THURSDAY, OCTOBER 27, 1898.

THE FIRST VOLUME OF HUXLEY'S MEMOIRS.

The Scientific Memoirs of Thomas Henry Huxley. Edited by Profs. M. Foster, Sec.R.S., and E. Ray Lankester, F.R.S. Vol. i. Pp. xv + 606. (London: Macmillan and Co., Ltd., 1898.)

THE editor, whose commands it has long ceased to be possible for an old contributor to gainsay, has desired me to write some notice of the first of these volumes. That his choice should have fallen upon a botanist is perhaps singular: for though there was no branch of biological science to which Huxley was not sympathetic, the bulk of his work is entirely beyond my powers of criticism. Other hands, I understand, will do justice to it as the successive volumes appear.

My task, at any rate, is merely introductory. And in that sense I gladly undertake it. For the appearance of this stately volume is to me a matter of peculiar satisfaction. I think it cannot be doubted that Huxley stood in the public eye as something other than a great man of science. The outside world saw that he had the scientific world at his back when it made him first Secretary and then President of the Royal Society. But why it was so, it may be confidently stated that the vast majority of persons had not the vaguest idea. They knew that he had a great literary gift: "at least," said Mr. Balfour at the Memorial meeting, "he will go down to posterity as a great master of English prose"; they knew that he had a singularly lucid and impressive power of oral exposition; they saw that he spent no small part of his life and of his strength in public work and the service of the State: most marvelled at the dexterity with which he wielded the perilous weapon of controversy; a necessarily smaller number delighted in the charm with which he played the part of the brilliant man of society; and perhaps some, fewer still, recognised his place amongst the great thinkers of his time.

The splendid gifts which led to success in so many and such varied fields threw the real Huxley which science will hand down to posterity somewhat in the background. I was one of those who were extremely anxious that this side of him should be brought into due prominence by the collection of his scientific work. The project was beset with many difficulties, and it would never probably have been achieved but for the chivalrous loyalty with which the publishers of this journal came to the rescue.

I have stated one reason why, personally, I desired it done. From the point of view of establishing Huxley's place in scientific history, it will be no unworthy *apologia pro vitâ suâ.* But there are others about which a few words may be said.

Not long ago Mr. Lionel Tollemache quoted Mr. Gladstone as saying that while he allowed genius to Romanes he could not concede it to Huxley. The dictum is of no critical or, indeed, of any other value, except as giving an insight into Mr. Gladstone's own ways of thought. For what do we mean by genius? I take it that it is the power of seeing further into the nature of things than is possible with the ordinary insight possessed at the time by a man's contemporaries. Genius, then, is essentially prophetic. And being so, the validity of its utterances can only be judged by posterity. When one walks in a wood, how can one judge the relative height of the trees; viewed from a distance it jumps to the eyes. For my part, then, I regard it as at once polite and politic to allow genius to all my friends.

But the juxtaposition of Romanes with Huxley suggests some interesting considerations of quite another kind. I knew both pretty intimately; both are dead, and I would not utter a word of criticism which would be unkind to the memory of either. Romanes was peculiarly interesting to talk to; his writing gave me less satisfaction. The bent of his mind was essentially deductive; his mental processes pursued an abstract course aloof from facts, and if he ever descended to them, it was from a sort of condescension to the weaker brethren amongst us. When he arrived at a conclusion, he looked about for facts to verify it. The method was quite logical and correct. Only unfortunately, in common with others who have followed the same line, he never really grasped the fact that biological science is very far indeed from admitting at present of deductive treatment at all.

Huxley, on the other hand, was supremely objective. Animated throughout his life by the most intense "curiosity" in the higher sense, the establishment of accurate observations was a positive passion with him. If facts came into collision with theory, with Romanes it was so much the worse for the facts; with Huxley, so much the worse for the theory. Even I, in turning over the pages of this handsome volume, can trace the dissipation of the mists of hazy transcendentalism in the middle third of the century as Huxley's ardent sun rose stronger and stronger above the horizon. I suppose, but I speak with all diffidence in such a matter, that it was in its full fervour when he wrote the classical paper with which this volume concludes, "On the theory of the vertebrate skull." I myself was too early to come under Huxley's influence in this direction, but I can yet remember the dreary Okenism with which the Comparative Anatomy Lecture-room was pervaded before Huxley's teaching had sunk to the level of the schools.

But the insatiate pursuit of fact, by which I mean the achievement of accurate objective knowledge without prepossession of any kind, was not Huxley's only scientific characteristic. It was accompanied by extraordinary powers of generalisation. He was not a mere compiler of observations. Sparing no pains to see the phenomena accurately, he was equally keen to make them tell their hidden story. Perhaps sometimes he was too keen; but if the story, as Huxley read it, would not always bear subsequent examination, at any rate the original documents on which it was based were always available to test it by.

But there is a curious fascination in turning over the collected work of a man such as Huxley, and tracing the mental paths by which his own ideas shaped themselves. It is not the habit now to study anything but the last and most fashionable text-book. Yet I am persuaded that any biologist who wishes to cultivate accurate habits of thought might profit exceedingly by a careful study of these pages. The method of research of a great master of the art is laid bare for us; and the acquisition of a right method is a greater thing than a mere knowledge of the results: πλέον ἥμισυ παντός.

Take as an illustration the interesting indications of the way in which Huxley's mind was feeling its way towards a grasp of evolution. The comparison of the results of philology and embryology in the lecture "On the common plan of animal forms" is curiously suggestive (p. 283). It throws light on what some of us thought a hard saying in his last (as I suppose) public speech made at Oxford, when he said that whether the Darwinian theory remained or fell, the fact of evolution would survive.

It has been said that Huxley made a "stalking-horse" of Darwin, and there is just the amount of truth in this as in every jest. It is evident that Huxley's morphological studies had brought him to the precise point where the "Origin of species" gave him the illumination of which he stood in need. And he seized it with characteristic ardour and enthusiasm. In the case of the cell-theory his mind was not so receptive because not so prepared. "Its value," he says, "is purely anatomical" (p. 220). He could not foresee, and perhaps would not have been justified in foreseeing, that it would supply the future key of our physiology.

And here I must acquit myself of the task which I have reluctantly undertaken. To do any adequate justice to the wealth of accomplished work included in this volume alone is, as I began by saying, wholly beyond my powers. But no intelligent student can turn over these records of Huxley's work without realising the truth of the remark of the editors, that "the progress of biology during the present century was largely due to labours of his of which the public knew nothing." And whatever else such a student may take away from their study, he cannot at least fail to learn how to treat of the most technical matters with the extremity of pregnant and lucid expression. W. T. THISELTON-DYER.

THE SCIENCE OF APPLIED ELECTRICITY.

Magnets and Electric Currents. By Prof. J. A. Fleming. Pp. xv + 408. (London: E. and F. N. Spon, Ltd., 1898.)

THIS work, as Prof. Fleming explains in his preface, has grown out of, and may be considered as taking the place of, his well-known smaller work, "Short Lectures to Electrical Artisans," published about twelve years ago.

"In recasting the information in such a manner as to conform more nearly to the present state of knowledge the author still desired to fulfil the original aim of supplying electrical artisans and engineering students with a brief and elementary account of the scientific principles underlying modern applications of electricity in engineering. With this object in view the use of mathematical symbols has as far as possible been avoided, but at the same time an endeavour has been made to give the reader clear notions on the quantitative measurements which lie at the root of all application of electrical facts in the arts."

This endeavour is more than justified by the present admirable volume.

After two introductory chapters, one on magnets and magnetism describing the simpler properties of permanent and electro magnets, and the other on measurement and units in which the bases of physical knowledge and the principles of "absolute" measurement are explained, the quantitative connection between currents and their magnetic effects is discussed under the heading "Magnetic force and magnetic flux." This discussion might equally well have been entitled "the magnetic circuit," as it virtually amounts to an explanation of that useful conception; and it is appropriately followed by a comparison of the present system of measurement with the "rational" system suggested many years ago by Mr. Oliver Heaviside, the advantages of which are particularly striking in magnetic circuit problems. Chapters iv. to vii. deal with electric currents and the theory of their measurement, electromagnetic induction, and electromagnets, with a discussion under the last head of magnetic curves, hysteresis, and the molecular theory of magnetism; chapters viii. and ix. are on the theory of alternating currents and on measuring instruments respectively; and chapter x., a longer one than the rest, is devoted to the various methods of generating currents. The book concludes with an appendix on the measurement of the earth's horizontal magnetic field strength, a table of natural sines, cosines, and tangents, and an index.

From what has been said, it is plain that Dr. Fleming's work is far more than a mere enlarged edition of the "Lectures to Electrical Artisans." It may be best described as a clear and brief—sometimes, we are tempted to think, almost too brief—but always admirably clear account of those parts of electrical theory which should be grasped by the better class of junior student of practical electricity. Such an account has, we venture to think, long been needed. Valuable as are descriptions of such things as Coulomb's balances and Wimshurst machines in the ordinary text-book, the importance of early guiding the thoughts of the youthful electrician into the channels which lead most directly to the regions of his subsequent activity cannot be too strongly emphasised. Life is too short, for all but the very gifted men, to do more than make a distant acquaintance with what, from the electrician's point of view, are the ornamental parts of his science; and it is largely because Dr. Fleming recognises the truth of this, that his book cannot fail to be of very great value to both teachers and students of electrical technology. A. P. C.

OUR BOOK SHELF.

Natural Hygiene or Healthy Blood, the Essential Condition of Good Health and how to attain it. By H. Lahmann, M.D. Translated from the German by Dr. H. Buttner. Pp. v + 253; plates 5. (London: Swan Sonnenschein and Co., Ltd., 1898.)

THE book before us is a learned exposition which aims at two very laudable objects—the reform of clothing and diet, and the banishment of disease. With regard to clothing little is said: the author's children are represented in a state of nudity; this, together with the prescription of constant air baths, and declamations against the amount of clothing worn by man at the present day, makes one think that in his heart of hearts the author regards the entire disuse of all clothing as the beau ideal.

With regard to diet and disease much is said, and much that is both interesting and instructive. For instance, we are informed that although bacilli may occasion disease, they only play a subordinate part. The essential cause of all disease is "dysæmia," or a deficiency or wrong proportion of the "vitalised" mineral constituents in the blood. All dysæmia is dietetic, and arises from too much water (the author, by the way, seems to have an objection to water baths: air baths are the things to have), too much common salt, or the too limited consumption of uncooked fruit and vegetables. Mankind in general, except the author, his children and the inmates of his sanatorium, appear to be suffering from this "dietetic dysæmia," and will be a ready prey to the first bacillus that settles on them. The whole book is unfortunately pervaded by the spirit of fanaticism, but nevertheless is well worth reading. Although doubtless the importance of the so-called inorganic salts in their combination with organic substances is greatly exaggerated, still the physiological chemistry of the subject is well considered, and the author has spared no pains to collect the results of erudite researches which tend to support his theory. His remarks upon the cooking of vegetables are well worthy of attention, especially in this country. F. W. T.

Applied Geology. By J. V. Elsden, B.Sc. (Lond.). Part I. Pp. vii + 96. (London: "The Quarry" Publishing Co., Ltd., 1898.)

THE author of this work states in his preface that circumstances have made it necessary to publish the earlier chapters separately, and that, therefore, these chapters scarcely give an adequate idea of the scope of the completed work.

The part thus published contains little but what can be found, often in much more detail, in such well-known books as the work on field geology by Sir A. Geikie, Mr. W. H. Penning's "Field Geology" and his "Engineering Geology," and the "Treatise on Ore Deposits" by J. A. Phillips and Prof. Louis.

The first chapter deals chiefly with geological surveying, but far too briefly to be of much use. Outcrops are then illustrated by figures resembling those of Sopwith's geological models.

The rule given on p. 14 for ascertaining the thickness of beds by multiplying the breadth of the outcrop, in a direction at right angles to the strike, by the sine of the angle of true dip, should be supplemented by the proviso "having, in case the surface is not horizontal, first reduced the observed outcrop to that which would be observed if the surface were horizontal."

The second chapter is devoted to problems relating to dip and strike, the method of solving which, both by trigonometry and by construction, is clearly explained. In the third chapter unconformity, overlap, curved strata and normal faults are defined and illustrated. In the fourth chapter problems relating to faults are dealt with in a similar method to that made use of in the chapter on dip and strike.

The fifth and last chapter of the part published describes, in the space of twenty pages, stratified ore deposits of gold, platinum, tin, iron, manganese, aluminium, copper, &c., at various typical localities.

Taking the volume as a whole, it is obvious from the small number of pages devoted to so great a variety of subjects that some matters are inadequately dealt with. On the other hand the book is well illustrated by fifty-seven figures, the explanations are clear, and the work is calculated to be of considerable practical use, more particularly in the case of dip, strike and fault problems.

An ideal work on applied geology should, in addition to taking hypothetical cases, discuss, as far as possible, problems in mining, tunnelling, water supply, &c., which have been actually met with, and should be illustrated by concrete examples from definite localities in which the theory of the geologist has been tested by the execution of the engineering work. May we hope that we shall not have long to wait for such a work?

Flora of the County Donegal. By Henry Chichester Hart. Pp. xxiv + 392; with a map. (Dublin: Sealy, Bryers, and Walker, 1898.)

THE publication of a flora of one of the dampest parts of our islands—one of the most uniformly peat-buried, and one of the hitherto least worked—is pleasing; and the pains evidently bestowed on this book make it welcome. Less than one half of the "Flora" is taken up by the enumeration of the phanerogams, ferns and *Characeæ* of Donegal; of the rest, over sixty pages are occupied by a long report on the climate, and one hundred by a discussion of the distribution in Ireland and Great Britain of the plants of the county. New observations on the altitudinal range of plants, and new statements of their times of flowering are things pleasant to see: from the latter, it appears that the "perpetually recurring storms" and the "deficient summer heat" retard the vegetation, so that blossoms appear even later than in the East Highlands. Mr. Hart does not call attention to this; it is a point deserving inquiry. Too long have authors of works such as this been content to copy or to make approximations at dates of flowering. In discussing the vegetation, the lines laid down long ago by H. C. Watson are carefully followed. As a common basis for comparison of different floras they are valuable; but one can only wish that the splendid chance which so uniform a vegetation offers had led to a consideration of vegetative formations—a subject only just touched upon. This discussion of the vegetation contains several suggestive observations, of which by no means the least in interest is that on the poverty of Donegal in *Cruciferæ*, *Leguminosæ*, *Umbelliferæ*, *Compositæ* and *Orchidaceæ*: of the last order, *Orchis maculata*, we are told, alone is able to live on the outlying islets; yet these plants, with their tuberous roots, might be expected to be able to tide over bad seasons.

It is a pity that the old error of calling *Neottia* a parasite should appear here; but such errors are rare; and the book, if not strikingly original, will at any rate be serviceable to all who find an interest in the botany of North-west Ireland. I. H. B.

The Reliquary and Illustrated Archæologist. Edited by J. Romilly Allen. New Series. Vol. iv. Pp. 288. (London: Bemrose and Sons, Ltd., 1898.)

THIS attractively produced quarterly review of archæology is "devoted to the study of the early Pagan and Christian antiquities of Great Britain; mediæval architecture and ecclesiology; the development of the arts and industries of man in the past ages; and the survivals of ancient usages and appliances in the present." The volume now before us, containing the numbers published this year, is well up to the high standard of its forerunners. The articles will interest students of the archæology of Great Britain; and they are so well illustrated that all who are interested in antiquities may derive pleasure from reading them. Many of the articles are noteworthy. Mr. Leader Scott describes a Gallic necropolis discovered in Italy, on a tract of land at the foot of an indentation of Mount Montefortino, near Arcevia (Ancona). In addition to the archæological aspects, the necropolis affords an interesting study from an ethnological point of view. Mr. Henry Balfour contributes a short paper on the modern use of bone skates and sledges with bone runners. The editor writes on primitive anchors, pot-cranes and their adjustments, and other subjects; Mr. R. A. Gatty describes the objects found in the Barrow at How Tallon; Mr. H. Ling Roth contributes a paper on Benin art, and there are numerous notes on archæology and kindred subjects.

LETTERS TO THE EDITOR

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Asymmetry and Vitalism.

IN your issue of September 22, Prof. Pearson, referring to the views expounded by Prof. Japp in his interesting address on "Stereochemistry and Vitalism," shows that, if chance be the only factor at work in the replacement of asymmetrical groups in symmetrical molecules, the production in nature of an excess, however small, of compounds of one-sided asymmetry must undoubtedly have taken place. But, ignoring the mechanical interpretation of the phenomenon (thus avoiding the stumbling-block hinted at by Prof. Pearson), and taking, according to present experience, for granted that, in the artificial introduction of asymmetry into a symmetrical compound, equal amounts of two inversely-active bodies are formed, so as to give rise to an optically inactive mixture or compound (in a way recalling to mind the separation of equal and corresponding amounts of positive and negative electricity), other objections may, in my opinion, be brought against Prof. Japp's views.

The point at issue is this: out of inactive material, vegetal and animal organisms are building up substances with asymmetrical molecules, and optically-active, such as albumins and carbohydrates. In which fact, joined with the chemists' then ascertained inability to prepare artificial active compounds from inactive substances, Pasteur saw an essential difference between the forces that are acting in living nature and such as are coming into play in our laboratories; he called, accordingly, the former asymmetrical, the latter symmetrical forces. This alleged barrier fell to the ground after the successful preparation, by Perkin and Duppa and by Jungfleisch, of racemic acid from succinic acid, and the separation, by means of a simple crystallising process, of sodium ammonium racemate into dextro- and lævo-tartrate, differing by their inverse hemihedral faces, and mechanically separable from one another. Being aware that the spontaneous separation of racemic acid into its two active forms afforded a strong argument against his theory, Pasteur uttered the belief that, even in that phenomenon, some asymmetrical outward agent, such as the organic germs contained in the atmosphere, might be the separating cause; but that hypothesis, inadequately supported by Joubert and Bichat with the doubtful evidence afforded by their experiments, cannot hold its ground against the facts discovered by Scacchi and Wyrouboff, and especially by Van 't Hoff and Deventer, respecting the so-called "transition-point" of some double salts, a class of compounds among which the racemates are but a particular case.

On Prof. Japp's view the asymmetrical forces are brought into play in another way and at another moment than on Pasteur's. He contends that, while simple asymmetry (exemplified by dextrotartaric or lævotartaric acid) is caused by asymmetrical actions, double asymmetry, as displayed by racemic acid, is caused by symmetrical actions: no asymmetry comes into play in the latter case, not even when the racemate is separating into its two enantiomorphs, as for every right-handed crystal a corresponding left-handed one is formed. But here is the point. When "the two kinds of crystals are to be picked out, and placed each in a vessel by itself," the intervention of an intelligent force, the intelligent and living (whether mediate or immediate) act of man is needed, as, both kinds having the same solubility, specific gravity, melting point, &c., behave in the same way towards all the separating symmetrical and non-living agents we dispose of in our laboratories. The conscious separation, carried out by man, may be compared with the unconscious one caused by bacteria and moulds, which agents are also able to destroy one kind rather than the other: the common side of both actions is that they are brought about by living organisms, formed of asymmetrical material, and therefore able to act asymmetrically.

Now, granting that, according to Prof. Japp's interpretation of facts, intervention of life cannot be dispensed with in the above separation, I believe that, supposing no substance endowed with molecular asymmetry to exist on our planet, it would be, not merely conceivable, but actually possible to produce as much simple asymmetry as might be desired, by means of an amount of one racemic compound (such as some racemate) liable to separation into active kinds, by the crystallising process, without any interfering asymmetrical force. In point of fact, after the spontaneous separation (the suitable temperature being granted) into the enantiomorphous crystals, we may always imagine a force, neither intelligent nor living, and acting in a symmetrical way, that would by chance single out *one* crystal: from that single, asymmetrical crystal (whether right- or left-handed), as was shown among other similar instances by Fischer and Wallach, other compounds can, on introducing asymmetrical groups, be prepared, displaying (without any previous separation into enantiomorphs) *simple* asymmetry. For, while a racemic compound always comes into existence when we start the synthetic process with a symmetrical and therefore inactive substance, such is not the case when we are operating on active, already asymmetrical compounds, as one active kind rather than its enantiomorph (with respect to the newly-introduced group) may be formed, the other one being partially or totally excluded. The pre-existing asymmetry has a directing influence upon the newly added atoms: asymmetry begets asymmetry, as life begets life. This argument does not only fit the hypothesis that a single crystal be selected: provided that the supposed force act for so short a time as to allow but a small part of the crystals to be removed, there is some chance for there being an excess, however small, of either one or the other enantiomorph to which the above remarks may as well apply.

The following illustration may perhaps convey a clearer idea of the fact stated. Supposing molecular asymmetry to have come on to our planet from outward space (an origin ascribed by some to life), let us imagine one primordial racemic compound to have spontaneously separated into its two enantiomorphs, and these to have been whirled round and scattered about vacant space by some vortex, so as to allow one simply asymmetrical particle to reach our globe. This may, without the intervention of any peculiar force differing at all from such as are acting in chemical synthesis, have originated all the now existent asymmetrical compounds. Some other planet might nevertheless have been reached by a particle of the other enantiomorph; the ensuing molecular asymmetry would accordingly have been the perfect reverse of ours: that celestial body might be inhabited by living creatures akin to ourselves, but built up of dextrogyrous albumins; its vine-grapes would yield *l*-glucose instead of *d*-glucose, &c. I do not mean to contend that there is any probability of such events having taken place, and am only pointing out that such an hypothesis is in no way absurd or inconceivable. Nay, it might even be enlarged. Although unlikely, a universe (in which our planet might well be included) can be imagined, being formed by pairs of celestial bodies endowed with equal and inverse asymmetry, so as to be comparable with a set of enantiomorphous crystals, into which a mixture of racemous compounds would separate. It matters little whether the enantiomorphs be near one another, as in the case of a crystallising solution, or as wide apart as the celestial bodies we are considering: there is in both cases *in a determinate point of space* one kind of simple asymmetry (the other one being excluded), a result attained without any absolutely asymmetrical action, and especially life, coming into play.

That the way followed by living organisms in their preparation of active substances, differs from the processes carried on in laboratories, is quite another question: the capital point is that, in one way as in the other, the final result is the same, and that the formation of the first asymmetrical group is not necessarily connected with that of the first living particle, as Prof. Japp contends. In my opinion, the problem of spontaneous generation is not likely to be ever reduced to the far simpler question of the origin of molecular asymmetry.

Turin, October. GIORGIO ERRERA.

I WILL endeavour to reply to the various criticisms which have appeared in NATURE on my address to the Chemical Section of the British Association.

Prof. Karl Pearson points out—what was, of course, obvious—that if only a small number of asymmetric molecules—say twenty—were to be formed under the influence of symmetric forces, there might be a preponderance of either right- or left-handed enantiomorphs, or even that all might be of one kind. He then goes on to suggest that such asymmetric compounds might have been spontaneously formed in the past, and might "be endowed with a power of selecting their own asymmetry from other racemoid compounds," and might thus act as "breeders."

This is a view which, as I have found in private discussion, is held by several organic chemists. My reason for rejecting it is that it attributes to the "breeding" process (to employ Prof. Pearson's concise, but, as we shall see, not altogether accurate expression) an efficiency which experiment does not justify. I will explain this important point in detail, as those who appeal to the "breeding" process seem to me to do so in a somewhat vague and elastic way.

This influence of already existing asymmetric molecules, or of asymmetric groups within the molecule, manifests itself in two ways, which I will distinguish as *asymmetric induction* and *asymmetric selection*.

(1) *Asymmetric Induction*.—If we introduce into an asymmetric molecule a fresh asymmetric carbon atom, or if we render asymmetric a carbon atom which was not previously so, the asymmetry already present will influence the character of the new asymmetry, and of the two possible arrangements of the new asymmetric carbon atom, one will predominate, or may even be the sole form. This influence, however, is entirely *intra*molecular; all attempts to convert asymmetric induction into an *inter*molecular action have failed. Thus various attempts have been made to obtain an optically active substance by allowing a reaction which under symmetric conditions would yield a racemoid mixture, to proceed in a solution containing another optically active substance; but this dissolved substance was invariably found to be without influence on the course of the reaction, and the resulting product was optically inactive. This influence, therefore, so far as experiment goes, does not extend from molecule to molecule, although within the molecule it is very powerful.

If the protoplasmic theory of vital synthesis is correct, according to which the molecules of carbon dioxide and other non-living molecules first combine with the living protoplasm and are afterwards eliminated in the form of asymmetric compounds, this asymmetric induction probably determines the asymmetry of the resulting compounds. But even supposing living protoplasm to consist of molecules—of which we have no proof—such molecules exercise their peculiar synthetic functions only under the influence of life, and are, therefore, useless as "breeders" for the purposes of Prof. Pearson's argument. Prof. Pearson's twenty non-living asymmetric molecules, formed by the chance play of mechanical forces, would, so far as experiment informs us—although I freely admit that mere negative results are not conclusive—have no more influence on the asymmetry of other molecules formed in their neighbourhood than one toss of a coin has upon another toss.

(2) *Asymmetric Selection*.—This is of two kinds. The first is that discovered by Pasteur, in which the different degree of affinity of one asymmetric base for two enantiomorphous acids (or of one asymmetric acid for two enantiomorphous bases) comes into play, and a separation may be effected, depending on the different solubilities of the resulting salts. But how this process would be available for Prof. Pearson's purpose, one hardly sees. In the most favourable case, his twenty asymmetric molecules would combine with a limited number—twenty, or some simple multiple or sub-multiple of twenty—of molecules from some racemoid mixture that happened to be present, and there would be an end of their action. There is no question of "breeding" here. Their number would not be increased by the process.

The other kind of asymmetric selection, which is a modification of that described by Pasteur, was discovered by Kipping and Pope. It depends on the fact that certain asymmetric substances, when in solution, alter in a different degree the solubility of two enantiomorphs, without actually, as in the previous case, entering into definite chemical combination with them. In this way a partial separation of enantiomorphs may sometimes be effected. But the applications of the method are very limited. Thus Kipping and Pope found that whilst, by means of a concentrated solution of glucose, they could effect a partial separation of sodium ammonium dextro- and laevo-tartrates—substances which spontaneously crystallise separately (*i.e.* not in racemic combination) at ordinary temperatures—in the case of mandelic acid, which is racemic at ordinary temperatures, no separation was effected, the tendency to form a solid racemoid overcoming any tendency to separation due to the presence of the glucose. Moreover, this action has never been observed, except with concentrated solutions of the selective substance; and it is, therefore, quite impossible that Prof. Pearson's twenty molecules—doubtless in a state of unlimited dilution—could in any way influence the solubility of other substances present.

My contention with regard to this "breeding" question, so far as non-living matter is concerned, therefore is: Asymmetric induction "breeds" only within the molecule, and without thereby adding to the number of molecules; asymmetric selection does not "breed" at all.

In fact, I do not see what Prof. Pearson is to do with his twenty molecules when he has got them. They will not "breed" in the sense he contemplates. On the other hand, if the process which produced them should go further, so as to yield a sensible quantity of substance, both enantiomorphs must be formed; and as the chances are equal in favour of the two asymmetric events; as, moreover, the occurrence of either event does not influence that of the other; and as the number of molecules in a sensible quantity is very great, Le Bel's ratio,

$$\frac{\text{Number of occurrences of event I.}}{\text{Number of occurrences of event II.}}$$

will not differ sensibly from unity.

Several of my critics seem to think that a mere sensible preponderance of one enantiomorph is sufficient. This is not the case unless the minority can be "bred" out of existence; and I do not think that under symmetrical conditions this is possible. We must bear in mind that, in the case of at least 99 per cent. of those optically active compounds which are products of the living organism, only one enantiomorph is found. It is the total disappearance of the opposite form which we have to explain.

Prof. Pearson, referring to the hypothesis of the asymmetric carbon atom, says: "Such a *geometrical* hypothesis cannot give the *dynamical* explanation of rotatory polarisation required by the physicist."[1] Every chemist, of course, fully recognises this; and in addressing an audience of chemists, I did not think it necessary to introduce so obvious a qualification of my statements. In the present undeveloped state of stereochemistry we are compelled provisionally to treat, as statical, problems which are in reality dynamical. The atoms are considered as being at rest in the positions of equilibrium about which they actually oscillate or revolve. Or, as Van 't Hoff puts it, the problems of stereochemistry are tacitly treated in the form in which they might be conceived to present themselves at the absolute zero of temperature.

Prof. Fitzgerald makes two suggestions, either of which, he considers, would dispose of my contention that single asymmetric forms cannot arise under chance conditions. In the first of these he supposes a mixture of two enantiomorphs to separate spontaneously into its right- and left-handed crystalline forms. If life then started from a few such centres, there would probably be a preponderance of one or the other form; "if it started from a single centre, it *must* have been either right- or left-handed."

In reply I would point out that this spontaneous separation of enantiomorphs is confined to crystalline substances; and I should have thought it fairly obvious that crystalline substances cannot possibly form the organic structural material of living organisms. Can Prof. Fitzgerald imagine crystallised protoplasm?

Prof. Fitzgerald's second suggestion is that life "probably started either in the northern or in the southern hemisphere, and in either case the rotation of the sun in the heavens may be a sufficient cause for a right- or left-handed structure in an organism growing under its influence."

In attributing the origin of the molecular asymmetry of compounds produced in the living organism to the apparent diurnal motion of the sun, Prof. Fitzgerald has been anticipated by Pasteur. I had, therefore, carefully considered the question before writing my address. I do not assign any importance to the negative result of the experiment which Pasteur made with the object of detecting such an influence. Indeed, we need not consider Pasteur's experiment at all, inasmuch as nature has been carrying out for us on this very point an experiment of a similar character which has lasted from the first appearance of life on our planet to the present day—and has equally yielded a negative result. For, if this supposed influence were at work, the asymmetric compounds of vegetable origin produced in the northern and southern hemispheres respectively ought to display

1 Prof. Pearson waives any objections to my reasoning "arising from the fact that it is based on a purely geometrical hypothesis as to the constitution of molecules," &c. But even if Prof. Pearson feels inclined to put forward these objections, he will find that I point out, towards the close of my address, that the reasoning is independent of this hypothesis, and that it holds good equally of the hemihedral crystalline forms of these asymmetric compounds about which there is no hypothesis at all.

asymmetry in opposite senses. But nothing of the kind is observed. Cellulose, starch, saccharose, have the same right-handed asymmetry, each in its particular degree, whether the plant that produces them grows north or south of the equator.

Mr. Bartrum suggests that two enantiomorphs may crystallise from their equimolecular mixture with an unequal distribution of the right and left crystals; that then partial re-solution may occur, "roughly on the lines of the distribution of the two varieties of crystals," giving an optically active solution. Unlike Prof. Fitzgerald, Mr. Bartrum does not propose to vivify this crystalline substance offhand; he merely suggests that it may have been "the first ancestor of lævo-rotatory protein."

This is vague. As far as I follow its meaning I should read it: "Leave a soluble, crystallisable, asymmetric organic compound, of suitable character and composition, which has been formed and separated by the chance play of mechanical forces, long enough exposed to the action of other matter under the influence of these forces, and it will, in due course, first turn into protein and then come to life." I do not think that this statement misrepresents Mr. Bartrum's position, and I will leave it to speak for itself.

Mr. Bartrum's process of separation is also open to the objection that it would at best yield only an optically active *mixture*—*i.e.* with a mere preponderance of one enantiomorph; and as I have already pointed out, that is not a solution of the problem.

Mr. Herbert Spencer considers that I have ignored a universal law of "segregation" which he formulated in 1862 in his "First Principles," and which he there referred to three "abstract propositions" now quoted by him. He asserts that this law of segregation would account for the separation of dextro-protein and lævo-protein, if these were once formed; and he instances the formation of hæmatite nodules and flints in chalk-formations as an illustration of the power of segregation in nature.

I think that Mr. Spencer does not quite realise to what extent enantiomorphous molecules are alike. Every symmetric form of energy (such as heat), and every symmetric material agent, is identical in its action upon two enantiomorphs: whatever happens to the one happens to the other. And in none of these facts is there the slightest violation of the law of the conservation of energy—although Mr. Spencer's corollary to his third proposition would suggest the contrary. As regards the separation of enantiomorphs, I do not know whether Mr. Spencer would interpret his third proposition to mean that they must be separable by diffusion; but from the foregoing illustration which he gives of segregation in the inorganic world he would seem to have some such process in his mind. In that case I may point out that, as indeed follows from what I have already said, the rate of diffusion of enantiomorphs is the same; no such separation is possible.

If Mr. Spencer will consider this absolutely identical behaviour of enantiomorphs under all symmetric influences, I think he will perceive that the phenomena of "the formation of hæmatite nodules and flints in chalk-formations, or of siliceous concretions in limestone"—phenomena in which only crystalline or crypto-crystalline compounds of symmetric molecular structure are concerned, and which occur under the influence of symmetric forces—are not comparable with the separation of two enantiomorphous colloids such as dextro-protein and lævo-protein. Short of some asymmetric influence, nothing could separate these; and I am still waiting for my critics to tell me where, prior to the existence of life, such an influence was to be found.

Prof. Errera writes with special knowledge of the subject of molecular asymmetry, and I have nothing to criticise in his statements, so far as they deal with known fact or accepted theory. Some of his suggestions are exceedingly ingenious. I must admit, for example, that a force neither intelligent nor living—a symmetric mechanism—might be conceived which would pick out a *single* crystal from a mixture of crystallised enantiomorphs, and thus yield a single asymmetric compound. This is, then, so far a solution of the problem, although not a solution in the sense which I contemplated, since the mechanism cannot be trusted to effect the separation of the same asymmetric form twice running, whereas the living organism, or the intelligent operator, can do so any number of times. This is the essential difference between symmetric chance and asymmetric life. It is a feat which no mechanism could perform, unless its constructor had first embodied in it the idea of asymmetry, when it would cease to be symmetric, and would be an asymmetric product of living intelligence.

Moreover, as Prof. Errera will perceive from my reply to the arguments of Prof. Fitzgerald and Mr. Bartrum, I do not consider that the separation of enantiomorphous *crystals* brings us much nearer to the spontaneous formation of those *non-crystalline* asymmetric substances that build up the living organism. Prof. Errera, it is true, goes a step further in this direction than my other critics by pointing out that—as indeed I emphasised in my address—the further chemical transformation of such an asymmetric compound by the introduction of new asymmetric groups need not yield more than a single asymmetric compound.

Prof. Errera admits that his suggestions as to the manner in which the separation of enantiomorphs may have occurred before the origin of life—thus, that different asymmetric crystals may have been whirled by a vortex into different planets—are not very probable. In fact, all my critics seem to be moving in that unreal world where a fount of type, if jumbled together sufficiently often, ends by setting up the text of *Hamlet*.

In conclusion, I repeat that it is the impossibility of any mechanical (symmetric) force *constantly* producing the same asymmetric form, or *constantly* selecting the same one of two opposite asymmetric forms—a constancy which is manifest in the same processes when effected by vital agency—to which I referred in my address. I certainly nowhere used the word "constantly"; but the idea is present throughout.

Most of my critics clearly recognise this impossibility, and therefore seek to avoid the difficulty by supposing only a few asymmetric events—or even, only a single asymmetric event—to occur. The desired result having been obtained, the initial process is assumed to stop. But in making this assumption they seem to me to do violence to all probability. Given a practically unlimited period of time, why should a particular set of mechanical conditions, acting by pure chance in a given way, not act over and over again? One can understand a gambler stopping after a run of luck in his favour; but why should a mechanical process do so?

I see no reason to withdraw any of the conclusions at which I arrived, although, had I to write my address over again, there are parts which, to guard against misunderstanding, I might express differently.

I wish to point out that the term "tetartohedral," used in my printed address in describing the asymmetric facets of quartz, is erroneously given in the NATURE report (this vol., p. 454, col. 1) as "tetrahedral." F. R. JAPP.

The University, Aberdeen, October 24.

Potential Matter.

ALLOW me to refer once more to the subject of my letter of August 18, in order to draw attention to two previous investigations with which, at the time of writing, I was unacquainted. Prof. Karl Pearson has, under the title of "Ether Squirts" (*American Journal of Mathematics*, vol. xiii. No. 4), worked out mathematically the theory of matter considered as sources and sinks of fluid, and draws attention to the fact that this theory implies the existence of "negative matter," which may exist outside the solar system. More recently A. Föppl, in a communication to the Munich Academy, dated February 1, 1897 (*Sitzungsber. der k. b. Akad. d. Wiss.*, 1897, i. p. 93), has published a short paper under the title, "Ueber eine mögliche Erweiterung des Newton'schen Gravitations-Gesetzes." Starting from the idea that there is a difference in kind between the electrical and magnetic fields of force on the one hand, and the gravitational field on the other, because the flux of force through a sphere converges towards zero with increasing radius of the sphere for the electric and magnetic fields, but not, as usually defined, for the gravitational field, Föppl gives the necessary extension to Newtonian law of gravitation in order to remove the distinction. This, of course, implies "negative matter." There is a marked difference between the expression for the energy of the gravitational field on Föppl's hypothesis with that which is derived from the ether squirt theory; but it is not necessary to enter into this question.

There are some points in my former communication, to which previous writers on the subject have, however, not, as far as I know, drawn attention. Among them is the insufficiency of the ordinary hypothesis to account for the rotational momentum of our solar system which cannot be self-generated, the possibility of having evidence of anti-matter in comet tails and coronal streamers, and the idea of potential matter.

ARTHUR SCHUSTER.

Solar Radiation.

AT the conclusion of his British Association lecture on Phosphorescence,[1] Mr. Jackson makes a suggestion with regard to solar radiation which will doubtless receive due attention from those who are interested in solar physics. It is one of especial interest to me because, by an entirely different train of thought, Mr. Jackson has arrived at a possible explanation of the relation between sun-spots and terrestrial magnetic disturbances which is practically identical with a suggestion I have recently put forward in a paper on "The cause of the darkness of sun-spots," published in the *Astrophysical Journal* (April 1897).

In this paper I attempted to show that absorption by relatively cool material offers no satisfactory explanation of the darkness of sun-spots, and that the spectroscopic evidence is really quite compatible with a relatively high temperature even in the umbra of a spot.

But in abandoning the absorption hypothesis, one is brought face to face with an apparent contradiction of Kirchhoff's law. Thus it is certain from the low mean density of the sun that the interior region under enormous pressures must be vastly hotter than the photosphere. If, therefore, spots are really breaks in the photospheric clouds through which we obtain a glimpse of the interior, why is it that the radiation from them is apparently so much less intense than from the photosphere? The clouds of condensed matter may, of course, possess a much higher radiating power than the gaseous mass below them; but this, according to Kirchhoff's law, should be entirely compensated by the enormous depth of the feebly radiating interior mass.

To meet this difficulty I suggested that the radiation from the interior, at the transcendent temperatures which must exist even a few thousand miles below the sun's visible surface, may possibly not be apparent as visible light, but may occur in wave-frequencies of a higher order than the known spectrum; and "may be effective in producing those magnetic disturbances which are characteristic of large umbræ."

Mr. Jackson however, if I have rightly understood him, supposes that it is not so much a question of temperature as of molecular structure that determines the wave-frequency of the radiation; and he regards the light of the photospheric clouds as a phosphorescent glow induced by undulations of a high order of frequency which are emitted by the simpler uncondensed materials. The condensed clouds containing more complex molecular groups acting as a screen, and converting the invisible radiant energy of high frequency into ordinary light.

With regard to this interesting speculation, one would like to know more particularly what is the nature of the evidence on which the idea is based that very simple molecular systems give rise to undulations of high frequency? There can scarcely be any analogy between the behaviour of matter in highly exhausted tubes and under the enormous pressures and temperatures which must exist within the photosphere.

The case of the phosphorescent limes is an exceedingly interesting one; but is there any ground for the belief that the lime obtained from organic salts, and giving a blue phosphorescence, is really simpler in molecular structure than a lime which glows red?

J. EVERSHED.

Kenley, Surrey, October 14.

Hibernating Reptilian Embryos.

WILL you allow me space to correct an error that has crept into the account given in the *Christchurch Press*, and reprinted in the last number of NATURE (p. 609), of Prof. Dendy's successful investigation of the development of the egg of the Tuatara lizard, *Sphenodon*.

The fact of an embryo hibernating within the egg was not, as stated, unknown among vertebrates, an exact parallel being offered by no less well-known a reptile than the European pond-tortoise (*Emys orbicularis*). This was first observed in Austria, in the last century, by Marsigli, whose statement has been corroborated by Miram in 1857, eggs laid in his garden at Kieff in May hatching eleven months later, and by Rollinat in 1894, the latter author concluding that hatching does not, as a rule, take place in France before the twenty-second or twenty-third month after oviposition.

I need hardly add how pleased I feel at the result of Prof. Dendy's investigations showing the close resemblance which the development of *Sphenodon* bears to that of the tortoises, since I believe to have been the first systematist to follow Cope (1885) in placing the Rhynchocephalia in close proximity to the Chelonia with the remark: "The affinities of the Rhynchocephalia to the Chelonia are at least as great as to the Lacertilia" (Cat. Chelon., 1889, p. 1).

G. A. BOULENGER.

British Museum (Natural History), October 23.

Organic Variations and their Interpretation.

I SHOULD be glad if Mr. Cunningham would tell us upon what evidence he founds his opinion that, in crabs, "it is certain that the number of ecdyses depend on age, not on size."

This assumption lies at the base of Mr. Cunningham's criticism of Prof. Weldon's arguments; but, even apart from that, the matter is one of such general biological interest that I hope he will respond to an invitation to substantiate a view which to me, at any rate, is altogether novel. I have always understood that exuviation was a phenomenon essentially connected with the process of growth in Crustacea rather than with the mere passage of time, and it is needless for me to remind Mr. Cunningham of the familiar facts and published statements which support this generally accepted view. Will Mr. Cunningham, on the other hand, tell us how many cases of exuviation, unaccompanied by growth, he has observed among Crustacea?

Unless Mr. Cunningham can revolutionise the present state of knowledge on this subject, his criticism, based on the greater relative growth of young crabs in 1893 than in 1895 and 1898 (which in itself is probable enough), falls to the ground; for he admits that "change in the proportions of a crab occurs only at the ecdysis." In assuming that, on the whole, similarity of size in young shore-crabs indicates an equal number of moults, Prof. Weldon appears to me to be quite in accord with our present knowledge of the subject. Certainly—to modify Mr. Cunningham's phrase—the frequency of exuviation in different *Carcini* corresponds much more closely with their relative growth than with the periods of time occupied.

Plymouth, October 22. WALTER GARSTANG.

Wall Mirages.

MR. R. W. WOOD, who describes a mirage on city pavements, in NATURE of October 20 (p. 596), may like to refer to the second volume of NATURE (p. 337, August 25, 1870), where he will find an account of mirages seen by looking closely along a wall, which was exposed to a hot afternoon sun. The mirage must be very common, and needs only looking for. Mr. Wood's interesting letter may lead others to photograph this curious phenomenon in our own country. A wall will be easier to deal with than a pavement.

C. T. WHITMELL.

Leeds, October 22.

A White Sea.

I HAVE received several letters respecting this phenomenon (see p. 496), and have distributed the samples of water to two gentlemen who were desirous of examining it. Will you permit me to say that subsequently I received another application from a bacteriologist on the continent, and that the letter was unfortunately lost before complete perusal. Hence my failure to acknowledge its receipt.

JAMES W. BARRETT.

22 Cavendish Square.

SURFUSION IN METALS AND ALLOYS.[1]

THE author points out that metals and alloys may be maintained in a fluid state at temperatures which are many degrees below their true freezing points, and states that this fact has been but little studied. As regards salts, the question of surfusion has recently received much attention. Ostwald (*Zeit. für Physikal. Chem.*, 1897, vol. xxii. p. 3) has shown, as the result of an investigation

[1] NATURE, October 6, p. 562.

[1] "Surfusion in Metals and Alloys." By Prof. W. C. Roberts-Austen, C.B., F.R.S. (Abstract of a paper read at the Royal Society, May 26.

of great interest, that a very minute quantity of a solid will cause a mass of the same substance to pass from the surfused to the solid state. His work, moreover, has led him to distinguish between the *meta-stable*, or ordinary condition in which surfusion takes place, and the *labile* condition which occurs at temperatures much below the melting point. Ostwald's paper, and one by M. Brillouin (*Ann. de Chim. et de Phys.*, 1898, vol. xiii. p. 264), on the theory of complete and pasty fusion, led the author to offer the Royal Society the results of his own experiments on the surfusion of metals.

Metals do not appear to have been examined from the point of view of surfusion until the year 1880, when some excellent experiments on the surfusion of gold were made by the late Dr. A. D. van Riemsdijk (*Ann. de Chim. et de Phys.*, 1880, vol. xx. p 66), by whose early death, which occurred last year, Holland has lost a skilful physicist. He pointed out that :—

"Faraday, in his memoir on regelation, published in 1858, stated that acetic acid, sulphur, phosphorus, many metals and many solutions, may be cooled below the freezing temperature prior to solidification of the first portions" ("Experimental Researches in Chemistry and Physics," p. 379). On the other hand, in their treatises on physics, Danguin (vol. i., 1855, p. 892) and Jamin (vol. i., 1859, p. 105) mention tin as the only metal which is capable of remaining liquid at a temperature 2·5° below the true solidifying point of the metal.

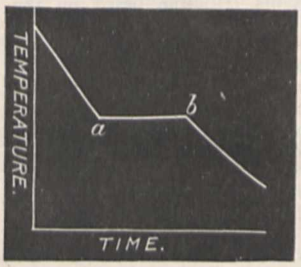


Fig. 1.

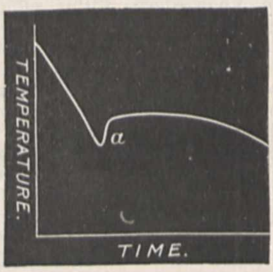


Fig. 2.

Van Riemsdijk's contribution to the subject of surfusion of metals consisted in showing that the well-known phenomenon of *éclair*, the brilliant flash of light which often attends the solidification of the metal in the ordinary assay of gold, is really due to surfusion. He also pointed out that surfusion could be either stimulated or hindered by suitably modifying the conditions, but he made no attempt at thermal measurements. It was not until ten years after van Riemsdijk's work that the recording pyrometer, which the author submitted to the Royal Society in 1891 (*Proc. Roy. Soc.*, 1891, vol. xlix. p. 347), enabled such measurements to be readily effected.

After a brief description of this appliance, the nature of which is now well known, it is stated that the freezing point of a metal, or the initial freezing point of an alloy, may be represented by one or other of three typical curves. Two of these are shown in the accompanying figures, which indicate the nature of the curves, traced by the recording pyrometer. Fig. 1 shows the freezing point curve of a pure metal, the horizontal portion, *a b*, indicating the actual solidification of the mass, the sharpness of the angles at *a* and *b* attesting the purity of the metal. The initial freezing point of most alloys would resemble Fig. 1 in having the corner *a* sharp, while the point *b* is generally rounded off.

The third type of curve, which may be a modification of the other two types, indicates the occurrence of surfusion, the bend at *a*, Fig. 2, showing the amount of surfusion which was observed. The author has detected pronounced cases of surfusion not only in gold, but in copper, bismuth, antimony, lead, and tin. Surfusion, moreover, is not confined to pure metals, and he showed in 1893, that the eutectic alloy in the bismuth-copper series presents a marked case of surfusion. In order to study surfusion, it is necessary to make the galvanometer (to which the thermo-junction is attached) very sensitive. The method of effecting this is described, the thermo-junction itself being in all cases suitably protected and placed in the cooling mass of metal or alloy. A curve, traced by the aid of such a sensitive method, if it represents the surfusion of a metal or an alloy, does not merely show a slight depression as in the case of pure gold shown at *a*, Fig. 2 : the slight depression becomes a deep dip. It is, in fact, possible by the methods described by the author to ascertain what takes place during the surfusion of an alloy, and the results are shown in two plates appended to the paper. From these plates one illustration (Fig. 3) has been selected. It is the autographic representation of the surfusion of an alloy of 64 parts of

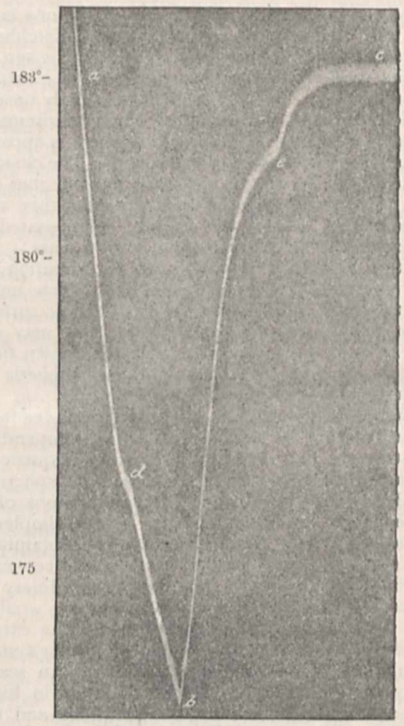


Fig. 3.—64 tin, 36 lead.

tin and 36 parts of lead. The line *a b* represents the surfusion of the mass which, as the scale shows, fell 10 degrees below its true point of solidification before it actually became solid. The solidification of the mass is recorded by the horizontal line *c*. This autographic record also shows that something happened during surfusion, for there are points at *d* and *e*. These proved to be due to the falling out of lead at *d*, and to its having to be remelted at *e*. The entire mass then became solid.

Experiments such as the one described have enabled the author to trace the crossing of solubility curves of certain metals in each other in the same way as had previously been effected in the case of salts by H. le Chatelier and by Dahms.

This crossing of the solubility curve of lead and tin is shown in Fig. 4, but for a description of it reference must be made to the original paper.

The first experimental evidence as to the identity of the behaviour of saline solutions and metallic alloys as regards selective surfusion, has thus been afforded by

Prof. Roberts-Austen. The question is, as he shows, one of much theoretical interest, and should lead to further experiments.

The author then adopts a method previously used by Spring (*Bull.* Acad. Roy. Belg., vol. xxviii., 1894, p. 40). He proceeds, after quoting experiments by Ostwald, Demarçay, Pellat, Colson and Russell, to show that alloys may be formed by the vaporisation of certain metals *in vacuo* at so low a temperature as 50° C. He concludes by pointing out that the results given in the present paper reveal additional points of similarity between the behaviour of alloys and that of ordinary saline solutions. He trusts, therefore, that it may be useful as a continuation of his investigation on the "Diffusion of Metals," which formed the subject of the Bakerian Lecture of 1896.

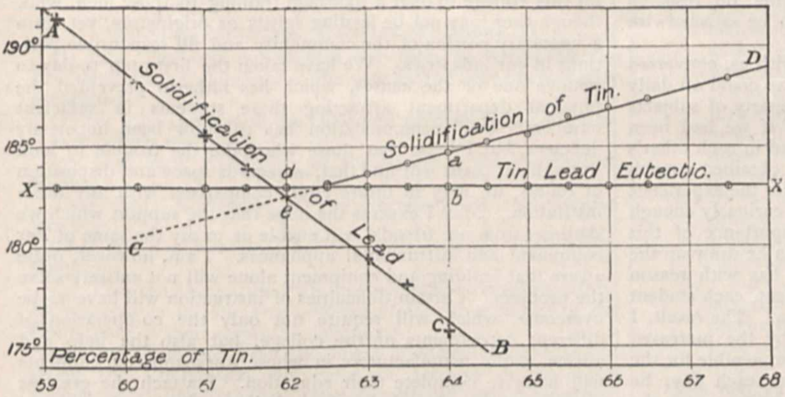
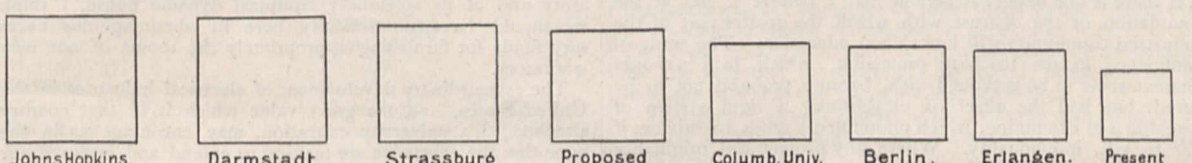


FIG. 4.—Freezing point curves of lead tin alloys.

THE NEW PHYSICAL LABORATORY OF THE OWENS COLLEGE, MANCHESTER.

THE laboratory of which the foundation-stone has been laid, on the twenty-fifth anniversary of the occupation of the present Owens College buildings, will be the largest and most completely equipped in this country. It stands on a separate plot of ground adjoining the Owens College site, and consists of a main building and a large annexe, the latter being more especially intended for electro-technical work. The principal building is 100 feet long and over 60 wide, and consists of a basement and three stories. The diagram gives the comparison as regards dimensions with some of the principal laboratories abroad. The squares represent square area of floor space of the working rooms, *i.e.* all corridors, cloak-rooms, &c., are excluded, and the floor space of the different stories added up. It will be seen that the only laboratories materially larger than the proposed building are those of Baltimore and Darmstadt. But provision has been made for future extension, the plot of ground secured by the College being sufficiently large to double, if necessary, the size of the building.



The cost of the building with fittings and new apparatus is estimated at 30,000*l.* Of this sum 17,000*l.* has been subscribed, in one sum of 10,000*l.*, one of 5000*l.*, and two of 1000*l.* In the plan ample provision for research work has been made. Two large rooms, for instance, are exclusively devoted to spectroscopic work, one of them being arranged to hold a large Rowland grating. It is intended to have at least one room set aside for constant temperature work, and to establish a small plant for the production of low temperatures. An electro-technical laboratory will be added, in which large currents will be available for electric furnaces.

One of the features of the laboratory will be a carefully planned system of ventilation combined with an attempt to exclude dust, as far as possible, from all rooms, and especially from the instrument cases. The plenum system, much used at present, had to be rejected, because it takes up too much valuable basement space, because it is ineffective as regards exclusion of dust, and because the inevitable noise and mechanical shaking due to the fans would have seriously interfered with the work of the laboratory. The architect is Mr. J. W. Beaumont, who, before finally drawing the plans, was sent by the Council of the Owens College to visit the principal modern laboratories of Germany.

In seconding a vote of thanks to Mr. Henry Simon for laying the foundation-stone, Prof. Schuster gave a short description of the building. In the course of his remarks he said :—

In the general plan of the building I have departed considerably from that adopted in some of the recent continental buildings. The designer of a laboratory may take either one or other of two opposite views, according as he wishes to differentiate as much or as little as possible between different rooms and between different classes of students. The present tendency is to adopt the former course, and to draw a rigid line of separation between the rooms set aside for elementary and for advanced work. This system is carried out to such an extreme in one of the most recent and, in some respects, most perfect of German laboratories that a separate division with a staircase of its own is provided for the elementary students, who thus can never be brought into contact with their more fortunate colleagues admitted to the main part of the building.

I have adopted the opposite course, for I consider that a free intercourse between different classes of students is of great benefit and educational value. My object has been to throw the students together and not to separate them, so that the beginner may occasionally see his more advanced colleagues at work, and the latter will have an opportunity to overlook and sometimes to assist their juniors. To some extent, the separation of students is necessitated by the requirements of space and apparatus, but we may do much to minimise instead of exaggerating the division.

Most of the rooms devoted to the highest kind of work are sufficiently large to accommodate several students. In this matter also I have not followed the practice now in fashion, which favours small rooms for single students. It is no doubt very convenient to those engaged in original investigations to have undivided command over a space in which they are absolutely undisturbed, and in which they may leave their apparatus secure against interference. But having regard not only to individual convenience but to the general good of the laboratory, my experience leads me to believe that the advantage lies with the older and less luxurious times, when space was valuable and a number of men were forced to work together.

I remember the old laboratory of the great Helmholtz, in which we were about half a dozen students carrying on research work in a room in which each of us had to be satisfiedwith a table.

The Professor used to spend an hour a day with us, conversed with each about the work he was doing, and we could all daily hear him speak and give his advice about a variety of subjects in a way which would have been impossible if we had been shut up in single rooms. We became interested in each other's work, and thus increased our experience and obtained a much broader view of the range of physics. I consider the experience thus gained to have been quite invaluable, but curiously enough the Professor did not himself realise the importance of this mutual intercourse, and a few years later, when he drew up the plans of a new laboratory, he adopted what has with reason been called the principle of solitary confinement, each student having a separate small room assigned to him. The result, I think, showed that the advantage secured by the increased privacy was too dearly paid for. It became impossible for the Professor to make the round of all the rooms each day, he ceased to exercise the same supervision as before; and the students, left to themselves, soon only looked after their own individual interests and lost touch with their comrades.

A great deal of attention has recently been given to the splendidly equipped laboratories of the German polytechnic schools, and the remarkable development of German industry is not unnaturally ascribed to their influence. But if it be our wish to emulate these laboratories, we should remember that the polytechnic school is only one part of a complete system of education which is not possible to copy here. We should inevitably be led to failure if we tried to solve the educational problem of this country by importing one particular type of institution, without regard to the previous training of the students attending it, and what is more difficult to ascertain, their future career or position in life.

We shall do better if we attack the problem by forming a clear idea as to whom we want to educate, and then doing the best we can with the material at our command.

In the industrial life of a country two distinct classes of men are needed. There are in the first place the leaders, on whom all the burden of further progress will fall. We look to them for future discoveries and inventions, and we must provide them with the proper tools to work their way, and weapons to overcome their obstacles. Though necessarily few in numbers, these men who are specially endowed to serve their country by their intellect and enterprise should receive our first attention. At first sight their education seems easy enough, for what can we do more than lead them up to the highest level of their subject. Yet there is one danger so serious that I believe it lies at the foundation of the distrust with which the greater part of the industrial community still looks upon education. The want of confidence in the teaching profession, which is a national characteristic to be reckoned with, because probably not to be cured, has had the effect of establishing a rigid system of teaching and examining, which undoubtedly tends to subdue, if not to kill, individuality. Where any pronounced originality exists, our whole effort should be to foster and develop it. I feel no doubt that the success of our university will entirely depend on the manner in which we allow room for individuality and originality in our courses, while the continued success of our college must depend on the freedom which we claim for individual teaching, even if in special cases the students should be kept out of the university altogether. Far better that a man of original mind should go through life without a a degree, than that he should artificially be driven into the broad path of common-place reasoning. This country has never been wanting in men of the type I am speaking about; they are not brought up in the polytechnic schools of Germany, and never will be brought up by any schools formed on that pattern. Whatever success Germany has achieved is due to the stringent slavery of its schools, followed and corrected by the absolute freedom of its universities.

I have spoken of two types of students, and the second is no less important than the first. The great majority of men are neither discoverers nor inventors, and they are for that very reason all the more in need of an education which will fit them for their life's work. It is in the instruction of this numerous class of students that we have most to learn, and it is in the intelligent organisation of their teaching that we are behind other countries. It has become a matter of vital importance for this college to offer a thorough training to those men, who, though they may not be leading spirits or originators, yet form a necessary portion of the community and fill responsible positions in our industries. We have taken the first step to-day to remove one of the causes, which has hitherto prevented the physical department attracting these students in sufficient numbers. Our accommodation has till now been hopelessly deficient, but I hope that those who take the trouble to look through our plans will find that, as regards space and disposition of rooms, we may in future court comparison with any other institution. May I express the hope that the support which we shall get from our friends, will enable us to say the same of our equipment and instrumental appliances. I am, however, quite aware that building and equipment alone will not entirely solve the problem. Certain difficulties of instruction will have to be overcome, which will require not only the co-operation of different departments of the college, but also the help and advice of the manufacturers in whose workshops our students will have to complete their education. I attach the greatest importance to this help, and believe that real progress in what may be called the highest branch of technical education can only be secured by a frequent and sympathetic consultation between the teachers and employers of labour.

One further remark I should like to make in order to remove the objection which I know has been urged against our college, that we wish to unite in it students of different classes, and that, as in Germany, the university instruction should be entirely separated from that of the polytechnic school. But the separation of the two kinds of institutions in that country has not been chosen deliberately to secure the best educational result. It has been the consequence of the very high standard of classical education, which the universities require, and which it was not possible to enforce on the technical students. No one can urge that the literary requirements of our college or of our university are such as unjustly to exclude any one who is fitted to receive a higher technical education. If we want to find a country the educational institutions of which have grown unhampered by historical tradition, we must go to the United States of America, and amongst their universities we shall find some whose success we need not be afraid to emulate. I confess that no other institution has ever impressed me so much as regards efficiency in teaching organisation and completeness of laboratory organisation as the Cornell University at Ithaca. The class of students visiting that university are nearly akin to those we wish to attract, and if the citizens of Manchester could see the appliances of the physical laboratory and of its splendidly equipped dynamo house, I think we should have no difficulty here in obtaining the neces sary funds for furnishing appropriately the rooms of our new laboratory.

The extraordinary development of electrical industries in the United States, and the great value which is in that country attached to a university education, may encourage us in the hope that the efforts we are making to extend and improve our electrical teaching will meet with some success.

I hope that the stress I have laid on our intentions regarding electro-technical teaching will not give rise to the impression that we mean to neglect other branches of physics. Our laboratory will provide arrangements for optical and more particularly spectroscopic work, which will at least be equal to that of any other institution; nor shall we forget the necessary machinery to produce very low temperatures by means of the liquefaction of air.

I had some hope originally to add a small astronomical observatory, but although the plans are such that it could be added at any time, the question of expense has for the present prevented us from carrying out a project for which there was no such pressing necessity.

THE INTERNATIONAL CONFERENCE ON SCIENTIFIC LITERATURE.

THE official report of the proceedings of the second International Conference on Scientific Literature, recently held in the rooms of the Society of Antiquaries, the rooms of the Royal Society being under repair, is given below. The names of the delegates who attended the Conference have already been published in NATURE (p. 579).

ACTA.

OPENING MEETING, TUESDAY, OCTOBER 11.

(1) Prof. Darboux moved that Sir John E. Gorst be the President of the Conference. The vote having been unanimously accepted—

(2) Sir John Gorst took the chair and welcomed the delegates. It was then resolved—

(3) That Prof. Armstrong be the Secretary for the English language.

That Prof. Korteweg be the Secretary for the German language.

That M. La Fontaine be the Secretary for the French language.

(4) That the Secretaries, with the help of shorthand reporters, be responsible for the *procès verbal* of the proceedings of the Conference in their respective languages.

(5) Prof. Foster read out the names of delegates appointed to attend the Conference, and gave an account of the correspondence relating to the non-representation of certain countries.

The following resolutions were then agreed to:—

(6) That the ordinary hours of meeting be 11 a.m. to 1 p.m., and 2.30 to 4.30 p.m.

(7) That each delegate shall have a vote in deciding all questions brought before the Conference.

(8) That English, French, and German be the official languages of the Conference, but that it shall be open for any delegate to address the Conference in any other language, provided that he supplies for the *procès verbal* of the Conference a written translation of his remarks into one or other of the official languages.

(9) Prof. Foster having formally presented the Report of the Committee of the Royal Society, copies of which were forwarded, in April last, to the several Governments represented at the Conference, the discussion of the recommendations was opened, and it was resolved—

(10) That the Conference confirms the principle that the Catalogue be published in the double form of cards and book.

(11) That Schedules of Classification shall be authorised for the several branches of science which it is decided to include in the Catalogue.

(12) That geography be defined as limited to mathematical and physical geography, and that political and general geography be excluded.

(13) That anatomy be entered on the list as a separate subject.

(14) That a separate schedule be provided for each of the following branches of science.

Mathematics.	Palæontology.
Astronomy.	Anatomy.
Meteorology.	Zoology.
Physics.	Botany.
Crystallography.	Physiology (including Pharmacology and Experimental Pathology).
Chemistry.	Bacteriology.
Mineralogy.	Psychology.
Geology (including Petrology).	Anthropology.
Geography—Mathematical and Physical.	

(15) That each of the sciences for which a separate schedule is provided shall be indicated by a symbol.

(16) Prof. Foster announced the reception of a letter from the German Chargé d'Affaires to the President of the Royal Society, stating that Geiheimer Regierungs-Rath Professor Dr. Klein, of Goettingen, had been appointed German Delegate to the Conference.

The regulations to be observed in the preparation of cards or slips were then taken into consideration, and it was resolved—

(17) That Italian should be added to the list of languages not requiring translation.

(18) That for each communication to be indexed at least one slip, to be called a *Primary Slip*, shall be prepared, on which shall be either printed or type-written or legibly hand-written in Roman script—

(i.) *Title-entries.*—The author's name and the full title of the communication, in the original language alone if the language be either English, French, German, Italian, or Latin.

In the case of other languages, the title shall be translated into English or such other of the above five languages as may be determined by the Collecting Bureau concerned; but in such case the original title shall be added, either in the original script, or transliterated into Roman script.

The title shall be followed by every necessary reference, including the year of publication, and such other symbols as may be determined. In the case of a separately published book, the place and year of publication, and the number of pages, &c., shall be given.

(ii.) *Subject-entries*, indicating as briefly as possible the principal subjects to which the communication refers. Every effort shall be made to restrict the number of these subject-entries.

Such subject-entries shall be given only in the original language of the communication if this be one of the five previously referred to, but in other cases in English or in such other language as has been used in translating the title.

[The Belgian delegates stated that they abstained from voting on the part of this resolution relating to subject-entries.]

SECOND MEETING, WEDNESDAY, OCTOBER 12.

(19) Prof. Korteweg having expressed the desire to be relieved of his office, it was resolved that Prof. Weiss be appointed Secretary for the German language.

The following resolutions were adopted:—

(20) That the registration symbols used in the Catalogue be based on a convenient combined system of letters, numbers, or other symbols, adapted in the case of each branch of science to its individual needs, and in accordance, as far as possible, with a general system of registration.

(21) That the authoritative decision as to the Schedules be entrusted to an International Committee, to be hereafter nominated by this Conference.

(22) That the Conference is of opinion that the Delegates should be requested to take steps in their respective countries to organise local committees charged with the study of all questions relating to the International Catalogue of Scientific Literature, and to report within six months to the International Committee.

(23) That the International Committee (Resolution 22) be instructed to frame a report, not later than July 31, 1899, which shall be issued by the Royal Society, and incorporated in the decisions of the Conference.

(24) That in all countries in which, or wherever, a Regional Bureau is established, as contemplated in the 16th Resolution of the International Conference of 1896, the Regional Bureau shall be responsible for the preparation (in accordance with Reg. 7 of the Royal Society's Report) of the slips requisite for indexing all the scientific literature of the region, whatever be the language in which that literature may appear.

That each Regional Bureau shall transmit such slips to the Central Bureau as rapidly and as frequently as may be found convenient.

That in the case of countries in which no Regional Bureau is established, the Central Bureau, failing other arrangements, shall, upon special mandate, endeavour to undertake the work of a Regional Bureau.

[The Belgian delegates stated that they abstained from voting on this resolution.]

(25) That the following recommendations of the Royal Society relating to the preparation of the Book Catalogue be referred to the International Committee for their favourable consideration, viz.:—

"At determined regular intervals, not necessarily the same for all sciences, the Central Bureau shall compile from the slips and issue in a book form both an authors' and a subject index of the literature published within that period.

This Book Catalogue shall be obtainable in parts corresponding to the several sciences for which slips are provided, and in such divisions of parts as may be hereafter determined.

In compiling the authors' index, in each of the sciences, the authors' names shall be arranged in alphabetical order, and each name shall be followed by the title of the paper and the necessary reference, and any other such symbols as may be determined.

The Book Subject Catalogue shall be compiled from the slips, as follows:—

(i.) The subject entries shall be grouped in sections corresponding to the registration letters on the slips, *i.e.* to the several sciences.

(ii.) In each science the several subject entries shall be arranged under headings corresponding to the registration numbers on the slips, the which headings and numbers shall be those contained in the authorised schedules of classification.

(iii.) The divisions indicated by registration numbers may be further subdivided by means of significant words or symbols.

(iv.) The nature of the subject entry may vary. Thus, as suggested in the cases of Mathematics and Physiology, it may be the title only; whilst in other sciences a special entry, more or less different from the title, may be provided on each slip. In all cases, the number of subject entries to be copied from a slip shall be determined by the number of registration numbers on the slip.

(v.) The mode of arranging subject entries under a registration number, or under the subdivisions of a number afforded by significant words or symbols, may vary. They may either be arranged in the order of authors' names placed alphabetically, in which case the author's name shall precede the subject entry in the Book Catalogue, or they may be arranged either in an arbitrary order, or in some order suited to the particular series of entries.

When in preparing an issue of the Book Catalogue, it is found that a registration number has no entries collected under it, the number and corresponding heading may be omitted from that issue.

To each part of the Book Catalogue corresponding to an authorised schedule, there shall be appended an alphabetical index of the headings, and if expedient, also of the significant words appearing in that part, showing on which page of the part each may be found.

After the publication of the first issue of the Book Catalogue, the Director of the Central Bureau shall consult the Committees of Referees as to the desirability of making changes in the classification, and shall report thereon to the International Council, who shall have power to authorise such changes to be made as they may think expedient."

(26) That the following recommendations of the Royal Society providing for International Conventions in connection with the Catalogue be adopted:—

"Each region in which a Regional Bureau is established, charged with the duty of preparing and transmitting slips to the Central Bureau for the compilation of the Catalogue, shall be called a 'constituent region.'

In 1905, in 1910, and every tenth year afterwards, an International Convention shall be held in London (in July) to reconsider and, if necessary, revise the regulations for carrying out the work of the Catalogue authorised by the International Convention of 1898.

Such an International Convention shall consist of delegates appointed by the respective Governments to represent the constituent regions, but no region shall be represented by more than three delegates.

The rules of procedure of each International Convention shall be the same as those of the International Convention of 1898.

The decisions of an International Convention shall remain in force until the next Convention meets."

27. That the following recommendations of the Royal Society relating to the constitution of an International Council, which shall be the governing body of the Catalogue, be adopted:—

"Each Regional Bureau shall appoint one person to serve as a member of a body to be called *The International Council.*

The International Council shall, within the regulations laid down by the International Convention, be the Governing Body of the Catalogue.

The International Council shall appoint its own Chairman and Secretary.

It shall meet in London once in three years at least, and at such other times as the Chairman, with the concurrence of five other members, may specially appoint.

It shall, subject to the regulations laid down by the Convention, be the supreme authority for the consideration of and decision concerning all matters belonging to the Central Bureau.

It shall make a report of its doings, and submit a balance sheet, copies of which shall be distributed to the several Regional Bureaux, and published in some recognised periodical or periodicals, in each of the constituent regions."

(28) That the following recommendations of the Royal Society relating to International Committees of Referees be referred for consideration to the International Council when constituted:—

"The International Council shall appoint for each science included in the Catalogue five persons skilled in that science, to form an International Committee of Referees, provided always that the Committees shall be as far as possible representative of the constituent regions. The members shall be appointed in such a way that one retires every year. Occasional vacancies shall be filled up by the Committee itself, subject to the approval of the Chairman of the International Council, and a member thus appointed shall hold office as long as the member whose place he fills would have held office.

It shall be the duty of the Director of the Central Bureau to consult the appropriate Committee or Committees, by correspondence or otherwise, on all questions of classification not provided for by the Catalogue Regulations; or, in cases of doubt, as to the meaning of those Regulations.

In any action touching classification the Director shall be guided by the written decision of a majority of the appropriate Committee, or by a minute if the Committee meets.

Provided always that when any addition to or change of the schedule of classification in any one branch may seem likely to affect the schedule of classification of some other branch or branches, the Committees concerned shall have been consulted; and provided also that in all cases of want of agreement within or between the Committees, or of other difficulty, the matter shall have been referred for decision to the International Council.

All business transacted by the Committees shall be reported by the Director to the International Council at their next ensuing meeting."

THIRD MEETING, THURSDAY, OCTOBER 13.

The following resolutions were adopted:—

(29) That the Committee contemplated in Resolution 21 be constituted as follows:—

Prof. Armstrong.	Prof. Poincaré.
Prof. Descamps.	Prof. Rücker.
Prof. M. Foster.	Prof. Waldeyer.
Dr. S. P. Langley.	Prof. Weiss.

That this Committee be at liberty, if any of those named are unable to serve, to appoint substitutes, and also to co-opt two new members.

(30) That the International Committee be termed the "Provisional International Committee."

(31) That the Provisional International Committee shall be governed by the decisions of the Conference, but shall have the power of introducing such modifications in detail as may appear necessary.

(32) Dr. Adler, referring to Resolution 20, said that he desired to place on record his view that the concluding words—"and in accordance, as far as possible, with a general system of registration"—the addition which he had agreed to as an amendment of his original Resolution, must not be regarded as modifying the first part of the Clause, or as in any way throwing open the whole question of notation and classification.

(33) Prof. Rücker having made a statement as to the probable cost of the undertaking, aud the Delegates having stated what assistance in their opinion might be expected from their respective countries, it was resolved—

That the Delegates to this Conference be requested to obtain information, and to report at an early date to the "Provisional International Committee," as to what assistance, by subscription or otherwise, towards the support of the Central Bureau, may be expected from their respective countries.

(34) M. Mascart called attention to Resolution 22 as being, in his opinion, incorrect in English, the intention being that the

local Committees therein referred to should report to the International Committee.

(35) The Royal Society was requested to undertake the editing, publication, and distribution of a verbatim report of the Proceedings of the Conference.

(36) It was resolved that the *procès verbal* of the Conference be signed by the President and Secretaries.

(37) On the motion of Prof. Armstrong, the thanks of the Conference were accorded to the Society of Antiquaries for the use of their rooms.

(38) On the motion of Prof. Klein, a vote of thanks to Sir John Gorst for presiding over the Conference, and his conduct in the chair, was passed by acclamation.

(39) On the motion of M. Darboux, a vote of thanks was passed to the Royal Society for their work in preparation for the Conference and their cordial reception of the Delegates.

(Signed) JOHN E. GORST, President.
HENRY E. ARMSTRONG, H. LA FONTAINE, E. WEISS, Secretaries.

NOTES.

THE British Institute of Preventive Medicine, which was founded with the view of establishing in this country a national home for bacteriological work in all its branches, has made considerable progress towards the achievement of this aim during the past few years. The bacteriological laboratories are now fully organised, the serum therapeutics laboratory is on a firm footing, whilst the applications of bacteriology to hygiene are finding full recognition. A further addition has just been made to the departments of the Institute in response to the growing demands of the times. A large laboratory at Chelsea has been assigned to investigation and instruction in technical bacteriology. In this laboratory the agriculturist, the chemist, the brewer, and others will find the instruction provided that they individually require for successfully employing the living agents of fermentation. Investigations will also be undertaken, and it is hoped that the laboratory will become a centre of useful work, and promote the advancement of a line of research of the greatest importance to the industries of the country. We have had hitherto to rely upon the research work of foreign laboratories in this direction. The laboratory has been named the Hansen laboratory, in recognition of the pioneer work of the distinguished investigator, and will be under the superintendence of Dr. G. Harris Morris. The formal opening of the British Institute will take place early in the new year, when the public will have an opportunity of inspecting the provisions made for furthering the objects of the Institute. The occasion will also be marked by the issue of a fresh volume of *Transactions* of the Institute, the first number of which was recently reviewed in these columns.

A LETTER signed "D.Sc. (Lond.)," referring to a Science and Art Department's examination, appeared in NATURE of September 8 (p. 435), and in it the writer gave the following as an instance of anomalies which occur in examinations:—"A student sat for the examination in May last in the advanced stage of practical organic chemistry. He was required to answer two questions, and to analyse two substances (unknown), as well as to find the halogen element present in an organic solid, and to determine the melting point of this solid. The written questions were correctly answered, the analyses were correctly done, the halogen was correctly determined, and the melting point of the substance was less than 1 per cent. too low. The description of the practical work was also fairly well done; but this student is returned as having failed, notwithstanding that there are two classes of success, first and second class. It would be interesting to know, in the face of this, the standard the examiners require for a first class success." Particulars which enabled the examiner to again look at the paper worked by the candidate referred to were afterwards furnished us by "D.Sc.," and the examiner now reports upon it as follows:—"The written questions were partly answered, and on this part of the examination the candidate would have been allowed to pass; but the analyses were both very badly done, and the answers quite wrong. For the detection of the halogen and for the melting point he received the full marks awarded to this part of the work." It will be seen from this that the suggestion of unfair marking made by our correspondent is entirely without foundation. With regard to the other point referred to in the letter, we are sorry to say that "D.Sc." would not furnish us with the name of the class in which he said that by the new rules the earnings this session will be reduced 75 per cent. We regret having unconsciously done an injustice to the Department of Science and Art by the publication of his letter.

THE following interesting announcement appears on a page in the catalogue of Messrs. Johnson, Matthey, and Co., Hatton Garden, London:—"In furtherance of scientific research, Professors and recognised scientific investigators will with pleasure be supplied with metals of the platinum group, in moderate quantities, and for periods to be arranged, free of charge, on condition that the precious metals are ultimately returned (in any form), and that the results of the investigations are furnished."

A PLEA for a national Antarctic expedition is made by Sir Clements Markham in a pamphlet published by the Royal Geographical Society. As was pointed out last week, in referring to the special Antarctic number of the *Scottish Geographical Magazine*, the only hope of maintaining the credit of our country in the work of exploration lies in an appeal to the patriotic feelings of those who possess the power which wealth supplies of providing the funds. The Council of the Royal Geographical Society generously offers to head the list with a subscription of 5000*l*. This example should be the means of showing that geographers are willing to help geographical discovery so far as their funds permit; and it also expresses their views upon the necessity of Antarctic exploration in an unmistakable form. It is earnestly to be hoped that the desire to enable the nation to retain its position as the first in exploration and discovery will inspire our wealthy countrymen to provide an amount equal to at least ten such subscriptions as that of the Royal Geographical Society. If this sum is not forthcoming, the prestige and credit won by former explorations will have to be resigned, and other nations will take our place as leaders in the work of geographical discovery.

AN attendant at the Pathological Institute attached to the Vienna General Hospital, died on October 18 from bubonic plague. It is believed that the man became infected by handling cultures of the plague bacillus. His duties were to look after the animals kept for the bacteriological study of the plague, but how he became infected has not yet been discovered. There is no doubt that the case was one of plague, an examination of the sputum having revealed the presence of the plague bacillus. Since the assistant's death, Dr. Müller, who attended him, has also died from the plague, and a nurse infected by it is described as in a condition which leaves little hope of recovery.

THE *Times* correspondent at Copenhagen announces that an international monument in honour of the famous Danish physician, Dr. Hans Wilhelm Meyer, who died three years ago, was unveiled in that city on Tuesday. Dr. Meyer was the discoverer of what are called adenoid growths. He found that the space between the nose and the throat, which ordinarily is an open cavity, is in certain persons suffering from deafness more or less closed by a large, soft mass. He found that this mass in numerous cases causes lifelong deafness, obstructs nasal respiration, and greatly retards the mental development of the patients, who are generally young. He succeeded in removing these growths by operation. Shortly before his death his discovery was universally recognised as being of the utmost importance. The monument has been erected by international subscriptions, committees having been formed in almost every civilised country, and not only members of the medical profession but also grateful patients contributing. The memorial is a bronze bust of more than life-size, resting upon a granite base. In front stands a figure of Hygeia, beneath which is inscribed the name "Hans Wilhelm Meyer." On the other side may be read the names of all the countries which have contributed to the memorial. The monument is the work of the Danish artists Bissen and Runeberg.

THE death is announced of Dr. Eugenio Bettoni, director of the Fisheries Station at Brescia, at the age of fifty-three years.

SIR JOHN MURRAY, F.R.S., has resigned the post of scientific member of the Fishery Board for Scotland, to which he was appointed by the Crown in January 1896.

THE opening meeting of the new session of the Institution of Electrical Engineers will be held on Thursday, November 10, when a paper will be read by Prof. Silvanus P. Thompson on "Rotatory Transformers." The annual dinner will take place in the Grand Hall of the Hôtel Cecil on Wednesday, December 7.

A DISPATCH describing a series of attempts to climb Mount Sorata, or Illampu, in the Eastern cordillera of the Andes of Bolivia, has been sent to the *Daily Chronicle* by Sir Martin Conway. The highest point attained was well over 23,000 feet, and probably as much as 24,000 feet, but the summit was not reached.

WE learn from *Science* that the U.S. Fish Commissioner has presented to Cornell University a collection of fresh-water and salt-water fishes, numbering between four and five hundred thousand specimens. The collection, in so far as it consists of living fishes, will be of great value not only to the zoological department, but also to the College of Forestry, in which a course in pisciculture and venery is to be introduced. It is understood that duplicates of this collection are to be presented to other institutions.

THE existence of a number of species of silk-spinning worms in the Sewaliks and Himalayas, and the extensive use of silk as a material for dress, make the question of sericulture in India a matter of great interest. The *Pioneer Mail* of October 7 prints an account of three different sets of experiments in progress during the current year in the North-Western Provinces to establish silkworm rearing—one in the plains, another in the Dun Valley, and the third on the Himalayas. All were carried out under different conditions of temperature, and all achieved a degree of success that is encouraging.

THE *Athenæum* announces that Don Francisco Coello de Portugal, who occupied in Spain the foremost rank as a geographer, has just died at Madrid at an advanced age. He had originally embraced the military career, and after having quitted the army in 1865 with the rank of colonel, he devoted himself chiefly to the science of geography, and published an excellent "Atlas of Spain and its Colonies," which will now, of course, be out of date. He was president of the Geographical Society of Madrid, and frequently represented Spain as delegate at scientific congresses.

A CORRESPONDENT sends us a letter he has received from Ballyarthur, in the Vale of Ovoca, County Wicklow, with reference to a curious object observed in the sky on Wednesday evening, October 19, about six o'clock. The object was visible in the south-west, and looked like a three-quarter moon. It was moving gradually from south-east to north-west, and appeared to the observer to go down behind the Croghan Kinsella mountain. It was of a golden colour, and was seen for four or five minutes. The suggestion is made that the object was a meteor, but it may have been merely an escaped balloon. Perhaps it was seen by other observers in Wicklow or Wexford, who could give further particulars concerning it.

WE learn from the *British Medical Journal* that it has been determined to appoint a special commission, to consist of five members, to conduct investigations regarding plague in India. The specific duty of the commission will be to inquire into the origin of the various outbreaks of plague, and the manner in which the disease is spread. An official statement also is required as to the efficacy of the serum treatment and the prevention of plague by means of inoculation. So far as the nominations to serve on this commission have been made public two Indian civilians, Messrs. J. R. Sewwett and A. Cumine, have already been appointed; but it is understood that three other members will be nominated by the Secretary of State for India, to proceed from this country, of whom one will act as chairman, while two will be experts.

PARTICULARS concerning the expedition which will leave England in the course of the next few days for the purpose of visiting the almost unexplored island of Sokotra, situated about 150 miles east-north-east of Cape Guardafui, are given in Tuesday's *Times*. The party will consist of Mr. W. R. Ogilvie Grant, of the department of zoology in the British Museum; Dr. H. O. Forbes, the director of the Liverpool Museums; and Mr. Cutmore, taxidermist attached to the latter institution. The Royal Society, the Royal Geographical Society, and the British Association have provided part of the funds for the undertaking. The expedition will sail for Aden, proceeding thence to Sokotra by the Indian Marine guardship *Elphinstone*, which, in compliance with a request made by the authorities of the British Museum, has been kindly placed at the disposal of Mr. Grant and Dr. Forbes for the purpose of conveying them to the island and back to Aden on the termination of their stay. The main object of the expedition is to investigate thoroughly the fauna of the place and make large and complete collections in every branch of zoology.

THE space to be devoted to the various sections of the Paris Exposition of 1900 has been arranged by the Commissioner-General as follows:—

	Sq. ft.
Agricultural and food products	20,000
Army and navy	3,300
Chemical industries	5,160
Education, instruments, practical sciences, and arts	11,470
Fine arts	(not yet known)
Forestry, hunting and fisheries	3,300
Heating apparatus	4,500
Horticulture	(not yet known)
Machinery and electricity	50,000
Manufactures	25,000
Mines and mining	7,700
Textiles	13,000
Transportation and civil engineering	20,000
Total	163,430

IT is of great importance that those who are engaged in archæological research should be properly trained; therefore the British School at Athens by supplying the needful training is doing very valuable work. This was the text of the remarks made by the Bishop of London at the annual meeting of the

School, held on Thursday last. Referring to the excavations at the prehistoric capital of the island of Melos, discovered at Phylákopi, the director of the School, Mr. Hogarth, said that the School began to excavate it in 1896, little suspecting the great importance of the site. It was proving a second Hissarlik, an undisturbed repository of the products of the primitive civilisation of the Ægean from the "Mycenæan" age back to the Neolithic period. Much had been eaten away by the sea, but what was left was equal in extent to Tiryns. Mr. Hogarth picked up the work where Mr. Cecil Smith left it, and after determining the limits of the city on south and east, and digging test trenches to obtain a relative chronology of the potsherds, in which the site was marvellously rich, proceeded to open out the great barrack-like structures on the north and west. Here were remains of three settlements, divided by layers of débris, the middle and lower ones being singularly well preserved. The best rooms were on the higher ground to the west. The blocks were divided by narrow lanes with covered drains down the centre. The depth varied from seven metres to three metres. In the two lower settlements was found a mass of pottery, and almost as many vessels, complete or little broken, as in a large cemetery. These covered the whole development of the potters' art up to the fine Mycenæan work. Fabrics, shapes, and decoration were in many cases new. The most notable vase was pipe-shaped and decorated with four scantily-clad figures, bearing fish in either hand. This was about the most interesting primitive Ægean vase in existence. In several rooms painted fresco was found, in one case white and gold lilies on a red ground; in another a beautiful scene of the sea with flying fish and marine growths, and a man working a casting net. Of the primitive symbols now attracting so much attention on Cretan stones, &c., over fifty distinct examples were found scratched in clay before baking. Many fine steatite vases, clay lamps (unknown previously on early sites), and other stone utensils and implements came to light. There was a little bronze and bone, but no gold or silver.

REFERRING to the collection of mollusca in the Madras Government Museum, Mr. Edgar Thurston states, in his report for the year 1897–98, that a right-handed chank shell (*Turbinella rapa*), that is, a chank shell with its spiral opening to the right, was acquired in the Madras bazaar for the small sum of Rs. 150. A shell of this nature, found off the coast of Ceylon at Jaffna in 1887, was sold for Rs. 700. Such a chank is said to have been sometimes priced at a lakh of rupees (Rs. 1,00,000); and, writing in 1813, Milburn says ("Oriental Commerce") that a chank opening to the right hand is highly valued, and always sells for its weight in gold. Further, Baldæus, writing towards the close of the seventeenth century, narrates the legend that Garroude (Garuda) flew in all haste to Brahma, and brought to Krishna the chianko or kink-horn twisted to the right.

IT has been suggested by several people that the recent wreck of the *Mohegan* on the Manacles Rocks was due to a local deviation of the compass of the ship. In a letter to the *Times*, Prof. A. W. Rücker points out that a disturbance of a magnitude sufficient to have caused the disaster is most improbable. He remarks:—"During the magnetic survey of the United Kingdom, carried out by Dr. Thorpe and myself, observations were made at twelve places in Cornwall. Of these Lizard Down, Porthallow, and Falmouth were the nearest to the scene of the disaster, and at all of them the deviation of the compass from the normal magnetic meridian was extremely small. The largest disturbance of this kind which was observed in Cornwall occurred at St. Levan, near the Land's End, and only amounted to eleven minutes of arc, or less than two-tenths of a degree. The largest disturbance of the dipping needle was at Mullion, and was only fourteen minutes."

MR. F. H. GLEW, of 156 Clapham Road, sends us a photograph of an oscillatory electric discharge which was taken in daylight. The photographic shutter was connected with a coherer and relay, so that the first component of the discharge operated the shutter and allowed an image of the succeeding components of the discharge to be caught. Mr. Glew suggests that a similar arrangement might be employed for photographing lightning in the day-time. Mr. Glew also sends us a photograph of a flash of lightning taken with a vibrating lens. From the multiple image produced, and the rate of vibration, he calculates that the total duration of the flash in question, which appears to have been of a triple compound nature, was one-nineteenth of a second, or less than one-half of a single vibration of the lens. He has also used a rotating photographic plate, but finds the vibrating lens to be more satisfactory.

TWO papers on the circulation of the residual gaseous matter in Crookes' tubes, read before the Physical Society by Mr. A. A. C. Swinton, appear in the October number of the *Philosophical Magazine*. The experiments described lead to the conclusion that "at very high exhaustions there exists a molecular or atomic stream from anode to kathode, which carries a positive charge and travels at considerable velocity outside of the opposite kathode-stream."

WHEN the poles of an arbitrary plane are taken, with respect to the conics of a Steiner's surface, it is known that another Steiner's surface is obtained. Prof. Brambilla, writing in the *Rendiconto* of the Naples Academy, iv. 7, has extended the same property to the two non-ruled surfaces of the fourth order, one in four-dimensional and the other in five-dimensional space, which, when projected from one or two arbitrary centres on our space, produce Steiner's surfaces.

THE phenomenon of equilibrium in mixtures of isomorphous substances has been studied by Küster since 1890. A further investigation, leading to somewhat different conclusions, is given by Signor Giuseppe Bruni in the *Atti dei Lincei*, vii. 5, who finds that the curve of congealment of a mixture deviates to a marked extent from the straight line obtained by Küster, and that serious objections can be raised against the latter's views on the coefficient of distribution. Signor Bruni, however, concludes that, in respect both of the variations in the temperature of congealment and of the distribution of the solid and liquid phases, isomorphous mixtures always follow completely the general theory of Van t' Hoff on solid solutions.

A SOMEWHAT novel line of investigation, which bids fair to throw light on certain problems of petrology, has been taken up by Prof. R. V. Matteucci in connection with the last eruption of Vesuvius. The method consists in artificially cooling portions of flowing lava, so as to partially or totally check the crystallisation of the substances contained in their magma; and in this way it appears possible to obtain information as to the exact stages at which different minerals separate out from the first exit of the lava up to its final consolidation.

THE coasts of Japan are peculiarly liable to incursions from spring tides, of which the one occurring on June 15, 1896, in the course of eighteen minutes swept away 9381 houses and 6930 boats, killing 21,909 people and wounding 4398. To minimise the damage done to life and property by such inroads, protective forests have been planted at various places along the littoral. Dr. Seiroku Honda, Professor of Forestry in the University of Tokyo, writing in the *Bulletin* of the Imperial University College of Agriculture, gives an interesting account of these protective forests, and advocates their further extension to parts as yet unprotected. The action of these forests is threefold: they check the force of the tidal wave; they delay its advance, giving more time for saving the lives of inhabitants living behind the forest; and, lastly, they prevent houses and property from being washed away into the sea. Dr. Honda gives a list of the trees which are best adapted for this purpose.—In the same number of the *Bulletin* Dr. Diro Kitao, by the use of the equations of elasticity, has endeavoured to reduce the calculation of the shrinkage and swelling of wood to mathematical principles.

THE interpretation of death among the lower organisms is ably dealt with by Signor Angelo Andres in the *Rendiconto del R. Istituto Lombardo*, xxxi. 13. The author, after pointing out the objections to Weismann's views, starts with the conception that living organic matter does not in itself possess any reason for dying, and that, on the other hand, this reason pertains to single individuals; in other words, that living matter remains in itself immortal, and that only the modality of the individual dies. An examination of the lowest forms of algæ leads Signor Andres to the conclusion that the first indications of true death occur in the Diatomaceæ, in which the process of subdivision leads to a gradual diminution in the size of the frustules, as also in the Volvocinæ, where the phenomenon of death is still more marked. The cause of death, it would appear, is to be sought in the differentiation which, by the specialisation of structure and function, leads to the perfectioning, both anatomically and physiologically, of different species.

THE *Engineering Magazine* for September contains an article on the bacterial process of sewage purification, which is at the present time attracting a considerable amount of attention; and is under investigation by a Commission appointed by the Local Government Board. The purification of sewage is a process of destruction of the organic matter by means of bacteria, and finally of the bacteria themselves from inanition. These bacteria are divided into two classes—ærobic, which require oxygen for their growth, and do their work best when sewage is exposed to the air; and the anærobic, which do not require oxygen for their growth, and do their work best in the dark. The former process of purification has been in use in this country for some years, having been first adopted by Mr. Bailey Denton in the system known as intermittent filtration through beds of earth. The latter system is of more recent origin, and has only been prominently before the public since the septic system was adopted by Mr. Cameron, the Borough Surveyor at Exeter. Besides the works at Exeter, others are in operation at Sutton and Yeovil, all of which are described and illustrated in the article by Mr. Rudolph Hering.

A SHORT account of the various steps that have been taken in the acclimatisation of trout in South Africa is given by Mr. J. D. F. Gilchrist in his report of 1897 on the sea and inland fisheries of Cape Colony. So far back as 1884 Mr. Lachlan Maclean began the experiment of trout acclimatisation in the Colony by importing 20,000 ova; and in spite of various difficulties and failures he proved its practicability. His experiments showed that the rearing of trout from imported eggs was feasible, and it is due to his success that the rivers are now being gradually stocked with valuable fish. The Cape Government took up the subject in 1890, and about a year later a hundred thousand trout ova were procured from Guildford, Surrey, a large number of which were hatched successfully. The work has since been carried on by the Cape Agricultural Department, and has undergone a steady progressive development. The trout turned into the rivers thrive exceedingly well, and many of them attain a large size.

AMONG other matters mentioned in the Report of the Marine Biologist referred to in the foregoing note, are trawling experiments performed with a view to introduce new and improved methods of fishing. It has been demonstrated that there is an excellent trawling ground rivalling the North Sea in productiveness, within easy reach of Cape Town. A satisfactory feature of the work is the discovery that soles occur abundantly on the fishing grounds, and can be readily got by trawling. As a scientific result of the experiments it may be mentioned that six different kinds of flat fish, one of which is new to science, have been discovered. The subject of temperatures, currents, &c., of the sea in relation to the scientific side of fishing investigations is being taken up; and Mr. Gilchrist announces that arrangements have been made at about a dozen different places for physical observations of this kind to be carried on.

THE latest number of *Janus*, a journal which is open to contributors from all parts of the world in divers tongues on subjects relating to the history of medicine and medical geography, contains an interesting and well-written article on medical archæology dealing with the significance of the plant Silphium and its therapeutic value amongst the ancients. Dr. Kronfeld of Vienna is the writer, and he has illustrated his article by a reproduction of the well-known dish of Arkesilas, now in the "Cabinet des Médailles" of the National Library in Paris. Graphic and very realistic scenes are depicted indicating the immense store set by the ancient Greeks upon this remarkable plant, whose habitat was located in Cyrene. Its applications seem to have been as diverse as they were valuable, and amongst its numerous uses we find it treasured as furnishing the earliest and most delicate of vegetables, also spice, whilst its therapeutic reputation was almost as universal as that claimed for some of our modern nostrums by their inventors! Silphium has long since disappeared from Cyrene, but Falconer has found in the northern parts of Cashmere a plant which is regarded as bein very closely allied to its historic predecessor.

THE Geological Survey has just published a brief Supplement to the Memoir on the Geology of Flint, Mold, and Ruthin, by Mr. A. Strahan. This Supplement contains records of borings put down in the reclaimed portion of the estuary of the Dee; and these are of importance as proving the presence of Upper Coal-measures, which do not appear at the surface and were not previously known to exist in Flintshire or West Cheshire. In this region the Middle Coal-measures are the productive strata, and the new information shows that the Upper Coal-measures may underlie much or all of the Cheshire Trials, and would consequently have to be penetrat.d in winning the coal. The price of the Supplement is 2*d.*

IN the *Journal of Applied Microscopy* (Bausch and Lomb) for August is a description of the Histological Laboratory of the Harvard University at Washington, D.C.

WE have received part 4 of vol. xxv. of Engler's *Botanische Jahrbücher*, occupied chiefly by a continuation of Pfitzer's review of the classification of Orchideæ, and the commencement of a systematic paper on the Monimiaceæ by J. R. Perkins.

WE have received the Reports of the Botanical Exchange Club for 1896 and 1897, both bearing the date 1898, the latter edited by Mr. G. Claridge Druce. They are both occupied by a record of new British localities, and by remarks on "critical" British species. The discovery is recorded of a new British sedge, *Carex chordorhiza.*

IN the *Revue Générale des Sciences* for September, M. L. Mangin has a short article on the sexuality of Fungi, in which he shows the remarkable advance that has been made during recent years in the discovery of sexual organs in various classes of Fungi. Especial reference is made to the researches of Harper on the Ascomycetes, of Dangeard on the same class of Fungi, of Sappin-Trouffy on the Uredineæ, and of Thaxter on the Laboulbeniaceæ.

THE *Journal* of the Royal Microscopical Society for October contains a continuation of Mr. F. W. Millett's report on the recent Foraminifera of the Malay Archipelago collected by Mr. A. Durrand, and the usual summary of current researches in zoology, botany, and microscopy. Among the latter is an abstract of Mr. Lewis Wright's important paper on microscopic images and vision.

THE *Biologisches Centralblatt* continues its useful summaries of recent researches in vegetable biology and physiology. In the numbers for September 15 and October 1 are contributions by Dr. R. Keller on the nyctitropic movements of leaves; on the mechanical action of rain on plants; on the flexibility of axial organs, and their capacity for resisting strain; on the comparative intensity of assimilation in plants in the Tropics and in Central Europe; and on the freezing of plants.

THE *Naturwissenschaftliche Wochenschrift* for October 2 contains an interesting paper by F. Schlechert, being a report of observations on several points of vegetable physiology. He finds that the highest temperature in the interior of a stem (12 cm. depth from the surface) occurs about midnight, the lowest between noon and 3 p.m. While the chief factor which governs the temperature of the interior of a tree is the temperature of the surrounding air (about twelve hours earlier), it is also affected by the temperature of the soil, by that of the ascending current of water, and by the degree to which the twigs are exposed to direct sunlight.

THE papers read at the third annual congress of the South-Eastern Union of Scientific Societies, held at Croydon in June, are printed in the *Report and Transactions* just published by Messrs. Taylor and Francis. Among the subjects of the papers are: The place of geology in education; entomology as a scientific pursuit; the nature of the soil in relation to the distribution of plants and animals; natural gas in Sussex; photography in relation to science; ideals for natural history societies, and how to attain them; and the folk-lore of amulets and charms. The volume thus contains information of interest and value to the members of all natural history societies.

FROM the United States we have received the following official publications:—"Principal Poisonous Plants of the United States," by V. K. Chesnut (U.S. Department of Agriculture *Bulletin* No. 20); a very useful publication, but the illustrations to which might be improved; we should hardly have recognised two of the imported European weeds, *Solanum nigrum* and *Conium maculatum*. Sixth Report on Kansas weeds (*Bulletin* No. 30, Experiment Station of the Kansas State Agricultural College, Manhattan); in this, and in other similar American publications, we note the introduction of the practice of noting graphically, by small maps, the distribution of the various species throughout the different States.

THERE is an interesting paper in the *American Naturalist* for September on some European museums, especially from a geological and mineralogical point of view, by Mr. E. O. Hovey. The small "Roemer Museum" at the quaint mediæval city of Hildesheim, near Hanover, is especially commended. Those visited by the writer in Russia presented no particular features of interest, the value of the magnificent collection of minerals in the Imperial Mining Institute at St. Petersburg being greatly marred by a faulty arrangement. Brief accounts are also given of the Natural History Museum at Berlin; the University Museum at Naples; the Museum of Natural History at the University of Geneva; the collection of minerals at the Jardin des Plantes, and the splendid collection of the École des Mines, Paris; the Museum of Practical Geology in Jermyn Street, London, and the collection at the British Museum; and the Woodwardian Museum, Cambridge.

MR. W. S. BLATCHLEY, State Geologist of Indiana, has issued his report (occupying 1197 pages) on the work accomplished by the department of geology and natural resources during the year 1897. A large proportion of the energies of the department were employed during that year in collecting data for a detailed report on the coal area of the State, shortly to be published. The present report includes papers of economic importance relating to petroleum, stone and clay resources of the State, the reports of the chiefs of the divisions pertaining to mines, natural gas and illuminating oils, and a long descriptive illustrated catalogue (666 pp.), by Mr. A. W. Butler, on the birds which have been observed within the State of Indiana, with an account of their habits.

A SECOND English edition of Prof. von Meyer's well known "History of Chemistry from the Earliest Times to the Present Day" has just been published by Messrs. Macmillan and Co., Ltd. The first English edition, translated from the original German edition by Dr. George McGowan, appeared in 1891. Dr. McGowan is alike responsible for the present volume, which is translated from the second German edition, with numerous additions and alterations. It is unnecessary to refer here to the value of the work, or to add to the account of it given in our review of the English version (NATURE, vol. xliv. p. 289). It is sufficient to say that in the second edition, published in 1895, Prof. von Meyer made use of all the additional sources of information on subjects of historical chemistry which had become available since the original work was written. "Among these," Dr. McGowan remarks, "are the Berzelius-Liebig and the Liebig-Wöhler *Letters*, the *Letters* and *Journals* of Scheele, Priestley's *Letters*, and the autobiographical fragment which Liebig left behind him. In addition, there are the recently published and valuable historical researches of Berthelot on the chemistry of the early Middle Ages, and the writings of Ladenburg, Schorlemmer, Thorpe, Grimaux, and others on the development of chemistry within certain definite periods, or on the life and work of particular chemists." These additions add to the value of what has always been a volume of great interest to students of chemistry, and we do not doubt that the new edition will be even more successful than the former one.

THE additions to the Zoological Society's Gardens during the past week include a Siamang (*Hylobates syndactylus*) from Sumatra, a Thick-necked Terrapin (*Bella crassicollis*), a Siamese Terrapin (*Damonia subtrijuga*), a Burmese Tortoise (*Testudo elongata*) from Siam, an Amboina Box Tortoise (*Cyclemmys amboinensis*) from Borneo, presented by Mr. Stanley S. Flower; a Negro Tamarin (*Midas ursulus*) from Guiana, presented by Mr. E. F. Brooker; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Miss Abchurch; a Pig-tailed Monkey (*Macacus nemestrinus*) from the East Indies, presented by Mr. R. O. Bell; a Common Paradoxure (*Paradoxurus niger*) from India, presented by Mr. H. A. Cottrell; two Capybaras (*Hydrochærus capybara*) from South America, presented by Mr. Basil J. Freeland; a short-winged Weaver-bird (*Hyphantornis brachyptera*) from South Africa, presented by Miss Alice Heale; an Emu (*Dromæus novæ-hollandiæ*) from Australia, presented by Sir Cuthbert Peek, Bart.; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Miss Peek; two Starred Tortoises (*Testudo elegans*) from India, presented by Mr. J. Freeman; a Smooth-headed Capuchin (*Cebus monachus*) from South-east Brazil, a Rabbit-eared Bandicoot (*Peragale lagotis*) from Western Australia, a Vulpine Phalanger (*Trichosaurus vulpecula*) from Australia, deposited; six Mute Swans (*Cygnus olor*) from Holland, purchased; two Rosy-faced Love-birds (*Agapornis roseicollis*), bred in the Menagerie.

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES IN NOVEMBER :—

November 1. 5h. 17m. to 6h. 0m. Occultation of 103 Tauri (mag. 5·5) by the moon.
5. 4h. Mars 3° 41′ N. of the moon.
12. Mars rises at 9h. and visible afterwards throughout the night in Cancer. Diameter of the planet 9″·6.
15. Mars. Illuminated portion of disc = 0·900.
15. Jupiter becomes visible as a morning star. Polar diameter 28″ 8.
15. Meteoric shower from Leo in the early morning hours. (Radiant 149° + 23°.)
16. 7h. Vesta 10′ N. of the moon.
17. 10h. 11m. Minimum of Algol (β Persei).
20. 7h. Minimum of Algol (β Persei).
22. 7h. 9m. to 8h. 13m. Occultation of 19 Piscium (mag. 5·2) by the moon.
23–24. Meteoric shower from Biela's comet. (Radiant 25° + 43°.)
25. 12h. Uranus in conjunction with the sun.
28. 14h. 7m. to 15h. 6m. Occultation of 103 Tauri (mag. 5·5) by the moon.
29. 11h. 19m. to 11h. 49m. Occultation of the star DM + 24°, 1033 (mag. 6·0) by the moon.

COMET BROOKS.—A telegram from Kiel, dated October 21, informs us that at 7h. Geneva time, on October 20, Brooks found a comet in position R.A. 14h. 32m. 8s. and Decl. + 60° 26′. It was moving in a south-easterly direction, and was described as "bright." A later telegram, dated October 22, gives the position of the comet from an observation made at Pola on October 21, at 16h. 48·5m. Pola mean time; this was R.A. 15h. 4m. 12s. and Decl. + 57° 50′.

The comet was thus situated in the constellation of Draco, and moving to the south of the star β Draconis.

THE LARGE SUN-SPOT.—In this column for September 8 we drew attention to the large spot that had a few days previously made its appearance on the eastern limb of the sun, and remarked that "the spot will be well worth watching during the remaining period of its visibility, especially as many years may perhaps elapse before observers are favoured with another spot of similar size."

At the present time there is another great spot, larger, perhaps, than the one referred to above, which is now beautifully visible near the centre of the solar disc. This very compact group, which is not situated very far from the solar equator, is composed of two large umbræ surrounded by rather irregular shaped penumbræ, with several small spots scattered around them. Another smaller spot is also following the large one.

It will be well for observers to look out for an aurora and magnetic storm when the spot passes the central meridian, as was the case when the spot, previously referred to, was in the same position on September 9 last.

THE HARVARD ASTROPHYSICAL CONFERENCE.—It was on the occasion of the dedication of the Yerkes Observatory that the Americans held their first astrophysical conference. So great was the success of this, their first trial, that it was expected that more would be held in after years. For this we had not long to wait, and we have now before us a full account of the conference recently held at Harvard, the oldest observatory devoted to astrophysical research, contributed to *Science* (October 7) from the pen of Prof. M. B. Snyder. There could have been no more appropriate place of meeting in America for the second conference than that of the Harvard College Observatory, for Prof. E. C. Pickering's vast organisation of work in all modern branches of astronomy is second to none.

The conference was presided over alternately by Prof. J. R. Eastman, of the United States Naval Observatory, and Prof. Hale, of the Yerkes Observatory, and the meetings were not only held on August 18, 19 and 20, but were carried over to a series of adjourned meetings held during the course of the subsequent week.

The papers read were very numerous, and dealt with all kinds of astrophysical work. The work carried on at Harvard formed, perhaps, the chief item in the programme. Some of the papers dealt with were as follows :—

Prof. George Comstock, on "Some investigations relating to zenith telescope latitudes"; Dr. Harold Jacoby, on "Photographic researches near the pole of the Heavens"; Mrs. Fleming, on "Stars of the fifth type in the Magellanic clouds," which establishes another connection between these objects and the Milky Way.

Prof. Solon Bailey presented a paper on "Variable stars in clusters," which is a subject most interesting in the light of recent investigations.

We notice that general plans for observing the total eclipse of the sun on May 28, 1900, were briefly discussed, and a committee appointed to consider the work of organisation.

Another important question brought up at the conference was the creation of a permanent astronomical and astrophysical society. This proposal was formally accepted, and a committee, consisting of Profs. Hale, Comstock, Pickering, Newcomb and Morley, was appointed to consider the organisation.

THE KNIGHT-DARWIN LAW.[1]

THE law under the above title is known to botanists through H. Müller ("Befruchtung der Blumen," Eng. trans., p. 4), who says that Andrew Knight "laid down the law that in no plant does self-fertilisation occur for an unlimited number of generations." This he call's Knight's Law, and later, in substantially the same form, it becomes the Knight-Darwin Law. For the statement of Knight's Law the reader is referred to that author's celebrated paper: "An account of some experiments on the fecundation of vegetables" (Phil. Trans., 1799). The words, however, do not occur in Knight's paper, and I imagine that Müller got them from Charles Darwin's paper on the fertilisation of papilionaceous flowers, where occurs the passage (*Gardener's Chronicle*, 1858): "Andrew Knight many years ago propounded the doctrine that no plant self-fertilises itself for a perpetuity of generations."[2] The words are not given in inverted commas, and I strongly suspect that, with a singular lapse of his usual accuracy, my father was merely giving his own interpretation of the conclusion which seemed to flow from Knight's expressions when taken with the whole of the context. For in the "Effects of Cross- and Self-fertilisation," 1876, p. 7, he quotes Knight's actual words. After referring to Sprengel, he goes on: "Andrew Knight saw the truth much more clearly, for he remarks: 'Nature intended that a sexual intercourse should take place between neighbouring plants of the same species' . . ." and again: "'Nature has something more in view than that its own proper males should fecundate each blossom.'" Here we have simply the general statement that hermaphrodite flowers are not necessarily self-fertilised; a statement of fundamental importance in floral biology. If the positive statement that "no plant self-fertilises itself for a perpetuity of generations" is to be found elsewhere in Knight's writings, I think Darwin would have quoted it.

In the "Origin of Species" (edition i., p. 96) he refers to Knight in the following words: "Nevertheless I am strongly inclined to believe that with all hermaphrodites two individuals, either occasionally or habitually, concur for the reproduction of their kind. This view, I may add, was first suggested by Andrew Knight."

Lastly, in 1868 ("Variation of Animals and Plants," ii. p. 175), after speaking of his own hypothesis, "that it is a law of nature that organic beings shall not fertilise themselves for perpetuity," he adds: "This law was first plainly hinted at in 1799, with respect to plants, by Andrew Knight." If he had known any positive expressions—going beyond the nature of a hint—in Knight's writings, would he not have quoted them? It seems, therefore, that, as far as Knight is concerned, the law should be a general statement of the tendency to cross-fertilisation of hermaphrodites, and not the positive statement quoted by Müller.

When we pass from Knight's share in the law to Charles Darwin's—there are difficulties in fixing on the most authentic wording of the law. The earliest form is that occurring in the "Origin of Species" (ed. i. p. 97).

"These facts alone incline me to believe that it is a general law of nature (utterly ignorant though we be of the meaning of the law) that no organic being self-fertilises itself for an eternity of generations; but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable."

In the sixth edition of the "Origin," 1872, he retains the above passage with the omission of the words "utterly

[1] A paper read before Section K (Botany) at the British Association, 1898.
[2] This sentence is quoted by Müller, "Historical Introduction," p. 29.

ignorant though we be . . . law," and with the addition of a reference to his own experiments, *i.e.* to those on cross-fertilisation.

This is the most strongly worded form of the Law, and one which is generally adopted. But shortly after the publication of the "Origin," *i.e.* in 1862, the Law took a vaguer form in the "Fertilisation of Orchids" (ed. i., 1862, p. 359), where he wrote: "Nature thus tells us, in the most emphatic manner, that she abhors perpetual self-fertilisation." This form of the Law is adopted in the "Effects of Cross- and Self-fertilisation" (p. 8), where he writes: "If the word perpetual had been omitted, the aphorism would have been false. As it stands, I believe that it is true, though perhaps rather too strongly expressed."

The aphorism is clearly not a literal statement of fact, and in describing it as "true," he probably meant that perpetual self-fertilisation is very strongly and very generally guarded against in nature. For he well knew that "some few plants seem to be invariably self-fertilised" ("Cross- and Self-Fertilisation," p. 3). With regard to these cases he makes the just remark: "These exceptions need not make us doubt the truth of the above rule, any more than the existence of some few plants which produce flowers, and yet never set seed, should make us doubt that flowers are adapted for the production of seed and the propagation of the species."

It is only fair to add that this argument also occurs in the "Variation of Animals and Plants under Domestication" (vol. ii. p. 91, 1868), and was therefore of considerably earlier date than his book on "Cross- and Self-Fertilisation" (1876).

To sum up:

(1) If the expression, Knight-Darwin Law, is to continue in use, it ought to be applied to a statement on which Knight and Darwin are undoubtedly agreed, *i.e.* that "nature intended that a sexual intercourse should take place between neighbouring plants of the same species."

But the name of Knight-Darwin Law is now firmly associated with the positive statement "that no organic being fertilises itself for an eternity of generations," and it would be useless to suggest a new nomenclature.

(2) If we are to take a Darwinian version of the Law, it seems to me fairer to take the form, "nature abhors perpetual self-fertilisation," which my father adhered to in his later books.

An example of what seems to me the misuse of the Knight-Darwin Law occurs in my friend Mr. Willis' excellent book, "Flowering Plants and Ferns" (vol. i. p. 46). "In Myrmecodia, &c., Burck has found crossing absolutely prevented, the flowers never opening. Hence the Knight-Darwin hypothesis must be abandoned." If the abandonment of the hypothesis means the recognition of cases of apparently continuous self-fertilisation, the abandonment was made in 1868 by Darwin himself, as I have already shown. But Willis' abandonment seems to me part of an implied contention that Charles Darwin's generalisations are no longer a sufficient basis for floral biology. He seems to think that if the Knight-Darwin Law is not true, the fundamental principles underlying the study of the mechanism of flowers must be sought elsewhere than in Charles Darwin's works. In this point of view I think he is mistaken.

The attitude of the earlier writers towards the problem of cross-fertilisation seems, if I may venture to say so, to be elsewhere rather hastily treated by Willis (*loc. cit.*, p. 45). Take the following passage: "The advantages of cross-fertilisation are often great, and frequently enormous, and as at the first glance they appear to be obtained at little or no cost, we are inclined to expect this method of propagation to prove almost universal. The earlier workers at this subject in fact set out with the idea that cross-fertilisation was, so to speak, the primary object of a flower's existence, whilst self-fertilisation was actually harmful." Almost the whole of this seems to me to be unintentionally misleading.

That all "earlier workers" did not consider cross-fertilisation the primary object of a flower's existence, is shown by the following passage from "Cross- and Self-Fertilisation," p. 3: "We should always keep in mind the obvious fact that the production of seed is the chief end of the act of fertilisation; and that this end can be gained by hermaphrodite plants with incomparably greater certainty by self-fertilisation than by the union of the sexual elements belonging to two distinct flowers. Again, reviewing in 1876 ("Cross- and Self-Fertilisation," p. 8) his own treatment of the question in the "Fertilisation of Orchids" (1862), Darwin says: "I should have added the self-evident proposition that the propagation of the species, whether by self-fertilisation or by cross-fertilisation . . . is of paramount importance." Willis, therefore, seems to me completely wrong if he includes Charles Darwin among the earlier who considered cross-fertilisation the primary object of a flower's existence.

Nor, I think, is self-fertilisation ever treated by Darwin as positively harmful, though *perpetual* self-fertilisation is so treated. Self-fertilisation is constantly and correctly considered as less advantageous than cross-fertilisation—and in this sense (always bearing in mind the paramount importance of fertilisation of some sort) it may be said that self-fertilisation is relatively harmful.

Whatever may be the case with other naturalists, Darwin was certainly not inclined to expect cross-fertilisation to prove almost universal. Speaking of orchids, he says ("Fertilisation of Orchids," ed. i. p. 359): "Considering that the anther always stands close behind or above the stigma, self-fertilisation would have been an incomparably safer process than the transportal of the pollen from flower to flower. It is an astonishing fact that self-fertilisation should not have been an habitual occurrence." He saw clearly that plants pay a price for being so constructed that cross-fertilisation is possible; in fact, he saw that the evolution of the flower is the result of a gain and loss account between the advantage of cross-fertilisation and the risks and injuries consequent on the flower being open instead of closed, and therefore chasmogamic instead of cleistogamic. And this is in all essentials the theory which Willis ("Flowering Plants and Ferns," p. 46) gives as Macleod's, and proposes as a basis for floral biology, when the Knight-Darwin Law has been abandoned, and H. Müller's theory also given up. I am not able to read Macleod in the original Dutch, but it would appear from Willis's paper in *Science Progress*, 1895, that Macleod's contribution to the subject is full of valuable matter, but the essence of his theory (as given by Willis) seems to me to contain nothing with which my father was not familiar. What I object to is the tendency to condense Charles Darwin's contribution towards floral biology to a Knight-Darwin Law, and then, when the abbreviated statement does not explain everything, to abandon—not so much the law—but the general point of view which can only be gathered from Darwin's books as a whole.

The fact is that some modern biologist uses the Knight-Darwin Law in an inverted way, *i.e.* in a manner the reverse of Charles Darwin's way of using it. It was not to him a basis for the investigation of floral structures, but a generalisation extracted from that subject to serve as a foundation for the study of wider questions, such as the origin of sexuality. This is clearly shown in a passage from the first edition of the "Fertilisation of Orchids," where, after enunciating nature's abhorrence of perpetual self-fertilisation, Darwin goes on ("Fertilisation of Orchids," 1862, p. 359): "This conclusion seems to be of high importance, and perhaps justifies the lengthy details given in this volume. For may we not further infer as probable . . . that some unknown great good is derived from the union of individuals which have been kept distinct for many generations." H. Müller, perhaps, understood my father's use of the Law when he said ("Fertilisation of Flowers," p. 22) that the Knight-Darwin Law is not necessary for the elucidation "of the forms of flowers." But he would hardly have said as much of Knight's statement, that hermaphrodite flowers are adapted for intercrossing—which is the very foundation of the science of floral mechanism.

I now pass on to another writer—Knuth—who, in his useful "Blütenbiologie," seems also to be open to criticism in his treatment of the Knight-Darwin Law. In speaking of H. Müller's great work, he says ("Blütenbiologie," vol. i. p. 25): "The laws of Knight, Darwin, Hildebrand, Delpino gave no explanation of the numerous cases of efficacious self-fertilisation, nor of cleistogamy." Here Knuth does not seem to remember the conditions of thought under which the Knight-Darwin Law came into existence. As Loew ("Einführung in die Blütenbiologie," p. 143) has well said, self-fertilisation was formerly assumed to be the rule in hermaphrodite plants. In calling attention to the existence and importance of cross-fertilisation in hermaphrodites, Knight and Darwin assumed the existence of self-fertilisation. From the point of view of floral biology the important thing was the recognition of cross-fertilisation, and the law in which, unfortunately, this conclusion has been entangled need not "explain" the facts which the framers of the law assumed to be a part of common knowledge. With regard to cleistogamy, in its ordinary sense it is clear that there is no contradiction between the Knight-Darwin Law and the facts, as Loew has clearly pointed out ("Einführung," p. 144).

After the passage quoted above, Knuth goes on: "In the place of the one-sided law of the above-named naturalists (of which the general truth remains unproven) Müller set up a law, proved directly by Darwin's experiments and indirectly by the reproductive arrangements of plants in general, but especially by those of flowering plants. The law, namely, that 'when the offspring of cross-fertilisation come into serious conflict in the struggle for life with the offspring of self-fertilisation, the former (cross-bred) win the day. Only when this contest is absent can self-fertilisation suffice for reproductions for many generations.'"

I confess that this law is to me unsatisfactory. We ask ourselves "when is the struggle between cross- and self-bred offspring[1] absent?" Clearly when all the offspring are of one kind, *i.e.* all cross-bred or all self-bred: in a diœcious plant where all offspring are cross-bred, there is no question of self-fertilisation. In a plant with purely cleistogamic flowers, all offspring would be necessarily self-bred, and the law would imply that cleistogamic perpetuation may suffice. The law, therefore, amounts to this: that self-fertilisation will suffice only when it is unavoidable. This is as much as to say that any form of fertilisation is better than none. It is best to neglect this form of Müller's hypothesis, and to seek his meaning in his simpler and broader statements. In summing up his discussion in the "Historical Introduction," he says ("Fertilisation of Flowers," p. 23): "There is a good foundation, therefore, for the demand that the explanation of floral mechanisms shall rest only on the sufficient and demonstrable assumption that cross-fertilisation yields more vigorous offspring than self-fertilisation."

We have therefore as the chief points in Müller's theory:

(1) Fertilisation at any price.

(2) The increased vigour of cross-bred offspring.

Let us consider these more fully, and first for the conclusion that self-fertilisation is better than no fertilisation. This is a proposition which Müller has insisted on in the most interesting and instructive way, but it surely is not very novel in principle.[2] In a passage already quoted, Darwin reviewing, in 1876, his work of 1862 ("Cross- and Self-Fertilisation," p. 8), says: "I should have added the self-evident proposition that the propagation of the species, whether by self-fertilisation or by cross-fertilisation . . . is of paramount importance. Hermann Müller has done excellent service by insisting repeatedly on this latter point." No one had a higher respect than my father for Müller's work, and he had no disrespectful intention in describing Müller's contribution to the theory as self-evident. The interesting point is that these views did not strike him as original, because they had already occurred to himself.

That Müller based the explanation of floral mechanism on the experimental results of cross-fertilisation cannot be considered as a new departure. I should have imagined it to be notorious that this was Charles Darwin's view, if it were not that we find Knuth and others describing Müller's theory (in which this is the essential thing) as a great law of nature.

In a letter ("Life and Letters," iii. p. 291) to the late Asa Gray (September 10, probably 1866), Charles Darwin wrote: "I have seen the young seedlings from the crossed seed exactly twice as tall as the seedlings from the self-fertilised seed. . . . If I can establish this fact . . . in some fifty cases . . . I think it will be very important, for then we shall positively know why the structure of every flower permits, or favours, or necessitates an occasional cross with a distinct individual."

It seems to me that Charles Darwin's generalisations in regard to flowers may be summed up thus:—

(1) First comes what he called the self-evident proposition that fertilisation of some sort is of paramount importance. This is of the nature of an axiom.

(2) Then comes the direct observation that the vast majority of flowers are open. From this fact alone we should be justified in concluding that there is some advantage in cross- as compared to self-fertilisation, which advantage makes it worth while for flowers to run the risks and incur the expenditure necessarily connected with openness, and avoidable by cleistogamy. The innumerable adaptations for pollen-transport suggest and strengthen the same conclusion. But this is, properly speaking, only an elaboration of the fact that flowers are open.

(3) Direct experiment demonstrates the nature of this surmised advantage of cross-fertilisation over self-fertilisation.

As already pointed out, the Knight-Darwin Law in its usual form, *i.e.* no plant is self-fertilised *ad infinitum*, or in its improved form—"Nature abhors perpetual self-fertilisation"—is a generalisation drawn from observations on structure and experiments on crossing, the value of which in Darwin's opinion was rather its applicability to the problem of sex in a wide sense, than its use as a basis for understanding the mechanisms of flowers.

The point which seems to me important in the history of the subject, is that the above generalisations, which are in substance to be found in Darwin's works, are still the foundation-stones of floral biology, and would stand as firmly if the Knight-Darwin Law had never been formulated. For the naturalist who takes a wider field, and studies the origin of sex and the action of changed conditions, the existence or non-existence of perpetual self-fertilisation must always be an important question; but the law in which its non-existence is formulated, is not a fundamental canon of floral biology.

Francis Darwin.

[1] I use the words *cross-bred* and *self-bred* to denote the offspring of cross- and self-fertilisation; we thus avoid the slightly obscure phrases cross-fertilised and self-fertilised seedlings which occur in Darwin's books.

[2] I am far from wishing to suggest that H. Müller's work does not contain much that is new and valuable; I am here considering only its fundamental bases.

BOTANY AT THE BRITISH ASSOCIATION.

THE subject of alternation of generations in plants played a prominent part in the work of the Botanical Section. The President (Prof. Bower) devoted a considerable portion of his address to the controversial questions connected with "the great enigma of the alternation of generations" in green plants. Mr. Lang, of Glasgow University, and Prof. Klebs, of Halle, contributed important papers on this subject, and these were followed by a general discussion on the problems of alternation. Mr. Lang gave an excellent summary and critical review of our present knowledge concerning alternations of generations in the Archegoniatæ. The recent work of this investigator on some striking cases of deviation from the normal life-history of ferns, must be ranked among the most important contributions germane to this subject which have appeared in recent years. In concluding his account of some of the main factors in alternation, the author suggested three subsidiary questions as worthy of attention—the probable line of descent in archegoniate plants, the bearing of the cytological facts on the question, and the significance to be attached to apospory and apogamy.

Prof. Klebs' paper dealt with the alternation of generations in the Thallophyta, a subject which he was particularly well fitted to discuss from a critical standpoint. After taking a general survey of the various divisions of the Thallophyta, Prof. Klebs referred more especially to certain cases which have a more direct bearing on the question of the first appearance of a regular alternation of generations. The majority of the Algæ and Fungi have two or more kinds of propagation, each of which necessarily depends upon definite external conditions. According to the conditions the different kinds of propagation may appear on the same or on different individuals, independently or in any succession. The fertilised ovum in sexual forms does not differ essentially on germination from another propagative cell. In none of these cases is there any reason for speaking of an alternation of generations. In conclusion, the author briefly referred to the possible connecting links between the Algæ and Archegoniatæ. Sir Edward Fry, Dr. Scott, Profs. Marshall Ward and Marcus Hartog took part in the discussion which followed the two contributions by Mr. Lang and Prof. Klebs.

Another important item in the programme of Section K was a semi-popular lecture by Dr. F. F. Blackman, on the breathing mechanism of plants. The lecturer gave a clear and interesting summary of the progress of experimental work on the phenomena of gaseous exchange between a green plant and the medium in which it grows, concluding with an account of some recent investigations which have not yet been published.

Algæ and Fungi.—The Committee on Fertilisation in the Phæophyceæ reported very satisfactory progress in the researches on the Fucaceæ and Dictyotaceæ. Mr. Lloyd Williams, of Bangor, whose researches have been carried out under the auspices of the Committee, gave an account of his important work on the reproduction of *Dictyota dichotoma*. *Dictyota*, an annual brown seaweed, germinates during the summer and begins to form its reproductive cells in July. The tetraspores are produced throughout the season, but the sexual cells show a remarkable periodicity. The author described the fertilisation of the oospheres by the motile antherozoids, and expressed the opinion that there are strong reasons for concluding that the factor which determines the maturation and liberation of the sexual cells, and the fertilisation of the oospheres, is the amount of illumination to which the plants are exposed.

Prof. Phillips, of Bangor, contributed a paper on the form of the protoplasmic body in certain Florideæ. In *Ceramium rubrum* and other species a strong strand of protoplasm runs along the axial cells from pit to pit. In *Dasya coccinea*, the branches of limited growth run out into pointed uncorticated filaments, the cells of which are large. Across the vacuole of these cells running from pit to pit occurs a thread of protoplasm much more delicate than the corresponding structure in *Ceramium*. In *Callithamnion byssoides*, threads of protoplasm, which exhibit incessant movement, radiate from a cushion lying over the pit and end blindly on the vacuole. All these phenomena point to the great physiological importance of the pit-communication between cell and cell.

Prof. Errera, of Brussels, communicated the results of some recent work on the structure of the yeast cell; his investigations led him to the following conclusions: (1) a relatively large nuclear body exists in each adult cell; (2) young cells contain no such body; at a later stage the old nuclear body divides, one of its two daughters wanders through the narrow connecting channel into the young cell; (3) after the division is complete, the two cells are kept together by a mucilaginous neck-shaped pedicel; (4) carbohydrates are stored up in yeast in the form of glycogen, which accumulates or disappears from the vacuoles very rapidly, according to conditions of nutrition and growth.

Mr. Harold Wager also presented a communication on the same subject; he referred to the existence of a deeply stainable body, regarded by most observers as a nucleus, and of a vacuole in close contact with the nucleus. During the division of a cell a portion of the nucleus and of the vacuole passes into the daughter-cell. Mr. Wager pointed out certain errors in the work of Hieronymus; and expressed the opinion that the "nucleus" of the yeast possibly represents an early stage in the development of the vegetable nucleus; it might be fitly designated a proto-nucleus. Mr. Wager also gave an account of his researches on the rare fungus *Polyphagus euglenæ*, a parasite on *Euglena viridis*. The material was obtained from a filter-bed at Keighley. Mr. Wager was able to follow in detail the methods of spore and zygospore formation; he noted the interesting fact that the male cell is larger, and possesses a larger nucleus than the female cell.

Prof. Marshall Ward gave an account of a new potato disease which appears to be fairly common, but has hitherto usually been confounded with the disease caused by *Phytophthora*. The pathology of the disease was dealt with, and the author referred to certain external symptoms which enable a practised eye to distinguish diseased plants from those suffering from the attacks of *Phytophthora*. An interesting feature of the disease is that the fungal hyphæ appear to prepare the way for the entrance of bacteria and other organisms into the tissues of the host-plant. The same author contributed a second paper, in which he described the action of *Penicillium* as a wood-destroying fungus.

Mr. Trow, of Cardiff, gave an account of the cytology and reproduction of *Achlya americana* var. *cambrica*. He described the nuclear division in the oogonium and antheridium; also the occurrence of fertilisation as in *Saprolegnia mixta* and *S. diclina*.

Mr. Ellis, of Cambridge, contributed a note on a method of obtaining material for illustrating smut in barley.

Pteridophytes and Gymnosperms (*Recent and Fossil*).—Mr. Lang announced the discovery of the prothallus of *Lycopodium clavatum*. A few prothalli were found wholly imbedded in the peaty soil underlying a patch of moss; three of them bore young plants, and a number of slightly older plants, the prothalli of which had disappeared, were found in the same spot. The prothalli, which present a general resemblance to those of *Lycopodium annotinum*, are of considerable size, completely devoid of chlorophyll, and fairly well provided with rhizoids. Their form is that of a thick fleshy cake, which soon becomes thrown into folds by the unequal growth of the margin. The sexual organs are borne on the upper surface; both antheridia and archegonia may be present at the same time.

Dr. Scott gave a short account of some of his recent work on the anatomy of Coal-measure plants; the most important of his contributions was a description of the structure of a new form of the genus *Medullosa* from the Lower Coal-measures of Lancashire. This extinct type of Palæozoic plants has not hitherto been recorded from a British locality, and has not previously been found in rocks of Lower Coal-measure age. The material on which the description was founded was obtained by Mr. Lomax, and the excellent sections, of which micro-photographs were shown on the screen, were prepared by this able worker. Dr. Scott showed that the type of structure represented by the Lancashire *Medullosa* is that of a polystelic *Heterangium* which bore *Myeloxylon* petioles. The same author exhibited photographs of an unusually fine specimen of the Halonial branch o. a *Lepidodendron*, allied to *L. fuliginosum*, recently discovered by Mr. Lomax. Other contributions by Dr. Scott dealt with an English example of the interesting Palæozoic fern *Botryopteris*, and with a remarkably fine example of *Zygopteris* from the Williamson Collections of Coal-measure plants in the British Museum.

Mr. A C Seward described the external features, internal structure and geological history of the Malayan fern *Matonia*. The anatomical investigation was founded on some material received through the kindness of Mr. Shelford, of the Sarawak Museum. The stem of *Matonia pectinata* is characterised by an arrangement of vascular tissue which appears to be unique among recent ferns; there are two annular steles, and occasionally also an axial strand of xylem and phloem traversing the creeping rhizome. The genus *Matonia* has usually been regarded as a type apart, and the anatomical characters emphasise the isolated position of the genus. The two living species of *Matonia* are no doubt the survivors of a tribe of ferns widely distributed during the Rhætic and Jurassic periods.

Mr. C. E. Jones, of Liverpool, contributed a paper on the anatomy of the stem of certain species of *Lycopodium*; his communication was of the nature of a preliminary note on the subject of the general anatomical investigation of *Lycopodium*, on which he is at present engaged.

Mr. Pearson, of Cambridge, described the apogeotropic roots of the Australian Cycad *Bowenia spectabilis*; he drew attention to the occurrence of colonies of *Anabæna* in the intercellular spaces of the cortex.

Physiology and Natural History.—Prof. Errera discussed the theoretical calculation of an osmotic optimum. Recent researches made by Dr. F. Van Rysselberghe, of Brussels, have shown that vegetable cells generally answer an *osmotic stimulus* by an appropriate *osmotic reaction*, and that the relation between stimulus and reaction follows, within wide limits, the "law of Weber." Hence results the possibility of predicting the existence and value of an *osmotic optimum*. The same author also contributed a note on the unit to be adopted for osmotic measurements.

Mr. Francis Darwin read a paper of special interest on the Knight-Darwin Law. (This paper is printed in full in another part of the present number.)

Prof. Reynolds Green gave an account of some results which he had obtained confirmatory of Buchner's work on the enzyme of the yeast plant. Prof. Green found that if the yeast experimented on is in a state of active fermentation, the alcohol-producing enzyme can be procured as Buchner has stated. He described the method of investigation adopted, and concluded by stating that the enyzme obtained from yeast agrees in an important respect with other enyzme.

Prof. C. de Candolle, of Geneva, gave the results of a comprehensive comparative study of peltate leaves, with special reference to the number of species possessing such organs, their distribution among the various natural orders, and their mode of growth.

Mr. Burkill, of Kew, dealt with changes in the sex of willows. In the genus *Salix* flowers of both sexes are occasionally present in the same catkin, and the sexual organs are sometimes found to be intermediate in structure between stamens and carpels. Mr. Burkill gave the results of his examination of an extensive series of specimens and published records.

Mr. S. T. Dunn contributed some notes on the origin o. railway-bank vegetation.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

SPEAKING as chairman of a meeting of the Associated Societies of Edinburgh University on Tuesday, Mr. Balfour expressed his views on examinations as follows:—I think the time is not very far back when the idea was prevalent that after all a University was little more than an examination machine for stamping a certain number of students with a hall-mark, indicating that they had satisfied a certain number of examiners, and that they possessed a certain amount of knowledge on a certain amount of subjects. But that idea belongs to the past, and everybody who realises how the University machinery may do the work of higher education in the country has long recognised that the University to be at its best must not be an examining University merely or particularly, but what is wanted is a teaching University. I do not wish to overstate the case against examinations. I have always insisted that they are a necessity. They are evils, necessary evils, evils which no skill on the part of the examiner, no dexterity on the part of those responsible for University organisation, could wholly remove. That an examination may be a good test of intellectual capacity I cannot deny when I remember the numbers of eminent men who in after life have been in the very first rank of scientific and philosophical investigators, or in the very front rank as men of letters, and who were all so distinguished in examination. But while they were reading for examination they were occupied in considering not what was the sort of truth, not what was the best method of advancing the special study in which they were engaged, and so increasing the science of the world. Not at all. They were occupied to a large amount with an immense variety of subjects, different altogether and at the same time ready for immediate use—the last thing a practical man ever does. The serious man puts out of his head that which is not necessary and is, indeed, superfluous. He focusses his mind upon the work immediately before him, and, though no doubt he may see to the right or to the left more collateral subjects which have a bearing upon the main question, he certainly is never in the position of the unhappy victim of examination, who is going over in his head, before entering the room, all the various problems it is necessary to have at his finger ends if he is to satisfy the gentleman who is examining him.

SIR W. H. WHITE delivered an address on engineering education at the Institution of Junior Engineers on Friday last. In the course of his remarks he said he was constantly asked what course of training he would recommend for youths intended to become engineers. His advice had always been the same, and it was based on personal experience and extensive observation. Practical training in the workshop, factory, ship-yard, or other engineering establishment was, he considered, best begun when a lad was fresh from school. "Roughing it" then came easy, observation was quick, while the facility for acquiring handicrafts and manual dexterity was greatest. Familiarity with the habits and modes of thought of workmen was readily gained also, and was a valuable acquisition. During this period of practical training it was most desirable that scholastic knowledge should be maintained or extended. If this programme were carried out, a young man finished his practical course without loss of educational knowledge; and if he had the means and the capacity, he was well prepared for entry into a technical college at an age which permitted him to obtain the full benefit of theoretical training and laboratory work. With ability and energy commensurate to the task, a student thus prepared, and bringing with him considerable practical experience, ought to reap the greatest advantage from the higher course of study, and to be ready for actual work when it was completed. His observation and experience as student and professor convinced him that many youths entered technical colleges who, from want of preliminary education or of ability, could never hope to benefit much, if at all. It would be a kindness in such cases if entry were guarded by such preliminary tests and inquiries as would prevent waste of time, and permit other and more suitable training to be undergone. Perhaps the ideal system of training was that which permitted an engineering pupil to continue his scholastic training side by side with the preliminary practical experience, as the medical student attached to a hospital did. Selected men, having proved their capacity, could then proceed to a course of higher technical training without losing all contact with practical work. The latter condition could be met by arranging suitable intervals when students would suspend their studies of theory and go out to the scenes of engineering operations, where they could compare the lessons learnt in the study and laboratory with actual procedure in carrying on work.

WE are glad to see that an attempt is being made to co-ordinate the educational institutions in Bristol, and so prevent the present overlapping of work and conflict of interests. At a recent meeting of the Technical Instruction Committee of the County Borough Council the following resolution was adopted: "That the governors of the Bristol University College, the governors of the Merchant Venturers' Technical College, and the Bristol School Board, be requested to send three representatives each to meet a sub-committee of the Technical Instruction Committee, for the purpose of taking into consideration the needs and resources of the city as a whole, with a view of combining all parties in one comprehensive plan for the supplying of such technical instruction as the circumstances of Bristol require."

Science reports the following gifts to educational institutions in the United States:—The will of the late Colonel Joseph M. Bennett, who during his life-time had made generous gifts to the University of Pennsylvania, leaves to the University property valued at 400,000 dollars. The money is to be used for the higher education of women.—A sum of money, said to be 158,000 dollars, has been given by friends of Barnard College to pay the entire indebtedness of the College due to its removal to the new site adjacent to Columbia University.—A donor, whose name is withheld, has given Wellesley College an astronomical observatory and a telescope, said to be of large size.—Vassar College receives 10,000 dollars by the will of the late Adolf Sutro, of San Francisco. The same College has been given 1000 dollars by Senator Coleman, of Michigan, the income to be used to purchase books and instruments for the astronomical observatory.—The annual report of President Low to the Trustees of Columbia College states that during the year the University received 346,409 dollars for permanent endowment and 43,909 dollars for current uses.

THE *Athenæum* states that the Joint Committee of the bodies concerned in secondary education, which includes representatives of the universities and the administrative authorities, has been summoned to meet on November 5, when the Government Education Bills will be taken into consideration. It seems probable, from what has taken place during the recess, that the constituent bodies will not deem it advisable to urge the Government to immediate legislation on the subject of local authorities.

SCIENTIFIC SERIALS.

American Journal of Science, October.—The compressibility of colloids, with applications to the jelly theory of the ether, by C. Barus. Various colloids were compressed in capillary tubes with mercury terminals. A solution of gelatine or albumen in water was found to have a low compressibility, and a solution of india-rubber in ether was taken as a type of a highly compressible colloid. When the colloid was compressed by the mercury, the meniscus would occasionally give way, and a droplet of mercury be projected through the substance of the colloid to a distance of 12 cm. or more. This has an interesting application to the problem of the motion of material bodies through a solid ether. The mechanism of this motion is not yet explained, but there is probably a temporary liquefaction of the colloid in front, and a subsequent solidification behind the moving body.—Eolian origin of loess, by C. R. Keyes. The amount of dust brought up out of the Mississippi valley into St. Louis is about one-hundredth inch in a day when the wind blows. An open book placed in a protected nook was after a few hours of wind so covered with dust that the print could not be distinguished. Probably the rate of deposition of the Mississippi loess is one-tenth of an inch per annum. Being spongy and absorbent, the loess retains moisture in the dry season, and gives rise to a luxuriant vegetation.—Detection of sulphides, sulphates, sulphites, and thiosulphates in the presence of each other, by P. E. Browning and E. Howe. To about 0·1 gr. of the substance dissolved in 10 cc. of water, add an alkali to slight alkaline reaction. Add zinc acetate in distinct excess, and filter. The precipitate may be tested for SH_2, on acidifying, in the usual manner. To the filtrate add acetic acid in slight excess, and barium chloride, and filter through a double filter. To the filtrate add iodine until the solution takes on a permanent yellow tinge, and then bleach with stannous chloride. A precipitate indicates the sulphite. Filter, add bromide water in faint excess to the filtrate, bleaching again with stannous chloride. A precipitate on adding bromine indicates a thiosulphate originally present.—The origin and significance of spines, by C. E. Beecher (concluded). Spinose forms were simple and inornate during their young stages, and were all derived from non-spinose ancestors. Spines represent an extreme of superficial differentiation which may become fixed in ontogeny. Spinosity represents a limit to morphological and physiological variation. After attaining the limit of spine differentiation, spinose organisms have no descendants, and out of spinose types no new types are developed.

THE following are the titles of the more important papers in systematic and geographical botany contained in the *Journal of Botany* for August-October :—Two new genera of Compositæ, *Psednotrichia* and *Adenogonium*, from Africa, by W. P. Hiern ; the Mosses of Cheshire, by J. A. Wheldon ; a new genus of Ericaceæ from Angola, *Ficalhoa*, by W. P. Hiern ; critical notes on some species of *Cerastium*, by F. N. Williams ; new species of *Crassula*, by S. Schönland and E. G. Baker ; the Flowering Plants of Novaya Zemlya, by Colonel H. W. Feilden.—Mr. W. Whitwell establishes the occurrence of *Botrychium matricariæfolium*, and of its subspecies (or distinct species) *lanceolatum*, as British plants. In their Notes on Freshwater Algæ, Messrs. W. and G. S. West propose the establishment of a new genus *Stipitococcus*, near to *Perionella*.

SOCIETIES AND ACADEMIES.

MANCHESTER.

Literary and Philosophical Society, October 4.—J. Cosmo Melvill, President, in the chair.—The President referred to the loss sustained by the Society through the deaths of Mr. H. M. Ormerod, Dr. R. M. Pankhurst, Dr. James Rhodes, and Mr. John Wright, ordinary members ; and of Prof. Ferdinand Cohn, Lord Playfair, and Mr. Osbert Salvin, F.R.S., honorary members.—Mr. H. W. Freston exhibited a male specimen of *Asagena phalerata*, an extremely rare species of spider which by itself represents the genus *Asagena*, whose nearest congener is the genus *Steatoda*. The present individual is the only male that has been found, at all events in recent years. The habitat of this species has hitherto been unknown, but it would seem now that it is a simple Theridion snare in grass amongst rocks. The most striking features of the genus are a denticulated edge to the cephalothorax, and a denticulated socket in the front of the abdomen, forming a stridulating apparatus which would produce a squeaking noise when rubbed against the rough hinder edge of the thorax.—Mr. John Butterworth read a paper on the structure of some fragmentary specimens of a new *Psaronius*, which he concluded to be the roots of *Heterangium tilioides*. The special feature of these roots was the presence of a distinct secondary thickening, which is unknown in the other species of *Psaronius*.—In a second paper, Mr. Butterworth dealt with the presence of a leaf-sheath surrounding the nodes of some of the Calamites of the Lancashire Coal-measures. Such a sheath has not been described before from British Calamites.

PARIS.

Academy of Sciences, October 17.—M. van Tieghem in the chair.—On an old alloy, by M. Berthelot. The alloy contains copper, lead, and small quantities of tin and zinc. The oxidation has taken place in such a manner, that removal of the external coating of rust shows an apparently reddish metal underneath, probably cuprous oxide. From its external appearance the metal might have been taken for pure copper.—Physiological researches on the contraction of the *sphincter ani*, by MM. S. Arloing and Edouard Chantre. Experimental results confirming the conclusions arrived at in a previous paper with regard to the existence of a number of sensitive fibres in the two symmetrical nerves of the sphincter.—On the resultant of two equations, by M. P. Gordan.—On differential equations of the second order with fixed critical points, by M. Painlevé.—On the variation of dielectric constants with temperature, by MM. H. Pellat and P. Sacerdote. Measurements were carried out with paraffin and ebonite at temperatures varying between 11° and 33°. The dielectric constant of paraffin diminishes with rise of temperature, that of ebonite, on the other hand, increasing on warming.—On the duration of emission of Röntgen rays, by M. Henri Morize. The rays from a Crookes' tube were allowed to fall through a narrow slit upon a photographic plate, the latter being rapidly rotated at a constant known velocity. The effect of rotation would be to widen the photographic image of the slit if the time of emission were appreciable. The results obtained were in general agreement with those of M. Colordeau, several images of the slit being formed, separated by equal intervals for each discharge in the primary in the coil, corresponding to successive discharges in the tube. The average duration of total emission was about one-thousandth of a second. On a new action undergone by light in traversing certain metallic vapours in a magnetic field, by MM. D. Macaluso and O. M. Corbino. A ray of polarised sunlight is passed through a sodium flame placed in an intense magnetic field, then successively through a second nicol and a cylindrical lens, is then received on a concave Rowland grating, and the second diffraction spectrum observed through a micrometer eye-piece. Under these conditions, on completing the circuit round the electromagnet parallel, bands appear on each side of the two D lines, which are displaced on rotating the analyser, the axis of each ray following the direction of the current. A lithium flame exhibits similar phenomena, but not so well marked as with sodium.—On a new hydrated chromium oxide, by H. G. Baugé. The new hydrate, which has the composition $Cr_3O_4,3H_2O$, is obtained by the action of boiling water upon the carbonate in the absence of air.—Action of sodammonium upon arsenic, by M. C. Hugot. The single product of the reaction is $AsNa_3$.—Researches on double iodides and borates, by M. H. Allaire. A mixture of a borate and a metal is heated in iodine vapour. The double salts obtained in this way were of the type $6RO.8BO_3RI_2$, where R = Mg, Zn, Cd, Mn, Ni, Co, or Fe.—On the solubility of camphor, by MM. C. Istrati and A. Zaharia. Camphor is appreciably soluble in water, the camphor in solution affecting the density, and having a perceptible rotation. The solubility is much greater in aqueous hydrochloric acid, a chlorhydrin perhaps being formed. The solubility in the latter case appears to diminish with rise of temperature.—Researches on incandescent lamps charged with an explosive mixture of methane and air, by MM. H. Couriot and J. Meunier. The glowing filament of an incandescent lamp was allowed to come in contact with an explosive mixture of marsh gas with air, under varying conditions. In no case did an explosion take place.—On the transformation of fat by direct oxidation, by M. Hanriot. Fat, treated with ozonised oxygen, gained considerably in weight ; in one case as much as 23 per cent. No reducing substance appeared to be formed, tests for sugar, starch, cellulose, formic and oxalic acids giving uniformly negative results. The products of the oxidation appear to be chiefly fatty acids.—On the cause of the spiral structure of the roots of certain Chenopodiaceæ, by M. Georges Fron. The asymmetrical structure, which gives the fibrovascular bundles in a transverse section the appearance of a double spiral, is caused by the mechanical compression of the cotyledons in the radicle.—On *Blepharopoda jauriana*, by M. E. L. Bouvier.—Anatomy and physiological functions of the arborescent organs or aquatic lungs of some Holothuria, by M. L. Bordas. These organs appear to have numerous functions, as they are concerned in breathing, moving, in excretion, and in the production of numerous amæbocysts.—The pegmatic and granulitic lodes of the rock masses in contact with the granite of Ariège, by M. A. Lacroix.—On the circulation of water in the Rhône glacier, by M. F. A. Forel. Fluorescin was introduced at various points, and the times which elapsed before its appearance in the main torrent noted. The velocities found were of the same order as those for the free stream, whence the conclusion is drawn that in the interior of the glacier the water circulates without stopping in basins, reservoirs, or lakes, and hence there is no sub-glacial lake under the Rhône glacier.—Results obtained in an experimental balloon ascent on August 23, by MM. G. Hermite and G. Besançon. The curves obtained from the self-registering baro-thermograph were unusually good, the greatest height registered being 7300 metres, with a corresponding minimum temperature of −60° C.

NEW SOUTH WALES.

Linnean Society, August 31.—Mr. E. G. W. Palmer in the chair.—Contributions to a knowledge of the fauna of British New Guinea. No. i. Communicated by T. Steel. This communication consists of a number of papers by various authors describing a collection sent to Mr. Steel from Fife Bay, New Guinea, by the Rev. H. P. Schlencker. The only form new to science is a snake described by Mr. J. Douglas Ogilby as *Dendrelaphis schlenckeri*. Mr. T. Whitelegge notes the occurrence of a shrimp, *Palaemon affinis*, not previously recorded for New Guinea. Amongst the lizards, *Gehyra oceanica*, *Gymnodactylus pelagicus* and *Lepidodactylus lugubris* are recorded, apparently for the first time from New Guinea, by Mr. A. H. S. Lucas, while several other species, including the interesting form *Homolepida englishi*, described in 1890 by De Vis, are now recorded for the second time —New genera and species of fishes, by J. Douglas Ogilby. In this paper there are described as new a xiphiodontid, two species of silurids, a genus of plotosids, two pleuronectids, and a small fish, the position of which is uncertain.—On the Echinoderm fauna of New Zealand, by H. Farquhar. The Echinoderm fauna of New Zealand, as at present known, comprises two Crinoids, sixteen Ophiuroids, twenty-eight Asteroids, twenty-three Echinoids, and twenty-one Holothurians: total, ninety species. It is not homogeneous, nevertheless it contains a large number of peculiar forms which give it a strongly distinct character of its own. Its affinities are strongest with that of Australia. Omitting doubtful and deep-water forms, fifty-eight per cent. of the known species are endemic, thirty-six per cent. occur in Australia, and only six per cent. have been found elsewhere and not in Australia.—Notes on the subfamily *Brachyscelinæ*, with descriptions of new species, Part v., by W. W. Froggatt.—Descriptions of six new species of Mollusca, by John Brazier.—A contribution to a knowledge of the Arachnidan fauna of New Guinea, by W. J. Rainbow. In this paper sixty-eight species are enumerated, and of these fourteen are described as new. The most interesting specimen of the collection is a species of the family Aviculariḍæ, for the reception of which a new genus, *Antrochares*, is proposed. This makes the third known genus of the six-eyed Aviculariḍæ.—Descriptions of the eggs and nests of four species of Australian birds, by Alfred J. North.

AMSTERDAM.

Royal Academy of Sciences, September 24.—Prof. Van de Sande Bakhuyzen in the chair.—Prof. Bakhuis Roozeboom communicated the results of a theoretical inquiry into (1) the phenomena occurring during the congealation of a mixture of two substances, when during the process "mixed" crystals exclusively are formed, which may either be continuously mixable or not so; and (2) the changes which the solid mixture may undergo, when the two components on further cooling are transformed into other stable modifications.—Prof. Haga communicated that the phenomena of "maxima and minima of brightness as a consequence of an optical delusion," mentioned by himself on behalf of Dr. Wind at the meeting in May, were alreadyknown and described by E. Mach in the *Wiener Berichte*, II. Abth. Bd. 52, 54 and 57.

DIARY OF SOCIETIES.

FRIDAY, OCTOBER 28.

PHYSICAL SOCIETY, at 5.—An Influence Machine: W. R. Pidgeon.—The Repetition of an Experiment on the Magneto-optic Phenomenon discovered by Righi: Prof. S. P Thompson, F.R.S.—The Magnetic Fluxes in Meters and other Electrical Instruments: Albert Campbell.

TUESDAY, NOVEMBER 1.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Address by W. H. Preece, C.B., F.R.S., President, and Presentation of Medals and Prizes awarded by the Council.

WEDNESDAY NOVEMBER 2.

ENTOMOLOGICAL SOCIETY, at 8.

THURSDAY, NOVEMBER 3.

CHEMICAL SOCIETY, at 8.—A Determination of the Equivalent of Cyanogen: George Dean.—Note on the Action of Light on Platinum, Gold, and Silver Chlorides: E. Sonstadt.—Methanetrisulphonic Acid: E. H. Bagnall.—A Composite Sodium Chlorate Crystal in which the Twin Law is not followed: W. J. Pope.—On the Composition of American Petroleum: Dr. Sydney Young, F.R.S.—(1) On the Separation of Normal and Iso-heptane from American Petroleum; (2) On the Action of Fuming Nitric Acid on the Paraffins and other Hydrocarbons: Dr. F. E. Francis and Dr. Sydney Young, F.R.S.—On the Boiling Points and Specific Gravities of Mixtures of Benzene and Normal Hexane: D. H. Jackson and Dr. Sydney Young, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Practical Mechanics: S. H. Wells (Methuen).—University College of North Wales, Calendar for the Year 1898-99 (Manchester, Cornish).—Naturae Novitates (Berlin, Friedländer).—Key to Algebraical Factors: D. H. Vachha, 3rd edition (Longmans).—On the Instincts and Habits of the Solitary Wasps: G. W. and E. G. Peckham (Madison, Wis.).—Quantitative Exercises for Beginners in Chemistry: A. H. Mitchell, Part 1, 2nd edition; Ditto, Part 2 (Reading, National Publishing Association).—Aids in Practical Geology: Prof. G. A. J. Cole, 3rd edition (Griffin).—Almanaque Nautico, 1900 (San Fernando, Gay).—British Museum: a Guide to the First and Second Egyptian Rooms (The Trustees).—Handbook of Insects injurious to Orchard and Bush Fruits, with Means of Prevention and Remedy: E. A. Ormerod (Simpkin).—First Stage Inorganic Chemistry (Practical): Dr. F. Beddow (Clive).—Gas and Petroleum Engines: translated and edited by A. G. Elliott (Whittaker).—Through Asia: Sven Hedin, 2 Vols. (Methuen).—An Elementary Text-Book of Botany: Prof. S. H. Vines (Sonnenschein)—Bibliotheca Geographica, Band iv. (Berlin, Kühl).—Gesammelte Botanische Mittheilungen: S. Schwendener, 2 Vols. (Berlin, Gebrüder Borntraeger).—Die Moderne Entwicklung der Elektrischen Principien: Prof. F. Rosenberg (Leipzig, Barth).—Übersicht der Lepidopteren: Fauna des Grossherzogtums Baden: C. Reutti, Zweite Ausgabe herausgegeben von Meess und Spuler (Berlin, Gebrüder-Borntraeger).

PAMPHLETS.—On the Forestry Conditions of Northern Wisconsin: F. Roth (Madison, Wis.).—Antarctic Exploration: a Plea for a National Expedition: Sir C. R. Markham (R. G. S.)

SERIALS—Proceedings of the Liverpool Geological Society, Part 2, Vol. viii. (Liverpool)—U.S. Department of Agriculture: Division of Biological Survey, Bulletin Nos. 9, 10, 11 (Washington).—Scottish Geographical Magazine, October (Edinburgh).—Journal of the Franklin Institute, October (Phil[a].).—Quarterly Review, October (Murray).—Zoologist, October (West).—Journal of Anatomy and Physiology, October (Griffin).—Geological Survey of Canada Report, Nos. 627, 628, 651, 657 (Ottawa).—Bulletins de la Société d'Anthropologie de Paris, 1898, Fasc. 2 (Paris, Masson).—Mémoires de la Société d'Anthropologie de Paris, Tome ii (3[e] série), 2[e] Fasc. (Paris, Masson).—Journal of the Chemical Society, October (Gurney).—Bulletin of the American Museum of Natural History, Vol. xi. Part 1 (N.Y.)—Sitzungsberichte der K. Akademie der Wissenschaften, Math.-Naturw. Classe: Anatomie, &c., 1897, January to July, October to December; Ditto, Mineralogie, &c., 1897, January to July, October to December; 1898, January to May; Ditto, Mathematik, &c., 1897, January to July, October to December; 1898, January and February; Ditto, Chemie, 1897, January to July, October to December; 1898, January to March; Ditto, Register zu dem Banden, 101 to 105 (Wien, Gerold).—An Illustrated Manual of British Birds: H Saunders, 2nd edition, Parts 9 to 12 (Gurney).—Journal of the Royal Horticultural Society, October (117 Victoria Street).—Bulletin of the American Mathematical Society, October (N.Y., Macmillan).—Agricultural Gazette of N.S.W., August (Sydney). Monthly Weather Review, July (Washington).

CONTENTS. PAGE