was not very serious. Combinations of inductive and non-inductive coils exhibit marked peculiarities, particularly if the inductive one be a transformer coil. This last arrangement gave distinct evidence of interference, or difference of phase, when the secondary of the transformer was open, but when closed and with a moderate load, the difference of phase disappeared, thus showing that under these circumstances the primary coil had no appreciable self-induction. On the subject of alternator characteristics, a graphical method of drawing the E.M.F. curve from the terminal curve was described, and the dependence of the terminal curves on the character of the external circuit pointed out. Keeping the speed, exciting current, and armature current constant, the pressure between the terminals was shown to be dependent on whether the external circuit consisted of condensers, resistances, or inductive coils, the pressure being greatest in the former case, and least in the latter. The true E.M.F. of the dynamo, however, was the same in all the cases, but it became less as the armature current increased. From these results the authors conclude that the drop in E.M.F. with large currents is due to reaction on the field, but that the change of terminal-pressure cannot be all attributed to this cause. Transformers, it was shown, are powerful controllers of phase, for the primary and secondary currents are nearly always circulating in opposite senses; the phase-angle for a 1 to 1 Mordey transformer experimented on varying from 170° at no load, to 180° at full load. The relation between the strength of the primary and secondary currents A_p and A_s was found to be a linear one, of the form $\frac{P}{s} A_p = \alpha + \beta A_s$, where P and S are the numbers of turns in the primary and secondary respectively, and α and β constant, α representing the exciting current, and β being nearly unity. The phase-angle, ϕ, between the currents was given by the equation—

$$-\cos\phi = \frac{\frac{1+\beta^2}{2}A_s + \alpha\beta}{\beta A_s + \alpha}.$$

The results of numerous experiments on a transformer of the Mordey type, in which coils having different numbers of turns were put in parallel with each other, were given. In some cases resistances were put in circuit with the coils, and in others one or more of these resistances were cut out. Remarkable interference effects were thus produced, for in some combinations the volts or currents were additive, whilst in others they were nearly differential. In connection with the Ferranti effect, experiments had been made by putting a condenser on the terminals of the secondary of a transformer, and noting the resulting increases in pressure, both in the primary and secondary circuits. The results obtained with a given condenser and approximately constant secondary pressure show:—(1) That whether the transformation be up or down, the percentage rise in the secondary is greater than that in the primary. (2) That these percentage rises diminish as the secondary current increases. (3) That they increase with the ratio of transformation. (4) That the rise in the secondary may be considerable without that in the primary being appreciable. (5) That the rise in the secondary still persists even when large currents are flowing. These facts lead the authors to believe that the "Ferranti effect" is due to some kind of interaction between the cable condenser and the self-induction of the transformer, and that it is not only due to armature reactions in the dynamo. In the discussion on the paper, Mr. Swinburne said the character of the "Ferranti effect" had been wrongly stated, for he understood Prof. Ayrton to say that the pressure between the Ferranti mains was greater at the London end than at Deptford. This, he contended, was impossible. He also said the effects now described, of putting a condenser on the secondary of a transformer, and noticing the rise in both primary and secondary volts, were due to the waste field of the transformer. In large transformers, such as those at Deptford, the magnetic leakage was proportionately much less, and no such effects would be noticeable with them. Referring to the relation between the primary and secondary currents in transformers, he said it was convenient to look at the primary from two points of view, and consider one part of it as producing the magnetization, and the other as neutralizing the effect of the secondary current. Mr. Mordey described an experiment on armature reactions in alternators, in which one of the armature coils of a 50 h.p. machine was isolated from the rest and connected directly to a Cardew voltmeter. On varying the load from 0 to the full output (20 amperes), the voltmeter on the isolated coil was quite stationary, thus showing that no appreciable armature reaction occurred. Mr. E. W. Smith remarked that the formula connecting the primary and secondary currents holds true over a very wide range, for he had experimented on a Kapp transformer which gave α constant and $\beta = 1$ for frequencies varying from 20 to 200. Mr. Blakesley gave the following formula as applying to transformer currents generally,

$$\left(\frac{A_p}{A_s}\right)^2 = \frac{n^2}{m^2}\left\{(1 + kr_2^2) + \frac{qr^2}{v}\right\},$$

where r_2 = resistance of secondary, v is proportional to the frequency, m and n the primary and secondary turns, and k and q are constants depending on the disposition of the iron. This formula he found to very nearly represent Ferrari's results, and he hoped others would test its applicability to various types of transformers. He believed himself to be the first who carried out an interference experiment, when he put a condenser in parallel with an inductive coil six years ago. He mentioned the matter now, because that arrangement had been put forward by Mr. Glazebrook in explanation of the "Ferranti effect." Mr. Glazebrook, however, had made an error in stating that under certain conditions one of the currents may become infinite. Dr. Thompson thought the word interference had been used in a somewhat different sense from its ordinary usage in optics, and asked for an authoritative definition of its meaning in the paper under discussion. The experiments on the alternating arc he considered very remarkable, as well as those on the dynamo, which gives various terminal pressures when its E.M.F. and current were the same, but the external circuit of different character. Prof. Ayrton pointed out that the measurements on the rise of volts by putting on condensers, mentioned in the paper, were not strictly analogous to those made by Mr. Ferranti, for his were made by pilot transformers, one placed between the primary terminals of the Deptford transformer, and the other on the secondary of the London transformer; whereas in the cases now brought forward the volts measured were those at the primary and secondary of the same transformer. On the subject of arc lamps, he said that Messrs. Kolkhorst, Thornton, and Weekes, who made the experiments, found that great lag only occurred when the carbons were bad and the arc hissing. If the apparent and the true power spent in such an arc were measured, a great difference existed between them. Dr. Hopkinson, he said, had shown that this might be expected, if the arc had a constant back E.M.F. which changed sign with the direction of the current.—A paper on the theory of dissociation into ions and its consequences, by Prof. S. U. Pickering, F.R.S., and another on some points in electrolysis, by Mr. J. Swinburne, were postponed.

Entomological Society, March 4.—The Right Hon. Lord Walsingham, F.R.S., Vice-President, in the chair.—Mr. F. P. Pascoe exhibited, and made remarks on, a curious Coleopterous larva, with a case somewhat resembling that of the Lepidopterous genus *Psyche*, which was found at the Theatre of Bacchus, Athens.—Mr. J. W. Douglas sent for exhibition specimens of *Icerya ægyptiaca*, which, through the kindness of Mr. A. D. Michael, he had received from Alexandria on January 19 last. It was stated that in travelling most of them had become loose, and had lost their waxen appendages; but a few still remained on the stems of their food-plant. In connection with this subject Mr. G. H. Verrall alluded to a Dipterous parasite of *Icerya* from Adelaide—*Lestophonous iceryæ*, Williston—which had been bred from *Icerya purchasi*, last February.—Mr. R. Adkin exhibited a long and interesting series of *Triphæna comes* from various parts of the south of England, Yorkshire, Forres, the Isle of Man, the Isle of Lewis, and the north of Ireland. —Mr. G. F. Hampson exhibited a series of varieties of *Plotheia frontalis*, Walk., which was the only species in the genus, and confined to Ceylon. He said that the varied forms of this species had been described under twenty-one different names by Walker, Felder, and Moore.—Mr. F. Merrifield showed a number of specimens of *Selenia illustraria*, of three different stocks, proving that the spring brood of this species, which passed the winter in the pupal stage, was, like the summer pupa, materially affected in colouring by the temperature to which the pupa had been exposed in its later stages. He thought this fact, coupled with similar results ascertained with respect to the single-brooded *Ennomos autumnaria*, indicated that the operating cause was one of wide general application. Captain Elwes said that in his experience in many parts of the Palæarctic region, where there was a combination of heat and moisture, all the commoner species of Lepidoptera occurring in this country attained a larger size and a greater brilliancy of colouring than in colder and drier regions; and he referred to such species, amongst others, as *Pieris brassicæ* and *Argynnis paphia*. The discussion was continued by Mr. Jacoby, Mr. Fenn, and others.—Mr. W. H. B. Fletcher exhibited a long series of *Zygæna loniceræ* from York, and *Zygæna filipendulæ* from Shoreham, Sussex; also a series of hybrids obtained by crossing these two species. He stated that the eggs obtained from these hybrids were all infertile. Lord Walsingham said this latter fact was extremely interesting.—Mr. F. W. Frohawk exhibited a living specimen of an ichneumon which had just emerged from a chrysalis of *Papilio taunus*.—Mr. C. J. Gahan exhibited a number of species belonging to the genera *Lema* and *Diabrotica*, and read a paper on them, entitled "On mimetic resemblances between species of the Coleopterous genera *Lema* and *Diabrotica*." Lord Walsingham, Mr. Jacoby, Colonel Swinhoe, and Mr. Champion took part in the discussion which ensued.

Chemical Society, March 5.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Crystalline form of the calcium salt of optically active glyceric acid, by A. E. Tutton. This paper presents the results of a complete crystallographical investigation of the calcium salt, $Ca(C_3H_5O_4)_2, 2H_2O$, of the optically active (dextro-rotatory) form of glyceric acid described by Frankland and Frew. The crystals are monoclinic, and are hemihedral. The ratio of the axis is $a : b : c = 1{\cdot}4469 : 1 : 0{\cdot}6694$.—Fermentations induced by the *Pneumococcus* of Friedländer, by P. F. Frankland, A. Stanley, and W. Frew. Brieger has pointed out that this micro-organism is capable of fermenting suitable solutions of glucose and cane-sugar. The authors have confirmed Brieger's observations, and have found that the organism also ferments maltose, milk-sugar, raffinose, dextrin, and mannitol, but not dulcitol. The fermentations of glucose and mannitol were specially studied. The products are, in each case, ethyl alcohol, acetic acid, generally a little formic acid, and a trace of succinic acid, carbon dioxide, and hydrogen. The glucose is less readily attacked than mannitol and cane-sugar, and both are only partially fermented. Quantitative results of the products of fermentation are given. For mannitol, the several products are in very close accord with the molecular proportions—$9C_2H_6O : 4C_2H_4O_2 : 12CO_2 : 8H_2$.—The volumetric estimation of tellurium, Part 2, by B. Brauner. When a solution of tellurium dioxide in sulphuric acid is titrated with permanganate, the following interchange occurs:—

$$2KMnO_4 + 4H_2SO_4 + 4TeO_2 = K_2SO_4 + Mn_2(SO_4)_3 + 4H_2O + 4TeO_3.$$

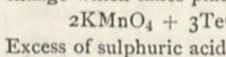
The manganic salt is destroyed with decinormal ferrous sulphate or oxalic acid, and then permanganate is added in slight excess. The quantity of tellurium dioxide is calculated from the volume of permanganate used, *minus* that corresponding to the amount of ferrous salt or oxalic acid added, as if the following action had taken place:—

$$2KMnO_4 + 3H_2SO_4 + 5TeO_2 = K_2SO_4 + 2MnSO_4 + 3H_2O + 5TeO_3.$$

The result is greater by 1 per cent. than that calculated, owing to the evolution of some oxygen. In alkaline solution, the change which takes place is

$$2KMnO_4 + 3TeO_2 = K_2O + 2MnO_2 + 3TeO_3.$$

Excess of sulphuric acid is added, and oxalic acid used for re-titration; the results are 0·35 per cent. too high.—Chloro- and bromo-derivatives of naphthol and naphthylamine, by H. E. Armstrong and E. C. Rossiter. The preparation of 1 : 4-dichloro-β-naphthol and 1 : 4 : 4′-trichloro-β-naphthol is described. The latter melts at 157°–158°; its acetyl derivative at 129°. 1-chloro-, 1 : 3- and 1 : 4-dichloro-, and 1 : 3 : 4- and 1 : 4 : 4′-trichloro-β-naphthol are all sulphonated with great ease, and yield derivatives of 1 : 3′- (Schaefer's) β-naphthol-sulphonic acid. Whereas 3′ : 1-dibromo- and bromochloro-β-naphthol—in which the position occupied by the sulpho-group in Schaefer's acid is occupied by bromine—are sulphonated less readily. Bromochloro- and dibromo-β-naphthol yield, on oxidation, 1 : 3 : 4-bromophthalic acid; and the bromochloro-β-naphthol, when distilled with PCl_5, gives 1 : 2 : 3′-trichloronaphthalene as one of its products. The authors find that, contrary to Smith's statement, the preparation of tetrabromonapththol is attended with considerable difficulty. The product of the action of excess of bromine on β-naphthol consists chiefly of tribromonaphthol. Nearly all the chloro- and bromo-derivatives of β-naphthol yield quinone derivatives when submitted to the action of nitric acid. The final product of the action of nitric acid is a phthalic acid. Alkaline permanganate gives a series of oxidation products: for example, the main product of its action on dibromo-β-naphthol is the acid,



, which, on distillation, yields bromophthalide.

Linnean Society, March 5.—Prof. Stewart, President, in the chair.—Mr. D. Morris exhibited a dwarf species of *Thrinax* which he found growing plentifully in the Island of Anguilla, West Indies, and which was apparently undescribed.—Mr. T. Christy exhibited the fruit of some undetermined species of plant which had been introduced into commerce by the name of Monchona, but the origin of which had not been ascertained.—Mr. J. E. Harting exhibited several instantaneous photographs (taken by Mr. W. H. St. Quintin in Yorkshire) of a living Great Bustard (*Otis tarda*), and gave a brief account of the recent visitation of several of these birds to England. Between December 9 and February 5 no fewer than seven had been shot, in Norfolk, Suffolk, Essex, Sussex, Hants, Wilts, and Carmarthenshire.—On behalf of Miss E. Barton, Dr. D. H. Scott gave the substance of a paper communicated by her, and entitled a morphological and systematic account of the fucaceous genus *Turbinaria*.—Mr. George Murray described some new species of *Caulerpa*, with observations on the position of the genus. In elucidation of this paper Mr. E. M. Holmes exhibited a large series of specimens, showing the extreme variability of the species of seaweeds which had been referred to this genus.—A paper was then read by Dr. John Lowe, on the specific identity of two forms of parasitic Crustacea, *Lerneonema spratta*, Sowerby, and *L. eucrasicholi*, Turton, the only two of the genus which had been hitherto recognized in Britain. A third species had been described by Dr. Salter (*Ann. Nat. Hist.*, 1850, p. 56), from the eye of the herring, and to this he gave the name of *L. Bairdii*, but his figures show clearly that they were drawn from imperfect specimens of *L. spratta*, which had been forcibly removed from the fish's eye, leaving the head behind. The parasites in question had only been found on the sprat, herring, and anchovy.

Royal Meteorological Society, March 18.—Dr. C. T. Williams, Vice-President, in the chair.—Mr. G. J. Symons, F.R.S., read a paper on the history of rain-gauges. It appears that Sir Christopher Wren, in 1663, designed not only the first rain-gauge, but also the first recording gauge, although the instrument was not constructed till 1670. The earliest known records of rainfall were made at the following places: Paris, 1668: Townley, in Lancashire, 1677; Zürich, 1708; and Londonderry, 1711. Mr. Symons gave a very full account of the various patterns of rain-gauges, and in most instances pointed out the merits or defects of each.—Mr. A. W. Clayden showed, on the screen, a number of interesting transparencies of photographs of clouds, lightning-flashes, and other meteorological phenomena.—The meeting was adjourned at 8.30 in order to allow the Fellows to inspect the exhibition of rain-gauges, evaporation-gauges, and new instruments, which had been arranged in the rooms of the Institution of Civil Engineers.

PARIS.

Academy of Sciences, March 16.—M. Duchartre in the chair.—Determination of the constant of aberration, by MM. Lœwy and Puiseux. (See Our Astronomical Column.)—On the equilibrium of fluid dielectrics in an electric field, by M. H. Poincaré.—On the different manifestations of phosphorescence of minerals under the influence of light and heat, by M. Henri Becquerel. The phosphoroscope contrived by the father of the author of this paper for the investigation of phosphorescence is well known. M. H. Becquerel has placed various specimens and varieties of fluor-spar in the phosphoroscope, and has observed the spectra they emit when illuminated by the electric spark or subjected to heat. A number of luminous bands are seen under either condition, and the wave-lengths of these have been determined. Chlorophane was one of the most interesting bodies examined. In rotating the disks of the phosphoroscope with increasing velocity, this substance emitted light of different tints, which tints corresponded to the appearance of bands of different refrangibilities in the emission spectrum. For a very slow movement of the disks the spectrum extended from about $\lambda 543$ to $\lambda 478$ with a maximum from about $\lambda 531$ to $\lambda 497$. On more rapid rotation, bands appeared at wave-lengths 557, 592, 606, and 492-478; afterwards, the velocity increasing, a band appeared at $\lambda 542$, and this became more brilliant than any of the others, whilst the maximum at 531-497 was replaced by a band at 492-478. Similar variations were observed when chlorophane and other varieties of fluor-spar were subjected to changes of temperature. One of the conclusions deduced from the results obtained is that the different spectra given by the same body are due to the presence in the body of different substances which form definite compounds under certain conditions of illumination and temperature.—On a new method for the determination of critical temperatures and pressures, and in particular, the critical temperatures and pressures of water, by MM. L. Cailletet and E. Colardeau. The determination of the critical temperature of water is extremely difficult by ordinary methods, because the tubes of glass burst under the enormous pressures required. The authors have devised a method which permits the experiment to be made without observation of the disappearance of the terminal surface of the liquid. The method not only gives the critical temperature but also the critical pressure, and the curve of tensions of the saturated vapour of the liquid up to the critical point. The manometer used for the determinations of the high pressures necessary registers up to 400 atmospheres, and is installed in the Eiffel Tower. No results are as yet given.—On the fossils found at Gourbesville by M. de Lapparent, by M. Albert Gaudry. As a complementary note to a previous one, M. Gaudry states that he has discovered the teeth of *Palæotherium magnum*, *Mastodon angustidens*, *Carcharodon*, and others, in the Gourbesville conglomerate.—Effect of cold on marine fishes, by M. A. F. Marion. The author gives some observations made at Provence on the resistance of certain species of fish to severe cold.—On the application of M. Lie's groups, by M. L. Autonne.—Graphical method for the determination of the relative values of the force of gravity in different places, by M. Alphonse Berget. One of the arrangements proposed consists of a pendulum carrying a small lens which concentrates the light from a lamp upon a movable sensitized film. A continuous, sinuous curve is thus obtained; which may be compared with that given by a standard pendulum at Paris or some other centre.—On the degree of complexity of gaseous molecules, by M. Marcel Brillouin.—On the transformations which accompany the carburation of iron by diamond, by M. F. Osmond. Some experiments which are described show—(1) that diamond itself is not cemented by iron, but first undergoes, from contact with this metal, a molecular transformation which renders it capable of cementation; (2) that the diffusion of carbon in iron has for a corollary a diffusion of the iron in the transformed diamond.—On the formation of coloured lacs, by M. Léo Vignon.—Researches on the dispersion in organic compounds (ethers), by MM. P. Barbier and L. Roux.—On the ptomaines, by M. Œchsner de Coninck.—Influence exercised by the extractive matters on the alcohol in spirits, by M. Ch. Blarez.—On the toxicity of the soluble products of tuberculous cultures, by MM. J. Héricourt and Charles Richet. A healthy rabbit was not affected by an injection of two grammes of the lymph, but died when the dose was increased to three grammes. A rabbit suffering from tubercle, but otherwise healthy, was killed in forty-eight hours with a dose of about the eighth part of the latter. It would therefore appear that much care should be taken in administering Dr. Koch's lymph to patients.

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