THURSDAY, MAY 8, 1890.

CHEMICAL TECHNOLOGY.

Chemical Technology; or Chemistry in its Applications to Arts and Manufactures. Edited by C. E. Groves, F.R.S., and W. Thorp, B.Sc. Vol. I., "Fuel and its Applications." By E. J. Mills, D.Sc., F.R.S., and F. J. Rowan, C.E. (London: Churchill, 1889.)

THIS work is described as substantially a new edition of the well-known "Chemical Technology" of Richardson and Watts, which in its turn was founded on the German work of Knapp. In its new form, however, it bears about as much resemblance to its prototype as the famous horse of Wallenstein does to the original animal—"The head, neck, legs, and part of the body have been repaired; all the rest is the real horse." How much of the real Knapp is left in the work takes some time and trouble to discover. We recognize here and there a woodcut—not always in the best state of preservation—but the descriptions appended even to these particular cuts are in most cases entirely recast, if not wholly rewritten. As the present work is to all intents and purposes an original production, it would have been better to have so described it. It may be that a sort of good-will has grown up around Richardson and Watts's "Technology" which the publishers desire to retain; but the connection between the two works is so slight that they are practically independent.

The present volume deals exclusively with fuel and its applications. The term fuel is employed in its widest possible sense, and its applications are treated of no less generally. The special employment of fuel in chemical manufactures is reserved for future treatment in the volumes which are concerned more particularly with these subjects. The most superficial comparison of this work with that upon which it is assumed to be founded will serve to show how enormous has been the advance in knowledge of the principles upon which the proper consumption of fuel depends. Take, for example, the question of smoke-prevention. In the preface to the 1856 edition it was stated that a method of smoke-prevention, although much wanted, had not then been discovered. The present work shows that we have changed all that. The idea of "consuming smoke" is obsolete. The conditions of complete combustion are to-day so well understood that it is only the indifference of manufacturers or the apathy of the authorities which prevents the greater part of industrial firing with solid fuel from being practically smokeless. Even if this were not so, gaseous fuel, the use of which is largely extending, is absolutely smokeless. This kind of fuel might be applied to many industries which have not yet adopted it, and without in any way hampering them, and would indeed be so applied if the authorities could be brought to regard it as coming within the definition of "the best practicable means" (to quote the words of the Act) for preventing smoke. We do not, of course, intend by this to insist on the exclusive adoption of gaseous fuel, although the time may come when, partly on economic and partly on sanitary grounds, such adoption may, as the late Sir C. W. Siemens predicted would be the case, become compulsory. How much the community might save, both in health and pocket, by the more systematic adoption of smoke-preventing arrangements, has been demonstrated over and over again, and we may well hope that the various exhibitions of appliances which seek to realize a consummation so devoutly to be wished may bring about this result in a not too distant future. The public may rest assured that efficient smoke-preventing appliances do exist, as the work before us abundantly demonstrates, and it ought to be the duty of the various centres of local government to insist on the more general adoption of these appliances. What can be done by a benevolent despotism in such a matter was well shown by the action of Lord Palmerston in the case of the metropolis, and there is nothing to prevent even such towns as Glasgow, Sheffield, Birmingham, and Newcastle from having atmospheres at least as sootless as that of London.

The out-put of coal in this country up to 1883 practically followed the law of Jevons as modified by Marshall. In that year it attained a maximum of nearly 164 million tons, or about four times the amount raised in 1850. In 1884 the quantity raised was 161 million tons, and in the following year it fell to about 159 millions. This diminution is due to various causes, partly natural and partly economic and social. It is, however, safe to say that a more intelligent appreciation of the principles which determine the conversion of the store of energy existing in coal into actual work has more than compensated for the smaller out-put. The world to-day gets more duty from its coal than it did even six years ago. Authorities differ slightly as to the manner in which the coal raised is distributed. According to Peckar, whose estimate seems to be preferred by the authors, it is somewhat as follows:—[1]

Destination.	Per cent.
Iron manufacture	32·40
Factories	21·87
Dwelling-houses	16·36
Gas- and water-works	6·46
Mining	6·38
Steamers	6·46
Railways	1·76
Copper-works	0·72
Sundries	0·64
Export	10·54

Although it is no necessary part of a treatise which is mainly concerned with the applications of fuel, the authors devote some considerable space to what may be termed the chemistry of coal-getting, *e.g.* the occurrence and nature of fire-damp, the relations of atmospheric temperature and pressure to its escape, fire-damp indicators, the influence of coal-dust on explosions, &c. On the whole, the information given is sound and accurate, and brought fairly well up to date. And the authors steer a very even and judicious course among points on which much difference of opinion still exists. We think, however, that the very careful and remarkable analyses of certain North Country explosions by the Messrs. Atkinson are worthy of more notice than they have apparently received, as they seem to be absolutely conclusive on the point that explosions can be originated and propagated by coal-dust alone.

[1] No explanation is given of the fact that these umbers add up to 103·59.

The question of the spontaneous ignition of coal scarcely meets with the treatment which its importance merits, and no reference is made to the work of the Royal Commission appointed in 1875 at the instigation of the Board of Trade and the Committee of Lloyd's to inquire into this subject. Many hundreds of vessels have without doubt been lost by the spontaneous ignition of coal cargoes, and there is a general belief that, with the considerable increase of temperature in steam-ships due to the introduction of high-pressure boilers and triple-expansion engines, the liability to spontaneous firing in the coal-bunkers is greatly augmented. The old idea of Berzelius, that the tendency to spontaneous ignition was mainly due to the presence of readily-oxidizable pyrites is now exploded. The experiments of Richters, Durand, and, in quite recent times, of Prof. Vivian Lewes, have shown that this substance has quite a subordinate effect. The cause is rather to be ascribed to the effect of absorbed or occluded oxygen upon finely-divided carbonaceous matter, *e.g.* dust or fine slack. The authors are also of this opinion, and state that the only method of preventing fire from such a cause is to keep the temperature of the mass of coal as low as possible by means of thorough ventilation by currents of air. In a paper recently read before the Institution of Naval Architects, in which this question is discussed, Prof. Lewes comes to the conclusion that this so-called ventilation is undoubtedly one of the most prolific causes of spontaneous ignition, and he instances the cases of the four colliers, *Euxine*, *Oliver Cromwell*, *Calcutta*, and *Corah*, which were loaded at Newcastle under the same tips, at the same time, with the same coal, from the same seam. The first three were bound for Aden, and were all ventilated. The *Corah* was bound for Bombay, and was not ventilated. The three thoroughly ventilated ships were totally lost from spontaneous ignition of their cargo, whilst the *Corah* reached Bombay in safety. Prof. Lewes points out that for ventilation to do any good, cool air would have to sweep continuously and freely through every part of the cargo, a condition impossible to attain, whilst anything short of that only increases the danger, the ordinary methods of ventilation supplying about the right amount of air to create the maximum amount of heating. A steam coal absorbs twice its bulk of oxygen, and takes about ten days to do it under favourable conditions, and it is this oxygen which in the next phase of the action enters into chemical combination and causes the serious heating.

One very remarkable change which is slowly making its way in this country is seen in the more extensive adoption of coal-washing machinery. Coal-washing machines have long been in use in Germany, France, and Belgium, and the exigencies of our iron manufacture are gradually necessitating their introduction in Great Britain, although the process has not yet reached the same degree of perfection as on the Continent. The effect of washing is to free the coal from pyrites and other mineral impurities. Of course it is only under special conditions that it can pay to subject the coal of this country to this process, but there is no doubt that as soon as the price of coal touches a certain point many coals which are practically unsaleable at present will be so treated. The authors describe a number of the more important coal-washing machines, and give details of their duty and cost.

The question of coking and coke-ovens naturally comes in for a very considerable share of attention, and practically all the more important methods are described and fairly well illustrated, and the general nature of the tars yielded by the various kinds of ovens is set forth, mainly on the authority of Mr. Watson Smith. One of the most valuable features in the work is the account given of the methods of using liquid fuel, and of the results obtained on various railways (principally Russian) and with various types of marine and stationary engines. The principles of domestic heating by solid and gaseous fuel, and relative efficiency of the various forms of open and closed grates and of gas stoves, are carefully stated, and considerable space is given to an examination of the modes of warming public buildings.

Analyses of boiler performances, and of the results obtained by mechanical stokers and by the application of gas-firing to boilers, methods of evaporation, with special reference to multiple effect apparatus, occupy a large portion of the section devoted to fuel in its applications to vaporization and evaporation ; and the concluding portions of the book are occupied by descriptions of special forms of kilns and furnaces.

The work is admirably printed, and on the whole well illustrated, and, what is very important in a book which is mainly a work of reference, it is furnished with an excellent index. T. E. Thorpe.

THE SELKIRK RANGE.

Among the Selkirk Glaciers; being the Account of a Rough Survey in the Rocky Mountain Regions of British Columbia. By William Spotswood Green, M.A., F.R.G.S. (London : Macmillan and Co., 1890.)

The Canadian Pacific Railway, after crossing the watershed of the Rocky Mountains by the Hector Pass, descends for some four thousand feet into the valley of the Columbia River. This, for a hundred and seventy miles, flows in a northerly direction, parallel with the crest of the range. Then, after a great sweep to the west, it flows southward, parallel to its former course, till it receives the Kootenay, the head waters of which rise only a mile and a half away from its own. The mountain-tract insulated by these rivers is called the Selkirk Mountains. It lies roughly parallel with the Rockies, and yet further west are the Gold Range and the Cascades. Thus the railway traverses a mountain region until the valley of the Frazer River, by which it finally emerges, begins to broaden out towards the sea. It is, to use Mr. Green's words, " a region of vast ravines and wide valleys, whose sides, when not bare rock precipices, are clad in sombre forests, through which wild mountain torrents rush from glacier sources to placid lakes, where, after resting for a while and reflecting the hoary cedars and hemlocks, they issue forth as great rivers, and with swift current hurry on to lose themselves in the Pacific."

Though the peaks of the Selkirks look down upon the railway, their recesses, before Mr. Green's visit, were still almost unknown. The reason is not difficult to discover. The forests which clothe their lower slopes are unusually dense—often almost impenetrable; the traveller often has to force every step of his way among great trees, upright and prostrate, hampered by a frequent undergrowth, till the forest at last gives place to alder scrub which seems to possess all the offensive properties of the dwarf pine in the Eastern Alps, and to be a yet greater obstacle to the mountaineer. Even hunting-parties of Indians but rarely visit the Selkirks.

Mr. Green left England in the summer of 1888, with the intention of exploring and making a rough survey of the chief peaks and glaciers in the Selkirk Mountains. He was accompanied by a relative, the Rev. H. Swanzy, and took with him a mountain outfit and the requisite scientific instruments, lent by the Royal Geographical Society, who had made a grant in aid of the expense of the expedition. Convenient head-quarters were found at Glacier Hotel (4122 feet), where the trains, as at Göschenen on the St. Gothard, halt for meals; but many nights of the six weeks passed in this region were necessarily spent under canvas.

The portion of the Selkirk Range explored by Mr. Green lies mainly to the south of Rogers Pass (where the railway crosses the watershed of that range at a height of 4275 feet). It is bounded on the east by Beaver Creek, a tributary of the Columbia, and on the north by the Illecellewaet River, by which the railway descends. Many of the peaks rise above 10,000 feet; few, if any, surpass 11,000; Mount Sir Donald, which is possibly the highest, being 10,645. But the average elevation of the range is considerable, and as the peaks rise precipitously some 6000 feet above the valleys, the scenery is very fine. Though not comparable with that of the Pennine Alps, the mountain outlines are not inferior to those in some districts of the Tyrol; and the forests, where spared by the frequent fires, are far more grand. The snow-line is at about 7000 feet, the forests ending at about 6000 feet; the glaciers are numerous, and sometimes large, the most important, named the Geikie Glacier, being about 4 miles long and 1000 yards wide. As usual, old moraines and huge erratics indicate that they formerly extended far below their present limit. The Selkirk Mountains, it may be observed, correspond in latitude with the Mendip Hills.

The dominant rocks are rather fine-grained micaceous schists, the structure of which has evidently been much modified by pressure, so that it is difficult to say whether this has produced crystallization of the constituents or modified a rock once more coarsely crystalline. A snow-white quartzite or quartz-schist is also very conspicuous, and not seldom caps, and no doubt has helped to determine, the higher peaks. In one part the rocks have a yet more ancient aspect, while to the north of the railway, near some lead-mines, a black slate exhibits certain markings which may possibly be the remains of graptolites. So far as can be inferred from the specimens brought back by Mr. Green, the Selkirks are composed of either later Archæan or earlier Palæozoic rocks—probably, in great part, of the former.

That Mr. Green can use the pen as well as the pencil was proved by his former book, on "The High Alps of New Zealand." The present one deals with a more limited district, and does not include any climb equal in difficulty to that of Mount Cook; but the ascent of Mount Bonney, the second, if not the highest, peak in the Selkirks, offered more than one "bad place," and the difficulties of these excursions were enhanced by being made without guides, and in many cases only by the two travellers. Thus they not only had to be their own porters, and often carry a load of forty pounds over rough ground and up steep ascents, but also were only "two on the rope," a phrase which is significant to mountaineers. Their toils and hardships—and these were many—the physical features and natural history of the country, are all graphically described: in short, Mr. Green has written a book which not only is a record of a mountain survey carried out under exceptional difficulties, but also indicates that he possesses in an exceptional degree the qualifications for a scientific traveller, and that he can write as well as he can climb.

T. G. B.

THE ANATOMY OF THE FROG.

The Anatomy of the Frog. By Dr. Alexander Ecker, Professor of Human and Comparative Anatomy in the University of Freiburg. Translated, with numerous Annotations and Additions, by George Haslam, M.D., Scientific Assistant in the Medical Department in the University of Zürich; formerly Assistant-Lecturer in Physiology in the Owens College, Victoria University, Manchester. Illustrated with many Wood Engravings and Two Coloured Plates, executed by Hofmann, Würzburg, Bavaria. (Oxford: Clarendon Press, 1889.)

THE rapid advance of physiology and morphology since the completion of Profs. Ecker's and Wiedersheim's "Anatomie des Frosches" has intensified the desire for a text-book which should deal in the most exhaustive manner with the anatomy of the frog, "the physiological domestic animal." Dr. Haslam remarks in his preface that he has done his best to bring the original of "Ecker's Frog" up to date, and in this task he has thoroughly succeeded. More than one hundred new figures, of which one-third are original, have been added, and copious lists of references to frog literature have been drawn up. He has restricted himself to the most careful and concise description of the various organs, and has abstained from entering into the discussion of such morphological questions as bear upon the comparison of the Anura with other Vertebrata. It would therefore be out of place to criticize the retention of names which—like atlas for the first free vertebra—if applied to the frog alone, are perfectly clear in their meaning, although their true morphological value, and therefore true denomination, may possibly be different. Every anatomist knows the difficulties connected with the frog's first spinal or hypoglossal nerve; its description on p. 182 will enable the reader to form his own opinion as to which of the three or four names he may adopt.

The first section of the book, dealing with the bones and joints, has remained unaltered, but the nomenclature generally used by English anatomists has been adopted throughout. The different nature of the clavicles and the precoracoid elements has been correctly described according to investigations made since the appearance of the German original in 1864. Howes's researches on the composition of the carpus and tarsus came too late to be embodied in the English edition, but the occipito-cervical region might have been more extensively treated.

The second section, on muscles, has not been changed. Here Fuerbringer's and de Man's nomenclature might, with great advantage, have been added to, or rather given instead of, many of the antiquated synonyms of Zenker and Dugès. These, however, are points of minor importance, and are, after all, matters of opinion.

The account of the central nervous system, and that of the sympathetic system, are quite new. The same applies to the heart. Some excellent figures illustrating the anatomy of the heart have been added, or have taken the place of old illustrations. The account of the arterial system remains practically unaltered, but many additions have been made to the venous system, and the description of the lymphatics has been rearranged. Much labour has been devoted to the histology of the alimentary canal and its appendages, to a great extent based upon original research made by the translator himself. A summary tabulation of the researches on the structure of the intestinal epithelium will be found on pp. 288–290. Section VI. is devoted to the respiratory organs and the neighbouring glands. A second carefully-finished and coloured plate contains many histological drawings, mostly original, of lungs, liver, and kidneys. The histological account of the thyreoid and thymus is almost entirely new, and a pair of lymphatic glands in the hyoid region, hitherto mentioned by Tolldt only, have been rediscovered and have been described as tonsils.

"A very large number of preparations have been made to investigate the vessels and uriniferous tubes of the kidneys; and the description of the remaining organs of this section (genital organs, adrenals, and fat-bodies) has received large additions from recent publications." The description of the minute structure of the fat-bodies, with illustrations, is a new and original contribution. The eighth, or last section, treats of the skin and sense-organs. The latter were treated somewhat summarily, and meagrely illustrated, in the original edition. This defect has been amended by so many new illustrations, and by the addition to the text of the results of so much recent research, that the whole section has assumed a completely new aspect. Especially may be mentioned the structure and distribution of the peculiar tegumentary papillæ and other tactile organs. The various other sense-organs, especially the ear, and above all the eye, have received much painstaking attentention, and have been amply illustrated by copies from the works of the most recent writers.

Of the 227 woodcuts we may say that they have been so carefully cut, and come out so clearly on the good paper, that the blue and red colours used for the vessels and muscles in the original edition are not at all missed. It would be going far beyond the scope of a general review to point out all the important additions and emendations which the new book contains (by the way, clearly indicated by square brackets), nor would it be fair to search for mistakes—of which, after all, there seem to be remarkably few. Those who use the work, whether for the sake of the many hundreds of references to the literature, or in order to be guided in the dissection necessary for a delicate experiment, will soon find that Dr. Haslam has greatly improved a book which was already good.

H. G.

OUR BOOK SHELF.

Syllabus of Elementary Dynamics. Part I. Linear Dynamics; with an Appendix on the Meanings of the Symbols in Physical Equations. (London: Macmillan and Co., 1890.)

THIS is a small pamphlet in which the author defines the terms usually found in works on dynamics. When dealing with measurements of quantities, he adopts an appropriate series of capital letters for specified units, and another set where the units are left undefined; thus equations containing the latter class of symbols possess more generality. Other units of higher dimensions are represented by capitals which are over-marked. Thus an acceleration of 10 feet per second per second is written as $10\bar{F}$. These are very useful for the author's purpose, though it requires no little time to become used to them. A few formulæ and results are obtained in connection with falling bodies, energy, and centre of mass.

Where quantities are represented by certain units and multipliers, it becomes necessary in every case to state any existing relations between the units employed. Instead of being under any such necessity, many physicists prefer to regard these multipliers as completely representing the quantities themselves. The advantages of such a system are discussed in the appendix, and a series of examples worked out in parallel columns, showing the advantage of the one or the other of the two methods suggested.

G. A. B.

Organic Evolution, as the Result of the Inheritance of Acquired Characters according to the Laws of Organic Growth. By Dr. G. H. Theodor Eimer. Translated by J. T. Cunningham, M.A., F.R.S.E. (London: Macmillan and Co., 1890.)

THE work of which this is a translation we have already reviewed. The only additional matter contributed by the translator seems to be an ill-advised reference to NATURE in the preface.

LETTERS TO THE EDITOR.

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

Bison not Aurochs.

IN his excellent article on the extermination of the American bison, "R. L." remarks (NATURE, p. 11) on the transatlantic practice of miscalling that animal a "buffalo"; but on the next page he writes of "its European congener the Lithuanian aurochs." This is to perpetuate a common error at least as bad. The "aurochs" (= ox of yore), Latinized by Cæsar in the form *urus*, is or was the *Bos primigenius* or *B. urus* of scientific nomenclature. It is wholly by mistake that in its extinction as a wild animal its ancient name was transferred to the bison, or *Zubr*. I would invite "R. L." to turn to the word "Bison" in Dr. Murray's "New English Dictionary," where he will find a reference to an article "*Wisunt*" in Schade's "Altdeutsches Wörterbuch," which ought to settle the question. I only wish one could ascertain to what animal the name "buffalo" strictly belongs. There unfortunately Dr. Murray does not help us.

ALFRED NEWTON.

Magdalene College, Cambridge, May 4.

Unstable Adjustments as affected by Isolation.

IN a brief passage in his very suggestive volume on "Darwinism," Mr. Wallace refers to a principle which seems to me to be worthy of much wider application than he has given. It is a key which requires only a little filing to prepare it for unlocking some difficult problems in divergent evolution. Speaking of the infertility of crosses, he says:—"It appears as if fertility depended on such delicate adjustment of the male and female elements to each other that, unless constantly kept up by the preservation of the most fertile individuals, sterility is always ready to arise. . . . So long as a species remains undivided, and in occupation of a continuous area, its fertility is kept up by natural selection; but the moment it becomes separated, either by geographical or selective isolation or by diversity of station or of habits, then, while each portion must be kept fertile *inter se*, there is nothing to prevent infertility arising between the two separated portions" (p. 184). Here is an application of the principle of segregation, or of like to like in groups that do not cross, in which indiscriminate separation is followed by increasing divergence in the different portions, not because they are exposed to different environments, not because there is any advantage in such divergence, not because there is any need that the function should be performed more perfectly in one portion than in the other, but because intergeneration, which is the principle by which correspondence of function is secured, has been suspended for some generations; and, in the absence of intergeneration, neither natural selection nor any other principle is capable of preserving complete correspondence. In organisms that reproduce sexually, the causes of divergence are many, though they may all be classed as causes of segregation, while the causes of correspondence with variation, whether of functions or of structures, are causes of intergeneration between partial segregations. If the environments surrounding the isolated portions are the same, the *use* of the environment, and therefore the forms of natural selection, may become divergent; if the use continues unchanged, some *useless divergence in the method of securing the use* may appear. Or, if all the relations to the environment, whether useful or useless, remain unchanged, "the adjustment of the male and female elements to each other" are liable to become slightly divergent, producing *mutual infertility*. Or the *preference of the sexes* for certain shades or arrangements of colour in their mates may become slightly different. Through some slight difference in the hereditary elements, distributed in each separated portion at the first, one or all of these causes of accumulated divergence may be introduced. I think it is evident that we have here a general principle, which is as applicable to a wide range of divergences as it is to the divergence that produces mutual infertility and sterility.

The context shows that the prominent idea in Mr. Wallace's mind was divergence in the adjustment of the male and female elements through correlation with "some diversity of form or colour," resulting from divergent forms of natural selection, that had been induced by exposure to "somewhat different conditions of life." But if the reasoning is correct in the sentences I have quoted, it gives an explanation of similar divergences when the separated portions are exposed to the same environment, *and where there is no possible advantage to be gained by divergence*. This is one of the principles I have used in the explanation of the divergences of Sandwich Island land mollusks; and I think that in the earlier stages of the development of infertility between allied forms it is often the only explanation that is applicable. It should, however, be remembered that, for divergence of this kind, it is not always necessary that the isolation should be either complete or very long continued, and that, when the forms that are not fully fertile with each other meet and more or less commingle, there is, through the very laws of propagation, without any aid from natural selection, a constant increase in the ratio of the pure breeds to the mongrels, and an accumulating intensity in the segregative instincts and the physiological incompatibilities. As this point has been fully discussed in my paper on "Divergent Evolution," I do not need to enlarge on it here (see Linn. Soc. Journ., Zoology, vol. xx. pp. 246–72).

There is, however, another phase of the subject which is indicated by Mr. Wallace's suggestion that infertility depends on "such a delicate adjustment" that it is more easily affected by isolation than some other adjustments. This is, I think, a very interesting point, as it suggests how it is that, in some cases at least, physiological divergence of this kind *is one of the first forms of divergence that arises*. But in some species other adjustments seem to be more delicate than this, and therefore more easily disturbed; while in others, several sets of adjustments, as colours and other recognition marks, with the preferences that correspond, and the habits of feeding and defence are in a state of equilibrium, the stability or instability of which is about the same as of that which determines the relations of the male and female elements. In this last class of cases, several forms of divergence may arise during the same stage of development, and that too when the isolated portions are exposed to the same environment. In some species a large number of characters are in a state of unstable adjustment. As Prof. Lankester has suggested near the close of his review of Mr. Wallace's book, this cause of divergence seems to be specially operative in the case of human faculties. But variability with plasticity of type is not the only condition that affects the stability of segregated portions of a species. Other things being equal, a single pair of any species is much less likely to represent the average of all the characters of the species than a million pairs. This consideration throws light on the comparative lack of divergence between the land animals of England and those of Ireland, which lack has been referred to by Mr. Wallace as an objection to my theory. In this case, many millions of some of the species were probably existing in each district at the time of the separation. As Prof. Lankester has pointed out, the representatives of the human species in the two districts have somewhat diverged; and the probability is, that if we were equally acquainted with the other species, we should find other examples of divergence in minor points. If the isolation is made more complete, and is longer continued, I believe the divergence will gradually become more apparent.

Mr. Wallace has mentioned another class of divergences which are best explained by the principle we are now considering —as he seems to have apprehended, though the process is not stated here as clearly as when discussing the divergences that produce infertility. The passage is as follows:—"The enormously lengthened plumes of the bird of paradise and of the peacock must be rather injurious than beneficial in the birds' ordinary life. The fact that they have been developed to so great an extent in a few species is an indication of such perfect adaptation to the conditions of existence, such complete success in the battle of life, that there is, in the adult male at all events, *a surplus of strength, vitality, and growth-power which is able to expend itself in this way without injury*. That such is the case is shown by the great abundance of most of the species which possess *these wonderful superfluities of plumage*. . . . Why, in allied species, the development of accessory plumes has taken different forms, we are unable to say, except that it may be due to that individual variability, which has served as the starting-point for so much that seems to us strange in form, or fantastic in colour, both in the animal and vegetable world" ("Darwinism," p. 293. The italics are mine).

It is no small gratification to me that Mr. Wallace has found this principle of unstable adjustment worthy of application to two important classes of divergences; and that, in the case of one of these classes, he has recognized that correspondence in such adjustments cannot be continuously maintained between the isolated portions of a species. I, moreover, have some hope that, when he understands the relation in which instability and isolation stand to each other in my theory, he will admit that it throws some light on the remarkable divergences of Sandwich Island land mollusks. The subject was only incidentally touched upon in my paper on "Divergent Evolution through Cumulative Segregation," but will be more fully discussed in a supplemental paper on "Intensive Segregation."

26 Concession, Osaka, Japan. JOHN T. GULICK.

Coral Reefs, Fossil and Recent.

MANY Alpine geologists believe the limestone and dolomite mountains which form so peculiarly beautiful and interesting a part of our Eastern Alps to be in great part composed of Triassic coral reefs. If this be so, their geological structure must necessarily contribute much towards elucidating the discussion concerning the origin of atolls and other forms of recent coral reefs. In this discussion, which has chiefly been carried on in England, the structure of our Triassic limestone mountains has been left out of account in a manner very surprising to me, considering that authorities like Richthofen and Mojsisovics have declared them to be remnants of coral reefs.

I have made a number of Alpine ascents in the dolomites of South Tyrol, chiefly in the districts of the Höhlensteinthal, Primiero, and the Langkofel, and have satisfied myself that the theory of the coralligenous origin of great part of these mountains is the only one which will explain the position and nature of the rocks composing them.

Not only do we observe in many places the massy dolomite alternating at its margin with sedimentary deep-sea deposit of partly non-calcareous nature, but we even find old reef surfaces exposed to view. The volcanic porphyritic lava, or rather tufa, which was spread over the sea-bottom after the termination of the Buchensteiner period (middle Triassic, Mediterranean province) covers the deep-sea deposits of earlier date, but leaves the masses of dolomite free. Here and there, as on the *Plattkofel*, it can be seen overlying the foot of the actual reef precipice and there ending. It is covered by similar dolomite of a later date.

Many observations by Dana, Walther ("Korallriffe des Rothen Meeres"), and others, have shown that old coral reefs, about the nature of which there can be no doubt, are often dolomitic. The structure of Tertiary coral reefs on the Sinai peninsula, about the origin of which no doubt can be entertained, is actually identical with the structure of some Triassic dolomite I have examined here. I believe myself, for these, and many other reasons, justified in agreeing with Richthofen, &c., and in assuming that many Tyrolese Triassic limestones and dolomites are coralligenous.

The zones of the Mediterranean Trias differ altogether from the Trias in Germany. Other limits must here be recognized. In this respect I follow Mojsisovics. In the lowest Triassic no coral reefs are observed; also in the zones corresponding to the German Muschelkalk, we find, although these deposits are usually calcareous, no reefs of any size. It is in the zones distinguished as Buchensteiner, Wengener, and Cassianer-Schichten, that the great massy, unstratified reefs of South Tyrol are met with. The Upper Triassic layers, known as Raiblerschichten and Dachsteinkalk, are in South Tyrol mostly stratified, and in my opinion sedimentary, not coralligenous. Numerous faults traverse South Tyrol and break up the whole, only slightly folded Triassic system, into numerous plates (*Tafeln*) which are elevated on one (usually the northern) and depressed on the other (usually the southern) side. Liassic, Jurassic, and even Cretaceous layers rest on the depressed margins of the plates. Elsewhere these have been entirely removed, and the underlying Triassic reefs, capped with remains of sedimentary Dachsteinkalk and Raiblerschichten, have been laid bare. On the elevated margins along the faults also the Triassic layers have been removed. It is clear that somewhere between the subsided margin of the plates covered with Jurassic deposit, and the elevated margin, laid bare down to the Dyas, there must be places where the erosion has just reached the middle Triassic reefs. Here it is that we find parts of natural reef surfaces exposed.

The Lower Triassic Werfener Schichten are divided from the middle Triassic by deposits of gypsum, which show that the sea receded after the Werfener Schichten had been formed. Afterwards the sea returned, and it is clear that it must have risen at least as high as the later layers are thick, whilst or before they were deposited. The fossils in the sedimentary deposits between the masses of structureless dolomite show that the depth increased during their deposition. These sedimentary deposits alternate, as above stated, at their margins with the dolomite at the foot of the reef precipices. Therefore the dolomite also was formed during the rise of the water, for which I shall henceforth use the more exact expression introduced by Suess—positive shifting of the coast-line.

The dolomite masses are coral reefs. They have been formed during a period of positive shifting of coast-line, therefore we may assume that the high coral reefs now living and growing may have attained their astonishing altitude over the sea-bottom during a period of positive coast-line shifting. The dolomite masses of the Wengener and Cassianer period show no trace of stratification, such as is observed in the lower Muschelkalk and in the higher Dachsteinkalk. Moreover the dolomite has the same perfectly uniform structure from top to bottom; and on the vertical cliffs produced by erosion, which are often several thousand feet high, no trace of a stratified basal part can be detected. Everywhere the massy dolomite rests on discordant older layers, or—as usually at the reef margin—on the simultaneously deposited deep-sea sediment.

I will now proceed to utilize the facts here outlined in criticizing the discussion between the advocates of Darwin's and Dr. Murray's theories concerning the origin of coral reefs. I may say at once that all the phenomena observed in the dolomites of South Tyrol corroborate Darwin's subsidence theory, whereas they do not find explanation if we accept Dr. Murray's. It is the latter, therefore, which requires a closer examination.

Dr. Murray says that on slight elevations of the sea-bottom calcareous sediment accumulates, whereas in the greater surrounding depths this is not the case in consequence of the increase of dissolving power of sea-water with increasing depth. I accept this, and I believe that the caps of stratified Dachsteinkalk on the summits of many of the middle Triassic reefs in South Tyrol have been formed in this manner after the growth of the reefs had terminated. The positive coast-line shifting led to a horizontal extension of the Triassic Mediterranean northward, and a junction with colder northern seas, which caused a cooling of the water in the bay of South Tyrol, and thereby terminated the existence of reef-building corals. The positive coast-line shifting continued, and during its progress the Dachsteinkalk was deposited on the summits of the reefs, whilst the intermediate deeper spaces were left free from calcareous deposit—in accordance with Dr. Murray's view. There is, however, as above stated, nowhere a trace of stratified calcareous sediment forming a *basement* or nucleus to any one of the dolomite masses.

Dr. Murray then goes on to say that, as soon as the accumulating calcareous sediment has reached the region of coral growth, say the 20-fathom line, corals will grow on it, and an isolated atoll rising precipitously, perhaps ten thousand feet from the sea-bottom, will be formed. Against this it must be objected that the soft Globigerina ooze, which forms the whole of the atoll-peak, with the exception of an insignificant coral cap, could never attain such precipitous slopes as the atolls usually have. A slope of 45° or more could never be formed. The fossil deposits of this kind observed in South Tyrol (*Uebergussschichten* of Mojsisovics) nowhere terminate abruptly like the reefs. Neither is a slope of this kind anywhere observed in the region of the Dachsteinkalk.

Then, according to Dr. Murray, an atoll is formed by the solution of the lime in the centre of the reef. Although the Oolithes discovered in reef regions by Walther show clearly enough that there cannot be any solution other than what is compensated by redeposition, in any enclosed lagoon, I would like to draw attention to the logical discrepancy in this part of Dr. Murray's theory. First, a limestone cone is built, because the lime is deposited more rapidly than it can be dissolved. Then a lagoon is formed because the solution exceeds the accumulation, and this on the same spot, in still shallower and less powerfully dissolving water, and in spite of the relative stagnancy of the water in the lagoon and the limestone material, which is continually washed into the lagoon from the parts of the surrounding reef, which lie above the level of the sea. I think that gives the *coup de grâce* to Dr. Murray's atoll theory.

There remains yet something to be said on his ideas concerning the lateral growth of reefs. There can be no doubt that there is such lateral growth, and that the band of living coral on the reef margin can advance towards deeper water on a basis of coral fragments which have fallen from the outward growing face of the reef. As the corals near the surface grow more rapidly than those further down, the advancing reef face must ever tend to become overhanging; parts of the living coral must therefore frequently break off, fall down, and accumulate below. But there is a limit to this lateral growth which restricts it so considerably that it will by no means explain the formation of masses of dolomite 4000 feet thick, like the Cimon della Pala, for instance; and far less will it enable an atoll rising 10,000 feet or more from the bottom of the sea to extend horizontally. The amount of material *available* for the formation of a basis whereon the laterally growing corals may find footing is limited, and grows only in proportion to the circumference of a reef. The amount of material *required* for this purpose grows in a much more rapid proportion because it has to cover the surface of the growing cone.

Take a simple case. An ordinary straight fringing reef advances on a bottom of 10° inclination straight outward. At a distance of 280 metres from the shore a depth of 50 metres is reached. Further lateral growth is only possible on a talus of coral *débris*. 560 metres from the shore the depth is 100 metres. If the talus will lie at an angle of 45°, an amount of 50 cubic metres extended over a surface of 71 square metres will be necessary for the advance of each 1 metre's length of reef a distance of 1 metre. This talus is contributed from 50 square metres of growing coral (vertical reef face). At a distance of 5600 metres from the shore the depth will be 1000 metres. Every 50 square metres of growing reef face will have to furnish 950 cubic metres of material to enable the reef to advance 1 metre. These 950 cubic metres will be distributed over a surface of 1350 square metres. In other words, the reef will advance nineteen times as slowly as it does 560 metres from the shore, whilst the surface which is exposed to the dissolving effect of the sea-water has also increased nineteen-fold. Where an ocean-current strikes such an incline, no Globigerina ooze can be deposited on it, and here the dissolving action of sea-water will balance the accumulation of coral *débris* long before such a height—of 1000 metres—is attained. It is clear that as soon as such equilibrium is reached there is a limit to the further extension of the reef in that direction. On the opposite side, however, where ooze will accumulate and protect the advancing reef from solution, such advance would be possible, but on that side the growth of coral is notoriously slow. Certainly, when the foot of the reef has advanced to depths below the zone of protecting Globigerina ooze no further lateral growth in *any* direction will be possible, and on the whole I should not think that lateral growth can play any considerable part in the formation of great reefs. Only positive coast-line shifting has such a result. In places where there is no such coast-line shifting (Gulf of Suez) the reefs are exceedingly small and insignificant.

Although therefore lateral growth no doubt does take place, it is not the actual cause of the formation of the great coral reefs.

We must, I think, revert to Darwin's subsidence theory, which is equally proved by the untenability of the hypothesis suggested for the purpose of superseding it, and by the direct evidence of the structure of the Triassic reefs in the Eastern Alps, which have actually attained their immense thickness during a period of positive shifting of the coast-line. R. von Lendenfeld.

Innsbruck.

Slugs and Thorns.

In Nature, vol. xli. p. 393, I pointed out that thorns might not always be a protection from snails and slugs, since they were found in the stomach of a European slug, *Parmacella*. In further confirmation of this view, I have to-day dissected a number of sharp, straight, reddish-brown thorns, over a millimetre long, from the intestinal tract of *Ariolimax niger*, var. nov. *maculatus*, a slug of rather doubtful affinities (possibly referable to *A. andersoni*), received from Dr. J. G. Cooper, who found it under drift-wood at Haywards, California. It is curious that the thorns do not pierce the intestine, but they appear to cause no inconvenience. T. D. A. Cockerell.

West Cliff, Colorado, April 21.

COMETS OF SHORT PERIOD.

It is now generally accepted that the periodic comets of our system did not originate in it, but are bodies captured from outer space by one of the planets, the parabolic orbits in which they approached the system being transformed into elliptical ones. On account of the perturbing action of Jupiter, however, the orbits of short-period comets are liable to considerable modifications, and it is practically impossible to identify two apparitions of the same comet without laborious computations of the perturbations which it must have been subjected to between the two epochs. But even such computations may lead to a negative result, for frequently comets quite distinct have elements very much alike, probably because they are parts of an old comet travelling along the same orbit at greater or less intervals.

In some recent investigations on the capture theory of comets (*Bulletin Astronomique*, June and July 1889), M. Tisserand developed a relation that might be employed to determine the possibility of identity of comets whose elliptical elements are known. This criterion depends upon the fact that the velocity of a body revolving round a central one is the same for equal radius-vectors. In the case of a comet having a parabolic orbit coming under the influence of a planet, the latter plays the part of the central body, and the relative velocity of the comet with reference to it will be the same at the point of entry into the sphere of attraction as at the point of departure from it, the one point being in the old orbit, the other in the new one. If two comets are identical, their velocities with reference to the perturbing planet will be the same at these points.[1]

M. L. Schulhof has discussed the possibility of identity of several comets by means of M. Tisserand's formula (*Bull. Astr.*, November and December 1889, and *Astr. Nach.*, 2964), and the following tables contain the values of n found for those having periods from 3·3 to 8·8 years. In the first table, the comets whose periods are well known are given; in the second, those having uncertain periods. Comets which have undergone strong perturbations since discovery, and those for which perturbations prior to the first known apparition have been calculated, are given more than once, and the year indicated for which the elements are found. The symbols used have their usual signification, and l is the longitude of the comet at the point of nearest approach to Jupiter.

Comets of Known Period.

Name of Comet.	Elements of Orbit. n	π °	☊ °	i °	e	a	l °
1. Denning, V. 1881 ...	...0·414	19	66	7	0·83	4·23	223
2. Brorsen, 1842 ...	...0·466	112	104	46	0·76	2·99	284
,, 1890 ...	...0·475	116	101	29	0·81	3·10	284
3. Finlay, VII. 1886 ...	...0·483	8	52	3	0 72	3·54	205
4. Lexell, 1767	...0·483	26	352	33	0·33	4·45	163
,, 1770	...0·486	356	132	2	0·79	3·16	184
,, 1779	...0·478	159	178	18	0·91	60·10	184
5. Biela, 1772	...0·486	110	257	17	0·72	3·58	269
,, 1852	.. 0·482	109	246	13	0·76	3·53	269
6. Wolf, 1868	.. 0·492	6	208	29	0·39	4·18	211
,, III. 1884 ...	...0·497	19	206	25	0·56	3·58	210
7. D'Arrest, 1851 ...	...0·503	323	148	14	0·66	3·44	153
,, 1883 ...	...0·504	319	146	16	0·63	3·55	153
8. Faye, 1814	...0·509	55	225	7	0·56	3·83	212
,, 1880	...0·507	51	210	11	0·55	3·85	208
9. Winnecke, 1809 ...	...0·509	274	113	10	0·75	3·21	107
,, 1886 ...	...0·509	276	104	14	0·73	3·23	109
10. Tempel$_1$, 1869 ...	...0 527	43	297	5	0·66	3·11	223
11. Brooks, 1885	...0·531	203	179	8	0·39	8·99	185
,, V. 1889 ...	...0·530	1	18	6	0·47	3·68	185
12. De Vico, 1678 ...	...0·542	323	163	3	0·63	3·07	143
,, I. 1844 ...	...0·537	343	64	3	0·62	3·10	163
13. Barnard, II. 1884 ...	...0·556	306	5	5	0·58	3·08	126
14. Tempel$_2$, 1873 ...	...0·562	306	121	13	0·55	3·00	126
15. Tempel$_1$, 1856 ...	...0·591	236	103	6	0 53	3·13	56
,, 1885	.. 0·589	241	72	11	3·41	3·49	61
16. Encke, 1795	...0·591	157	335	14	0·85	2·21	335

Comets of Uncertain Period.

Name of Comet.	n	π	☊	i	e	a	l
1. Comet of 1585 ...	...0·484	10	38	5	0·70	3·61	213
2. Grischau, I. 1743 ...	...0·525	93	87	2	0·72	3·09	271
3. Helfenzrieda, II. 1766	...0·493	251	74	8	0·86	2·93	80
4. Pigott, 1783	...0·473	50	56	45	0·55	3·26	233
5. Blainpain, IV. 1819	...0·517	68	78	9	0·71	3·11	248
6. Tuttle, III. 1858 ...	...0·527	201	175	20	0·67	3·52	357
7. Coggia, VII. 1873	...0·484	86	249	26	0·76	3·19	255
8. Brooks, IV. 1886 ...	...0·553	230	53	13	0·61	3·41	54
9. Swift, VI. 1889 ...	...0·462	40	330	10	0·68	4·27	189

The value of n therefore found by the formula given is almost constant for the 21 known short-period comets, being contained within the limits 0·41 for Denning's comet, and 0·59 for Encke's and Tempel's comets.

It will also be seen that only five comets have their minimum distance to Jupiter's orbit between $l = 284°$ and $l = 112°$, while twelve have the point of nearest approach between $l = 153°$ and $l = 233°$. This unequal distribution along the ecliptic cannot be accidental, and

[1] M. Tisserand expressed the criterion very approximately by the formula—

$$\frac{1}{a_1} - \frac{1}{a_2} = \frac{2\sqrt{A}}{R^2} (\cos i_2\sqrt{p_2} - \cos i_1\sqrt{p_1}),$$

where a_1, a_2, p_1, p_2, i_1, i_2, are the semi-major axes, parameters, and inclinations of the old and new orbits; A and R the planet's semi-major axis and radius-vector at the point of nearest approach. This relation may be divided up into two parts, having the form—

$$n = \frac{1}{a} + \frac{2\sqrt{A}}{R} \cos i\sqrt{p}.$$

is in favour of the capture of comets by Jupiter. In fact, the accumulation of these points about $l = 192°$, which is the longitude of Jupiter's aphelion, may be partially explained by the circumstance that at this point Jupiter as well as the comets move more slowly than at perihelion, hence the sphere of attraction of the planet is sensibly extended, and its action exercised for a longer time on bodies moving in its neighbourhood.

The similarity of the elements of many comets is very manifest from the above tables, and M. Schulhof discusses in detail the probable identity of such. During last year Mr. Chandler brought forward evidence that Brooks's comet, V. 1889, was identical with the celebrated lost comet of Lexell, and the latter comet has also been asserted to be identical with that of Finlay, VII. 1886, to which it presents many points of resemblance. It is shown in the discussion that, by computing the orbit of Brooks's comet before 1886, the question of its identity with that of Lexell may be settled, while an extended calculation of the perturbations undergone by Finlay's comet indicates that it could not have been near Jupiter in 1779, and hence it is probably not identical with Lexell's comet. The elements of Finlay's comet are also very similar to those of Vico's comet, I. 1844. In order that the two may be identical, it is necessary that Mars should have augmented the period of Vico's comet by almost two years between 1844 and 1886.

The elements of Denning's comet present a certain analogy with Pigott's comet, but the two are shown to be certainly distinct.

Blainpain's comet, 1819, and that of Grischau, 1743, are most probably identical, and the elements of both these present a strong analogy with those of Tempel's comet, so that it is not impossible that this last comet is identical with the other two, or at least with Grischau's comet.

Whether Coggia's comet, VII. 1873, is identical with Pons's comet, I. 1818, is not settled. It is interesting to remark, however, that the value of $n_j = 0{\cdot}484$, corresponding to a period of 5·67 years for Pons's comet, is exactly equal to that of Biela's comet. This, therefore, appears to confirm the opinion that both Biela's comet and that of Coggia represent the *débris* of an old comet, for, in the case of the division of the materials forming a comet, n_j may be regarded as constant for each of the portions detached.

To decide the question of the identity of Winnecke's with Helfenzrieda's comet, the perturbations undergone by the former towards 1800, when it approached very near to Jupiter, have been found, and it is shown that for the identity to be possible it must have moved faster in its orbit before 1800 than it does now—that is, the period must have been shorter.

This discussion of cometary identities, coupled with M. Tisserand's elaborate investigations, supports strongly Laplace's hypothesis that comets coming from stellar space, and moving in parabolic orbits, only become periodic by the perturbing action of one of the planets. This theory best explains the origin of the families of comets that cluster round the major planets, and the well-established fact of the disintegration of certain periodic comets, as Biela's in 1846, and Brooks's in 1889. Indeed, such disintegration must eventually happen to all periodic comets.

RICHARD A. GREGORY.

THE JOURNAL OF MORPHOLOGY.[1]

THE issues before us constitute the first three parts of the third volume of this excellent journal. They contain 502 pages with numerous plates and a vast number of woodcuts. The chief contribution in the June number is that of Dr. Macmurrich, on "The Actinaria of the Bahama Islands." The author's material was collected during the summer of 1887, in connection with the work of the marine zoological station of the Johns Hopkins University. The monograph forms a very valuable contribution to the literature of the Actiniæ, and it may be regarded as a first step towards a rational comprehension of the tropical members of this group. It is the more welcome to us at the present juncture, in view of the revision of our native Actiniæ now progressing, in the hands of Haddon and his pupils; and we cannot but regard the excellent results obtained by Macmurrich as furnishing an additional argument in favour of the advantages of a peripatetic University marine laboratory, as compared with one of fixed habitat. Fourteen species are described, of which three are new. The illustrations are particularly good, and the following distributional conclusion is arrived at, viz. that

> "so far as the Actinaria are concerned two great areas of distribution can be defined,—the Indo-Pacific, including the Indian and Pacific Oceans and the seas connected with them, such as the Red Sea; and the Atlantic, including in this the Mediterranean. The Caribbean region of the Atlantic is, however, to be separated from the Atlantic region and united with the Indo-Pacific, the relationships of its Actinaria being very certainly with those of that region."

Of the remaining papers in the first part, two are by Dr. R. W. Shufeldt, and they treat respectively of "The Comparative Osteology of the Families of the North American Passeres," and of "The Anatomy of *Speotyto cunicularia hypogæa*." Both communications are written and illustrated in that peculiar style for which their author is notorious. In the first-named paper the author reverts to his recently expressed belief in a near kinship between the swifts and swallows, but he adds nothing of fresh interest in this debated question. His papers bear the mark of honest work, and we wish them a favourable reception.

The last communication is one of 8-9 pages upon the "Variation of the Spinal Nerves in the Caudal Region of the Domestic Pigeon," by J. I. Peck. Although short, it is the outcome of a laborious investigation instituted to ascertain whether the spinal nerves vary in the same ratio as the caudal vertebræ, "or whether they remain constant in number and position of exit from the vertebral canal, without reference to the number of vertebræ themselves." One very interesting result of the author's investigation is the discovery that the coccyx does not diminish in length proportionate to the increase in number of the free caudal vertebræ—on the contrary, it is longest where the said vertebræ are most numerous; therefore, the detachment of the supernumerary vertebræ from the pygostyle would appear to be due to influences at first productive of a lengthening of the entire caudal region. The interest of this topic is vastly increased, on reflection that the assiduity of a Parker has shown us that our swans and ducklings are the bearers of a tail potentially longer than that of the Saururian, Archæopteryx.

In the September issue there are two papers, and each is a valuable monograph of its kind. That which will command most attention is the thesis by Prof. Cope upon "The Mechanical Causes of the Development of the Hard Parts of the Mammalia." To this subject there are devoted 150 pages, 5 plates, and close upon 100 most admirable woodcuts.[1] The paper is, for the most part, an elaborated *résumé* of the author's earlier and scattered contributions upon the subject under discussion; and with

[1] *The Journal of Morphology*, June to December 1889. (Boston: Ginn and Co.)

[1] We wish we could see this author's voluminous treatises invariably illustrated in a manner similarly befitting their contents. We cannot refrain from comparing the one under review with that on the "Batrachia of North America" recently published under the auspices of the United States National Museum. The illustrations in this are as poor as those referred to above are excellent: carelessly drawn, badly planned, miserably lettered, and in places misleading (if not inaccurate), they "illustrate though they hardly adorn" the text to which they are appended, while they render a large portion of the same of little or no avail for working purposes.

these he has incorporated the allied work and generalizations of Ryder and other collaborateurs, the whole being woven into a connected argument. The author declares at the outset that he is the more convinced "that it is the habit that has given rise to the structures of animals, and not the structures which have forced animals to adopt their special habits," while he sets himself to discover, "in the light of the descent traced by palæontology, the mechanical causes for the existence of the salient characters of the skeleton and dentition of the Mammalia." The paper abounds in suggestive and ingenious passages, and the author sums up his conclusions in the words:—

"The general law which we may derive from the preceding evidence is, that in biological growth, as in ordinary mechanics, identical causes produce identical results."

We have, in all, that which savours of rank Lamarckism; and the assiduous author of the remarks we have cited is (as our readers have lately been made aware), clearly, no believer in the non-transmission of acquired characters. He asserts that

"since the modifications acquired by use during life are necessarily useful, it seems that, according to the post-Darwinians, the only way of acquiring useful variations known to us, is excluded from the process of organic evolution;"

and further, that

"were this hypothesis true, there would have been no evolution."

Again, he writes,

"in spite of Weismann's theory to the contrary, so long as the germ-plasma is subject to nutrition, it is subject to influences during the adult life of an animal, and it would be an exception to all other tissues were it not so."

The second and last paper in the September issue is by W. M. Wheeler, upon "The Embryology of *Blatta germanica* and *Doryphora decemlineata*." It is illustrated by seven exquisite plates, which, we are glad to note, are of native (American) origin. In testimony to the thoroughness of the author's work, it need only be said that he professes to be able to tell "just what position any oöthecal egg held in the ovary, or just what position any egg in the ovary will hold in the capsule." Evidence of direct cell-division is adduced, and the author's observations under this head have a most important bearing upon the allied researches of Carnoy. The author records the discovery of the very early appearance and paired arrangement of the Malpighian tubes, and he regards the facts to which he alludes as indicating "that at one time they opened on the surface of the body, and that their orifices were subsequently carried in by a deepening of the proctodæal invagination," and that "probably these tubes in insects are homologous with the anal tubes of *Echiurus* larva, which are modified segmental organs." Gegenbaur, as is well known, long ago postulated such an origin for the excretory apparatus of the Insecta, and Beddard has lately substantiated his belief, on argument from analogy to the Chætopod worms, in which he finds (*Acanthodrilus*) evidences of such an inturning of undoubted nephridia. The author has investigated, among other things, the orientation in oökinesis, and he draws the conclusion that "the force of gravitation has no perceptible effect on the development of the eggs of *Blatta*, but that these highly differentiated eggs, utterly unable to revolve in their envelopes, like the eggs of birds and frogs, have their constituents prearranged, and the paths of their nuclei predetermined, with reference to the parts of the embryo."

In the December number of the journal, Dr. Shufeldt communicates a detailed work "On the Position of *Chamæa* in the System" (28 pp.). In this welcome addition to his previous papers on the smaller Passerines, the author gives a short description of the pterylosis, visceral anatomy, and myology of the bird, and deals at greater length with its skeletal anatomy; he concludes that the Wren Tit is allied to the Bush Tits (*Psaltriparus*) rather than to the true Wrens. Dr. G. Baur rushes into print with two short notes, "On the Morphology of the Vertebrate Skull," and "On the Morphology of Ribs and the Fate of the Actinosts of the Median Fins of Fishes," respectively. In the twelve short pages devoted to the two, the conclusions are arrived at that "the doctrine of the 'otic' bones established by Prof. Huxley twenty-five years ago, and held since that time by nearly all morphologists, is incorrect," and that "the elements of the anal and caudal fins of fishes . . . are represented by the chevron bones of the tail vertebræ, which are the partial homologues of the actinosts." The author's proposal to revive the term "petrosal" for that element now known as the "pro-otic" is especially to be deplored. These notes, although not wholly destitute of merit, are premature. They deal with questions in morphology which have taxed the powers of the greatest anatomists, and which are not to be summarily disposed of in a succession of scrappy communications, any one of which may more or less completely contradict its predecessors. If, in respect to these leading topics, every intelligent inquirer is thus to dogmatically foist upon the public his musings upon facts observed in individual specimens, to say nothing of others pitchforked in second-hand, and which he has therefore not observed at all, what is to become of our already too voluminous literature? We cannot allow to pass unnoticed the contraction of Theromorpha to "Theromora"; life is too short for actions of this kind, even if etymologically justifiable. The remaining contribution is by E. B. Wilson, upon "The Embryology of the Earthworm" (55 pp.). It is an extended account of investigations previously announced in an earlier issue of the same journal; and it is, moreover, very welcome now that current research is revealing in the earthworms an altogether unexpected and intensely interesting range of modification. The author's results raise momentous questions affecting the most important of current morphological beliefs; while largely confirmatory of the recent work of Kleinenberg, they run counter to the same in matters of vital importance, and interest in them is thereby enhanced. The most important topics dealt with are the origin of the mesoblast and the development and morphology of the head (prostomium). The author asserts that Kleinenberg was in error in his account of the origin of the first-named, and he criticizes those facts and deductions which lead him to reject the ordinary conception of the mesoblast as an embryonic layer: he attempts to show that the cerebral ganglia do not arise independently of the rest of the nervous system, and that the cavity of the prostomium is from the first unpaired. These and other lines of investigation have led him to a reconsideration of the annelid Trochosphere, and that he regards as "a secondary larval form," which has "arisen from an elongated segmented ancestral form, . . . the head region or prostomium being enormously developed, . . . and the trunk region more or less reduced." The author confirms Kleinenberg's discovery of the remarkable "cleavage-pore," and suggests a probable significance for the same. He regards both muscular and glandular segments of the nephridia as ectoblastic in origin, and he adduces reason for suspecting that the Hirudinea may formerly have possessed setæ. The last-named is by no means the least suggestive point raised in this excellent paper, which fully realizes the expectations raised by its author in his preliminary note referred to.

We observe that the late publication of this journal, so conspicuous at the outset, is being persisted in. With respect to this, as concerning more than one of their scientific serials, our American brethren are establishing a dangerous precedent for which there is absolutely no excuse; and it is with much dissatisfaction that we note the adoption of a similar course nearer home.

G. B. H.

NOTES.

THE first *conversazione* of the Royal Society will be held at the Society's Rooms, Burlington House, on Wednesday next, May 14.

THE Royal Geographical Society is to be congratulated on the brilliant reception accorded under its auspices to Mr. Stanley at the Albert Hall on Monday. All the arrangements had been made with the greatest care, and the proceedings were in every way most successful. No one who was fortunate enough to be present could fail to see how fully the English people recognize, and how warmly they appreciate, Mr. Stanley's achievements.

THE Chancellor of the Exchequer will receive a deputation on May 15 from the Marine Biological Association of the United Kingdom in reference to the Treasury grant in aid of that Society's investigations of the natural history of marine food-fishes. A large monograph on the common sole, illustrated by many coloured plates, will be among the evidences of work done which the Association will submit to Mr. Goschen. The Fishmongers' Company have recently raised their contribution to the funds of the Association from £200 to £400 a year.

ON Monday evening various questions as to the effects of the dog-muzzling order were addressed to Mr. Chaplin in the House of Commons. He said:—"The return of deaths from hydrophobia since the muzzling order came into force are not at present in the hands of the Board of Agriculture. But I am glad to say, with regard to rabies, that in every county which has been placed under the regulations, and in the country as a whole, there has been a marked diminution in the number of outbreaks since the passing of the order. For instance, in 1889, for the last two quarters of that year there were 133 cases in the third, and 81 cases in the fourth quarter reported to the Board. For the first quarter of the present year they have been reduced to 39, and for the month of April there have only been seven cases throughout England, as compared with 11 for March, 14 for February, and 14 for January of the present year. In the metropolis and the West Riding, although there has been a large diminution, cases are still of constant occurrence, and there have also been comparatively recent outbreaks in Hampshire and West Sussex, in which latter county a muzzling order has been imposed by the local authority. With regard to Lancashire and the home counties of Essex, Hertfordshire, Surrey, and Kent, so far as they are not included in the metropolitan district, no cases have been reported for a considerable period, and if the reports continue to be as favourable in the case of the home counties as they have been of late, I shall hope to be able to modify the order, if it is not suspended, at no distant time. I may be allowed to add, as it will be of interest to the public, that since the order has been enforced, of the rabid dogs seized in public places, nine were properly and securely muzzled, and were thus prevented from doing mischief."

PROF. G. J. ROMANES, F.R.S., has been elected President of the Sunday Society, in succession to Sir James D. Linton, P.R.I., and will deliver his Presidential address at a meeting to be held in London in June.

THE Pharmaceutical Society will hold a *conversazione* at its house on Tuesday evening, May 20.

THE German Ornithological Society will hold its annual meeting at Berlin from May 9 to 12.

M. C. W. ROSSET has arrived at Hamburg after having been absent in Egypt, Cochin China, and China for three years. He has made a most interesting scientific collection, which will be presented to the Ethnographical Museum of Berlin.

THE recent investigations of Dr. Rudolf Kœnig, of Paris, into the composition of musical so unds and the theory of *timbre* will form the subject of an important paper to be read on May 16, at the meeting of the Physical Society, by Prof. Silvanus P. Thompson. Dr. Kœnig is sending over to this country for exhibition on this occasion a number of his wave-sirens and other expensive and elaborate apparatus, by which he has demonstrated the points of his research. Amongst the apparatus are some special appliances for producing audible beat-tones by the interference of two notes, each of which is too shrill to be separately heard. Musicians, as is well known, have never taken cordially to the current theories of Helmholtz respecting overtones and their relation to the consonance or dissonance of intervals and chords. As Dr. Kœnig's investigations have carried matters to a point beyond the speculations of Helmholtz, and not altogether in accordance with them, the occasion promises to be of unusual interest. It is expected that Dr. Kœnig will himself be present at the meeting, which is to be held at 6 o'clock at the Physical Laboratory of the Science Schools, South Kensington.

AT the Royal Academy banquet on Saturday, Sir William Thomson responded for "Science." He spoke chiefly of the mutual obligations of science and art. Aërial perspective, he said, first became known io scientific men through the artist's practical knowledge, and the use made of it in every conceivable representation of light and darkness, of house interiors and exteriors.

THE Select Committee on the sweating system refer in their Report to the evidence submitted to them as to the incompleteness of the education of workmen. "The remedies suggested," says the Committee, "are, on the one hand, a renewal of the apprenticeship system; and, on the other, the promotion of a larger system of technical education. We think that the encouragement of technical education for all classes of artisans is more likely to prove an efficient remedy than a recurrence to the old system of apprenticeship."

IT is reported from the ruby mines of Burmah that a ruby weighing 304 carats has been found.

A PUBLIC library is to be established at Hyderabad, and the Nizam's Government has also decided to undertake an archæological survey of the State.

FATHER FRANCIS DENZA, the Director of the new Vatican Observatory, is sending a circular in English to the Observatories of all English-speaking places, asking them to exchange publications with his institution. The authorities of the Vatican Observatory, which "now revives under the protection of His Holiness, Pope Leo XIII.," are anxious that it may render great service to science. Hence they feel the necessity of entering into communication with every existing scientific establishment of a similar kind. Father Denza expresses a hope that the directors to whom he appeals will let him have all the past publications of their respective Observatories.

MR. W. C. MILLS, Secretary of the Archæological Society of New Comerstown, Ohio, has found a Palæolithic flint implement in the gravel of the glacial terrace which everywhere lines the valley of the Tuscarawas river. Mr. G. F. Wright, to whom the implement was submitted, went to see the spot where it was discovered, and contributes to the *Nation* an interesting paper on his researches and conclusions. At this spot the surface of the terrace is thirty-five feet above the flood-plain of the Tuscarawas. The implement was found by Mr. Mills himself in undisturbed strata, fifteen feet below the surface of the terrace; so that it is "connected, beyond question, with the period when the terrace itself was in process of deposition." Thus it adds "another witness to the fact that man was in the valley of the Mississippi while the ice of the glacial period still lingered over a large part of its northern area." This is only the fifth locality in which similar discoveries have been made in America—the other places being Trenton, N.J.; Madisonville, Ohio; Medora, Ind.; and Little Falls, Minn.

At a recent meeting of the Washington Chemical Society, Dr. Thomas Taylor, of the United States Department of Agriculture, exhibited a new flash-light intended to take the place of several kinds which have proved highly dangerous. The composition, as described by *Science*, consists largely of charcoal made from the silky down of the milk-weed—a form of carbon which Dr. Taylor prefers to all others, because of its freedom from ash. A few grains of this composition placed on tissue-paper, and lighted by a "punk-match," produced a prompt and blinding flash. The paper on which the powder rested was not even scorched. The flash being instantaneous, the heat is not sufficient to ignite the most inflammable material on which the powder may rest. An inferior flash-light being used, with the same paper for a base, the paper at once caught fire. This was owing to the comparatively slow combustion of the chemicals used in the inferior grade. Dr. Taylor said the powder of his new flash-light would not explode either by concussion or by friction.

At the meeting of the French Meteorological Society on April 1, the President read a circular from the Minister of Public Instruction, with reference to the Congress of Scientific Societies to be held at the Sorbonne from May 27 to 31. The following questions to be discussed are those more particularly of interest to meteorologists:—The study of the mistral; earthquakes; researches on the presence of aqueous vapour in the air by astronomical and spectroscopic observations; comparison of the climates of the different parts of France; the causes which seem to induce a general decrease in the waters of the north of Africa, and a change of climate; to fix for certain localities of the Alps and Pyrenees the present superior limit of vegetation, and to study the variations which it has experienced at different epochs; the study of the periodical phenomena of vegetation, dates of budding, flowering, and maturation; coincidence of these epochs with that of the appearance of the principal species of insects injurious to agriculture.

The Meteorological Council have just published the monthly and annual results of the meteorological observations taken at the stations of the Royal Engineers and the Army Medical Department, for the years 1852–86, comprising thirty-three stations, in different parts of the world. In the year 1852, meteorological instruments were supplied to the principal foreign stations of the Royal Engineers, and the observations were continued till March 1862, when the instruments were transferred to the Army Medical Department, as directions had been given by the War Office for similar observations to be taken by the medical officers in the Army, wherever stationed. The observations were partially published by the Board of Ordnance and the War Office, but as it was pointed out in the *Meteorologische Zeitschrift* for March 1886, that it was "most desirable that this valuable store of observations, especially from stations for which hardly any other information for the period exists, should be worked up according to the modern requirements of the science, and then published," the Meteorological Council decided to undertake the work, and a large mass of original observations was handed over to that body. The result is the present volume of 261 + xiii. large quarto pages of carefully revised results for separate months and years. The combined results, for as many years as possible for each station, accompanied by a brief discussion of this valuable material, would no doubt be welcomed by meteorologists.

Dr. Dixon, Professor of Hygiene at the University of Pennsylvania, has been studying air and dust obtained in street cars. *Science* says he has found in them "the germs of many diseases, contagious and otherwise. Better ventilation and more effective cleansing are sorely needed."

In the current number of the *Zoologist*, Mr. E. L. Mitford writes of the survival of the beaver in Western Europe. Some fifteen years ago he saw in the museum at Bayonne a very large white beaver, which had been killed in the Rhone. He was told that it was the last of its race found in Europe. But this year, being at Hyères, where there is a museum with a very fine collection of indigenous birds and quadrupeds, he found another fine specimen, colour light brown, measuring three feet from snout to end of tail. This was obtained about four or five years ago, and is one of several that were sent to M. Fiépi, a naturalist and taxidermist of Marseilles. They were taken in the Rhone at St. Meree, in the neighbourhood of Arles. M. Catal, the naturalist of the Hyères Museum, talking of the subject with Mr. Mitford, said that beavers were more numerous formerly on the Rhone; that the great floods of 1846 had destroyed a large number, and made them more easily captured; and that subsequent inundations had made them much rarer. They are still to be found on the Rhone and its affluents, the Gardon, the Durance, and the Isère below Valence, also lower down the Rhone at Arles, Beaucaire, and Taralcon. They seem to have abandoned their custom of building huts and dams; the race no longer being sufficiently numerous to live in communities, they now live in deep burrows. In 1827 a number of the huts of the beaver were found on the Elbe at its meeting with the Nuthe, near Magdeburg.

The Director of the Norwegian Geological Survey, Dr. Hans Reusch, has lately published a small geological map of the Scandinavian peninsula, Finland, and Denmark. It includes also representations of Greenland, Iceland, Spitzbergen, and the Faroe Islands. The Norwegian terms used in the explanation of the colours are translated into English. The publishers are H. Aschehoug and Co., Christiania.

A work entitled "Dogs, Jackals, Wolves, and Foxes: a Monograph of the Canidæ," is being prepared by Mr. St. George Mivart, F.R.S. It will contain a description, with a plate drawn and coloured from nature, and often from life, of every species which the author thinks can fairly claim to be regarded as distinct, and also of various marked varieties of what he regards as probably one species. In addition to an account of the habits, geographical distribution, and life-history of each species, there will be given in an introduction, enriched with woodcuts, what the author deems a sufficient description of the anatomy of the group, of the structural relations of the Canidæ to other animals, their position in zoological classification, and the general relations they bear to the past and present history of this planet. The execution of the plates has been entrusted to Mr. J. G. Keulemans.

A new and most useful edition of the "Guide" to the exhibition galleries of the Department of Geology and Palæontology in the Natural History Museum, Cromwell Road, has been issued. In the preface, Dr. Henry Woodward explains that the publication of Mr. R. Lydekker's Museum Catalogues of the "Fossil Mammalia," parts i.–v. (1885–87), and the "Reptilia and Amphibia," parts i.–iv. (1888–89), has compelled the rearrangement of a great part of these collections, and changed the plan of the "Guide." Much additional information is given in the present edition, and the illustrations have been increased from 49 to 211. It has therefore been found necessary to subdivide the work into two parts. The first part deals with fossil mammals and birds, the second with fossil reptiles, fishes, and invertebrates.

MESSRS. NEILL AND CO. have issued a volume giving a list of the contents of the first thirty-four volumes of the Transactions of the Royal Society of Edinburgh, with an index of authors and an index of subjects. The Society was founded in 1783, and published the first volume of its Transactions in 1789, adopting demy quarto as the size of the page. A slightly larger size has now been chosen, that the Society's Transactions may be uniform with those of the Royal Society. A new series may thus be said to have been begun. Messrs. Neill and Co., who have been printers to the Society since its foundation, present a copy of the volume to each member, "as a slight acknowledgment of their appreciation of the favour shown their firm for more than a century."

WE have received the twentieth annual report of the Wellington College Natural Science Society. A good record of work in various departments is presented, and an account of some very interesting lectures is given in the "minutes of open meetings."

SOME time ago the Japanese Minister of Education summoned a committee to discuss the system of building best adapted to withstand earthquakes. For the use of this committee, Prof. Milne, of Tokio, compiled a great quantity of information respecting building in earthquake countries. The various reports collected by him, with some original articles of his own, he has now brought together in a work which is printed as vol. xiv. of the Transactions of the Seismological Society of Japan. The compilation ought to be of great service to builders in countries where shocks of earthquake are frequently felt.

MESSRS. WILLIAMS AND NORGATE have published a second edition of Mr. F. Howard Collins's "Epitome of the Synthetic Philosophy," which we recently reviewed. The work has been favourably received in America, and is being translated into French, German, and Russian.

A VOLUME on the Paris Exhibition of 1889, by M. Henri de Parville, has just been published by M. J. Rothschild. It is pleasantly written, and illustrated by 700 vignettes.

THE Geneva Society of Physics and Natural History has issued the second part of the thirtieth volume of its *Mémoires*. Besides the President's Report for 1888, a bibliographical bulletin, and a list of members, the volume contains papers on the movements of electrified bodies, by M. Ch. Cellérier; new or little-known locustides in the Museum of Geneva, by M. Alph. Pictet; on the flora of Paraguay, by MM. M. Micheli and R. Chadat; and on certain fossils of Japan, by MM. J. Brun and J. Tempère. The volume includes many illustrations.

MESSRS. MACMILLAN AND BOWES, Cambridge, have issued the first part of a Catalogue (No. 230) of books on the mathematics, pure and applied, containing many works of the old mathematicians, mathematical and astronomical journals, observations, &c., including many from the libraries of the late Arthur Buchheim, Fellow of New College, Oxford, and E. Temperley, Fellow of Queens' College, Cambridge.

A CATALOGUE of zoological and palæontological works has been issued by Messrs. Dulau and Co. It includes works on mammalia, and on anthropology and ethnography.

PROF. EVERETT sends us the following extract from the *East Anglian Daily Times*. It is by Mr. Herman Bidell, of Playford, who is well known in Suffolk, and is thoroughly competent to describe accurately phenomena he observes:—"I shall be glad to draw the attention of those interested in thunderstorms to a magnificent example of ruin by lightning we have in this village. The parish of Playford was visited on Saturday last (the 26th) by one of those volatile clouds heavily charged with electricity that so often remind us of the approach of summer. The tree struck stands about 300 yards north-east of the church, close by the footpath leading to Great Bealings, one of a row of 'old English' poplars running east and west. At the foot the tree is about 2½ feet in diameter, tapering more or less regularly to a 10-inch diameter at the top. Here the electric fluid came in contact with the trunk some 40 feet from the ground. The two topmost branches are intact, but the bark is completely stripped from top to bottom, the southern half of the body being riven into matchwood. The storm came up from north-west, with a very light breeze, shifting right and left of north. The cloud was a dense dark blue—the effect, in part, of the sun in front—a detached mass of vapour with fringed edges, differing little, except in density and its proximity to the earth, from others which during the forenoon had floated over. At half-past one o'clock a few drops warned four or five men at work close to the tree to take shelter under a stack 200 yards off—a fortunate warning, for no sooner had the cloud drifted overhead than a blinding flash, accompanied by a terrific peal of thunder, left the tree a magnificent ruin, spread over not less than two acres of land, more or less covered with bark, branches, and riven trunk. One solid piece of 5½ pounds was picked up 126 yards away from the tree. Other *débris* lies 70 yards in an opposite direction, and as an evidence of still more inconceivable force, small pieces of riven trunk or bark, some under half an ounce in weight, were found right in the face of the wind, nearly 60 yards from the tree. What force could have been applied to such light particles is beyond comprehension. Nothing that I have ever seen effected by lightning approaches this ruin. Larger trees have been shivered in this parish, but I never saw a tree completely barked all round, with one half literally riven into fibre, leaving the other half of the trunk a whitened stem, still standing as a forty-feet shaft to be seen a mile away. (The remnant is a conspicuous object in a north-westerly direction all the way from a point east of the old Kesgrave Schools for half a mile towards Ipswich on the Martlesham and Rushmere roads). The electric fluid left the tree at the foot, following the direction of the fence for about 15 or 20 feet, threw up a sod about a foot square, and there pierced the soil into the earth. Four hundred yards in a direct line north-east of the tree stands Playford Mount, the residence of Mr. Kemp-West, a commanding object in the landscape. Here some half-dozen of the fine glass plates in the front windows are shattered to atoms, the result, I apprehend, of concussion from the report of the explosion. I have never known this effect from the severest storm in the neighbourhood, but the thunderclap is described as terrific. The tree, as standing, is worth going to see, and, I will add, is of easy access from the road past the church."

THE *Engineer* and *Engineering* for May 2 contain much information concerning the proposed great tower in London. The drawings, plans, and designs of the competing schemes are now being exhibited in the Drapers' Hall, Throgmorton Street, E.C. *Engineering* appears to think that, if the tower is to pay, it must be provided with some attractions to bring the people again and again; and if these attractions could be raised 200 feet or 500 feet above the smoke, they would be immensely increased. The country cousin and the conscientious sight-seer would go to the summit, but the first stage would detain the bulk of the visitors. *Engineering* also observes that, in reviewing the various designs, we must frankly admit that none excels the Eiffel Tower in beauty and grace. No fewer than eighty-six competitors have sent in designs.

AN interesting paper upon cyanogen iodide, CNI, is communicated to the current number of the *Berichte* by Drs. Seubert and Pollard, of the University of Tübingen. On account, probably, of the extremely poisonous nature of this compound, rendered even more dangerous by reason of its great volatility, little has hitherto been done towards completing its chemical history beyond a mere description of its more evident properties. Determinations either of its density or its melting-point appear never to have been attempted, and it was with the object of supplying these deficiencies that the work in the Tübingen Laboratory was undertaken. Everyone who has ever prepared this exceptionally beautiful substance for lecture or other purposes will remember the exquisite manner in which it sublimes, forming long, delicate, colourless, but very highly refractive needles, bridging over from side to side of the wide tube or flask in which the operation is performed. Often these elongated prisms attain the length of half a dozen inches or more, and frequently form an interlacing network among which may be seen perched here and there star-shaped or flower-like aggregates of the smaller crystals. Perhaps the most remarkable property of these crystals is to be found in the manner in which they re-sublime from one side of the vessel to the other according as their position is varied as regards the direction of the light which falls upon them. Drs. Seubert and Pollard prepared their specimens by the old method first used by Sir Humphrey Davy, the action of iodine upon mercuric cyanide, $Hg(CN)_2 + 2I_2 = HgI_2 + 2CNI$. About 10 grams of the finely-powdered and well-dried mixture of iodine and cyanide of quicksilver, in the proportion of one molecule of each so as to avoid the presence of much free iodine in the sublimate, was placed in a wide test-tube and interpersed with glass beads in order to render the mixture as porous as possible. The test-tube was then placed at the bottom of a wider glass tube closed at the lower end, and fitted at the upper with a calcium chloride drying tube to prevent access of moisture. The apparatus was then allowed to stand for about three days in a position where it could receive direct sunlight; at the end of this time the reaction was almost complete, the mixture being brilliant red from formation of mercuric iodide. The lower end of the tube was then placed in a hot water bath, when the iodide of cyanogen sublimed in the manner above described into the upper cooler part of the tube. In order to determine the melting point, small quantities were placed in capillary tubes and hermetically sealed, for if the upper end were left open, as is usually the case in taking a melting-point, the cyanide would simply volatilize without fusion. The melting-point was in this way found to be 146°·5 C., and the solidifying point 143°. The vapour-density was determined by Victor Meyer's method, and found to be 5·28, corresponding to the simple formula, CNI. The lowest temperature at which the substance becomes completely and rapidly converted to the gaseous condition appears to be about 250°. The iodide is therefore analogous to the simple bromide and chloride of cyanogen, CNBr and CNCl, and not to the triple polymers tri-cyanogen bromide and chloride, $C_3N_3Br_3$ and $C_3N_3Cl_3$.

The additions to the Zoological Society's Gardens during the past week include the Wild Boars (*Sus scrofa* jv.) bred in Scotland, presented by the Lord Hebrand Russell; a Ring-tailed Coati (*Nasua rufa* ♂) from the Argentine Republic, presented by Mr. R. E. Moore; a Louisianian Meadow Starling (*Sturnella ludoviciana* ♀) from North America, a Black-bellied Sand Grouse (*Pterocles arenarius* ♀) from India, presented by Mr. W. H. St. Quintin; four variegated Sheldrakes (*Tadorna variegata* ♂ ♂ ♂ ♂) from New Zealand, presented by Captain C. A. Findlay, R.N.R.; a Rhomb-marked Snake (*Psammophylax rhombeatus*) from South Africa, presented by Miss Harris; three Common Vipers (*Vipera berus*) from Sussex, presented by Dr. C. W. Cousens; a Green Lizard (*Lacerta viridis*), a Three-toed Skink (*Seps tridactylus*) from France, presented Mr. J. C. Warbury; a Sooty Phalanger (*Phalangista fuliginosa* ♂) from Australia, deposited; a Black-headed Lemur (*Lemur brunneus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

Objects for the Spectroscope.

Sidereal Time at Greenwich at 10 p.m. on May 8 = 13h. 6m. 42s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 3321 (64 M) ...	—	—	12 51 19	+22 17
(2) G.C. 3572 (51 M) ..	—	—	13 25 13	+47 45
(3) 40 Comæ Ber. ...	6	Yellowish-red.	13 1 0	+23 12
(4) 91 Leonis...	5	Whitish-yellow.	11 30 48	+ 0 11
(5) α Virginis	1	White.	13 9 24	−10 36
(6) V Virginis	Var.	Yellow-red.	13 22 7	− 2 36

Remarks.

(1) The spectrum of this bright nebula does not appear to have been yet recorded. Smyth describes it as "a conspicuous nebula, magnificent in size and brightness." In the General Catalogue it is described as "a very remarkable object; very bright; very large; very much extended in the direction 120°; brightens in the middle to a small bright nucleus, which is possibly a double star." The nebula is in the constellation Coma Berenices, and is now very favourably situated for observations.

(2) This is the famous spiral nebula in Canes Venatici. The details of the nebula are so well-known that a full description is not necessary here. In all but the largest telescopes it simply appears as a double nebula with the two nebulosities running into each other, one of them being surrounded by a ring which is variable in brightness. Smyth likens this to a "ghost" of Saturn. According to Huggins the spectrum is continuous, but some parts were thought to be abnormally bright. Although the observations will not be easy, it is very important that the positions of such bright parts should be measured, if only approximately. In such cases as this it is probable that we shall find spectroscopic connecting links between the bright-line nebulæ and stars which are well advanced in condensation. Our knowledge of the relations between nebulæ and comets is also likely to be advanced in this way.

(3) This is a very fine star of Group II. The bands are very wide and dark, even in the ultra-blue, but especially in the red (Dunér). The star belongs to a late stage of the group, and may be usefully re-examined for further details.

(4) A star of the solar type (Vogel). The usual differential observations as to whether the star is increasing in temperature (Group III.), or decreasing (Group V.) are required. The spectra of this class of stars should be very carefully examined for remnants of the strong bands in the red, which characterize the later stages of Group II., and which are also seen in Aldebaran. If these are found, the star is obviously at an early stage of Group III. It has also become very important to determine whether there are any stars intermediate between stars like the sun and stars of Group VI., and in these more detailed observations this should be borne in mind. The carbon band near *b* will probably be the first to appear, and the presence or absence of traces of this band should be always noted. It is most likely to occur in stars of an orange or reddish tint.

(5) Spica has a spectrum of Group IV. The only observations likely to be of service are those comparing the thicknesses of such lines as *b*, E, and D with their thickness in other bright stars of the same group (*e.g.* α Lyræ). This will determine its relative temperature.

(6) This variable will reach a maximum about May 9. It ranges in magnitude from 8–9 at maximum, to <13 at minimum in a period of about 251 days. The spectrum is still doubtful; Gore writes it III.?

A. Fowler.

Structure of the Corona.—The Smithsonian Institution has had two plates prepared, containing nine photographs of the total eclipse of the sun of January 1, 1889, and distributed them amongst astronomers and others interested in solar physics. All the photographs have been reduced to a uniform diameter, and at Prof. Langley's request, Prof. Todd has contributed a descriptive note to accompany them. In the remarks on the structure of the corona it is noted:—

(1) The axis of symmetry of the corona does not coincide with the axis of revolution of the sun as determined from the solar

spots. The corona appears to be at least a triple phenomenon made up of—

(*a*) The polar rays, seen most prominently about the poles.

(*b*) The inner equatorial corona, the lower regions of which bear some resemblance to an outer solar atmosphere.

(*c*) The outer equatorial corona, consisting of the long streamers for the most part only visible to the naked eye.

(2) The polar corona consists of rays, straight or nearly so, and radial from neither the sun's centre nor the sun's poles. Rather they seem to radiate from areas the centres of which are adjacent to the sun's poles.

(3) The inner equatorial corona emits a large percentage of the total light of the corona; the streamers, however, are not generally so sharply defined as about the poles, and many of them appear to have a real curvature. Four large prominences are visible at about 35° of solar latitude, as if to suggest some connection between the protuberances and the corona.

(4) The equatorial streamers of the corona are very slightly curved, being convergent on the east side of the sun, and divergent on the west.

The fact of chief importance established appears to be the periodicity of the outer corona in a cycle probably of equal duration with that of the solar spots. The epoch of greatest extension of the equatorial corona appears to coincide very nearly with the epoch of minimum sun-spots.

Prof. Todd also directs attention to the most important points requiring elucidation, and throws out a few suggestions for future eclipse observations.

Brooks's Comet (*a* 1890).—The following ephemeris has been computed by Dr. Bidschof (*Astr. Nach.*, No. 2966), and is in continuation of that previously given (Nature, vol. xli. p. 571):—

Ephemeris for Berlin Midnight.

1890.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
May 6	20 53 21	+34 9·6	0·2874	0·2596	2·60
10	46 41	37 44·1	0·2857	0·2449	2·80
14	38 9	41 28·5	0·2843	0·2314	3·00
18	27 13	45 20·2	0·2832	0·2195	3·18
22	13 16	49 14·9	0·2824	0·2096	3·34
26	19 55 28	53 6·2	0·2818	0·2022	3·47
30	32 54	56 46·1	0·2815	0·1976	3·55

The brightness at discovery (March 21) has been taken as unity.

Discovery of Minor Planets.—Two more asteroids were discovered by Herr Palisa, at Vienna, on April 25, and observed independently by M. Charlois, at Nice, on the following night. The magnitudes of the planets are 13 and 12 respectively, and their numbers are (291) and (292). Prof. Krueger thinks that the latter is probably Scylla (*Astr. Nach.*, 2966).

THE INSTITUTION OF MECHANICAL ENGINEERS.

An ordinary general meeting of the Institution of Mechanical Engineers was held on the Thursday and Friday of last week; the President, Mr. J. Tomlinson, in the chair.

The second meeting of the year is not generally looked on as of great importance, but it is a long time since we remember one of such meagre proportions in one respect as that with which we are now dealing, for there was only one paper on the agenda; that and the President's address constituted the whole programme. What the proceedings lacked in variety and amplitude was, however, fully compensated for in solid value. The one paper, Prof. Kennedy's, is full of valuable information, and Mr. Tomlinson's address came as a most welcome surprise to a good many. In the first place it was short, and, secondly, it was practical—two virtues which appeal strongly to engineers when there is talking to be done; but beyond that it was one of the most interesting Presidential addresses we have heard for many a day at any of the Engineering Societies. The reason for this is not far to seek. Mr. Tomlinson simply narrated his own experience in plain language, eschewing those ornamental tags of rhetoric which many people look on as essential when they have to speak in public; and as his experience extends back to a very interesting period of railway engineering, the address proved an exceptionally happy effort.

Mr. Tomlinson has been, as he said, a railway man all his life; and, indeed, he has been connected with the engineering departments of more than half a dozen railways, from the Stockton and Darlington up to the Metropolitan. His father was passenger superintendent to the former line. His recollection therefore carries him back to the very early days of the locomotive. His first knowledge extends to the year 1837, when he was employed at the works of Timothy Hackworth, of Shildon. Perhaps no better instance could be given of the simplicity of those Arcadian days than the fact that Hackworth was at once locomotive superintendent and contractor to the railway. Such a dual position might cause invidious remarks on the part of shareholders in the present day. Mr. Tomlinson remembered the three original locomotives placed on the Stockton and Darlington line. One of them, the *Locomotive*, now stands on a pedestal in front of the North Road Station at Darlington. The load for this engine was about 22 tons of empty waggons to draw up hill; whilst down the hill to Middlesborough the waggons loaded, weighing 64 tons, were drawn. The weight of the engine and two tenders loaded was about 15 tons. Unfortunately there was no record kept of the consumption of fuel, but Mr. Tomlinson used to help put the coal on the tender, and he estimates the quantity to have been 16 to 17 cwts. for 48 miles, or about 40 lbs. per engine mile; but it must be remembered that the gradient was all in favour of the load—in fact, the greater part of the fuel was consumed on the return journey of empty trucks. The cylinders were 10 inches in diameter by 24 inches stroke. The eccentrics had to be changed for back and forward gear by hand, the boiler pressures were 30 to 35 lbs. per square inch, and the pistons were packed with a spun-yarn gasket. As the cylinders were vertical there were necessarily no engine springs. There were no brakes, no water-gauge glass, no head or tail lamps, and no whistle. We have not space to follow Mr. Tomlinson in his interesting engineering reminiscences. Perhaps, since Mr. T. R. Crampton has gone, there is only one other engineer living who could give us such unique personal experience of early locomotive days. If so, that engineer is Mr. E. Woods, Past-President of the Institution of Civil Engineers.

Prof. Alexander Kennedy's paper constituted the second report of the Research Committee appointed by the Council of the Institution to investigate the Marine Engineering question. Within the last few years the Institution has made quite a special feature of these research committees, and we know of no better way in which it could carry out the object of its existence, and, at the same time, keep down the ever-growing financial surplus. The Research Committee on Friction and the Research Committee on Rivetting would have been of great service to engineers if only from the fact that they collected and put in concise form the knowledge already existing on the subjects; but they did more than this, for they made experiments of their own by which doubtful points were cleared up and new possibilities were suggested. The Marine Engine Committee are following the same useful course under the guidance of their Chairman, Prof. Kennedy, who, it may be remarked, gained his first experience as an engineer in the once celebrated Thames-side marine engineering establishment of the Dudgeons.

As we have said, this is the second report of the Committee, the first, which was read last year, being on the trials of the s.s. *Meteor*, a London and Edinburgh steamer of 692 registered tons. The vessels since then under trial, and dealt with in the second report, are the *Fusi Yama*, the *Colchester*, and the *Tartar*. The first is an ordinary trading vessel of 214·3 feet long b.p., 29·3 feet beam, 20·5 feet deep, and of 2175 tons displacement at trial draught. The trial run was from Gravesend to Portland. The engines are by Samuelson, of Hull, and had just been overhauled. They are of the ordinary two-cylinder compound type. The *Colchester* is the latest built vessel of the Great Eastern Railway on the Antwerp service. She is 281 feet long, 31 feet beam, and 15·2 deep. Her trial displacement was 1675 tons. She is a twin-screw ship, the engines being ordinary two-cylinder compounds. The trial run was from the Humber to Harwich, the engines having been overhauled in the former river. The *Tartar* was selected as an excellent example of modern economical engines in a cargo-carrying steamer—what is generally known as an "ocean tramp." She is 332 feet long, 38 feet wide, and 27 feet deep. Her displacement tonnage on trial was 2250 tons. She has triple compound engines of the three-crank type. The trial run was from the Thames to Portland. The vessel was light, so that the engines were working at very low power, and, in addition to this, bad weather was met with on the voyage, so that the recording of results was much interfered with. It will be noticed that the figures bearing on the efficiency of the *Tartar's* boilers are not given in the table. The reason is that the coefficient based on the recorded data comes out so high that the boilers could hardly have been evaporating all the feed water pumped into them. In ordinary cases we should naturally attribute this to priming; but the power developed was so small that we hesitate to apply this solution in the present case. On the other hand, the phenomenon of excessive cylinder condensation would be induced by working a big engine at low power. We have not, however, sufficient data to enable anything positive to be advanced in this connection. We understand that in ordinary working the boilers show no sign of excessive priming, and the steam space is said to be ample. The *Meteor*, the first vessel experimented upon, is 261 feet long, 32·1 feet wide, and 19·3 feet deep. Her trial displacement was 2090 tons. The engines are of the triple compound type, with three cranks at equal angles.

It will be evident that we have not space to give details of the trials as set forth by Prof. Kennedy, and any fairly intelligible abstract is difficult to make. The paper itself is merely a record of facts—a most admirably arranged record we may say in passing—and each fact is so interdependent on others, that it is difficult to make a selection. We will, however, briefly state in the form of a table a few of the leading features and final results, referring those of our readers most interested in the subject to the report itself. We include the *Meteor*, as her record is necessary to make the matter complete.

	Name of vessel.			
	Meteor.	*Fusi Yama.*	*Colchester.*	*Tartar.*
Boiler pressure above atmosphere in pounds per square inch...	145·2	56·84	80·5	143·6
Vacuum in condenser below atmosphere in pounds per square inch... ...	12·17	12·48	12·49	12·9
Revolutions per minute	71·78	55·59	{ 86·0 87·1 }	70·0
Total mean indicated horse-power	199·4	371·3	{ 1022·5 957·2 }	1087·4
Coal burnt per square foot grate per hour	19·25	18·98	26·1	11·93
,, ,, ,, total heating surface per hour	0·602	0·437	0·987	0·367
,, ,, 1 horse-power per hour	2·01	2·66	2·90	1·77
Carbon value of coal	0·878	0·878	0·913	1·031
Feed water per square foot total heating surface per hour in pounds ...	4·49	3·48	7·39	4·13
,, ,, pound of coal	7·46	7·96	7·49	11·23
,, from and at 212° F.	8·21	8·87	8·53	13·06
,, per indicated horse-power per hour	14·98	21·17	21·73	19·83
Calorific value of 1 pound of coal as used in thermal units	12,770	12,760	13,280	14,995
Percentage of calorific value of fuel taken up by feed water	62·0	67·2	62·0	—
,, ,, ,, ,, carried away by furnace gases	21·9	23·5	28·0	22·1
,, ,, ,, ,, lost by imperfect combustion	3·6	0·0	1·3	0·0
,, ,, ,, ,, expended in evaporating moisture in coal	1·2	0 9	0·4	0·0
,, ,, ,, ,, unaccounted for	11·3	8·4	8·3	—
Efficiency of boiler per cent.	62·0	67·2	62	—
,, ,, engine	16·1	11·2	10·7	11·5
,, ,, ,, and boiler combined	10·0	7 6	6·6	9·7

A discussion followed the reading of the paper, the most interesting feature of which was a description, by Mr. Willans, of a device he had used for investigating the effect of condensed steam in an engine cylinder. For this and other points in connection with the trials we must refer our readers to the Transactions of the Institution.

THE SCIENTIFIC INVESTIGATIONS OF THE FISHERY BOARD FOR SCOTLAND.[1]

WHATEVER may be wanting to Scotchmen in the way of Home Rule, they have no cause to complain of a want of Home Rule in their fisheries. The Fishery Board for Scotland is a complete and independent body, exercising complete jurisdiction over all the Scottish coasts, provided with an ample staff, and in receipt of a considerable amount of Government money. We learn from the introduction to the present Report that the scientific staff consists of three trained naturalists and an assistant naturalist, and besides these there is a Committee of eminent scientific men, including representatives from all the Scottish Universities. Finally, the Board has a steamer, the *Garland*, specially devoted to scientific investigations, and is able to make use of the fishery cruisers for the same purpose.

Under these favourable circumstances, and especially in virtue of the powers granted by the Sea Fisheries (Scotland) Amendment Act, 1885, the Scotch Fishery Board has exceptional opportunities for making extensive and continuous scientific investigations. The investigations for 1888 are embodied in the Report which is here dealt with. The Report is divided into three Sections. Section A is largely devoted to the experimental trawling of the *Garland* in the areas closed against beam-trawling, and to a number of statistical tables drawn up for the purpose of comparison with those experiments. This experimental trawling requires some explanation. The Act above-mentioned empowers the Scotch Fishery Board, under stated circumstances, to make by-laws for restricting or prohibiting, either entirely or partially, any method of fishing for sea fish within any specified area in any part of the sea adjoining Scotland, and within the exclusive fishery limits of the British Islands.

In accordance with the Act, by-laws were framed, prohibiting beam-trawling in districts which may roughly be described as the Firth of Forth, St. Andrew's Bay, and the Firth of Tay, and part of the sea off the coast of Aberdeenshire and Kincardineshire. This by-law came into force on April 5, 1886. Since that date the *Garland* has trawled periodically over certain definite stations within the prohibited areas, and the catches have been carefully tabulated, both as regards size and quantity. The object of the experiment is, of course, to study the effect of an enforced period of rest on the piscine fauna of the inclosed and adjacent areas, and to obtain information under the following heads:—(1) Whether the cessation of beam-trawling would cause any marked increase in (*a*) the number, (*b*) the size of trawl-fish within the closed areas. (2) Whether the closure would affect the catches of line-fishermen working in those areas. (3) Whether the closure would affect the catches of trawlers and other fishermen in adjacent areas. No fault can be found with the method of investigation, which is the only possible one under the circumstances; but, as might be expected, the results are influenced by a number of secondary causes which obscure the effect of prohibiting beam-trawling in the places mentioned. This may easily be seen by reference to the published accounts of the experiments. It was found in 1887, a year after the closure, that the average take of fish per "shot" was much greater than in the previous year in the closed

[1] "Seventh Annual Report of the Fishery Board for Scotland, being for the Year 1888." Part III., Scientific Investigations. Presented to both Houses of Parliament in pursuance of Act 45 and 46 Vict, cap. 78. (Edinburgh, 1889.)

areas, and that the increase was chiefly in flat fish, though also in round fish. At the same time, there was an increase in the take of all classes of fish in the free waters outside, but in general the increase in this case was in round fish, rather than in flat fish. So far, then, the experiment promised to show an immediate and most beneficial result. In 1888, however, the take of fish was very much diminished. The average number of fish of all kinds captured in the Firth of Forth per "shot" amounted to 211. In 1887 the corresponding average was 351, and in 1886, 251. There was also a considerable reduction in the average take in offshore waters, but the reduction was less than that in the closed area. Moreover, the proportional decrease of flat fish was greater than that of round fish in the closed waters, and this was more marked in the offshore waters. In St. Andrew's Bay there was, similarly, a great diminution of all kinds of fish, especially of flat fish; but outside, in the free sea, there was an increase in the flat fish and a great decrease of round fish. These negative and partly contradictory results were, without doubt, due to the exceptionally stormy weather in 1888. It shows, however, the great difficulty and complication attending fishery investigations. Nothing could seem to be more obvious than that, if trawling were prohibited in a certain area, less fish would be caught, and that their numbers would increase. The first results of the trawling experiments go to show that this is by no means necessarily the case, but that there are causes more powerful than beam-trawling which affect the numbers of fish in any season.

There are also statistics showing the relative amounts of fish caught by line in restricted and unrestricted areas—that is to say, areas where beam-trawling is prohibited and where it is permitted. These statistics show an increase in the weight of fish caught by line has taken place in 1888, in both areas, but that it is proportionally larger in the unrestricted than in the restricted areas. The increase is not due to a larger number of boats and men engaged in fishing, for these have actually decreased. The statistics of line-fishing are certainly curiously contradictory to those of beam-trawling, for whereas, in 1888, the latter mode of fishing showed a decrease of flat fish in closed areas, the line-fishing showed an increase of flat fish.

It is really impossible to draw any conclusions from statistics extending over so few years. After ten years of work we shall be in a better position to judge the result of the experiment of closing certain inshore waters against trawlers. So far, it must be confessed that no case whatever has been made out against them, and the line fishermen seem to be quite as efficient in depopulating a district. From the way in which the summaries of the statistics are written, the Fishery Board may be suspected of an unconscious leaning towards the interests of the line-fishermen.

No fewer than 129 pages are devoted to the statistical tables referred to.

A very interesting Report is given in Section B (biological investigations) by Prof. Ewart on the spawning of British marine food-fishes. Space forbids a detailed criticism of this Report, but it is definite and satisfactory, and shows that, contrary to the common belief, the majority of British food-fishes do not come inshore to spawn, but at the spawning season they congregate in shoals in deeper waters. This Report is followed by a paper on the food of fishes, by Mr. Ramsay Smith. The greater part of the observations and records necessary for this work were carried out by Mr. Thomas Scott, who is a veritable giant in practical work at sea. The paper on the pelagic fauna of the Bay of St. Andrew's, by Prof. McIntosh, may be considered as complementary to Mr. Ramsay Smith's paper, since the pelagic organisms are considered from the point of view of food for adult and larval fish. Prof. McIntosh's paper, giving a record of all the pelagic organisms observed throughout the year, is a thorough and important contribution to our knowledge of the subject, and has a high practical value, especially that part of it relating to fish ova and larvæ. At the same time, it may be questioned whether the subject of fish food is not dragged in a little too much. Is it perfectly ingenuous to give a series of beautifully-coloured drawings of the metamorphoses of Actinotrocha, and to label them "Pelagic fish food"?

The descriptions of, and suggestions about, the mussel and clam beds are of obvious practical interest, and Dr. Edington's paper on the Saprolegnia of the salmon disease gives promise of a wide extension of our knowledge of a difficult subject.

The Report concludes with a careful record of physical observations made in the North Sea. The value of the physical work of the Board would be much enhanced if arrangements could be made for taking daily observations at definite stations around the Scotch coast. Such observing stations have been established by the United States Fish Commission and by the German Commission for the Scientific Investigation of German Seas, and have been fruitful of results.

The Fishery Board, it may be noticed, is only engaged in one *experiment*—that of closing certain areas against beam-trawling. The remainder of the work is in the preliminary stage of *inquiry*. In the earlier stages of fishery investigation, a large amount of biological and physical inquiry into the natural conditions of the sea is absolutely necessary, as a guide for future experiments upon marine organisms. To those who do not consider the matter attentively, these investigations may seem useless and superfluous, but they are not. It must be observed, however, that these inquiries are not an end in themselves, as in philosophical biology, but must be undertaken solely with the view of applying the experience gained to future attempts to solve the fishery problem. For example, an inquiry into the food of the different species of fishes of a district need only be made once; it is sufficient for practical purposes to know what they do generally eat, without inquiring what they may eat in exceptional circumstances. An inquiry into the relations of pelagic organisms is most useful as a guide to the life-conditions and food of fish larva and certain adult fish, but a great deal of strictly scientific work on this subject is useless; the morphology and phylogeny of each pelagic organism has not the slightest bearing on fishery questions.

The statement of the fishery question is perfectly simple. Given a continuous decrease in a number of valuable fish, due to over-fishing, how may the diminution be checked, and a continuous future supply be insured? The answer to the question is very difficult. Life in the sea is beyond control, and, to a large extent, beyond observation, for the trawl and dredge give a very insufficient idea of the conditions of marine life. There is not so close an analogy between agriculture and fisheries, as is sometimes implied in language. The sea cannot be parcelled out into inclosures; it cannot be cultivated with different kinds of crops at will; its fishes cannot be kept in confinement and protected from their enemies and the weather, nor can they be fed at regular periods as live stock are. It is misleading to talk of "reaping a harvest that is never sown," when the power of sowing *and caring for the crop* is out of reach. The ultimate aim of all scientific investigations in fishery matters must be to find out what circumstances are in human power to control, and to show how that control may best be exercised.

The first and obvious subjects for control are the fishermen themselves. If they are the cause of the depopulation of the seas, such a check may be put upon their proceedings as to obviate the evil. This may be done in one of two ways: by prohibiting fishing altogether in certain specified areas, as has been partially done by the Scotch Fishery Board, so as to afford centres from which fish may spread into the surrounding seas, or by the establishment of close seasons for different species of fish. Both methods are attended with great difficulties, which have been discussed over and over again. They may be summed up as hardship to the fishermen, and the impossibility of preventing the destruction of one species of fish whilst another is being fished for. To establish a close season which would prevent any breeding-fish being caught, would be to prohibit all fishing for three parts of the year. Secondly, the ova of breeding-fish may be artificially fertilized, the fry hatched out and turned out in great numbers to restock the waters that have been depopulated. This method is said to have been attended with success, and demands a further trial; but it must not be supposed that this process in any way resembles the rearing of domestic animals on land, or even the culture of fresh-water fish. The fry, once turned out, are lost sight of, are exposed to the attacks of numerous enemies, and are beyond all further human care. Thirdly, fish might be protected by the wholesale destruction of their natural enemies other than man, just as game is protected by the destruction of stoats, carrion-crows, and other vermin. No doubt a general massacre of cormorants, gannets, and dog-fish would make a great difference to the annual destruction of fish on our coasts, but in the case of the birds, such a course would meet with great opposition; and in the case of dog-fishes, extermination, or even an appreciable reduction in number, would be nearly impossible. Lastly, attempts may be made at culture *sensu stricto*. Young fish may be caught by the ordinary methods, and kept in suitably constructed fattening-ponds until they are of saleable size; or, to carry the process a step further, the larvæ reared in hatcheries may be turned into similar ponds and brought to maturity. These operations have been conducted with success in more places than one, but the only places where marine fish-culture forms an industry of any importance is in the Adriatic, where there are large inclosures known as *valli*, in which young fry, caught in the open sea, are inclosed and brought to a marketable condition. The possibility of cultivating mussels and oysters in the like manner is too well known to require further mention, and it is quite possible that it may be found practicable to apply the system of culture to lobsters.

These are the practical questions to which fishery officers will have to turn their attention. That a preliminary scientific training is necessary is obvious, for the art of culture requires the most exact knowledge possible of the animals under cultivation, and success will in each case depend on the extent to which the necessities of the organism are studied and supplied. But abstract scientific study must give way to practice; as soon as a man allows the problems of morphology and phylogeny to distract his attention, he will become less careful of his practical experiments, and they will end in disappointment. The Scotch Fishery Board has made an excellent beginning in its trawling experiments; in a short time it may be hoped that its staff will be engaged in numerous other experiments on the protection and production of fishes, crustacea, and mollusks, to which many of the observations published in the Seventh Report are but the preliminary. G. C. B.

THE FIXATION OF FREE NITROGEN.[1]

In a paper communicated to the Royal Society in 1887-88 (Phil. Trans., 1889), the authors discussed the history and present position of the question of the sources of the nitrogen of vegetation. The earlier results obtained at Rothamsted, as well as those of Boussingault, under conditions in which the action both of electricity and of microbes was excluded, led the authors to conclude that the higher chlorophyllous plants have not the power of taking up elementary nitrogen by means of their leaves, or otherwise. The conclusions arrived at were, that atmospheric nitrogen is not a source of nitrogen in the case of gramineous, cruciferous, chenopodiaceous, or solaneous crops, but with regard to the *Leguminosæ* it was admitted that there was not sufficient evidence to account for the whole of the nitrogen taken up. Of the recent researches bearing on the subject, those of Hellriegel and Wilfarth, first published in 1886, were considered the most striking and conclusive.

In 1883, Hellriegel grew plants of various families in washed sand containing the necessary ash constituents but no nitrogen; in one series nothing further was given, whilst to others varying known amounts of sodium nitrate were added. The gramineous and some other plants of the first series were all limited in growth by the amount of nitrogen contained in the seed, and in the other series the growth was largely proportional to the amount of nitrogen which was applied. On the other hand, whilst most of the peas of the series to which no nitrogen was added failed after a short time, some would develop luxuriantly; and it was found that the roots of the plants of limited growth were free from nodules, and that there was abundant nodule formation on the roots of the well-developed plants. These results led Hellriegel to make further experiments, the results of which showed that leguminous plants will not develop to any extent in sterilized sand free from nitrogen; whilst in the case of peas, vetches, and some other *Papilionaceæ*, the addition of a small quantity of soil extract containing an immaterial amount of nitrogen, causes the plants to grow luxuriantly. A soil extract, prepared from an ordinary soil, which produces such striking results with the plants just mentioned has no effect with lupins. The same result is, however, readily obtained with lupins by the application of a soil extract from a sandy soil in which lupins have been growing. With clover less definite results were obtained for some time, but more recently it has been observed that whilst the extracts from other soils produce little or no effect on clover, an extract from a root-crop soil brought about a considerable nitrogen fixation; but the result was less marked than with the other leguminous plants. In all cases the nitrogen assimilation was accompanied by nodule formation on the roots. Sterilized soil extracts were entirely without effect.

As stated in a postscript to the paper in the Phil. Trans. already referred to, a preliminary series of pot experiments on similar lines to Hellriegel's was commenced at Rothamsted in 1888. The plants selected were peas, blue lupins, and yellow lupins. They were grown in washed sand containing a small amount (0·0027 per cent.) of nitrogen and the necessary ash constituents; whilst for comparison all the plants were grown in a rich garden soil, and the lupins in a special lupin soil as well. As more normal and satisfactory growth was obtained with peas, only the results relating to these will be discussed here. The lupins, which are admittedly difficult to manage under the artificial conditions which must, more or less, prevail in experiments of this kind, gave no very definite indications in the first year's experiments, although, in 1889, the most striking of the results were those obtained with yellow lupins. Of the peas grown in sand, No. 1 had nothing further added, whilst to Nos. 2 and 3 an extract from the garden soil was added. All the peas germinated and grew well, but about five or six weeks after sowing, the plants of the pots seeded with soil organisms began to acquire a darker colour than those of the pot which was not so seeded, and from this time the plants gained both in leaf surface, and in number of leaflets, and maintained a brighter green colour. At the conclusion of the experiments it was found that the roots of the plants in the unseeded pot had many nodules; the roots of the plants of the seeded pots had many more and much larger nodules than those of the unseeded pot. That these had nodules at all is to be attributed to the impurity and non-sterilization of the sand. The root, too, was much more distributed through the whole of the sand which was seeded than through the sand which was not seeded. The roots of the plants grown in garden soil were very much developed, but showed comparatively few nodules, which were, moreover, smaller than those of the other pots. Owing to the lateness of the season none of the plants flowered.

With regard to the above ground growth at the end of the experiment, there was more vegetable substance produced in the pots seeded with soil organisms than in the unseeded pot; and this increased growth was without doubt connected with the development of the root nodules and their contents. But the greatest gain was in the total nitrogen. In fact, whilst the amount of dry produce in the seeded pots was less than one-fifth more than that of the unseeded pot, there was about twice as much nitrogen in the above ground growth of the seeded, as in that of the unseeded pot. In the case of the garden soil there was more growth, more dry substance, and more nitrogen than in any of the others. In all three pots with sand, the amount of nitrogen in the produce, and in the sand, at the end of the experiment was far greater than that of the seed sown, and the sand, at the commencement. In each case the amount of nitrogen in the sand remained practically unchanged, the gain, therefore, being in the plants. The same may be said of the garden soil, but with some reserve, owing to the great difficulty, to say the least, of detecting slight changes in the amount of nitrogen in a large bulk of rich soil. There is, at least, no evidence to show that either the sand or the garden soil have taken up nitrogen on their own account, independently of the plant.

In order to show clearly that the gain of nitrogen is far beyond the limits of experimental error, it will be well to give some numerical results showing the actual amounts which had to be dealt with. Leaving out of account the difference in the amount of nitrogen of the seeds sown in each pot—the exact amounts are recorded in the paper—and the slight difference in the initial and final amounts of nitrogen in the sand, the results will be as follows:—In the 9 pounds of sand which each pot contained there was nearly 0·1 gram of nitrogen. The three seeds sown in each pot contained nearly 0·03 gram of nitrogen. At the conclusion of the experiment the vegetable produce contained: pot 1, 0·28; pot 2, 0·54; pot 3, 0·44 gram of nitrogen; which, after deducting the nitrogen of the seed sown, corresponds with a gain of 0·25, 0·51, and 0·41 gram of nitrogen.

The experiments in the second season, 1889, included the following leguminous plants: peas, red clover, vetches, blue lupins, yellow lupins, and lucerne. The sand used this time was a coarse, white sand which was well washed and also sufficiently, if not absolutely, sterilized by heating for some days at nearly 100° C. The necessary ash constituents, mixed with an equal weight of calcium carbonate, were added to each pot. There were four pots to each series. No. 1 contained the prepared sand with nothing further added. Nos. 2 and 3 the same sand to which a soil extract was added—prepared from a good garden

[1] "New Experiments on the Question of the Fixation of Free Nitrogen (Preliminary Notice)," by Sir J. B. Lawes, Bart., LL.D., F.R.S., and Prof. J. H. Gilbert, LL.D., F.R.S. (Proc. Roy. Soc., xlvi. 85).

soil in the case of all the plants except the lupins, and for these from a special sandy soil from a field in which lupins were growing. No. 4 garden soil, or, for the lupins, the special lupin soil. With regard to the peas and vetches of the pots not seeded with soil extract, the growth was extremely limited, and the colour of the leaves pale green; in the second and third pots of the two series there was luxuriant growth, the plants being taller even than those grown in garden soil. On the other hand, the garden soil plants were more vigorous and produced flower and seed. An examination of the roots of the plants showed an entire absence of nodule formation on those of the pots where no soil extract was given, whilst on the roots of the other plants there was, coincidently with the increased growth, an abundance of nodules. As in the experiments of 1888, the amount of nodules on the roots of the plants grown in garden soil was less than in the sand treated with soil extract.

Still more striking were the results obtained with yellow lupins. Whilst the plants of pot 1 (without soil extract) barely appeared above the rim of the pot, those of pots 2 and 3 (with soil extract) were large branched plants, the largest being 2 feet high—larger even than those grown in the lupin soil. Moreover, unlike the peas and vetches, the yellow lupins grown in sand seeded with soil organisms, flowered and seeded freely. The superiority of these plants over those grown in the lupin sandy soil may be due to the fact that the lupin soil was much less porous than the sand, especially when watered, and perhaps on this account less adapted for favourable growth. The roots of the plants without soil extract seeding were of very limited development and quite free from nodules. In pots 2 and 3, with soil extract seeding, the root development was very great, and the roots showed several large swellings; the ends of the roots were thickly covered with root-hairs, probably indicating an effort to acquire water and mineral nutriment in quantity commensurate with the large amount of nitrogen fixed, and so rendered available to the plant. In the garden soil the root development and nodule formation were much less.

The blue lupins again failed, with the exception of one plant in one pot. The red clover and lucerne are left for further growth. In pot 1 (unseeded) of the lucerne the plants do not appear to have grown at all since a few weeks after the seeds were sown, and for a long time there seemed to be no increased growth in pots 2 and 3, which were seeded with garden soil extract. Pot 2 had, therefore, a fresh quantity of soil extract—this time from a soil where lucerne was growing—added; this also seemed for some time to have no effect, but subsequently there was some increased depth of colour and some increased growth. Pot 3 was watered with a dilute solution of calcium nitrate, which soon produced a very marked and beneficial effect. With regard to the red clover, the results are, as yet, uncertain; both in the pots to which soil extract was added, and in that which had no soil extract, there is much more growth than is believed can be accounted for by nitrogen in the seed sown. The glass house in which the experiments are made stands in the middle of allotment gardens where vegetables of all kinds are grown, and this fact, viewed in the light of Hellriegel and Wilfarth's more recent results, already referred to—according to which the best results with clover are obtained by seeding with organisms from a root-crop soil—points to a possible acquisition of organisms from the air as the most probable explanation.

Attention is drawn to the widely different external appearance of the tubercles of the different plants. In the case of peas, they occurred generally as agglomerations; on the roots of vetches the nodules were generally single. Lupins seem to have two kinds of tubercular development, the most prevalent being "swellings" which entirely encase the thick roots; the "nodules" are generally small, and are distributed on the root-fibres. The lucerne nodules are, again, quite different in form from any of those already mentioned, being long, and generally divided or branched.

Returning to the main object of the investigation, the results confirm those of Hellriegel and Wilfarth, in showing the fixation of free nitrogen under the influence of microbe-seeding of the soil, and the resulting nodule formation on the roots in the case of the leguminous plant.

It appears that, concurrently with the experiments made at Rothamsted, M. Bréal, of the Muséum d'Histoire Naturelle of Paris, has made various experiments instigated by the results of Hellriegel and Wilfarth, and his results also confirm those of Hellriegel.

Hellriegel agrees with the authors that the *Leguminosæ* utilize soil nitrogen. He considers that the soil would be drawn upon first, and that this source is supplemented by the elementary nitrogen of the air, brought into combination by means of the organisms; he also considers that there would be more or less fixation even with a soil rich in nitrogen. On the other hand, Vines found the formation of tubercles, and presumably also the fixation of free nitrogen, is much reduced, or even stopped altogether, by the application of much nitrate to the soil; and the Rothamsted experiments indicate, that with a rich garden soil there are far fewer nodules formed, than with a sand containing but little nitrogen, and seeded with soil organisms. If subsequent experiments should show this to be the case, the amount of nitrogen of a crop, derived from the air, and the amount derived from a soil, would vary very much according to circumstances; fixation would take place most freely in the case of a sandy, or poor and porous soil, and less in a richer soil.

Upon the whole, it is considered that the evidence at command points to the conclusion that, in the case of most, if not all our leguminous crops, more or less of their nitrogen will be due to fixation under the conditions supposed.

Regarding the mode in which the organisms, which, in symbiosis with the higher plants, bring about the fixation, although Marshall Ward, Praźmowski, and Beyerinck have already contributed interesting results as to the mode of life of these bodies, much has yet to be learnt on the subject before an adequate explanation of the phenomena involved can be given. The authors suggest the following alternatives:—"(1) That, somehow or other, the plant itself is enabled, under the conditions of symbiotic life, to fix free nitrogen of the atmosphere by its leaves—a supposition in favour of which there seems to be no evidence whatever. (2) That the parasite utilizes and fixes free nitrogen, and that the nitrogenous compounds formed are taken up by the host. On such a supposition, the actually ascertained large gain of nitrogen by the leguminous plant growing in a nitrogen-free, but properly infected soil, becomes intelligible."

In their former paper (*loc. cit.*) the authors had stated that all the evidence that had been acquired in lines of inquiry previously followed had failed to solve conclusively the question of the sources of the whole of the nitrogen of the *Leguminosæ*, and that hence it should not excite surprise that new light should come from a new line of inquiry.

"The question suggests itself, whether such, or allied agency, comes into play in the nitrogen assimilation of leguminous plants generally, or that of other than the agricultural representatives of the non-leguminous families to which we owe such plants, or of those of the numerous and varied other families of the vegetable kingdom.

"It is true that the families which contribute staple agricultural plants are but few, and that the agricultural representatives of those families are also comparatively few. The families so contributing are, however, among the most important and widely distributed in the vegetable kingdom; as also are some of the plants they contribute. As prominent examples may be mentioned the *Gramineæ*, affording the cereal grains, a large proportion of the mixed herbage of grass-land, and other products; also the *Leguminosæ*, yielding pulse-crops, many useful herbage plants, and numerous other products. As we have said, there does not seem to be an unsolved problem as to the sources of the nitrogen of other of our agricultural plants than those of the leguminous family. Obviously, however, it would be unsafe to generalize in regard to individual families as a whole, from results relating to a limited number of examples supplied by their agricultural representatives alone. Still, there is nothing in the evidence at present at command to point to the supposition that there is any fundamental difference in the source of the nitrogen of different members of the same family, such as is clearly indicated between the representatives of the leguminous, and of the other families, supplying staple agricultural products. On the other hand, existing evidence does not afford any means of judging whether or not similar, or allied agencies to those now under consideration, or even quite different ones, may come into play in the nitrogen assimilation of other families which contribute such a vast variety of vegetation to the earth's surface." N. H. J. M.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—At the meeting of the Philosophical Society, on Monday, May 12, at 4.30 p.m., the following papers are promised:—

Mr. Langley, effect of nicotin on the nervous system of the fresh-water crayfish.

Mr. Shipley, on a new species of Phymosoma, with some account of the geographical distribution of the genus.

Mr. Adami, on the action of the papillary muscles of the heart.

Mr. Harmer, exhibition of specimens of a Land-Planarian (*Rhynchodemus terrestris*, O. F. Müller) found in Cambridge.

SCIENTIFIC SERIALS.

The *American Meteorological Journal* for March contains the conclusion of M. Faye's articles on the theory of storms, based on Redfield's laws. The author maintains that cyclones are descending whirls with a vertical axis, that they originate in the upper currents of the atmosphere, and follow the course of these currents. He considers it very desirable that two different modes of drawing charts should be adopted, to distinguish between cyclones and statical depressions to which, in his opinion, the laws of storms do not in any way apply.—Prof. H. A. Hazen contributes an article on the spectre of the Brocken, and gives a summary of various explanations of the phenomenon. He gives the results of his observations upon a similar shadow seen upon Mount Washington. This number also contains an extract from a paper by Dr. Schenck on the climatic treatment of pulmonary consumption; the advantages of New Mexico, especially, are pointed out.

In the number of the *Journal* for April, M. Faye commences a series of articles on trombes and tornadoes; he deals principally in this number with the theories of various writers and with descriptions of the phenomenon.—Mr. A. H. Dutton analyses the laws laid down by Padre Viñes relative to the normal points of recurvature of West India hurricanes. The result of his inquiry is that less than 14 per cent. of the tropical storms obeyed the laws.—Mr. A. L. Rotch summarizes the proceedings of the International Hydrological Congress held at Paris in October last. The next Congress is to be held in Rome in 1892.

Department of Agriculture, Bulletin No. 4, July 1889 (by authority, Government Printing Office, Melbourne)—This Report embodies the results of State-aided scientific effort which is intended to benefit agriculture, as we sincerely trust it will. The contents are miscellaneous, although all have direct bearing on the theory and practice of agriculture. Reports on horse-breeding, the needs of plants, irrigation, liquorice, yields of milk, vineyards, fruit-culture, Danish dairying, &c., yield a varied diet for the omnivorous reader, and will be of special service to Australian cultivators. We plead guilty to a feeling in connection with the perusal of such *Bulletins* as this, that the work is official, and lacks spontaneity. There is, notwithstanding, much that is valuable. Take, for example, the raisin industry (p. 91). Here we find described the conditions for successful growth, varieties, cultivation, and drying. What can be more useful to a colonist up country than to possess trustworthy information in detail on such a subject? If he is engaged in the wider pursuits of horse or cattle ranching, he will find subject-matter—addresses of breeders, names of sires, and other information of solid value. The *Bulletin* will also be of interest to the increasing class of owners of land in Australia who reside in England, as well as to young men who are thinking of making Australia their home. Anyone writing for this class of information should secure the previous numbers and also the future issues, and these he would probably be able to obtain free of charge by application at the offices of the Agricultural Department at Melbourne, or in London, at the Australian Colonial Offices in Victoria Street.

SOCIETIES AND ACADEMIES.

London.

Royal Society, March 27.—"Measurements of the Amount of Oil necessary in order to check the Motions of Camphor upon Water." By Lord Rayleigh, Sec. R.S.

The motion upon the surface of water of small camphor scrapings, a phenomenon which had puzzled several generations of inquirers, was satisfactorily explained by Van der Mensbrugghe (*Mémoires Couronnés* (4to) of the Belgian Academy, vol. xxxiv., 1869), as due to the diminished surface-tension of water impregnated with that body. In order that the rotations may be lively, it is imperative, as was well shown by Mr. Tomlinson, that the utmost cleanliness be observed. It is a good plan to submit the internal surface of the vessel to a preliminary treatment with strong sulphuric acid. A touch of the finger is usually sufficient to arrest the movements by communicating to the surface of the water a film of grease. When the surface-tension is thus lowered, the differences due to varying degrees of dissolved camphor are no longer sufficient to produce the effect.

It is evident at once that the quantity of grease required is excessively small, so small that under the ordinary conditions of experiment it would seem likely to elude our methods of measurement. In view, however, of the great interest which attaches to the determination of molecular magnitudes, the matter seemed well worthy of investigation; and I have found that by sufficiently increasing the water surface the quantities of grease required may be brought easily within the scope of a sensitive balance.

In the present experiments the only grease tried is olive oil. It is desirable that the material which is to be spread out into so thin a film should be insoluble, involatile, and not readily oxidized, requirements which greatly limit the choice.

Passing over some preliminary trials, I will now describe the procedure by which the density of the oil film necessary for the purpose was determined. The water was contained in a sponge-bath of extra size, and was supplied to a small depth by means of an india-rubber pipe in connection with the tap. The diameter of the circular surface thus obtained was 84 cm. (33"). A short length of fine platinum wire, conveniently shaped, held the oil. After each operation it was cleaned by heating to redness, and counterpoised in the balance. A small quantity of oil was then communicated, and determined by the difference of readings. Two releasements of the beam were tried in each condition of the wire, and the deduced weights of oil appeared usually to be accurate to $\frac{1}{20}$ milligram at least. When all is ready, camphor scrapings are deposited upon the water at two or three places widely removed from one another, and enter at once into vigorous movement. At this stage the oiled extremity of the wire is brought cautiously down so as to touch the water. The oil film advances rapidly across the surface, pushing before it any dust or camphor fragments which it may encounter. The surface of the liquid is then brought into contact with all those parts of the wire upon which oil may be present, so as to ensure the thorough removal of the latter. In two or three cases it was verified by trial that the residual oil was incompetent to stop camphor motions upon a surface including only a few square inches.

The manner in which the results are exhibited will be best explained by giving the details of the calculation for a single case, *e.g.* the second of December 17. Here 0·81 milligram of oil was found to be nearly enough to stop the movements. The volume of oil in cubic centimetres is deduced by dividing 0·00081 by the sp. gr., viz. 0·9. The surface over which this volume of oil is spread is

$$\tfrac{1}{4}\pi \times 84^2 \text{ square centimetres};$$

so that the thickness of the oil film, calculated as if its density were the same as in more normal states of aggregation, is

$$\frac{0\cdot00081}{0\cdot9 \times \frac{1}{4}\pi \times 84^2} = \frac{1\cdot63}{10^7} \text{ cm.},$$

or 1·63 micro-millimetres. Other results, obtained as will be seen at considerable intervals of time, are collected in the table.

A Sample of Oil, somewhat decolorized by exposure.

Date.	Weight of oil.	Calculated thickness of film.	Effect upon camphor fragments.
	Mg.		
December 17 ...	0·40	0·81	No distinct effect.
January 11 ...	0·52	1·06	Barely perceptible.
,, 14 ...	0·65	1·32	Not quite enough.
December 20 ..	0·78	1·58	Nearly enough.
January 11 ...	0·78	1·58	Just enough.
December 17 ...	0·81	1·63	Just about enough.
,, 18 ...	0·83	1·68	Nearly enough.
January 22 ...	0·84	1·70	About enough.
December 18 ...	0·95	1·92	Just enough.
,, 17 ...	0·99	2·00	All movements very nearly stopped.
,, 20 ...	1·31	2·65	Fully enough.

A Fresh Sample.

January 28 ..	0·63	1·28	Barely perceptible.
,, 28 ...	1·06	2·14	Just enough.

For convenience of comparison they are arranged, not in order of date, but in order of densities of film.

The sharpest test of the quantity of oil appeared to occur when the motions were nearly, but not quite, stopped. There may be some little uncertainty as to the precise standard indicated by "nearly enough," and it may have varied slightly upon different occasions. But the results are quite distinct, and under the circumstances very accordant. The thickness of oil required to take the life out of the camphor movements lies between one and two millionths of a millimetre, and may be estimated with some precision at 1·6 micro-millimetre. Preliminary results from a water surface of less area are quite in harmony.

For purposes of comparison it will be interesting to note that the thickness of the black parts of soap films was found by Messrs. Reinold and Rücker to be 12 micro-millimetres.

An important question presents itself as to how far these water surfaces may be supposed to have been clean to begin with. I believe that all ordinary water surfaces are sensibly contaminated; but the agreement of the results in the table seems to render it probable that the initial film was not comparable with that purposely contributed. Indeed, the difficulties of the experiments proved to be less than had been expected. Even a twenty-four hours' exposure to the air of the laboratory[1] does not usually render a water surface unfit to exhibit the camphor movements.

The thickness of the oil films here investigated is, of course, much below the range of the forces of cohesion; and thus the tension of the oily surface may be expected to differ from that due to a complete film, and obtained by addition of the tensions of a water-oil surface and of an oil-air surface. The precise determination of the tension of oily surfaces is not an easy matter. A capillary tube is hardly available, as there would be no security that the degree of contamination within the tube was the same as outside. Better results may be obtained from the rise of liquid between two parallel plates. Two such plates of glass, separated at the corners by thin sheet metal, and pressed together near the centre, dipped into the bath. In one experiment of this kind the height of the water when clean was measured by 62. When a small quantity of oil, about sufficient to stop the camphor motions, was communicated to the surface of the water, it spread also over the surface included between the plates, and the height was depressed to 48. Further additions of oil, even in considerable quantity, only depressed the level to 38.

The effect of a small quantity of oleate of soda is much greater. By this agent the height was depressed to 24, which shows that the tension of a surface of soapy water is much less than the combined tensions of a water-oil and of an oil-air surface. According to Quincke, these latter tensions are respectively 2·1 and 3·8, giving by addition, 5·9; that of a water-air surface being 8·3. When soapy water is substituted for clean, the last number certainly falls to less than half its value, and therefore much below 5·9.

April 24.—"On a Pneumatic Analogue of the Wheatstone Bridge." By W. N. Shaw, M.A., Lecturer in Physics in the University of Cambridge. Communicated by Lord Rayleigh, Sec. R.S.

When fluid flows steadily through an orifice in a thin plate, the relation between the rate of flow, V, measured in units of volume of fluid per second, and the head H (the work done on unit mass of the fluid during its passage) may be expressed by the equation :—

$$H = RV^2,$$

where R is a constant depending upon the area of the orifice. If the head be measured in gravitation units, R is equal to $1/2gk^2a^2$, where g is the acceleration of gravity, a the area of the orifice, and k the coefficient of contraction of the vein of fluid, a factor which is independent of the rate of flow.

Measurements made upon the flow of air in order to determine the coefficient of contraction have been hitherto such as may be termed "absolute"; that is to say, the head and the flow have each been separately expressed in absolute measure and the value of R determined by taking the ratio of the head to the square of the flow. This process is exactly analogous to measuring the electrical resistance of a wire by finding the electromotive force between its ends and the current which flows along it.

M. Murgue, in a work on "The Theory and Practice of Centrifugal Ventilating Machines" (translated by A. L. Steavenson), has shown that the internal resistance of a centrifugal fan to the flow of air through it can be calculated from the effects produced on the flow by varying the size of a second orifice through which the air had to pass. This process is evidently parallel to calculating the internal resistance of a battery by finding the effect produced upon the current by varying the external resistance. The further development of the analogy seems to afford a "null" method of comparing resistances to the motion of air, and of verifying the laws of flow, and one which requires only a detector and not an anemometer, and is independent of the constancy of the flow. Whether it could be used practically to test the laws of flow and measure the pneumatic constants for various orifices to a higher degree of accuracy than has hitherto been attained, evidently depends upon the sensitiveness of the arrangement. In order to try this, the author constructed what may be called a pneumatic analogue of the Wheatstone Bridge. It consists of three wooden rectangular boxes, A, B_1, B_2. The ends of B_1 and B_2 abut against the side of A; between B_1 and A is a rectangular opening, a_2, 1 in. × ½ in., in a cardboard diaphragm; between B_2 and A a rectangular opening, a_4, 1 in. × 1 in., in a similar diaphragm. In the side of B_1 at a_1 is an adjustable slit, made by cardboard shutters sliding in cardboard grooves, and at a_3 in the side of B_2, opposite to a_1, is a similar adjustable slit. The tube connecting B_1 and B_2, or "galvanometer" tube, is a straight tube of glass, G, of about 1·1 inch internal diameter. It can be closed at one end by a small trap-door in the interior of the box B_1, which can be opened and shut by a steel wire passing through a cork in the top of B_1. The sensitiveness of the apparatus depends upon the indicator employed. There are many indicators that might be suggested; the one tried and found to work well consists of two very small parallel magnetized sewing needles, stuck through a cap of elder-pith, supported on a small agate compass centre; the needles carry very light mica vanes on one side of the centre, counterpoised by a small quantity of platinum wire. The whole is balanced on the point of the finest needle obtainable, and forms a very delicate wind vane. The needles take up a definite position of equilibrium with the planes of the vanes approximately north and south. The apparatus being so placed that the tube, G, is east and west, the vanes always set across the tube when there is no current. The needle-points enable the position of equilibrium to be clearly identified by the aid of a fiducial mark on the glass tube. The sensitiveness can be altered as desired by an external control magnet, just as that of a galvanometer needle can be. The little compass needle or wind vane, M, is very sensitive to the motion of air in the tube.

The head is produced by a gas-burner in a metal chimney fitted to the lid of the box A.

Of the four apertures of the bridge, two, viz. a_2 and a_4, are inaccessible without pulling the arrangement to pieces; they represent areas of ½ sq. in. and 1 sq. in. respectively, as accurately as a knife can cut them in cardboard.

The other two areas, viz. a_1 and a_3, are made by sliding shutters, as already mentioned. The edges were cut with a knife, and they probably are only rough approximations to areas in a truly thin plate.

If the coefficient of contraction may be assumed to be independent of the shape of the orifice, we get the condition for no flow through the "galvanometer" tube :—

$$\frac{a_1}{a_2} = \frac{a_3}{a_4},$$

where the a's represent the actual measured areas of the four orifices.

Observations have been taken with the apparatus—

(1) To verify the law of proportionality of areas, viz.

$$\frac{a_1}{a_2} = \frac{a_3}{a_4}.$$

(2) To verify the inference that the condition of no flow is independent of the total head.

[1] In the country.

(3) To compare a circular with a rectangular aperture.

The observations are sufficient to show that the width of the adjustable slit when there is no flow is a perfectly definite magnitude, and that a properly constructed apparatus is capable of making measurements of the effective areas of orifices with a very considerable degree of precision.

"On the Effect of Tension upon Magnetic Changes of Length in Wires of Iron, Nickel, and Cobalt." By Shelford Bidwell, M.A., F.R.S.

The *iron* used in these experiments was a piece of soft annealed wire, 0·7 mm. in diameter and 10 cm. in length between the clamps. The weights successively attached to it were equivalent to 1950, 1600, 1170, 819, 585, and 351 kilos. per square cm. of section.

The *nickel* wire was 100 mm. long, and 0·65 mm. in diameter. The loads under which it was examined were 2310, 1890, 1400, 980, 700, and 420 kilos. per sq. cm.

The *cobalt* used was a narrow strip measuring 100 mm. by 2·6 mm. by 0·7 mm., its cross section being, therefore, 1·82 sq. mm. It was not possible to obtain this metal in the form of a wire. The loads employed for the strip were equivalent to 772, 344, and 75 kilos. per sq. cm.

In all the experiments the loads were successively applied in decreasing order of magnitude, and before every single observation the wire or strip was demagnetized by reversals, without, of course, being removed from the coil. The magnetizing force was carried up to about 375 C.G.S. units for iron and nickel, and 500 units for cobalt.

The results are given in several tables and curves, and point to the following conclusions:—

Iron.—Tension diminishes the magnetic elongation of iron, and causes contraction to take place with a smaller magnetizing force.

Nickel.—In weak fields the magnetic contraction of nickel is diminished by tension. In fields of more than 140 or 150 units, the magnetic contraction is increased by tensional stress up to a certain critical value, depending upon the strength of the field, and diminished by greater tension.

Cobalt.—The magnetic contraction of cobalt is (for magnetic fields up to 500 G.C.S. units and loads up to 772 kilos. per sq. cm.) practically unaffected by tension.

Chemical Society, April 3.—Dr. Hugo Müller, F.R.S., Vice-President, in the chair.—The following papers were read:—Note on the hydrosulphides, by Messrs. S. E. Linder and H. Picton. The authors find that freshly-precipitated metallic sulphides almost always contain hydrogen sulphide, that they are, in fact, hydrosulphides or remnants of hydrosulphides, and that if, instead of adopting the usual plan of passing gas through the solution, the metallic salt be allowed to run slowly into a solution of hydrogen sulphide in water in the absence of too large an excess of acid, a solution of the hydrosulphide is obtained which can be freed from dissolved hydrogen sulphide by the current of hydrogen. The copper hydrosulphide, $7CuS, H_2S$, and mercury hydrosulphide, $31HgS, H_2S$, are described in the paper.—Researches on the germination of some of the *Gramineæ*, Part I., by H. T. Brown, F.R.S., and Dr. G. H. Morris. This investigation was undertaken with the view of throwing some light on the complex metabolic processes which take place in the germination of seeds. The authors, during the progress of the inquiry, have examined and experimented with the seeds of a great number of the grasses, but this, the first part of their paper, is confined almost entirely to a consideration of the changes which take place in barley during the earlier periods of its growth. In recording the visible changes which occur in the seed during germination, it is shown that a disintegration and dissolution of the cell-walls of the endosperm always precede any attack upon the cell-contents. This breaking down of the cell-wall is shown in a subsequent portion of the paper to depend on the production during germination of a special cellulose-dissolving or "cyto-hydrolytic" enzyme, which, like diastase, is soluble. The action of this enzyme on the cell-walls of some kinds of vegetable parenchyma is very energetic. The physiological importance of this cyto-hydrolyst is very great, for, owing to the non-diffusible nature of the amylo-hydrolytic enzyme—diastase—the previous breaking down of the cell-wall is a necessary prelude to the dissolution of the contained starch-granules. The authors show that the appearance of the cyto- and amylo-hydrolysts is due to a specialized secretory function of the layer of columnar epithelium which covers the outer surface of the scutellum. It has hitherto been considered that the function of this epithelium was exclusively that of an absorptive tissue: its absorptive as compared with its secretory functions are, however, of quite secondary importance. The natural food material—starch—does not appear to have any special power of stimulating the cells of the epithelium to increased secretion of a diastase, but the flow both of diastase and of the cyto-hydrolytic enzyme from these cells is affected in a very remarkable degree by the presence of certain carbohydrates. Providing the carbohydrate is one which is readily assimilable by the embryo, such as cane-sugar or maltose, secretion of ferment is checked or even entirely inhibited. No such inhibitory action is, however, produced by such substances as mannitol and milk-sugar, which are entirely without nutritive value. The authors' experiments in this direction point to the secretion of the amylo-hydrolytic and cyto-hydrolytic enzymes as being to some extent *starvation phenomena.* The power of secretion possessed by the epithelium is in some way or other so adapted to the requirements of the young plant as to be only exercised when the supply of tissue-forming carbon compounds begins to fail. The histological changes which take place in the cells of the epithelium during secretion are very similar to those which have been observed in certain secretory cells of the alimentary tract of animals, and in the secretory cells of some of the insectivorous plants. The authors confirm the important generalization of Sachs, that the relation of the embryo to the endosperm is that of parasite to host, and they have availed themselves of this relation by cultivating the embryo on suitable media after separating it from its endosperm. In this way they have obtained information with regard to the secretory powers of the embryo and the chemical modifications of its absorbed nutriment which it would have been impossible to obtain by any other means. The results of cultivating excised embryos on various nutrient solutions, more especially of the carbohydrates, are recorded, and it is shown that, whilst cane-sugar, invert-sugar, dextrose, lævulose, maltose, raffinose, galactose, and glycerol have all more or less nutrient value, milk-sugar and mannitol do not in any way contribute to the growth of tissue in the young plant. Of all the substances tried, cane-sugar has by far the greatest nutritive power. Maltose, although the natural food of the embryo when attached to its endosperm, is decidedly inferior in this respect to cane-sugar. This, at a later point in the paper, is shown to be due to the fact that maltose, directly it is absorbed by the growing embryo, becomes transformed into cane-sugar by the living cells, and in this form is passed from cell to cell. When cane-sugar is supplied ready formed to the young plantlet, there is manifestly a saving of energy to the living cell, which receives its nutriment in a form in which it is readily available for its requirements. An examination of the sugars produced during germination, and their mode of distribution in the grain, have convinced the authors that the transformed starch of the endosperm is absorbed by the embryo in the form of maltose, and that the seat of production of the cane-sugar which all germinated grain contains is the tissues of the embryo itself. The authors are continuing their work upon the germination of the grasses, and are applying the methods described in this first part of their paper to an elucidation of the chemical changes which the other reserve materials, especially the proteids, undergo in their passage from the endosperm, and of the agencies which are at work in bringing about these transformations. In the discussion which followed the reading of this paper, Prof. Marshall Ward, F.R.S., pointed out that in the seeds of the *Gramineæ*, *Cyperaceæ*, and other families of plants, there is a peculiar layer of cells, from one to three or more deep, surrounding the starchy endosperm, and distinguished from the latter by containing no starch, but relatively large quantities of proteids: this layer belongs to the endosperm, but as the seed ripens, the cells store special proteids instead of the starch-grains which predominate in other endosperm cells. In the oat there is such a layer, one cell deep, and it has been shown that, during germination, the dissolution of the starch and the cell-walls of the starch-containing cells begins near the surface of this layer, which itself persists, and the cells of which take up food and undergo changes so like those of excreting cells that it was concluded that they excrete the diastatic enzyme. Prof. Ward further remarked that the authors' suggestion that more than one enzyme may be excreted according to the nutrition of the cells, and their proof that a cellulose-dissolving enzyme exists in barley, are borne out by various recent researches, and by Wortmann's observations on the behaviour of bacteria in a mixture of starch and proteids. Wortmann proved that so long as the bacteria were fed with proteids they refused to excrete the diastatic enzyme which they produce in abundance when only carbohydrates are at their disposal. Prof. Green said that in the case of the date-stone his observations led him to believe that the enzyme was independent of the endosperm, and that probably it was located in the epithelial layer. But in castor oil seeds not only the embryo but also the endosperm cells appeared to be possessed of vitality, the fatty matter of the latter undergoing change even when not subject to the action of the embryo; probably the enzyme was present in the form of an enzymogen, as extracts of the seeds were rendered active by acids. Prof. Armstrong remarked that the authors had shown that in the plant maltose was converted into cane-sugar; dextrose, according to their observations, did not undergo conversion into cane-sugar, but gave invert-sugar—that is to say, it became partially converted into lævulose, these constituents of cane-sugar being apparently incapable of interacting. It was known from Emil Fischer's work that dextrose could be converted into lævulose, and that maltose was an etheric compound of the acetal type, formed from two molecules of dextrose, one of which acted as aldehyde, the other as alcohol; it was conceivable that if the "dextrose residue" in maltose underwent a change comparable with that which is involved in the conversion of dextrose into lævulose, a compound would be obtained which if not identical with cane-sugar would be convertible into it by hydration and subsequent dehydration. The authors had spoken of the maltose becoming incorporated with the protoplasm from which the cane-sugar was then elaborated; perhaps the effect was comparable with that exercised by phenylhydrazine in effecting the conversion of dextrose into lævulose through the agency of the osazone. Dr. Lauder Brunton and Mr. Thiselton Dyer also took part in the discussion.—The formation of indene-derivatives from dibrom-α-naphthol, by Prof. R. Meldola, F.R.S., and Mr. F. Hughes.—The action of hydrochloric acid on manganese dioxide; manganese tetrachloride, by Mr. H. M. Vernon. Contrary to the statements of Pickering (Chem. Soc. Trans., 1879, 654), the author finds that the original product of the action of hydrochloric acid on manganese dioxide is manganese tetrachloride, and that at first no free chlorine is formed.

April 17.—Dr. W. J. Russell, F.R.S., President, in the chair.—The following papers were read:—Phosphorous oxide, Part 1, by Prof. T. E. Thorpe, F.R.S., and Mr. A. E. Tutton. The authors describe a method of making phosphorous oxide by burning phosphorus in air. Pure phosphorous oxide crystallizes in thin monoclinic prisms, melts at 22°·5, solidifies at 21°, and boils unchanged in an atmosphere of nitrogen or carbon dioxide at 173°. When heated at 300°, it decomposes, and at 440° is wholly converted into phosphorus and phosphorus tetroxide: $4P_4O_6 = 6P_2O_4 + P_4$. Phosphorous oxide is readily acted on by light, and in bright sunshine its colour rapidly becomes yellow, and eventually dark red, the violet rays being most active in effecting the change. Its molecular weight, as determined by Hofmann's and Raoult's methods, corresponds with the formula P_4O_6; phosphorous oxide, therefore, in this respect is analogous to arsenious and antimonious oxides. The thermal expansion of liquid phosphorous oxide is expressed by the formula—

$$V = 1 + 0{\cdot}0_3 91377\, t - 0{\cdot}0_6 11175\, t^2 + 0{\cdot}0_8 38607\, t^3;$$

its relative density at the boiling-point is 1·6859, whence its molecular volume = 130·5; and its molecular refraction for A ($\lambda = 7604$) at 27°·4 is 60·5. Contrary to the usual statement of the text-books, cold water has very little action on phosphorous oxide: many days elapse before even a small quantity is dissolved; it then forms phosphorous acid. Hot water acts upon it with explosive violence, forming the red sub-oxide, phosphoric acid, and spontaneously inflammable phosphoretted hydrogen. Phosphorous oxide spontaneously oxidizes to phosphorus pentoxide on exposure to air or to oxygen, and the process of oxidation is attended under diminished pressure by a faint luminous glow; ozone is not formed as the oxidation proceeds. On gently warming the oxide in oxygen, the glow gradually increases in intensity until it passes into flame. In contact with ozone, phosphorous oxide glows at the ordinary temperature and pressure. Phosphorous oxide has a well-marked physiological effect, and it is not improbable that the action hitherto attributed to phosphorus, especially as regards its influence on the glycogenic functions of the liver and on tissue change, may be really due to this substance. The fumes from phosphorus consist largely of phosphorous oxide, and the odour of the product obtained by drawing air over phosphorus without allowing it to ignite is identical with that of the pure oxide; it is, indeed, highly probable, as Schönbein long ago surmised, that phosphorus vapour, as such, is odourless, and that the smell which phosphorus ordinarily possesses is a mixture of that of ozone and of phosphorous oxide.—The action of chlorine on water in the light, and the action of light on certain chlorine acids, by Prof. A. Pedler. As a general result of a number of experiments, it is found that, even in very strong tropical sunlight, water and chlorine interact to but a very slight extent when the proportion is about $100\ H_2O : Cl_2$; when the ratio is about $150\ H_2O : Cl_2$ action takes place to the extent of perhaps 50 per cent., and when more than $400\ H_2O : Cl_2$, to about 80 per cent. of the theoretical. Chlorine water containing about $708\ H_2O : Cl_2$ when exposed to direct tropical sunlight decomposes practically entirely in the sense of the equation $2H_2O + 2Cl_2 = O_2 + 4HCl$, an exceedingly small amount of chloric acid being formed; but when exposed in a south aspect to strong diffused daylight, gives much less oxygen and a variable amount of hypochlorous or chloric acids, very little oxygen but an increased amount of hypochlorous or chloric acids being formed when it is exposed in a north aspect to moderate diffused daylight. Hypochlorous acid, on exposure to light in dilute solutions, yields both oxygen and chloric acid, the proportion of oxygen being larger, the greater the intensity of the light. Solutions of chloric acid undergo little or no change. The author concludes that the action of chlorine on water is in its first stage similar to that which it exercises on cold, dilute aqueous potash or soda, and in its second stage to that on more concentrated hot solutions of these alkalis.—Note on the explosion of hydrogen sulphide and of carbon bisulphide with air and oxygen, by the same. The author finds that when a mixture of hydrogen sulphide, air, and oxygen is exploded, a normal result is obtained, sulphur dioxide and water being formed. But when carbon bisulphide vapour is similarly treated, a not inconsiderable proportion of the nitrogen of the air becomes oxidized, and sulphuric compounds are formed under the combined influence of the oxides of nitrogen and sulphur and of the moisture present.—The action of light on phosphorus, and on some of the properties of amorphous phosphorus, by the same. The author brings forward evidence to show that the term "amorphous phosphorus" is a distinct misnomer, and that, so far from the commercial amorphous phosphorus constituting a separate allotropic modification of the element, it is in reality the same substance as the form called rhombohedral or metallic phosphorus; the very slight differences in character noticed between the substances in question being explained by the difference in the state of division and the slight variations conditioned by their mode of formation. The change of red into ordinary phosphorus does not take place below 358°; above this the change takes place *in vacuo*, but exceedingly slowly, even at 445°.—The action of phosphoric anhydride on fatty acids, by Dr. F. S. Kipping. Heptylic acid yields 25–33 per cent. of dihexyl ketone when heated with phosphoric anhydride.

Royal Microscopical Society, April 16.—Dr. C. T. Hudson, F.R.S., President, in the chair.—Mr. J. Mayall, Jun., called attention to a spiral ruling on glass, sent by Mr. P. Braham, of Bath, which had been produced in an ordinary lathe, the diamond point being adjusted on the slide rest; also to a series of photomicrographs of diatoms, sent by Mr. T. Comber. These were of special interest from the fact that they were produced with sunlight, by which the maximum resolving power of the objective was obtained.—Mr. Mayall referred to an improved form of fine-adjustment, constructed and exhibited by Messrs. Powell and Lealand, in which the chief aim had been to construct a fine-adjustment which should combine extreme sensitiveness of action with accuracy and probable durability beyond what had previously been obtained. The essential feature was the application of what watchmakers would term a "jewelled movement." The whole of the contact surfaces by which the fine-adjustment was actuated consisted of polished steel and agate, the intention being to reduce the friction as much as was consistent with steadiness of motion. The result attained was undoubtedly an improvement on the old system, though the cost would probably limit the application to the few instruments required for very special and difficult investigations in microscopy. For high-class photomicrographic work, or where preparations had to be retained under observation for long periods of time, the new mechanism should be particularly useful, for the greater solidity of the general construction clearly pointed to greater precision and increased stability.—Mr. Goodwin exhibited a form of eye-piece for the microscope which gave a large field with considerable magnifying power.—Mr. A. W. Bennett gave a *résumé* of a paper, by Mr. W. West, on the fresh-water Algæ of North Wales. The paper described a collection of fresh-water Algæ, chiefly diatoms and desmids, made in various localities in North Wales and Anglesey, and it furnished what was beyond comparison the richest list of desmids which had ever been prepared in this country.—Prof. M. M. Hartog's paper, on the state in which water exists in live protoplasm, was read.—A paper descriptive of the method adopted by Mr. Halford in mounting the spermatozoa of the Salmonidæ was read, and specimens in illustration were exhibited by the lantern.—Mr. E. M. Nelson exhibited on the screen several slides showing under high powers (× 1350) the bordered pits of *Pinus* and *Tilia*. He also exhibited a small series of slides to show the qualities of a new apochromatic ¼-in. objective with fluorite lenses and of 95 N.A., which had recently been made by Messrs. Powell and Lealand.—Mr. Mayall mentioned that the gathering which was to have taken place at Antwerp, in celebration of the 300th anniversary of the invention of the microscope, was unavoidably postponed until next year.

Paris.

Academy of Sciences, April 28.—M. Hermite, President, in the chair.—On a class of differential equations of which the general integral is uniform, by M. Emile Picard.—On the characteristic equation of nitrogen, by M. Sarrau. In previous communications the author pointed out that certain experiments with carbonic acid verified an equation analogous to those proposed by Van der Waals and Clausius to represent the relation between the pressure, p, the volume, v, and absolute temperature, T. The following is the equation—

$$p = \frac{RT}{v - a} - \frac{K\epsilon^{-T}}{(v - \beta)^2};$$

where R, a, β, K, and ϵ are constants. A discussion of the experiments made by Regnault and by Amagat on nitrogen shows that its critical point may also be represented by this formula.—On the heats of formation and combustion of several nitrogenous bodies derived from albumenoid matters, by MM. Berthelot and André. The bodies experimented upon are glycollamine or glycocoll, alanine, leucine, tyrosine, asparagine, aspartic acid, and hippuric acid.—Researches on the condensation of benzine and acetylene vapour under the action of the silent discharge, by M. P. Schutzenberger. The benzine condenses into a clear, yellow, resinous solid. Analyses of the liquid employed and of the condensed product are given, and it is shown that the amount of oxygen contained in the latter could not have been taken up from the air, but must have passed through the glass tube.—On *Gomphostrobus heterophylla*, a coniferous prototype from the Permian of Lodève, by M. A. F. Marion.—Observation of Brooks's comet (a 1890) made with the Brunner equatorial at Toulouse Observatory, by M. E. Cosserat.—General theory of the visibility of interference fringes, by MM. J. Macé de Lépinay and Ch. Fabry. The consequences which follow from the theorem demonstrated are pointed out, and it is proposed to describe the experiments which verify them in a future communication.—On the phosphites and the pyrophosphite of lead, note by M. L. Amat.—The action of erythrite upon the alkaline alcoholates, by M. de Forcrand. The author gives a continuation of a previous paper, here discussing formulæ for the bodies discovered and giving thermal data which explain the behaviour of the new substances when heated.—The action of lead oxide upon toluene and the production of benzene, by M. C. Vincent. The paper treats of this reaction at temperatures below the melting-point of lead. The conclusions are drawn: (1) that oxide of lead attacks toluene below 335°, giving water, carbonic anhydride, and benzene; (2) that, at higher temperatures, less benzene and more stilbene and higher hydrocarbons are obtained; (3) that at a red heat, in addition to the above, hydrocarbons produced by the simple heat decomposition of benzene and toluene are obtained; (4) that diphenyl formed during this experiment in small quantity comes rather from the benzene formed by the action of oxide of lead upon the toluene than from benzene contained in the toluene employed.—Thermochemical researches on textile fibres (wool and cotton), by M. Léo Vignon.—Experiments relative to the loss and gain of nitrogen by fallow or cultivated land, by M. A. Pagnoul. The writer finds in the cases examined that the gain of nitrogen in two years is—(1) with bare soil 29 kg. per hectare; (2) with grass land 394 kg. per hectare; (3) with land laid down in clover 904 kg. per hectare.—Note by M. Ant. Magnin, on the parasitic castration of *Anemone ranunculoides* by *Æcidium leucospermum*.—On the discovery of a giant land tortoise at Mont Léberon, by M. Ch. Depéret.—On the action of the positive pole of a constant galvanic current upon microbes, and particularly upon the anthrax bacillus, by MM. Apostoli and Laquerrière. Among the conclusions drawn are the following: the heating effects of the current may be experimentally neutralized, but the destruction or weakening of vitality of the microbe still takes place; it is the positive pole only which acts upon the microbes, the negative pole and the intermediate space do not give any evidence of adverse action upon the organisms; the current *sui generis* has no effect upon the microbes; the action at the positive pole is due to the disengagement of acids and of oxygen, as will be shown in a further note.—On the existence of tuberculous endocarditis, note by M. Raymond Tripier.

Berlin.

Meteorological Society, April 1.—Prof. Schwalbe, President, in the chair.—Dr. Perlewitz spoke on the influence of the city of Berlin on local climatic conditions. To investigate this he had compared, for the year 1889, the meteorological records of two stations outside the city with those of three inside. As regards temperature, some allowance must be made for the fact that the exposure of the thermometers was not identical at all the stations. The differences in temperature between the city and the surrounding country were greater than for Vienna, the maximal difference showing itself in spring and summer, the minimal in winter. The differences were least at 2 p.m., greater at 7 a.m., and greatest at 9 p.m. The absolute humidity was much less inside the city than in the neighbouring country, and the difference was, as regards maxima and minima, the exact reverse of that which held good for the temperature; whereas, on the other hand, the relative humidity followed the same lines as for differences of temperature. The direction of the wind was generally different in the city from that in the surrounding country, but no definite relationship of the two could be deduced from the observations, and the same held good for the frequency and extent of clouds in the two localities. Thunderstorms were observed less frequently in the city than in the country, but here again it must be borne in mind that the conditions under which observations can be made in the former are much less favourable than for the latter.—Prof. Spörer spoke on the rotation of the sun, and came to the conclusion that the continued endeavours which have been made to determine the rotation of the sun from observations of sun-spots, cannot lead to any definite conclusions.

Physical Society, April 18.—Prof. du Bois-Reymond, President, in the chair.—Prof. Planck spoke on the difference of potential of two binary electrolytes. According to recent views there exists, in any uniform dilute solution of an electrolyte, a complete dissociation of the ions, the latter being in equilibrium, since the sum of the two electricities of the anions and kathions is equal and the osmotic pressure is everywhere the same, quite independently of the nature of the ions. The electrical charge of the ions and the osmotic pressure are the sole forces which are at work in the solution, and suffice to account for all the phenomena which take place inside it. But in order to calculate the above it is necessary to know the mobility of the ions; this has been determined experimentally by Kohlrausch for a large number of different ions, and he has also measured the electrical charge of the ions, this charge being independent of their nature. If the solution is not of uniform composition, the osmotic pressure leads to a movement of the ions from the more to the less concentrated parts of the solution. Now, since the mobility of the ions varies, being five times as great for hydrogen as for chlorine, it follows that a larger number of hydrogen atoms will pass from the more concentrated parts of the solution, than of chlorine. This, however, leads to an upsetting of the electrical equilibrium, and the electrical affinities work in a direction opposite to that of the flow of atoms. The speaker had developed a general mathematical formula to express what takes place in the case of two solutions of different concentrations which are in contact with each other through an intervening porous partition. By means of this formula he has calculated the magnitude of the differences of potential which establishes itself between the two electrolytes. Applying the formula to a special case, and calculating the difference of potential from the observed rate of flow of the ions and their known electrical charge, he showed that the values thus obtained correspond very closely with those obtained by direct measurement of the difference of potential.

Physiological Society, April 25.—Prof. du Bois-Reymond, President, in the chair.—Dr. Heymans spoke on medullated and non-medullated nerves. The medullary sheath of the former is characterized by the myelin formations which it yields under the action of water and the dark coloration with osmic acid. This last reaction is common to lecithin, protagon, and cholesterin, all of which are found in the medulla. When, however, lecithin has been treated with osmic acid it can no longer be extracted from the nerve, whereas protagon and cholesterin may be extracted by alcohol at 70° C. By taking advantage of this difference in their behaviour it becomes possible to test the statement that lecithin occurs in the neurokeratinous network of the medullary sheath, while protagon and cholesterin are present in the meshes of the network. Experiment does not support the above statement. The speaker had further used the reaction with osmic acid (2 per cent. solution) to investigate the occurrence of non-medullated nerves in certain places in which their presence is a subject of dispute. He found them in the sympathetic and olfactory nerves of the pike, but in much smaller numbers in the former than is usually stated to be the case. In many of the sympathetic fibres he observed a sheath composed of protoplasm which stained brown with osmic acid. He finally discussed fully the transition of medullated cerebrospinal nerves into the non-medullated processes of the ganglia.—Dr. Cowl spoke on methods of recording the variations of blood-pressure in an artery. He criticized the various forms of apparatus in use, and pointed out the errors arising from the use of elastic connections so frequently employed, as a result of which it is impossible to register the exact moment at which the pressure is zero. He had constructed an apparatus in which this source of error is avoided, and which admits of extremely delicate adjustment. He finally exhibited curves to demonstrate the advantages of the newer instrument.

BRUSSELS.

Academy of Sciences, March 1, 1890.—The following were the papers communicated:—Experiments made by Count Espiennes at Scy (Ciney), on the circulation of air during calm nights from the surface of broken ground, by M. F. Folie.—On certain inversions of temperature, and on the frost of September 16, 1887, at Spa, by M. G. Dewalque. In this and the preceding paper it is shown that cold strata of air lie in valleys, and many cases are given of places situated on elevated plateaus where the minimum temperature is habitually higher than that at places of less altitude lying in valleys.—Gustavus Adolphus Hirn, Associate of the Academy, born at Logelbach (Colmar), August 21, 1815, died at the same place, January 14, 1890. An account of his life and work is given by M. F. Folie.—Another obituary notice by M. F. Folie, on C. H. Buys-Ballot, born at Kloetingen, October 10, 1817, died at Utrecht, February 3, 1890.—On phillipsite crystals from sediments found in the centre of the Pacific Ocean, by M. A. F. Renard. In a previous note (February 1890) the physical characteristics and the composition of zeolite crystals from deposits in the Pacific were indicated; the author now shows the conditions under which the phillipsite and mineral matters which accompany it are found.—Determination of the variations in the coefficient and diffusion with temperature for liquids other than water, by M. P. De Heen. The liquids investigated are xylene, benzine, ethyl alcohol, amyl alcohol, amyl benzoate, and carbon bisulphide, at temperatures of 10°, 30°, 50°, 70°, and 90°.—On the nature of the polarizing matter of the beetroot in alcohol; rotatory power of pectous matters, by MM. L. Chevron and A. Droische. It is found pectine and its derivatives exercise an energetic action on polarized light; the rotatory power of these matters is three or four times greater than that of saccharose sugar.—Some properties of conics, by M. C. Servais.—On the centre of curvature of lines described during the displacement of a plane figure in its own plane, by the same author.—Solanidine from potato sprouts; preparation and properties, by M. A. Jorissen.—On semi-invariant functions, by Jacques Deruyts.

AMSTERDAM.

Royal Academy of Sciences, April 25.—Prof. van de Sande Bakhuysen in the chair.—M. T. Forster read a paper on the influence of smoking on tuberculous matter. He had formerly shown that tuberculous matter does not cease to be infectious after salting. Experiments subsequently made in his laboratory prove that salting *and* smoking do not kill the bacteria of tuberculosis. Not only tuberculous matter, but meat from tuberculous cattle is very infectious.—Prof. van de Sande Bakhuysen communicated an abstract of a paper published by him on an instrument for the determination of the absolute personal error in astronomical transit-observations.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Light, Heat, and Sound: C. H. Draper (Blackie).—Leçons sur l'Electricité, tome i.: E. Gerard (Paris, Gauthier-Villars).—A Hand-book of Descriptive and Practical Astronomy, vol. 2, 4th edition: G. F. Chambers (Oxford, Clarendon Press).—Official Year-Book of the Scientific and Learned Societies of Great Britain and Ireland (Griffin).)—Electrical Influence Machines: J. Gray (Whittaker).—Electric Transmission of Energy, 2nd edition: G. Kapp (Whittaker).—Zoologische Ergebnisse einer reise in Niederländisch Ost-Indien, Erstes Heft: Dr. Max Weber (Leiden, Brill).—Memoirs of the Geological Survey of India—Palæontologia Indica; ser. xiii., Salt Range Fossils; vol. iv. Part 1, Geological Results: W. Waagen (Trübner).—The Criminal: H. Ellis (W. Scott).—Notes on the Pearl and Chank Fisheries and Marine Fauna of the Gulf of Manaar: E. Thurston (Madras).—Food Adulterations: A. J. Wedderburn (Washington).—The Beginnings of American Nationality: A. W. Small (Baltimore).—Journal of the Royal Statistical Society, March (Stanford).—Traité Encyc. de Photographie, 15 April (Paris, Gauthier-Villars).—Journal of the Anthropological Institute, vol. xix. No. 3 (Trübner).—Brain, Part 49 (Macmillan).—Mass. Institute of Technology, Boston, Annual Cat., 1889–90 (Camb., Mass.).—Journal of the Royal Microscopical Society, April (Williams and Norgate).—Bulletin of the American Geological Society, vol. 21, Supplement 89, vol. 22, No. 1 (New York).—Missouri Agricultural College Experiment Station Bulletin, Nos. 6 and 9 (Columbia, Miss.).

CONTENTS.

PAGE