From this table it will be seen that, while the largest proportion of capital expenditure in the county and non-county boroughs, &c., is met by loans, the main source of supply of the County Councils is still the Residue grant. The extent of the assistance rendered by voluntary effort is also clearly indicated, as well as that given by the Science and Art Department; the smaller urban districts appear to have received the largest contributions from these two sources.

THE BRITISH ASSOCIATION.

BRISTOL MEETING.

SECTION K (BOTANY).

OPENING ADDRESS BY PROF. F. O. BOWER, SC. D., F.R.S., PRESIDENT OF THE SECTION.

SHORTLY before we met last year in the hospitable Dominion of Canada, two biologists, whose work relates to the questions I propose to discuss to-day, passed away. In both cases their services to science had received honourable recognition in this country. Johannes Japetus Smith Steenstrup, who had been for more than thirty years a foreign member of the Royal Society, died June 20, 1897, at the advanced age of eighty-four; Julius von Sachs, also a foreign member of the Royal Society, died May 29, 1897, aged sixty-five.

The former of these, a zoologist, was probably best known in this country for his work on "Alternation of Generations," a translation of which was published by the Ray Society in 1845. The title-page describes the phenomenon as "a peculiar form of fostering the young in the lower classes of animals." Botanists should remember that this term "alternation," which they often use in a sense peculiarly their own, was originally applied to the course of development in certain animals, by Chamisso in 1819. The first general statement of the subject from the zoological side was by Steenstrup in the work already named; even there no mention is made of such phenomena in plants, until the concluding paragraph, where there is an allusion in very general terms to the course of events in the life of seed-bearing plants. But when we remember that it was only in 1848 that Suminski discovered the antheridia and archegonia borne upon the prothallus of a Fern, we see plainly that Steenstrup could not have used the term "alternation" in the sense in which it is now generally applied to plants. The interest for us as botanists will therefore be that Steenstrup suggested in his work on alternation in animals how in the life of plants successive phases exist, and that these are comparable to those which he described in many animals.

The work of Sachs, on the other hand, has influenced every one of us. Some, including myself, have had the great advantage of his direct personal guidance; all must have derived pleasure as well as profit from his writings. I shall not here attempt any general summary of the achievements of this great man, for that has been done efficiently by the scientific press at large. I shall merely allude to one feature of his work, viz. the style of its presentment to the reader. He was always clear, usually concise. He was, in addition to his power as an investigator, a master with the pencil, as well as with the pen. It was this combination of qualities which made him the great text-book writer of his time. Never perhaps has a volume more fairly reflected the position of a science at the moment of its publication than did that of Sachs. It resembles the work of a snap-shot camera, and, like any instantaneous photograph of life in motion, it has fixed and perpetuated awkward positions. The morphological system of the time was stiff and unpromising; the text-book accurately depicted this, but it did not suggest or anticipate future developments; it did not bear the softened image of a longer exposure; it presents to us the angular attitude of a moment.

The powers of Sachs as a writer found their best scope in his "History of Botany," a work which will always retain its value as a masterly exposition of the results of very wide reading, arranged with a literary skill which is unfortunately rare among scientific men. I lay stress upon this power of Sachs as a writer, apart from his record as an investigator, because he was strong where so many of us are weak. The truth is that little effort is made by men of science to use a concise and transparent style; for the most part we write by the aid of such instincts as nature has given us; few cultivate composition. But it should, I think, be impressed upon the young aspirant that, when he writes, it is one of his first duties to consider his readers' convenience; he must use all endeavours to convey forcibly the result of his inquiry, but to make the least possible demand upon the patience of his readers. I should like to see certain papers selected as models of construction, to be studied as such by all candidates for our higher degrees; we should naturally include in the list those of the best masters of style in foreign languages, and among them would rank the late Julius von Sachs.

THREE PHASES OF MORPHOLOGICAL STUDY.

It will be in your memory that the Address of last year's Sectional President was largely devoted to branches of our science which touch the material and economic interests of man. It was pointed out to us how certain fungal diseases diminish agricultural profits to an extent which may be estimated in millions of pounds yearly. Beneficent microbes were also mentioned, such as those which govern the aroma and maturing of butter and cheese; these and many others, the study of which lies properly within the province of botany, affect not only the health, but, at the most varied points, the comfort and prosperity of mankind.

It is unnecessary for me to dwell further upon these matters, or to urge again the utilitarian argument for the proper support of botany. I propose, on the other hand, to invite your attention this morning to the Morphology of Plants. This is a department of science pure and simple. The results which it brings have not, and cannot be expected to have, any money value in the markets of the world. The present time is one of unusual bustle and change in morphology, consequent upon the discovery of new facts and the introduction of new methods. The development of the study may be divided into three periods, we ourselves standing upon the threshold of the third. The earliest phase was that of description and delineation of what might be observed of the mature form of plants; this includes the work of the herbalists and of the earlier systematists, who thus furnished the basis for classification. It is true that the mere description was enriched at times by comparisons made, but these often took a capricious form, as is shown by the many curious allusions which still survive in the nomenclature. Erasmus Darwin satirised the imaginative comparisons indulged in by early writers in his "Loves of the Plants"; an instance of this is seen in his lines referring to the legendary organism, half animal, half plant, suggested by the peculiar form of *Dicksonia* (*Cibotium*) *Barometz*:—

> "Cradled in snow and fann'd by arctic air
> Shines, gentle Barometz, thy golden hair.
> Rooted in earth each cloven hoof descends,
> And round and round her flexile neck she bends;
> Crops the gray coral moss, and hoary thyme,
> Or laps with rosy tongue the melting rime.
> Eyes with mute tenderness her distant dam,
> Or seems to bleat, a *Vegetable Lamb*."

The tendency to comparison thus already perceptible asserted itself strongly in the next phase of our study, to which it gave its character. And now the need arose for observing development; this was initiated by Schleiden, and carried to a triumphant climax by Hofmeister. Passing from the hands of these pre-Darwinian to those of post-Darwinian writers, the comparisons, while remaining virtually the same, received a new significance. Observers now pushed their inquiries into the details of anatomical structure and development, and in many cases attached an importance beyond what is justifiable to minute similarities or differences of cell-cleavage. Thus what might be called "cellular morphology" became a feature of the period. It has, however, been in a measure discredited by the excessive zeal of some of its votaries, who drew large conclusions from slight facts; a salient example of this is furnished by studies concerning segmentation of the ovum. But we must not assume that because it has been pursued indiscreetly, the study of segmentation is effete; there is still scope for valuable observation, which will bear a reasonable burden of argument; though conclusions from such a source must be compared with those derived from other data, and a due estimate of them must be made accordingly.

Morphology has lately passed to a third stage—that of experiment—with a view to ascertaining the effect of external agencies in determining form, and the limits of variability under varied circumstances. Development of itself shows only how a part originates; it does not demonstrate what it is, nor what it may become under special conditions. This new and growing phase of experimental morphology, together with comparison

from the point of view of descent, now tends to supersede the formal morphology of the second period, which in many minds implied or assumed ideal types or creative plans. It has become a general view that the facts of morphology are but the stereotyped facts of physiology, form being determined by function, but under the check of heredity. This third experimental phase of the study of plant-form is directed, as it were, to the very setting of the types, before the stereotype plate is cast. We watch nature's compositor at work, but we also ascertain that the plate itself, after it is cast, is much more plastic than some of us had thought.

These three phases of morphological inquiry have naturally overlapped one another; we recognise, however, that first description, then formal comparison, and now experiment, have been the leading features in morphological investigation during these successive periods.

HOMOLOGY.

The ideal aimed at in the study of the morphology of plants is to trace their real relationships and mode of origin, on the basis of the widest observation—in short, to reconstruct the evolutionary tree. In order to make comparison possible, or at least manageable, a terminology is necessary, and this not only of the plants themselves, but also of their parts. We may for the moment leave on one side that summing up of morphological opinion represented by the systematic arrangement of plants in a taxonomic system. I propose to-day to discuss not the classification of plants, but the classification of the parts of plants, their grouping according to their *homology*. And here I use a word which is probably explained to every class of elementary students; it is one of those terms a meaning of which is indeed revealed to the babes of the science, while those who teach are not at one as to its definition. We need not enter now into the various opinions which have been held on this point, nor need we make any antiquarian research into the introduction or early use of the word *homology*; it will suffice to state that it was already firmly established in the science before views as to descent gave it any intelligible meaning. We speak of the homologies recognised by Hofmeister, but it should be remembered that their great discoverer did not put an evolutionary interpretation upon them. Sachs points out in his history how "the theory of descent had only to accept what genetic morphology had already brought to view." Nevertheless, much remained ingrained in the very texture of the science which was incompatible with evolutionary thought. This was so even in the text-book of Sachs itself. The categories of root, stem, leaf, and hair are there laid down, and the parts classed under these several heads were held to be *homologous*. In their definition all those characters which refer to function were put aside, the definitions relating to origin and relative position; the reproductive organs were grouped with the rest, with the result that these parts were described as bearing a varying morphological value. But this purely formal morphology is now dead; it long survived a mere passive belief in evolutionary views, but their active practice has strangled it. The first step towards emancipation was the recognition of sporangia as parts *sui generis*. Eichler, agreeing with Braun and Strasburger, found it "highly probable according to the theory of descent" that such a structure as the ovule has universally the same morphological dignity. It remained for Goebel to make the general statement that sporangia stand in a category by themselves, and are probably not the result of modification of any vegetative part. It was in this way that the phylogenetic factor was first asserted as bearing on a question of importance in the morphology of plants. Adherents of descent no longer passively accepted the direct results of investigation; they began actively to check and control the interpretation of them; but this position was not attained till more than twenty years after the publication of Darwin's "Origin of Species." Since then, however, views as to descent have taken an increasingly important place in the province of morphology, till at the present moment a far-reaching comparison of allied forms, assisted by experiment, is the most potent instrument in the hands of the morphologist.

But various writers admit in varying degree this factor of comparison as controlling other considerations. There is indeed a wide range of difference on this point. I will cite only two extreme views. On the one hand is the view of Strasburger, which he enunciated so early as 1872. The enthusiasm for evolution in the Jena school found its botanical expression in the aphorism, "The highest problem of morphology is to explain the form of plants, but this problem can only be solved genealogically." This statement is repeated in a more definite form in Strasourger's text-book: "Phylogeny is thus the only real basis for morphology."

At the other extreme is the method of physiological organography put forward by Sachs in his Lectures. I am aware that he subsequently modified his views; I merely quote the system which he propounded in 1882, as being the antithesis to that of Strasburger. For in the physiological organography descent is hardly taken into account at all; parts which are plainly of distinct origin by descent are classed together. This organography of Sachs, though introduced with all its author's charm of style, never convinced the botanical world, for it treated plants too much as the creatures of present circumstance. It may be taken as illustrating the extreme reactionary swing of the pendulum from the non-physiological attitude of the formal morphologists; a protest against the exclusion of function from the morphological arena. The protest was salutary, but its form was extravagant.

Let us now consider whither "phylogeny, as the only real basis of morphology," may lead us. Let us take as our provisional view that *homology* in the strictest sense implies repetition of individual parts, in successive generations, just as the hand of the child repeats in position and qualities the hand of the mother. Though among seed-bearing plants, for instance, this repetition may apply for the plant-body as a whole, it will be at once apparent that such repetition as regards the individual is found in comparatively few cases in plants. The continued embryology of all the higher forms, the indefinite number of the parts successively produced, and the variety in detail of their arrangement show that in the strictest sense repetition of individual parts cannot be traced. In a pan of seedlings of the Sunflower, raised from seed of the same parent, the cotyledons in all cases may be regarded as homologous in the strictest sense, as they correspond in origin, number, position, and form to like parts in the parent. In a similar way the first root of the seedling appears to be individually identical with the first root of the parent, or of any other seedling of the batch. In those plants in which a foot or suspensor is present occupying a constant position with regard to the parts of the embryo, it will not be doubted that within near lines of affinity the foot in any one specimen corresponds to that of any other. The exact repetition which is thus found to exist may be regarded as the most complete type of homology.

Starting from this repetition of individual parts in plants nearly related, there is a divergence by gradual steps in two directions: Firstly, in the individual plant, where the later formed parts may assume forms and positions which may even raise a question of their essential correspondence. Thus in the batch of Sunflower seedlings there may be a varying number of leaves, with varying transition from the decussate to the alternate arrangement, intervening between the cotyledons and the capitulum. As they vary in number and position these cannot in the strictest sense be accepted as individually comparable, each to each by descent—the lineal representatives of like individual parts in the parent. The lateral roots also, though all essentially similar, do not correspond each to each, either in number or in position.

Again, to go a step further, a Fern prothallus produces antheridia and archegonia; their number and position are not uniform; by conditions of culture we have them under control, and can induce antheridia only, or we can induce a formation of archegonia upon the upper surface, where they are usually absent. Plainly these cannot be held severally as the exact representatives of like individual parts in a previous generation. Another exceptional, but most interesting, case is that of *Aspidium anomalum*, Hk. and Arn., which Sir William Hooker remarks is possibly an abnormal form of *Aspidium* (Polyst.) *aculeatum*, Sw. In this Fern the sori, instead of being all on the lower surface, as in allied Ferns, are often upon the upper surface of the leaf. There is no sign of torsion to explain the anomaly, while the sori themselves present no structural peculiarity except that they are sometimes quite destitute of indusium. There has doubtless been a transfer of developmental capability from the usual position of the sori to the anomalous one. In case of such transfers as these we do not doubt that the parts in question are to be ranked as comparable to those in the normal position; we contemplate here,

as in the case of the Sunflower leaves, an essential correspondence, but not an individual repetition of the parts, and we learn that parts thus essentially corresponding to one another may be transferred to unusual positions.

Secondly, in plants more or less nearly related, those which are less akin may show so slight a similarity in detail that again questions of the essential correspondence of the parts may arise. Within nearer circles of affinity these questions will affect only the appendages of minor importance, which show less constancy of occurrence and arrangement, such as emergences and hairs; but in case of plants less nearly akin the degree of correspondence of the larger members may become a matter of debate. Take, for instance, the three great phyla of living Pteridophytes, the Ferns, Equiseta, and Lycopods. While the sporophyte as a whole in each of these may be accepted as homologous by descent with that of the others, the question as to the true correspondence by descent of the leaves must still be open for discussion. It is a tenable view that the three phyla arose separately from a non-foliar ancestry, and that the assumption of a foliar development, having in each case a different habit, and a different relation to the sporangia, led to the distinctiveness of the three stocks. Opinion on the point of homology by descent of the leaves of these Pteridophyta must at present remain in suspense; but the case is different with the leaf of Pteridophytes as compared with the leaf of Bryophytes: unless the whole morphological system of the time be in error, we shall be right in maintaining that these foliar developments have been distinct in origin from the first.

Now all the foliar parts above quoted would in a system of merely formal morphology fall into the category of "leaves." But if phylogeny be accepted as the only real basis of morphology, we must be prepared to split up the category based on mere time, place, and mode of origin, and to recognise in some cases repetition of individual parts; in others essential correspondence, but not individual repetition, owing sometimes to transfer of developmental capability; in other cases again, a possibility of distinct origin by descent not actually proved; and lastly a reasonable certainty of distinct origin. The practical question for the morphologist is, having recognised these facts for himself, how is the matter to be best made intelligible to others?

A reconsideration of the term "homology" will thus be necessary; is it to be applied equally to such parts as are connected by lineal descent, and also to those which we have good reason to believe have resulted from parallel development in quite distinct phyla? Or, to put a finer point upon our inquiry, are we to distinguish in any way the cases of "individual repetition" from those of "essential correspondence"? In the latter case I think no good end would be served at present by accentuating this distinction by terms: the steps of divergence are so slight and gradual. None the less should it be clearly borne in mind that comparisons of parts commonly ranked as homologous in the plant body are based on a less complete individual correspondence than that of parts usually compared in the animal body.

But the case is different in dealing with parallel developments, and some doubt arises whether parts which probably, or it may be certainly, have arisen by separate evolutionary sequence in distinct phyla are to be classed as homologous in the same sense as those directly related by descent. This question was long ago taken up on the zoological side by Prof. Ray Lankester, and it was shown that the old word "homology" covered two things recognised as distinct from the point of view of descent. He defined as *homogenous* "structures which are genetically related, in so far as they have a single representative in a common ancestor." On the other hand, "when identical or nearly similar forces or environments act on two or more parts of an organism which are exactly or nearly alike: further, if, instead of similar parts in the same organism, we suppose the same forces to act on parts in two organisms, which parts are exactly or nearly alike, and sometimes homogenetic, the resulting correspondences called forth in the several parts in the two organisms will be nearly or exactly alike. . . . I propose to call this kind of agreement *homóplasis* or homóplasy." Now this distinction of terms requires also to be observed in plant-morphology, and I am surprised that it has never yet been adopted by botanists, though we have long recognised cases of parallel development. I do not propose now to spend time in assigning these terms to familiar cases: but to take the examples already cited, the leaf of a Fern would be homoplastic, though not homogenetic with the leaf of a Moss; or, taking examples from plants more nearly akin, it would appear possible that the leaves of the three distinct phyla of living Pteridophytes show merely homoplasy, not a true homogeny.

The successive foliage leaves of most plants are assumed in the individual to be the result of a mere repetition of development. But it is quite a possible view that in the plant-body (as is contemplated in the animal in those cases of "serial homology" which Lankester recognises as homoplastic) homoplasy may have had a place. We must inquire whether all those structures which we designate "leaves" have actually been the result of a development identical, or at least essentially similar as regards their origin in the race. The problem is, given a plant with numerous leaves of various form and function, to unravel the real story of their evolution. Two distinct factors may be contemplated as possibly occurring even in the individual, viz.:

(1) *Homogeny* of genetically related parts, with or without repetition of the parts formed.

(2) *Homoplasy*, an origin of two or more distinct categories of parts, not genetically related, on the same organism.

Working upon either of these, and thus complicating the problem by obliterating such distinctions as may have existed at first, may be the phenomenon of *metamorphosis*. This has lately received its evolutionary definition at the hands of Prof. Goebel, as restricted to those cases where there has been an obvious change of function. We see how change of function accounts for various forms of leaf in certain cases; but it does not follow that all leaf-forms on the same plant were so produced, by metamorphosis of a single original type.

The Lycopodineae are particularly interesting in illustration of this point. It appears probable that *Phylloglossum* is a more primitive type than other living Lycopods; it has two kinds of leaf, the protophylls borne in irregular number and arrangement on the protocorm, and the sporophylls of different form from these, and arranged regularly on the strobilus: commonly there are no intermediate steps between them. This condition in a plant, which on general grounds of comparison we believe to be primitive, is certainly interesting, and we shall ask whether the two types of leaf have not arisen by distinct evolutionary sequence? In the genus *Lycopodium* there are certain species, such as *L. Selago*, which show alternately sterile and fertile zones; examining the limits of the sterile zones, we find at the base of each leaf an atrophied sporangium, similar in position to that borne by a sporophyll. When we compare this condition with that of *Phylloglossum* it appears probable that the successive zones are the result of a metamorphosis of a strobilus, which had a continuous apical growth, and unlimited repetition of sporophylls, but that some of these suffered atrophy of their sporangia, with the correlative effect of a larger vegetative development. A differentiation of the strobilus thus results in the plant as we see it, a production of foliage leaves by sterilisation of sporophylls. Recognising this, some may suggest that the protophylls originated in the same way. It is possible that they did; but it is equally possible, and, in view of the peculiar case of *Phylloglossum*, I think more probable, that in these plants we have an example of homoplastic development of parts distinct as to descent, while the limits of the two still evident in *Phylloglossum* became obliterated in the more complex case of *Lycopodium*. The proof of the point will be difficult or even impossible, but the eyes of botanists should certainly be open to recognise such individual homoplasy, should it occur, and to inquire whether it has really had a place in plant-development.

Returning now to homoplastic development in distinct groups of plants, the morphology of the *foot* provides interesting material for comparison, and especially so since there is no question of repetition here; for the comparison is between parts of which only one appears on each individual plant.

The term foot has been applied to that part of the embryo in Pteridophyta which serves to connect it physiologically with the prothallus; the term has also been used for the base of the seta in Bryophytes. Parts performing a similar function, but not referable, as in other Phanerogams, to the metamorphosis of cotyledons, are also found in *Gnetum* and *Welwitschia*.

In the Bryophyta what is usually called the foot is no definitely specialised structure; it is merely the absorbent base of the seta. It would appear probable that in the Bryophyta a true homogeny holds in all cases, as the requirement for it will have been

uniform ; and its basal position is also uniform, though some difference of detail does appear in the relation of this absorbing body to the first segmentation of the embryo.

In the Pteridophyta it is exceedingly difficult to be sure of the correspondence by descent of the foot in distinct types, and indeed it should not be assumed that a specialised absorbent organ was always present, though general surface-absorption will naturally have taken place in all archegoniate embryos ; indeed the condition of some upright embryos is such that a foot would never have been described, were it not for comparison with other types. In *Equisetum*, *Isoëtes*, *Botrychium*—all forms without a suspensor, and with an upright growing embryo —the hypobasal half of the embryo, with or without a root, is absorbent as in the Bryophyta, and is described as a foot ; it is quite possible to see in them the continuation of a primitive absorbent organ. This may also be the case in the Marattiaceae, and it is specially noted by Campbell that "in *Marattia* all the superficial cells of the central region become enlarged and act as absorbent cells for the nourishment of the embryo." From such types we may imagine the more specialised foot of the Leptosporangiate Ferns to have been derived by a localisation of the absorbent function on one side only, which would be a natural consequence of the embryo taking the prone, in place of the vertical position.

A different course of events probably occurred in the Lycopodineæ. I am disposed to think that here the suspensor represents nothing more than a specialised part of the primitive absorbent organ ; this seems to be indicated by the details as shown in Treub's figures of *L. cernuum* and *L. Phlegmaria*, in which the suspensor is continuous with the foot. But what is then the "foot" of *Selaginella*, which is quite apart from the suspensor, the root intervening? On this point I think we obtain light from *Welwitschia* and *Gnetum*, for in these we see an absorptive organ formed at a comparatively late period ; and it corresponds in position and function, though not in time of origin or details of structure, with that of *Selaginella*. I conclude that the "foot" of *Selaginella* is probably a later formation, not comparable as regards descent either with the foot of *Lycopodium*, or with the "feeder" of *Welwitschia* or *Gnetum*. The latter are plainly of recent independent origin, as comparison shows, and their actual position is defined according to the position of the seed in germination. Probably, then, there is homoplasy in such cases, not true homogeny.

Similarly with such structures as the pinnae, stipules, indusium, corona, and still more so with such inconstant bodies as emergences and hairs ; when we speak of the "homologies" of these parts it is rarely the *homogeny*, or identity by descent, which we mean to express ; usually it is only *homoplasy*, a comparison of parts similar, it may be, in form and position, or even in development and function, though not shown to be comparable by descent.

ALTERNATION.

But the questions above discussed are mere matters of detail, compared with that great enigma of the alternation of generations in green plants, or of alternation at large. This is, after all, a question of degree of homology, not now of the parts only, but of the whole plant or "generation." How this greatest of all adaptations was really initiated, we cannot expect to bring to the point of demonstration ; at best we can only venture opinions of probability. Still this discussion commands at present more widespread interest among botanists than any other in the sphere of plant morphology.

There was a time when the attempt was made to reduce all plants to one scheme as regards their life-cycle, a method which not only prevented elasticity of theory, but was responsible for some unfortunate comparisons. It was characteristic of the period when the text-book of Sachs reigned supreme ; we find it there definitely laid down that "the doctrine of alternation has the object of reducing to one scheme the main phases of the life of all plants which bear sexual organs." But the controversy between Pringsheim and Celakovsky had, as one of its results, the recognition of various types of life-history, not of one scheme only. The tendency at present is towards the opposite extreme ; the frequency of the parallel developments now recognised has led some to accept a comprehensive polyphyletic view as regards alternation, and wherever difficulties of comparison arise, to take refuge in the plausible suggestion that the organisms compared represent altogether distinct lines of descent. But the view which should be confidently upheld, is that even where this may actually be the case useful comparisons may yet be made ; and that the method of progress within one phylum may illustrate the probable mode of progress in another. The green Algæ may thus throw light upon the probable origin of the sporogonium in the Bryophytes, though they may in no sense be in the line of their descent ; the Bryophytes may suggest valuable ideas for the comparative study of the Pteridophytes, though they may not represent their actual ancestry.

It is the alternation as seen in these green plants that I propose to discuss. Writers have distinguished various types of alternation, including under the term divers modes of "alternation of shoots" ; and it should be remembered that this was the original sense of the word alternation as applied by Steenstrup. But gradually the issue in the case of green plants has been simplified, and the question now centres round that alternation of phases which some of us describe as "antithetic," while others believe the phases to be really "homologous" as regards their origin.

Briefly put, the question is, How was the first start made? Has the neutral generation or sporophyte been the result of change of any other part of the sexual generation than the zygote itself? If so, the alternation is of *homologous* generations ; if not, then the alternation is what is styled *antithetic*. The whole discussion is like a purely historical inquiry, but with the minimum of documentary evidence ; for on this point the fossils give scanty help. In the absence of more direct evidence we are thrown back on other aguments, such as those based on comparison of normal specimens, and secondly upon the study of abnormalities. I shall not attempt to treat the matter exhaustively ; it will, however, be necessary for me to deal with certain points in the discussion which were raised in the able address of Prof. Scott at Liverpool. He there restated Pringsheim's view of homologous alternation as against the antithetic. I propose now to consider three matters which I think are most material to the discussion, viz. (1) the bearing of the Algae and certain Fungi on the question ; (2) the comparison from the Bryophyta ; and (3) the argument from abnormalities.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—At the conjoint examination for entrance scholarships, just completed, the following awards have been made in Natural Science. At Pembroke College : Scholarship, G. H. Delf, Camberwell Grammar School, 40*l*. At Gonville and Caius College : Exhibition, M. M. L. Rittenberg, Tonbridge School, 30*l*. At Jesus College : Scholarship, J. Hewitt, Derby School, 40*l*. At Christ's College : Scholarship, C. H. B. Epps, City of London School, 40*l*. ; exhibition, R. B. S. Sewell, Weymouth College, 30*l*. At St. John's College : Scholarship, G. C. E. Simpson, Mill Hill School, 60*l*. ; Lupton and Hebblethwaite Exhibition (open *pro hac vice*), J. F. Hough, Mason University College, Birmingham ; Johnson Exhibition (open *pro hac vice*), B. E. Mitchell, Brighton Grammar School. At Emmanuel College : Scholarship, H. U. B. Banham, Ipswich Grammar School, 40*l*. ; exhibition, A. C. H. Rothera, Market Bosworth School, 30*l*. Clare College : Scholarships of 60*l*. to E. B. Bailey, Kendal Grammar School, and W. Cartwright, Middlesburgh Grammar School. Trinity College : Minor Scholarship of 75*l*. to C. S. Coles, University College, London ; Exhibitions of 40*l*. to J. Frame, Mason College, Birmingham ; C. W. Hutt, St. Paul's School, London ; T. C. James, Aberystwyth University College ; H. Lambert, Perse School, Cambridge.

Mr. F. G. Hopkins has been appointed University Lecturer in Chemical Physiology.

The degree of LL.D. will be conferred on Lord Kitchener of Khartoum on November 24.

The Clerk Maxwell Studentship in Experimental Physics will be vacant at the end of this term. Candidates, who must have worked in the Cavendish Laboratory, are to send their names to Prof. Thomson by December 9.

It is proposed that Advanced Students shall be admitted to Part II. of the Mechanical Sciences Tripos, and that for the B.A. degree they shall be required to attain the standard of the Second Class at least.

The General Board of Studies have proposed a scheme for the establishment of an Allen Research Studentship under the