developed into upland running types, which competed with the Horses and Ruminants of the plains; while others were more likely frequenters of marshes and river-banks, like many of the Rhinoceroses of the present day. Neither the Hyracodonts or the Amynodonts ever developed horns, and all the early species of true Rhinoceroses had weak, hornless nasal bones, so that in external appearance they were probably more like large-sized Tapirs than the well-armed animals with which we are now familiar.

"They did not interfere with each other," writes the author, "because each enjoyed a different local habitat while occupying the same general geographical regions. The Hyracodonts dwelt in the drier grassy plains. The Amynodonts frequented the river and lake borders. Up to the time of the extinction of these two related families, the true Rhinoceroses maintained a somewhat uniform structure, both in Europe and America, differing so far as we know in size rather than in proportions. Their dentition and their feeding habits were probably similar to those of the *R. bicornis* of Africa, and the *R. sumatrensis* and *R. sondaicus* of Asia, namely upon shrubs, leaves, and softer herbage. After the extinction of the rival families, however, there was naturally a tendency on the part of the true Rhinoceroses to enter the peculiar local habitats previously occupied by the Hyracodonts and Amynodonts, and they accordingly diverged into upland and lowland, short and long-limbed, brachydont and hypsodont types."

From this it will be evident that Prof. Osborn by no means confines himself to the dry details considered sufficient by so many palæontologists, but endeavours to give his readers a mental picture of the habits of the animals he so well describes. He next proceeds to show that the *Rhinocerotidæ*, or true Rhinoceroses, diverged into four sub-families. These are, first, the *Aceratheriinæ*, or Hornless Rhinoceroses; second, the *Diceratheriinæ*, or Transversely-horned Rhinoceroses; third, the *Rhinocerotinæ*, or typical Rhinoceroses; and, fourth, the *Elasmotheriinæ*, represented only by the huge *Elasmotherium* of Siberia. And he further shows that while the first and second of these, like the Hyracodonts and Amynodonts, are common to the Old and New Worlds, the third and fourth are exclusively Old World types.

In the New World the Rhinoceroses became entirely extinct at the close of the Miocene period; and this, although it is not mentioned by the author, is doubtless the reason they never penetrated into South America, which up to that date was cut off from North America. No reason can at present be assigned for the sudden extinction of the group in North America, seeing that a profusion of animals, adapted apparently for a warm climate, flourished there during the Pliocene; while the case of the Woolly Rhinoceros and the Elasmothere indicates that the Rhinoceroses themselves were capable of fitting themselves to withstand sub-arctic conditions.

Whether the group first originated in the Eastern or the Western Hemisphere, the author, perhaps wisely, refrains from discussing. In both regions they appear to have come into existence at approximately the same period; and in both, up to a certain stage, they seem to have undergone a parallel development. This, as in the case of the Horses, would seem to suggest that during the middle portion of the Tertiary epoch the connection between the Old and the New Worlds was much more extensive than a mere narrow bridge across Bering Strait. But, on the other hand, the existence of large groups like the Civets and Hyænas which never succeeded in travelling from the Eastern to the Western Hemisphere, is, so far as it goes, in favour of only a narrow connection in high latitudes.

As already mentioned, the author includes all the typical Rhinoceroses in a single sub-family or group. On p. 84 this group is correctly termed *Rhinocerotinæ*, but in the table on p. 121 it is renamed *Ceratorhinæ*, which is obviously wrong. As with the *Aceratheriinæ*, the author considers that the group may be divided into a Dolichocephalic and a Brachycephalic section. The former section is taken to include all the Pliocene and Plistocene Old World species, with the exception of the Pikermi *R. pachygnathus*; while the latter embraces the Miocene and recent Old World types, except the living *R. sumatrensis*. To this classification we must take one exception. In our opinion the African "White Rhinoceros" (*R. simus*) is as dolichocephalic as the Plistocene *R. antiquitatis*. The figure of the skull of the former, which the author has copied from some previous writer, is misleading; and if he had the opportunity of seeing the fine series of specimens in the British Museum, he would in all probability amend the statement.

Space prevents detailed notice of the interesting observations which the author gives on the evolution of the cheek-teeth in the group. It may, however, be observed that he is in accord with previous writers in regarding the white and woolly Rhinoceroses as presenting the culminating point of molar evolution among the typical Rhinoceroses; *Elasmotherium* representing a still more specialised offshoot by itself. At present we are left in some degree of doubt as to the author's views with regard to the generic or subgeneric divisions of the Plistocene and recent Rhinoceroses; but light will probably be thrown upon this point as the work proceeds. So far as they have been carried at present Prof. Osborn's labours afford, in the main, a distinct advance in our knowledge of a very interesting group, and the completion of his memoir will be anxiously awaited by all who have made the subject a special study. R. L.

## *THE BRITISH ASSOCIATION.*

### BRISTOL MEETING.

### SECTION K (BOTANY).

OPENING ADDRESS BY PROF. F. O. BOWER, SC.D., F.R.S., PRESIDENT OF THE SECTION.[1]

II.

I. *Algae and Fungi.*

AT first sight those Algae and Phycomycetous Fungi which show a subdivision of the zygote appear to offer the key to the enigma of the first start of antithetic alternation, and such rudimentary fruit-bodies as those of *Oedogonium* and *Coleochaete* are frequently quoted as prototypes of sporogonia. My own position has been that they may be "accepted as suggestive of similar progress in the course of evolution of Vascular Plants." On the assumption that the zygote is equivalent in all cases—and this is itself a pure assumption—the fruit-body of such Algae or Fungi would be comparable to the sporophyte in higher forms; but it must be clearly remembered that it is not even then proved to be *homogenetic*. Dr. Scott has based a strong line of criticism of antithetic views upon these cases. He remarks: "The sudden appearance of something completely new in the life-history, as required by the antithetic theory, has, to my mind, a certain improbability. *Ex nihilo nihil fit.* We are not accustomed in natural history to see brand new structures appearing, like morphological Melchisedeks, without father or mother. Nature is conservative, and when a new organ is to be formed it is, as every one knows, almost always fashioned out of some pre-existing organ. Hence I feel a certain difficulty in accepting the doctrine of the appearance of an intercalated sporophyte by a kind of special creation."

In answer to this, I state that to me the zygote, from which our hypothesis starts, is not "nothing"; it is a cell with all the powers and possibilities of a complete cell. Vöchting, in his "Organbildung," has fairly concluded that "a living vegetative cell which is capable of growth has not a specific and unalterable function." I have myself demonstrated that cells typically sporogenous may develop as vegetative tissue, and conversely that tissues normally vegetative may on occasions become sporogenous. We may, therefore, say generally as regards the sporophyte, that "a living cell which is capable of growth has not a specific and unalterable function." This I conceive to have been the condition of the zygote, and of its early products.

I think that the words "intercalation" or "interpolation," as used by writers on antithetic alternation, have been quite misunderstood. I have contemplated no sudden development—indeed, on the first page of my "Studies" I have spoken of the sporophyte as "gradually" interpolated. Nor is the suggested development something "completely new," for I specially speak of elaboration of the zygote. This is the parent of these "morphological Melchisedeks"; and unless segmentation be held to be synonymous with "special creation," I confess I do not see where the initial difficulty arises. I agree that nature is conservative; what we contemplate is the fashioning of the sporophyte by a process of which the first step is segmentation, out of a pre-existing organ—the zygote. Such simple segmentation is seen in the case of certain Algae and Fungi, and these may be taken as suggesting how the sporophyte of the Archegoniatae may have come to be initiated. But I am not aware of having ever suggested that these segmented zygotes of Algae are the homogenetic prototypes of the more elaborate sporophytes.

[1] Continued from p. 69.

Dr. Scott further states that "the reproductive cells produced by the ordinary plant of an *Oedogonium* are identical in development, structure, behaviour, and germination with those produced by the oospore." Prof. Marshall Ward, also speaking of *Œdogonium*, remarks "the attempt to get over this by terming asexual spores borne by the gametophyte *gonidia*, and reserving the term *spore* for bodies indistinguishable from these gonidia by any morphological or physiological character whatsoever, beyond their origin from a so-called sporophyte, carries its own refutation." Now, as a matter of fact, Pringsheim's description and figures of *Oedogonium* give scanty details; in most of the germinating zygotes the nuclei themselves are not clearly shown; much less the details of behaviour of those nuclei on germination. Klebahn has described the fusion of the sexual nuclei in *Oedogonium*, but I am not aware that he, or any one else, has yet made detailed observations on the nuclear condition of the zoospores, or the changes which take place in the germinating egg. Till this is done I submit that it is premature and undesirable to make such assertions as those of Dr. Scott and Prof. Ward. We now know that important nuclear changes do take place on the germination of the zygotes of certain Algae and Fungi. These changes are connected with a division of the nuclei into four, which is the number of the zoospores usually produced on germination in *Oedogonium;* the details may differ, but in the zygotes of *Closterium* and *Cosmarium*, and in the formation of the auxospores of *Rhopalodia*, Klebahn has demonstrated this division into four; also Chmielewsky has described a similar production of four nuclei in the germinating zygotes of *Spirogyra*. When it is further stated that in some of these cases there is good reason to think that a reduction of chromosomes is connected with the division into four, just as a reduction is now known to accompany the tetrad division in Archegoniate and Phanerogamic plants, it is plain that such cases as that of *Oedogonium* ought not to be assumed to support an homologous view without any fresh observation of the facts.

With the whole question of alternation, the nuclear details and differences in number of the chromosomes on division are now intimately bound up. Though the observations are still few, so far as they go they are consistent with the generalisation first stated by Overton, and elaborated by Strasburger as regards the Archegoniate and Phanerogamic plants. It has now been seen in cases drawn from various groups, that the cells of the gametophyte show a certain number ($n$) of chromosomes, while those of the sporophyte show on nuclear division double that number ($2n$) of chromosomes. Since Section K has had the advantage of a statement on this subject from Prof. Strasburger himself at Oxford, and as Dr. Scott also discussed the matter at Liverpool, I need not enlarge. I shall only remind you that Strasburger took up the position that the number of chromosomes which appears in each sexual nucleus is that original number which the ancestors possessed in a pre-sexual period; while the reduction of the double number which results from sexual fusion is, in his opinion, to be regarded as an atavistic process. As far as investigation has yet gone, I see nothing to prevent the acceptance of this as a provisional theory.

It is now well known, however, from the observations of Farmer and of Strasburger, that the nuclear conditions of *Fucus* are peculiar; that the reduction only takes place on the formation of the sexual organs themselves, and that the *Fucus* plant, like a sporophyte in the Archegoniate series, has the double number of chromosomes. At first sight this might appear to be a fatal difficulty, and Dr. Scott, attributing to the adherents of the antithetic theory views from which I personally dissent, has landed them in a seeming *reductio ad absurdum*. He himself does "not think we are as yet in a position to draw any morphological conclusions from these minute differences, interesting as they are." But we need not accept either of these extreme positions, if only a certain elasticity of theory be maintained, which should come naturally to adherents of polyphyletic development. I think the difficulty will chiefly be felt by those who, like some of the earlier writers on alternation, attempt to reduce all plants which show sexuality to one stiff scheme; this has been found to fail in the case of alternation, and a healthy recognition of various types of alternation has been the consequence. So in the matter of chromosomes, and of the position which the event of reduction holds in the life-cycle; difficulties such as this in *Fucus* may be anticipated, if we assume that all plants will conform to one plan. But Strasburger has not considered it necessary to cast aside the nuclear details as a basis for morphological conclusions, because all plants investigated do not fall in with a preconceived scheme. On grounds of comparison of behaviour of the nuclei before and after conjugation in *Closterium*, *Cosmarium*, *Spirogyra*, in certain Diatoms, and finally in *Actinophrys*, he has arrived at the conclusion "that a shifting (Verschiebung) of the time of division into four, together with reduction, is possible in the history of development of organisms." It will doubtless be necessary later to put a precise meaning upon the word "Verschiebung," and to define how far in given cases it is to be understood as an actual shifting of the event within one line of descent, how far it merely expresses an initial difference maintained, or it may be, extended, in different lines. Meanwhile, those who accept Prof. Strasburger's position will see that while in various evolutionary sequences the reduction may take place at different points in the cycle, still it may have settled down to a fixed and constant position in any one sequence; that I conceive to have been the case for the Archegoniate series. The validity of this conclusion does not seem to me to be affected by the diverse state of things seen in so far removed a sequence as that of the brown Algae.

Here a brief reference must be made to the very beautiful results of Wager on the changes in the zygote of *Cystopus candidus*, which have been verified and extended by Berlese. Wager states that in this fungus the process of fertilisation does not differ in any essential particular from the process as it takes place in Angiosperms. On the division of the fusion-nucleus of the zygote the number of the chromosomes present before division appears to be considerably in excess of the number observed in the nuclei of the oogonium. "By counting as carefully as possible 20 to 24 or even more appear to be present, and the impression is produced that the number is certainly much larger than that observed in the oogonium." Divisions of the nucleus then follow to form 4, 8, 16, and finally 32, in which condition a period of rest ensues; and finally, it appears that a division of each into four follows, to form the nuclei of four spores. Wager believes the reduction to take place at this last division, and Berlese has established a strong probability that such a reduction actually does take place. Plainly these observations are not final or conclusive, and even if they were, the strict homogeny of this fruit-body with a rudimentary sporophyte of a green plant would not be proved. It must, however, rank at least as an important parallel case, illustrating how the reduction may be effected in a distinct line of descent.

We see then that in green Algae such as *Oedogonium*, *Sphaeroplea*, and *Coleochaete*, certain divisions follow fertilisation, but we are not yet in possession of the nuclear details. I prefer, therefore, to suspend judgment as to the nature of those divisions; but in view of the peculiar behaviour already seen in other zygotes, it may be distinctly anticipated that some form of reduction will be demonstrated at that stage. If that be shown then we shall be right in recognising in these small cell-bodies the rudimentary correlative of a sporophyte—the sort of beginning from which a neutral generation may have sprung in land-living plants. We cannot go further than this as regards the green Algae until we are in possession of the facts. There is no greater desideratum in morphology at the present moment than a detailed knowledge of the germination of zygotes such as that of *Oedogonium*.

Here I may remark that the admirable observations of Prof. Klebs, whom the Section will welcome as a distinguished guest, do not appear to me to touch this question. His very varied and convincing experiments show in a number of Algae and Fungi that, as regards the succession of vegetative and sexual modes of propagation, the experimenter has a very complete control. I do not find, however, any observations of his which touch the behaviour of germinating zygotes of green Algae as regards details of segmentation. I do not mention this as in the least impairing the brilliancy of Prof. Klebs's work, but because Prof. Ward has brought Klebs's results to bear upon the discussion on antithetic alternation in a manner which I do not think that the facts will support.

### II. *Bryophyta.*

Turning now to the Bryophytes, these plants stand at the moment in a somewhat discredited position. We have been warned by Dr. Scott that "there is no reason to believe that the Bryophyta, as we know them, were the percursors of the Vascular Cryptogams at all," and that "there is no appreciable resemblance between the fruit of any of the Bryophyta and the plant of any Vascular Cryptogam," and the suggestion has been thrown out afresh that they may really be "degenerate descendants of higher forms."

In view of statements such as these it may be well to examine the Bryophyta quite separately, without reference to Vascular Plants at all, and see what are their main bearings on theories of alternation. And if the Bryophytes were the only Archegoniate Plants in the world, I think the case for their origin by a progressive antithetic alternation would be an uncommonly strong one; the points which are especially noteworthy are: (1) The readiness with which they may be arranged in natural sequences which illustrate increasing vegetative complexity of the sporophyte as a consequence of progressive sterilisation; (2) The nuclear details, which are as yet known, however, in only few cases; (3) The constancy of the two alternating phases, the relations of which are very seldom disturbed by apospory, and never, to my knowledge, by apogamy.

The first of these matters has been dealt with at length in my "Studies." It is, of course, possible for any one to read such sequences as are there mentioned in reverse order, and to uphold a theory of simplification; but this must be shown to be in accordance with probability. Now it appears to me that the general probability in the case of the Bryophytes is against simplification, for the larger the number of spores which can be matured the greater the probability of survival; even in cases where, as in *Buxbaumia* and *Diphyscium*, there is an exiguous, and probably reduced Moss-plant, the sporogonium is not of a reduced type, but, on the contrary, unusually large. It seems to my mind much more probable that the Bryophytes as a whole illustrate a course of progressive complexity. A comparison of anatomical details frequently suggests a progressive sterilisation, a process which we see demonstrated both in Pteridophytes and Phanerogams, where actual conversion of potentially sporogenous tissue into temporary or permanent vegetative tissue does occur. When it is added that the nuclear evidence, scanty though it still is, shows the sporophyte with a double number of chromosomes, and the reduction taking place on the tetrad division of the spores, the comparison with the segmented zygotes of Algae and Fungi above mentioned seems inevitable. The position of those who hold views of antithetic alternation will, therefore, be that the simple sporogonium was produced as a post-sexual growth. The starting-point was probably some such multicellular body as we see nowadays in certain Algae and Fungi resulting from division of the zygote, but not necessarily homogenetic with any such body that we know now living. The land-habit imposed a restriction on fertilisation, and an alternative method of increase in numbers was an advantage. The multicellular body resulting from division of the zygote provided the means for this; the cells developed separately as dry, dusty spores. As the number of divisions increased, the powers of the plant to nourish, protect and disseminate the spores became the measure of the number produced. Hence followed the elaboration of the nourishing and disseminating mechanism, which has involved a diverting of some cells from their first office of spore-production, the start being, perhaps, made in a manner similar to the formation of the peridium in the Uredineæ. To my mind—taking the Bryophyta alone—there is an inherent probability in all this, which far counterbalances any of the obstacles which have been raised against it.

The greatest obstacle is the fact of apospory in Mosses. This departure from the usual alternation will be more generally discussed in relation to the Ferns, where it is more frequent. Besides its being artificially induced in Mosses by special treatment, it appears also to have been noted by Ugo Brizi in nature, in the case of atrophied capsules of *Funaria*, which had buried themselves in the soil. The essential point is the production of the sexual generation by direct vegetative growth from the neutral. This would appear to involve a reduction of chromosomes, but Pringsheim's drawings show nothing analogous to the usual process of tetrad division to form the spores; the reduction, if it occurs, must be effected in some other way.

A theoretical suggestion on this point will be made later. Meanwhile let us estimate its probable importance as regards the Bryophyta. It cannot fail to strike the observer how uniform is the alternation in these plants; there are, I believe, no recorded cases of deviation from the normal alternation in Liverworts. I know of only a single case of apospory among Mosses taken in the open, and then in atrophied capsules; apospory, when induced, follows such extreme treatment as chopping the sporogonium into pieces. And it is not as if the Mosses and Liverworts had escaped detailed observation; hardly any group of plants had been more carefully examined by competent observers. Deviations from strict alternation then are rare, and appear under physiological stress. This great group, which includes the simplest sporophytes among Archegoniate plants, is also singularly constant in its alternation. I think this is to be connected with the permanently dependent condition of the sporophyte; its equable physiological condition, nursed and protected by the Moss plant, finds its morphological expression in its comparative uniformity. Conversely the independent position of the sporophyte in Ferns, and its exposure to varied conditions may have elicited more freely in them unusual developments.

### III. *Abnormalities.*

And now I may pass to my third point, and discuss more generally the argument from abnormalities. I have no wish to prejudge the question by the use of this term as applied to apogamy and apospory, or in any way to detract from their morphological importance—I merely intend to express that they are departures from that order of events which is the most frequent in Archegoniate plants at large, and I particularly wish to point out that while such irregular developments are now shown to be frequent in Ferns, they are exceedingly rare in Bryophytes, and are not, I believe, hitherto recorded for Lycopodineae or Equisetineae.

While direct vegetative transitions from one generation to the other may appear as a *primâ facie* support of an homologous origin of the two generations, I must protest against their being used, as they have been, as evidence against an antithetic view. It has been said that the facility with which these transitions from one generation to another in Ferns take place "shows that there is no such hard and fast distinction between the generations as the antithetic theory would appear to demand." Why should it demand a hard and fast distinction? For my own part, I had already described apogamy and apospory as occurring in the same individual before I wrote on alternation. The presumption seems to be that a distinct course of evolution must have imposed "hard and fast" limits upon the potentialities of the parts evolved. But we ought to remember how the root, whether in Phanerogams or Ferns, has doubtless had a long course of evolution as a member distinct from the shoot; and yet we see it bearing adventitious buds upon it, as in the Rosaceae, Poplar or Elm; or even transformed at its apex into a shoot, as in *Platycerium* or *Anthurium*. Such cases as these, though not exact parallels, should suffice to show that hard and fast lines are not to be anticipated as a consequence of a distinct course of evolution.

There is another kindred, though almost converse, proposition which has been advanced by Pringsheim. He made his experiments on Moss fruits, "in the hope that he would succeed in producing protonema from the subdivided seta of the Mosses, and thus prove the morphological agreement of seta and Moss-stem." The point here appears to be that parts which are capable of producing similar growths are in "morphological agreement." I cannot assent to this proposition. In the case of the roots above quoted, the production of buds upon them, or the conversion of their apices into shoots, does not prove their "morphological agreement" with shoots upon which such developments are common.

By those who use such arguments it is to be borne in mind that the two generations, however distinct in their evolution, are still merely stages in the life-history of one and the same organism. The hereditary qualities of the race as a whole must be transmitted through the successive generations. It may be a question how far, and under what conditions, its various potentialities come into evidence, as, for instance, in the formation of an apogamous sporophyte, or of an aposporous protonema: but that some such potentialities are there is in no way inconsistent with the antithetic theory.

I have above pointed out how morphology has recently passed to an experimental stage, and I am glad to say that by means of the cultures of Dr. Lang and others we are beginning to gain an insight into the circumstances which lead to these phenomena. In certain Ferns direct apogamy occurs; that is, "the immediate production of vegetative buds by prothalli which are usually incapable of being fertilised"; the origin of this is still obscure. But apogamy may also be induced in various other species. Dr. Lang states that "the causes which appeared to induce apogamy in these prothalli were, the prevention of contact with fluid water, which rendered fertilisation impossible, and the exposure to direct sunlight. Possibly the temperature had some effect." It is further to be noted that in every case of induced apogamy "normal embryos were produced when conditions permitted fertilisation." Now the conditions of prevention of fertilisation, exposure to light, and possibly also a high temperature, all lead to a plethoric state, which we may thus recognise as a precursor of induced apogamy, possibly also of apogamy at large.

On the other hand, the circumstances which precede or accompany apospory are commonly those of deficient nutrition. In the case of Ugo Brizi's *Funaria*, it is mentioned that the capsules were atrophied and buried in the soil, where they could not obtain nourishment by their own assimilation. In the indnced apospory of Stahl and Pringsheim the growths appear upon parts of the chopped up seta, isolated from their usual sources of supply. Among Ferns, the conditions of nutrition which precede apospory have not been noted in all cases; but the following facts are interesting. *Athyrium Filix-foemina* var. *clarissima* is a pale chlorotic Fern with exiguous leafage, while the more or less complete arrest of the sporangia is a concomitant of apospory. In *Polystichum angulare* var. *pulcherrimum* there is no obvious disturbance of the vegetative organs, but I have specially noted the sporal arrest, which, in the specimens examined by me, appeared to be complete. This is, then, a concomitant of apospory, though it may be uncertain how far there is a casual connection. In the case of apospory in *Pteris aquilina*, reported by Farlow, there is an irregular diminution of leaf-area in the pinnules which show apospory; this is accompanied by various stages of abortion of the sporangia, though some fully-matured spores were found. Here, as also in *Polystichum angulare*, the tips are specially affected. Farlow remarks, "the sporangia became more and more irregular the nearer they were to the tip." In the case of *Scolopendrium vulgare*, the plants which showed apospory at so peculiarly early a stage had been raised by Mr. Lowe from prothalli which had been repeatedly divided, a process calculated to affect the physiological condition. The aposporous plants of *Trichomanes alatum*, *pyxidiferum*, and *Kaulfussii*, were all cultivated under artificial conditions, and are characteristically shade-loving plants, a habit which must affect their nutrition. Perhaps the most interesting case, however, is that described by Atkinson in *Onoclea*. In plants from which, by removal of the foliage leaves, the sporophylls had been induced to change their character and develop as foliage leaves, the sori were arrested. "When the leaf has lost so much of its reproductive function that the sporangia are becoming rare or rudimentary in the sorus, apospory frequently occurs, and the placenta develops among the rudimentary sporangia prothalloid growths." Here is, again, a case of deficient nutrition; the assimilating leaves, after formation, but before they could have carried their functions far, were removed. The plant makes an effort to supply their place at the expense of spore-production; arrest of sori and sporangia is the result, accompanied by cases of the direct vegetative transition to the prothallus. From these examples we see that deficient, or, at least, disturbed nutrition is frequently, perhaps always, a concomitant of apospory. Thus there is some countenance for the view that apospory and apogamy follow on converse conditions of nutrition.

We may next inquire how these converse conditions may lead to the changes in question; and especially the state of the nuclei ought to be considered. Owing to practical difficulties of observation the behaviour of the nuclei in apogamy and apospory has not been directly followed. But if the nuclear difference between the two generations be as it is believed, nuclear changes will be closely connected with these vegetative transitions. What could appear more natural than that apogamy, which presumably involves a doubling of the chromosomes, should follow a condition of plethora, and that apospory, which presumably involves a halving of the chromosomes, should follow deficient nutrition?

One further fact in either case appears to me to be specially noteworthy, that the changes are not confined to a single cell. The directly apogamous bud of *Nephrodium Filix mas* may perhaps be referable to a single cell, but Dr. Lang shows by numerous examples that the transition from characteristic tissue of the gametophyte to that of the sporophyte may arise at various points, and involve considerable tracts of tissue. Similarly I have shown in the case of apospory that the change may affect not one cell only, but cell-groups at various and distinct points on the same individual. It would seem that there is a widespread disposition of the tissues to undergo the change.

For my own part, I think the usual attitude on the chromosome question has been too absolute and arithmetical. Evidence is accumulating from various sources that the usual numbers are not strictly maintained; it is known that in vegetative cells there are often considerable differences of the number of chromosomes from those in the sexual cells of the same plant, while observers have noted the irregularities in the divisions of the pollen-mother-cells in such plants as *Hemerocallis* and *Tradescantia*. If there be any causal connection between the number of chromosomes and the morphological character of the sporophyte and gametophyte, irregularities such as these at least countenance the idea of nuclear instability being possible; it will be a question for special treatment and investigation how far nuclear instability is connected with disturbed nutrition. But into the mechanism of the presumable nuclear change, and the question whether it be sudden or gradual, we cannot enter with any more than a speculative interest, in the absence of direct observations. Whatever the nuclear details may be, I regard it as a matter of very great importance to recognise that special conditions of nutrition commonly accompany, if indeed they do not actually determine, those changes which we term apospory and apogamy. But the story of the past is not simply a matter of conditions of nutrition, as we see them now influencing Archegoniate plants in their present highly specialised state. The real question is a purely historical one, How *did* the present state of things come about?

(*To be continued.*)

## *THE TEACHING OF SCIENCE IN ELEMENTARY SCHOOLS.*[1]

YOUR Committee are able to report that the quantity, if not the quality, of the teaching of science subjects in elementary schools has made progress during the past year. The following table, made up from the return issued by the Education Department, gives the figures for the scientific class subjects as compared with English. In the report for last year it was mentioned that the number of school departments taking object lessons would greatly increase, as the Government code of regulations announced that they would become obligatory in the three lower standards on and after September 1, 1896. We now see the result, so far as the schools are concerned whose school year ended between August 31, 1896, and August 31, 1897, but the full effect cannot appear until the next year's return, the whole of which will be in the obligatory period.

| Class Subjects—Departments | 1890-91 | 1891-92 | 1892-93 | 1893-94 | 1894-95 | 1895-96 | 1896-97 |
|---|---|---|---|---|---|---|---|
| English ... ... ... | 19,825 | 18,175 | 17,394 | 17,032 | 16,280 | 15,327 | 14,286 |
| Geography ... ... | 12,806 | 13,485 | 14,256 | 15,250 | 15,702 | 16,171 | 16,646 |
| Elementary Science | 173 | 788 | 1,073 | 1,215 | 1,712 | 2,237 | 2,617 |
| Object Lessons ... | — | — | — | — | — | 1,079 | 8,321 |

The number of departments in "schools for older scholars" for the year 1896-97 was 23,080, all but 10 of which took one or more class subjects. But history was taken in 5133 departments, and needlework (as a class subject for girls) in 7397 departments, and sundry minor subjects in 1056, making, with the other four subjects of the table, a total of 55,456. This shows an average of more than 2½ class subjects to each department; but it must be borne in mind that the same subject is not always taken in all the standards, in which case three class subjects will appear in the return.

It was remarked in the last report that "the increased teaching of scientific specific subjects in the higher standards is the natural consequence of the greater attention paid to natural science in the lower part of the schools." The following table shows the correctness of this inference:—

[1] Report of the Committee, consisting of Dr. J. H. Gladstone (Chairman), Prof. H. E. Armstrong (Secretary), Prof. W. R. Dunstan, Mr. George Gladstone, Sir John Lubbock, Sir Philip Magnus, Sir H. E. Roscoe, and Prof. S. P. Thompson. (Read before Section B of the British Association at the Bristol Meeting.