by Lister and others, some species exhibit viviparous reproduction and the budding-off of young individuals. The viviparous young are formed inside the parent shell and emerge by the dissolution of the base. The process demands a sacrifice of the whole of the protoplasm and of the internal septa, whereas in zoospore-production the shells of the young are formed outside the parent, from material derived from the surrounding medium, and not from the internal septa. Another method of multiplication is to bud-off a young individual from the shell-aperture of the parent. To this process and to the occasional (possibly fortuitous) mingling of the extruded protoplasm of two or more shells, is generally attributable what has been described as "plastogamy" in the Foraminifera.

In regard to shell-making, Mr. Heron-Allen has some remarkable evidence to submit of a quality which he calls "intelligence," or "an apparent development of purpose." The Foraminifer may select out of a large supply of possibilities one particular kind of material, such as sponge-spicules, minute flakes of mica, Echinoderm plates, and it may use this material in a purposive way. Thus in *Technitella legumen* "the whole shell wall consists of two distinct layers of spicules, an outer layer in which the spicules are all laid down with their long axes parallel to the long axis of the test, and an inner layer of spicules laid with their long axes at right angles to the outer layer, giving as close an approximation to the woof and warp of a textile fabric as is possible with a rigid non-flexible material such as sponge-spicules. It is obvious that by the crossing of these two layers the strength and resistance of the test to strain is enormously increased."

Or again, while *Marsipella cylindrica* forms a long and very friable tube of broken sponge-spicules, "it was left for *M. spiralis* to make the same great discovery as did the prehistoric genius who invented string—it has clearly realised that a twisted yarn is stronger than an untwisted wisp of fibre." The author maintains that "the Foraminifera exhibit the highest functions and the most 'intelligent behaviour' of which undifferentiated protoplasm has been observed to be capable."

MATHEMATICAL AND PHYSICAL SCIENCE AT THE BRITISH ASSOCIATION.

THE Section of Mathematical and Physical Science met under the presidency of Sir F. W. Dyson, whose address on the construction of the heavens appeared in NATURE of September 9.

Radio-active Elements and the Periodic Law.

Following the address, a discussion on radio-active elements and the periodic law took place. The opener, Prof. F. Soddy, explained that the discovery of a large number of radio-active disintegration products seemed at first difficult to reconcile with the periodic table of the elements; for it was clear that the existing gaps would not provide for more than a few of them. But it had been found that among them there were only three new separable elements —radium, polonium, and actinium; the others were isotopes of known elements, *i.e.*, they had identical chemical properties, although differing somewhat in atomic weight. The fundamental discovery, which brought order amongst these diverse products, was that when an α particle was expelled a shift of two places to the left in the table took place, whilst the expulsion of a β particle caused a shift of one place to the right. Since an α particle carries two positive charges, and a β particle one negative charge, this suggests that position in the periodic table is a function of the charge. Moseley's work has extended this by showing that it is true from end to end of the periodic table. Another fundamental fact is Rutherford's discovery of the nucleus of the atom, which was detected by experiments in scattering. We arrive at the conclusion that isotopes have the same *net* nuclear charge, though the gross number of positive and negative charges differs. Isotopes cannot be separated by chemical means, and hitherto diffusion methods have been unsuccessful. In view of the numerous isotopes of lead the question of the variability of the atomic weight of lead derived from different minerals becomes important; variations from 206·05 (uranium lead) to 207·67 (thorite lead) have been found. The new view of the periodic table is that it is a relation between chemical character and nuclear charge, not between character and mass; and it is possible now to state that there are exactly 92 elements up to uranium (counting isotopes as one element), of which 86 are known. We seem to be returning to the view of the Greeks and alchemists that elements are qualities, in contrast to the later view that elements are constituents.

Dr. N. Bohr pointed out that the dimensions of the nucleus are so small compared with the outer rings of electrons, that the nuclear constitution would have negligible effect on the electric field in these outer parts, only the net charge being important. Consequently, properties depending on the outer rings of electrons would be the same for all isotopes. In the case of spectral vibrations, there occurs a small term depending on the mass of the central nucleus, and accordingly we ought to look out for a small but perceptible difference between the spectra of two isotopes. Dr. F. A. Lindemann gave a theoretical argument to show that you could not have identical chemical *and* physical properties when the atomic weight differed. If the chemical properties were the same, certain physical properties must differ, and *vice versâ*. Mr. A. Fleck compared the effects of taking away charges from uranium, (*a*) by reduction (uranous salts), and (*b*) by disintegration (UrX salts). Dr. Whytlaw Gray described experiments on minute quantities of RaD, showing that it answered the chemical tests for lead. He thought it would not be difficult to observe its melting point directly. Sir E. Rutherford said that it was surprising how simply the whole system of thirty-four new products had been absorbed in the periodic table. In one case we have seven isotopes, all radio-active except one (Pb). Those which show radio-activity are distinguishable from one another by that property. Several references were made to the loss of Mr. H. G.-J. Moseley, killed at the Dardanelles, to whose researches this subject has owed so much.

The Order of Stellar Evolution.

On Thursday morning, Prof. A. Fowler opened a discussion on spectral classification of stars and the order of stellar evolution. He described the order of the types of stellar spectrum, which, according to the Draper notation, form the sequence O, B, A, F, G, K, M, and pointed out that this sequence has come to be regarded not merely as a convenient mode of description, but as actually representing successive stages of evolution. The spectra present striking evidence of a continuity extending from one end to the other of the sequence, and there are links connecting the earliest type, O, to the gaseous nebulæ. The temperatures of the stars decrease in the order of the sequence from upwards of 10,000° C. for the B stars to 3000° C. for the M stars, and at the same time the colour changes continuously from white to red. Additional evidence in support of the sequence is obtained from laboratory researches; from types G to M, the stars show spectral lines characteristic of the "arc"; from B2 to F the "spark" lines are shown; and in the earliest divisions O to B1 the lines are for the most part unattainable in the laboratory, except by the most powerful electric discharge. These last may be described as "super-spark" stars. The evidence fully establishes a physical continuity corresponding to the Draper sequence. Prof. Fowler concluded by referring to the theory of Sir Norman Lockyer, and to Prof. Russell's hypothesis, which is closely akin to it. According to these, the stars fall into parallel series of ascending and descending temperature respectively, so that the hottest stars (types B and O) come midway in the order of evolution.

Sir Frank Dyson set forth the evidence, based on the observed luminosities and densities of the stars, which led Russell to the view that the red stars represent both the earliest and latest stages of evolution. It seems necessary to believe that some of the M stars are extremely diffused bodies, and others very dense. The Draper sequence gives the order of temperature; but the order of evolution is that of increasing density, not necessarily that of decreasing temperature. Prof. Eddington pointed out that the actual calculations of stellar density in particular cases compel us to admit that two stars having the densities respectively of water and of air can yet show the same type of spectrum; this seems to remove one of the chief objections brought against Russell's theory. The adoption of this theory would, however, play havoc with the regularity of many statistical results which have hitherto seemed orderly and intelligible. Father Cortie referred to the changes of a spectrum in a Nova, which seem to show that in these cases the nebular and type O spectra come, not before, but after the B and A stages. Prof. Nicholson and Mr. Merton discussed the nature of the Wolf-Rayet spectrum (type O). In his reply, Prof. Fowler pointed out a serious difficulty of Russell's hypothesis, that we have no celestial spectra which can be regarded as bridging the gap between primordial nebulous matter and the intensely bright giant stars of type M.

Thermionic Emission.

On Friday Prof. O. W. Richardson opened a discussion on thermionic emission. He said that he would confine his remarks to the emission of negative ions from hot bodies, which seemed to be an intrinsic property, whereas the emission of positive ions was not permanent and could be traced to impurities. The emission of negative ions increases rapidly with temperature according to the formula $C=AT^{\frac{1}{2}}e^{-b/T}$, where T is the absolute temperature. It was found, however, by H. A. Wilson and by Langmuir that the constants depend greatly on the experimental conditions. The temperature-law would follow theoretically from purely physical considerations. On the other hand, a similar law may be deduced if the effect is due to chemical action. In the experiments of Haber and Just the emission of electrons from alkali metals was observed under the action of water vapour and other agents. This is the only known case of the kind, and the distribution of energy among the emitted electrons suggests that it is something different from thermionic emission. On the question of the distribution of energy among the electrons during chemical action, Prof. Richardson had gained the impression from his experiments that it did not conform to Maxwell's law, but was more analogous to the laws governing the photoelectric effect; the facts of thermionic emission require Maxwell's law. As the best possible test between the physical and chemical theories, he had recently conducted experiments on a tungsten wire with special precautions against impurities; the emission was found to be much too great and persistent to be accounted for by residual gaseous impurities. He concluded that the action was not chemical, nor could it be a photoelectric action of the temperature radiation; and the physical theory seemed the most satisfactory.

Dr. F. A. Lindemann discussed the bearing of the experiments on the chain theory of electric conduction. Mr. E. Newbery criticised the use of tungsten in the crucial experiment on account of its great chemical activity at high temperature. Dr. J. A. Harker discussed the bearing of experiments on the electric arc under high pressure.

Miscellaneous Papers.

There was no subdivision of the Section this year, and the papers selected for reading were taken in full Section. Technical subjects were therefore avoided, and even the mathematical papers were such as would appeal to the members generally. Mr. G. H. Hardy greatly interested his audience with a paper on prime numbers. He gave an historical survey of the investigation of the distribution of primes, with particular reference to the theorem that the number of primes less than x approaches asymptotically to $x/\log x$ for large numbers. This theorem was first conjectured by Legendre, but was not proved until 1896. An explanation of the importance of the Riemann-ζ function in this connection was given. The paper is to be printed *in extenso* in the report. Prof. A. N. Whitehead, in a paper on space, time, and relativity, gave an account of the philosophical difficulties connected with space and time so far as they concern mathematicians. His remarkably simple method of arriving at the fundamental equations of the principle of relativity was especially valuable. Mr. A. A. Robb and Mr. H. R. Hassé took part in the discussion, the former explaining his method of logical development of the subject based on the idea of points arranged in "conical order."

Prof. W. H. Bragg's account of X-rays and crystal structure showed the power of the new methods of determining the arrangement of the atoms in a crystal. Instead of attempting to summarise this remarkable paper, we may refer the reader to Prof. Bragg's Bakerian lecture in the *Phil. Trans.* (vol. ccxv., p. 253), which covers similar ground. Prof. J. C. McClennan gave an account of his production of single-line spectra of cadmium and zinc, showing that the wave-lengths of the lines are connected with the ionisation potential by a relation depending on the quantum theory. Sir J. Larmor discussed the decomposition of the irregular vibrations constituting white light into regular trains of waves, when a grating or prism is used; for the prism, he gave an explanation based on the difference between wave-velocity and group-velocity in a dispersive medium.

Meteorology was represented by a paper by Mr. F. J. W. Whipple on the mechanism of cyclones, in which an account was given of the observed distribution and pressure at different heights, and the dynamical connection between the pressure-gradient and the inflow and outflow of air was discussed. In presenting the report of the committee on seismology, Prof. H. H. Turner stated that the work of plotting on a map the earthquake epicentres observed by Milne had now been completed. The epicentres were found especially on two great circles cutting at right angles. He referred also to improvements made in the Milne seismograph by Messrs. J. J. Shaw and J. H. Burgess.

The meeting concluded on Friday afternoon with a very interesting address by Prof. Pierre Weiss on new views of magnetism, in which he described his researches on the part played by the magneton, or definite unit of magnetism, in the phenomena of iron, nickel, cobalt, and their alloys.

THE BRITISH ASSOCIATION.

SECTION K

BOTANY.

OPENING ADDRESS[1] BY PROF. W. H. LANG, F.R.S., PRESIDENT OF THE SECTION.

Phyletic and Causal Morphology.

I PROPOSE to deal with some aspects of the study of plant-morphology. In doing so I shall not accept any definition of morphology that would separate it artificially from other departments of botany. I regard the aim of plant-morphology as the study and scientific explanation of the form, structure, and development of plants. This abandons any sharp separation of morphology and physiology, and claims for morphology a wider scope than has been customary for the past fifty years. During this period the problem of morphology has been recognised as being "a purely historical one," "perfectly distinct from any of the questions with which physiology has to do," its aim being "to reconstruct the evolutionary tree." The limitation of the purpose of morphological study, expressed in these phrases from the admirable addresses delivered to this section by Dr. Scott and Prof. Bower some twenty years ago, was due to the influence of the theory of descent. I fully recognise the interest of the phyletic ideal, but am unable to regard it as the exclusive, or perhaps as the most important, object of morphological investigation. To accept the limitation of morphology to genealogical problems is inconsistent with the progress of this branch of study before the acceptance of the theory of descent, and leaves out many of the most important problems that were raised and studied by the earlier morphologists.

In the history of morphology, after it had ceased to be the handmaid of the systematic botany of the higher plants, we may broadly distinguish an idealistic period, a developmental period, and a phyletic period. The period of developmental morphology, the most fruitful and the most purely inductive in our science, was characterised by an intimate connection between morphological and physiological work. Among its contributions were studies of development or "growth histories" of whole plants and their members. These were carried out, in part at least, in order to investigate the nature of development, and such general problems found their expression at the close of the period in the "Allgemeine Morphologie" of Hofmeister. The "Origin of Species" took some years before it affected the methods and aims of botanical work. Then its effect on morphology was revolutionary, and, as in all revolutions, some of the best elements of the previous *régime* were temporarily obscured. This excessive influence of the theory of descent upon morphology did not come from Darwin himself, but from his apostle Haeckel, who gave a very precise expression to the idea of a genealogical grouping of animals and plants, illustrated by elaborate hypothetical phylogenetic trees. Such ideas rapidly dominated morphological work, and we find a special "phylogenetic method" advocated by Strasburger. The persistence of the phyletic period to the present time is shown, not only in the devotion of morphology to questions of relationship, but in the attempts made to base homologies upon descent only. Lankester's idea of homogeny can be traced to the influence of Haeckel, and nothing shows the consistency of phyletic morphology to its clear but somewhat narrow ideal so plainly as the repeated attempts to introduce into practice a sharp distinction between homogeny and homoplasy.

Prof. Bower, in his address last year and in other papers, has dealt illuminatingly with the aims and methods of phyletic morphology. I need only direct attention to some aspects of the present position of this, which bear on causal morphology. The goal of phyletic morphology has throughout been to construct the genealogical tree of the vegetable kingdom. In some ways this seems farther off than ever. Phyletic work has been its own critic, and the phylogeny of the genealogical tree, since that first very complete monophyletic one by Haeckel, affords a clear example of a reduction series. The most recent and trustworthy graphic representations of the inter-relationships of plants look more like a bundle of sticks than a tree. Consider for a moment our complete ignorance of the inter-relationships of the Algæ, Bryophyta, and Pteridophyta. Regarding the Algæ we have no direct evidence, but the comparative study of existing forms has suggested parallel developments along four or more main lines from different starting-points in a very simple unicellular ancestry. We have no clue, direct or indirect, to the ancestral forms of the Bryophyta, and it is an open question whether there may not be as many parallel series in this group as in the Algæ. The Pteridophyta seem a better case, for we have direct evidence from fossil plants as well as the comparison of living forms to assist us. Though palæobotany has added the Sphenophyllales to the existing groups of vascular cryptogams and has greatly enlarged our conceptions of the others, there is no proof of how the great groups are related to one another. As in the Bryophyta, they may represent several completely independent parallel lines. There is no evidence as to what sort of plants the Pteridophyta were derived from, and in particular none that relates them to any group of Bryophyta or Algæ. I do not want to labour the argument, but much the same can be said of the seed-plants, though there is considerable evidence and fairly general agreement as to some Gymnosperms having come from ancient Filicales. The progress of phyletic work has thus brought into relief the limitations of the possible results and the inherent difficulties. As pointed out by Prof. Bower, we can hope for detailed and definite results only in particularly favourable cases, like that of the Filicales.

The change of attitude shown in recent phyletic work towards "parallel developments in phyla which are believed to have been of distinct origin" is even more significant. Prof. Bower spoke of the prevalence of this as an "obstacle to success," and so it is if our aim is purely phyletic. In another way the demonstration of parallel developments constitutes a positive result of great value. Thus Prof. Bower's own work has led to the recognition of a number of series leading from the lower to the higher Filicales. By independent but parallel evolutionary paths, from diverse starting-points in the more ancient ferns, such similarity has been reached that systematists have placed the plants of distinct origin in the same genus. In these progressions a number of characters run more or less clearly parallel, so that the final result appears to be due "to a phyletic drift that may have affected similarly a plurality of lines of descent." This conclusion, based on detailed investigation,

[1] Abridged by the author.