attenuated to the utmost, yet in no case did I ever observe rupture.

EXPLANATION OF FIGURES.

Fig. 1. Heart of Capercailzie. The right ventricle is opened by the usual incisions for exposing the valve; *av*, the septal surface of the valve, with the varying direction of its fibres; *bb*, the cut surfaces of the muscular arch, which closes in the pulmonary region; *c*, the accessory slip; *d*, the pulmonary artery disappearing behind the aorta.

Fig. 2. Heart of the Heron, showing the superficial fibres reflected at *aab*, and likewise those fibres *cd*, of the second layer, which are connected with the formation of the valve.

Fig. 3. Superficial fibres of the heart of the Swan, from the posterior aspect; *m* and *o* are the superficial fibres passing over into the valve; *n* and *p*, the fillet and the arch of muscle, which complete the anterior and upper portion of the right cavity.

Fig. 4. Heart of the Swan, exhibiting the third layer of fibres *cc*; *b*, the second layer reflected; *aa*, portions of the same which could not be raised entire on account of their peculiar bending over the valve.

Fig. 5. Internal fibres *aa* of the heart of the Swan, as they slant from the pulmonary region; *b*, the twist caused by the entrance and exit of fibres connected with the accessory slip.

Fig. 6. Heart of the Capercailzie; *h*, the great or oblique band; *ij*, diverging fibres, connected with the same; *k*, the prominent brim of the ventricle; *m*, the cartilaginous setting at the posterior coronary track into which many fibres are inserted; *n*, the smooth or septal surface of the ventricle; *o*, the mass of muscle wending round the apex; *p*, the aperture of the pulmonary artery; *s*, point of junction of the right or free ventricular wall with the septum.

Fig. 7. The same heart turned round to exhibit the course of the upper diverging fibres, *ii'*, of the oblique band, as they sweep to the region of the aorta, anteriorly, and to the left.

Remarks on the Nature and Peculiarities of the Fern-spore.

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The second volume of the Linnean Transactions contains a paper on the development of fern-spores, by Mr Lindsay. This was read before the Linnean Society in January 1792, and appears to be the earliest record of the production of these plants from spores. Since that period, the spore, its nature and peculiarities, have been the subject of frequent discussions, without eliciting, however, any perfectly satisfactory conclusions. This can scarcely be attributed to any inherent anomalies, but rather to an unfortunate desire, in many instances, to

* Read before the Botanical Society of Edinburgh, 12th June 1862.

homologate essentially distinct organs, *i.e.*, buds and spores. The fallacy of this view has been so repeatedly exposed, that a re-statement may appear quite superfluous, were we not confronted with the fact, that authors still persist in ignoring the distinction, so lucidly drawn by Carpenter, Berkeley, and others, between the terms Homology and Analogy. This tendency of studying from preconceived hypothetical standpoints, is, we think, the chief source of the intricacy pertaining to the subject we are about to consider.

The following remarks lay small claim to originality, being in most instances re-statements of those peculiarities and anomalous properties of the fern-spore which are said to render all explanations of its nature hitherto offered unsatisfactory. In one or two instances, however, there will be found notices of certain peculiar properties, which, so far as we are aware, have not been recorded before, and likewise a few remarks explanatory of those peculiarities, the correctness of which, however, we leave to the judication of others. On these considerations, then, and as a record of conclusions based on a series of careful observations of the fern-spore in its various stages of development, the following remarks may not, perhaps, be deemed superfluous.

The sporanges of ferns are generally attached to the lower surface of the frond, though in the *Polypodium anomalum* they are confined entirely to the upper. The spores are productions of cell-division within these, and, accordingly, unattached. They consist of simple cells, having two coats like pollen grains, the outer of which is variously marked with warts, spines, or reticulations. The history of their development is similar in every essential respect to that of pollen grains, being, like them, products of a free cell-formation. This, however, is a point entirely ignored by those who regard the spore as the homologue of the flower or leaf-bud, and hence, we are inclined to think, arise the difficulties in giving an explanation of its apparently anomalous properties. For it can scarcely be denied that all proposed explanations, at least of that peculiar facility afforded by spores for the reproduction and perpetuation of any accidental variation of the parts upon which they originate, are, upon the whole, insufficient. The most satisfactory, as we are inclined to think, is contained in a paper "On the Production of Ferns from Spores," by Professor Balfour, in the Trans. Bot. Soc., vol. vi. It is there remarked, "that the spores which are not the direct products of reproduction, may be regarded as analogous to parthenogenetic seeds." . . . "These may have a tendency to propagate particular forms of plants, just as is the case in Cœlebogyne, where female plants have always been produced by seeds said not to be acted on by pollen. In this way, then, we may account for the spores of those fully developed anomalous forms always reproducing varieties like their parents." This view, so far as regards the nature of the spore, appears to be perfectly satisfactory; it seems, however, scarcely to meet the exigencies of the case, when we consider the spore in connection with its peculiar properties. In the offspring of parthenogenetic seeds, we have no positive evidence of their propagating other characters differing from the normal specific type except as regards the sexual organs, which, in fact, are specifically normal; whereas in ferns other abnormal characters are regularly reproduced.

Another explanation has been proposed by Dr Lowe in his paper "On the Homologies of the Floral Organs of Phanerogamia and the Higher Cryptogams," of a very different nature from the preceding. In it the author believes that this peculiarity, *i.e.*, of reproducing anomalous forms, "can be explained only on the supposition that the spores are nothing more than buds, and that the real act of reproduction comes after their separation from the parent plant." But, aside from the difference of origin of the *bud* and *spore* before alluded to, we cannot but regard this explanation as entirely opposed to the laws governing the reproduction of variations in Phænogams. It appears to be generally admitted that the longer the incipient plant is developed under the direct formative tendency of the parent, the more will it partake of its individual peculiarities; that this is, in short, the cause of buds reproducing the peculiarities of their parents. In reproduction by seed, on the other hand, an embryo is separated from the individuality of the parent, "and develops itself out of itself, so that the influence which the parent plant exerts, even though it be definite and assimilative, is yet always an external one, and is modified by the peculiar vital power of the reproductive cell;" hence, in reproduction by seeds, the specific characters alone are in general reproduced. Now, in the fern spore, we have an organ still less developed, under the direct formative tendency of the parent, than even the seed; as, according to the theory we are examining, the embryo is not alone separated from the individuality of the parent, but all the floral organs are developed in perfect independence of it, so that the definite and assimilative influence exercised by the parent over the embryo of the seed, small though its individualising influence may be, is not even called into action in the case of the embryo developed from the spore. Hence we are certainly justified in asserting that were these organs the homologues of buds, floral or foliar, the reproduction of the morphological variations of the individual must be rare in the extreme—the exception not the rule—so purely dependent is the incipient plant on the specific morphological tendency.

Again, in a paper by Mr W. K. Bridgeman, entitled "The Influence of the Venation in the Reproduction of Monstrosities among Ferns,"* the author supposes that this peculiar property of spores is dependent on an abnormal distribution of the veins. This opinion is formed on a few experiments made upon certain abnormal forms of *Scolopendrium vulgare*, in which the venation was abnormal in one part of a frond and normal in the other. Selecting a frond of the variety *laceratum*, and taking spores from a reticulated and abnormal veined portion of the frond, the author found that the offspring from these spores exhibited the same characteristic forms as the parent frond. Those, on the other hand, taken from the normally veined portion of the frond, exhibited few of the characteristics of their parent, a large proportion of them being perfectly normal. On these, then, and one or two more experiments, the author believes, as before stated, that the reproduction of varieties is dependent upon an abnormal venation. This, however, is little more than a new mode of expressing the fact, that the spore reproduces the part upon which it originates. The statements differ from

* Mag. of Nat. Hist., December 1861.

each other, however, in this respect, that the view under examination appears to regard the form of the pinnæ as dependent on the venation, while the other avoids particularising, and makes no attempt to solve the question, whether the distribution of the veins influences the form assumed by the parenchymatous tissue, or *vice versa*. The latter view is held by Dr Dickie, who remarks that the quantity of cellular tissue in a leaf determines the development and position of the veins. Not pretending, however, to decide such a question, we may simply remark, that in the majority of ferns there is little or no departure from the normal venation of their respective species. Few will be found to exhibit so striking a departure as some of the crested forms of *Scolopendrium vulgare*. Nevertheless, it is an acknowledged fact, that the spores from normal-veined, though abnormal-formed, fronds, do reproduce with great regularity the latter peculiarity. The numerous varieties of the British ferns now annually raised from spores, in which there is no departure from the normal venation further than a greater or less development of them reciprocally with the tissue, affords sufficient evidence on this head. We should like to know in what manner the variegated ferns are dependent upon an abnormal distribution of the veins for the perpetuation of their peculiarities. In fine, we are induced to regard the produce of the spores from the differently-veined portions of the Scolopendrium as merely a casual concurrence, from the superabundance of evidence demonstrating the reproduction of variations with a perfectly normal venation. And there is not, so far as we are aware, a single instance on record, of the reproduction of an abnormally-veined frond or pinna, in which the form of the parenchymatous tissue was normal. An example of this kind is required to demonstrate the possibility of the reproduction of an abnormality in the venation alone. Instances of this kind would certainly appear to be very rare. Berkeley, alluding to a paper by Dr Hooker on the "Vegetation of the Carboniferous Period," as compared with that of the present day, observes that two pinnules are there figured of the *Callipteris malabarica*, in one of which the veinlets anastomose, in the other they are perfectly free. As we have not had an opportunity of seeing these figures, we cannot say whether they differ in form or not; but we have observed pinnules upon this fern with free venules, differing in no respect from that characterising a normally-veined pinnule. This, then, is an instance in point, and would serve to illustrate in a certain degree the power of the venation in the reproduction of its own abnormalities. As yet we have never had an opportunity of trying the experiment, as we have not succeeded in finding spores upon one of those abnormally-veined pinnules. Although Mr Bridgeman has, in our opinion, failed in showing conclusively the influence of the venation in the reproduction of variations, he has nevertheless furnished us with interesting illustrations of that peculiar individualising power possessed by the fern-spore, even in a continuous tissue, as occurs in Scolopendrium, by which it reproduces the form of the part upon which it originates.

It now behoves us to enter into a more particular examination of the spore in its different stages of development, to exhibit those physiological peculiarities which render it so distinct from the organs of phænogams having a similar functional import. And we trust that by thus exhibiting the very distinct conditions of the reproductive organs in ferns and the higher plants, we shall be enabled to understand the nature of that peculiar power of spores which affords such great facilities for the reproduction and perpetuation of individual variations.

The reproductive organs of phænogams, as is well known, are always products of two morphologically distinct organs, the stamens producing the pollen, the carpels producing the ovules; if they be not, as some authors are inclined to think, physiologically distinct, *i.e.* the *anther* a modified leaf, and the *ovule* a modified extremity of the stem. This separation of the reproductive structures, and their necessary union for the development of the embryo, must tend to impress more fully upon *it* the essential characters of the species. The embryo being in this case the *modified resultant* of two originally distinct organs, there will necessarily be a greater tendency to efface any individual peculiarities of these than would have been the case, had the embryo been the product of a single organ. And this is simply an application of the laws of hybridisation for the reproduction and continuation of the normal specific form. As it is well known that a hybrid may in a few generations, by repeatedly crossing the successive offspring with one of the original parents, be brought back, or nearly so, to the normal form of that parent, so in the case of the specific individual, the reciprocal action of distinct morphological organs—stamens and pistils—for the production of the embryo, will have a tendency to modify the occasional variations these organs may individually be liable to in the *resultant* of their combined action, and thus be a comparative safeguard against any sudden divergence from the characteristics of their parent.

The relations of the reproductive organs of thallogams to the morphologically perfect plant, are, on the other hand, very different from what obtains amongst phanerogamous plants, as the following summary will show. The fern-spore, as before stated, originates free in the sporangial sac, the latter being an immediate product of the foliaceous structure. After its discharge from the sporangial-sac, it produces, in favourable conditions, the prothallus, upon which the true organs of reproduction are ultimately produced. The formation of the latter organs in general closes the developmental history of the spore and its prothallus; an archegonial cell, or, as instanced by a prothallus of the *Adiantum pubescens* in the Edinburgh Botanic Garden, two of these cells have been impregnated, each of which has produced a perfect morphological representative of the parent. It occasionally happens, however, that none of these cells are impregnated, and in this case the prothallus frequently exhibits an interesting analogy in its gemmative powers to those lower order of plants which it morphologically resembles—each cell seeming to possess an independent formative power, and generating secondary prothalli, morphologically similar to the primary one. As this formative power seems not to have been observed, or at least recorded before, we shall take the opportunity of illustrating it in a subsequent part of this paper.

The phenomena indicated in the preceding brief detail of the origin and development of the spore, justify the following deductions explanatory of its peculiar individualising powers.

The spore has been defined, "the ultimate germinating cell of the plant"—*objectively*, an independent organ of generation, *subjectively*, however, and truly, part of the individual cycle of the plant upon which it originates. This latter relation is, we are aware, denied by Hofmeister and others, who, in accordance with Steenstrup's theory of alternation of generations, regard the spore and its immediate products as a distinct and perfect generation. This, however, as has been observed, confounds the true generative process with a mode of mere gemmative development; hence, we prefer to apply Huxley's definition of the zoological individual to the merely objective definition which Steenstrup's views afford, and, accordingly, regard the *phytological individual* as being equal to the total result of the development of a single fertilised ovule, the cycle of individuality being only completed with the generative act. The spore, then, being an independent self-developing organism, produced by a lateral organ, the frond, and undergoing no modification by the *immediate* action of any others, *similar to what obtains in the production of the Phanerogamic embryo*, is, consequently, less influenced by the *specific morphological tendency*; and this, we believe, affords a full explanation of the phenomena in question. The spore, being a product of a single lateral organ, and thus uninfluenced by the restrictive modifications of any other, inherits and reproduces the peculiarities of this organ, or rather that part of it upon which it originates, with *greater facility* than those characteristic of the species. This view is furthermore supported by the fact, that abnormalities of the pinnæ are inherited with much greater facility than a rachidal deviation, thus demonstrating, that even in those parts of the frond less directly connected with the spore, the *specific morphological tendency* becomes equally as energetic as the *individualising*. In illustration of this, we may refer to *Lastrea dilatata*, plants of which occasionally produce from a single stipe two normal fronds. Taking spores from such a specimen, as has been done in the Botanic Garden here, and sowing them, the offspring does not, in the early state, exhibit the bifurcated rachis of the parent frond; although, as the plants increase in age, a few of them produce these abnormal fronds pretty regularly; the majority of them, however, like their parent, produce them very irregularly. Spores having been taken again from the most regularly abnormal of these, we find that all the offspring exhibit the bifurcate fronds, the abnormal and normal being nearly equal. The plants are as yet, however, in a young condition, and will no doubt, as they advance in age, like their progenitors, produce with still greater regularity the abnormalities in question.

Before passing from this part of our subject, we would briefly direct attention to the agreement exhibited between the laws governing the inheritance of peculiarities in the Animal Kingdom, and those of the Vegetable. This law has been expressed in the following manner by an eminent naturalist, as obtaining amongst animals: "That at whatever period a peculiarity first appears, it tends to appear in the offspring at a corresponding age, *though sometimes earlier.*"* To clearly exhibit the concurrence of the law of inheritance in plants with that admitted to obtain amongst animals, we must necessarily know at what period in the life of the individual the peculiarity first made its appearance. Another point must also be taken into account in considering the doctrine of inheritance at different periods—viz., that in each successive generation the peculiarity appears to be exhibited at a slightly earlier period. Hence an apparent objection might be raised against the doctrine, by taking the third or fourth generation reproducing an abnormality, which had originally appeared on a frond from an old plant. As in either of these generations, the peculiarity, as has been already shown in the case of the *Lastrea dilatata,* may have begun to make its appearance in the earliest stages of the plant's life. In those ferns, however, which have dissimilar barren and fertile fronds, should the variation not appear in the early stage of the plant's life, we are inclined to suppose that the variation in the offspring will be confined to the fertile fronds, the barren retaining their normal form. In illustration of this, we may mention the *Doodia media,* var. *corymbifera,* which produces with the greatest regularity, in the mature state, the characteristic, erect, densely-tufted fronds. In raising this

* Darwin's "Origin of Species."

fern from spores, however, we find that the young plants, which produce only barren fronds, differ little, if at all, from the normal specific form, having the same spreading habit. As they advance, the cresting begins to exhibit itself on the lateral pinnæ, with a larger crest on the terminal; and they are usually two or three years old before the rachis begins to exhibit any ramifications. Occasionally, however, even in the mature state, as instanced by a plant in the Botanic Garden here, the normal barren frond is produced,—thus favouring the supposition that the variation has originated from a monstrous frond, produced by an otherwise normal representative of the species. Again, the *Blechnum Spicant*, vars. *crassicaule* and *cristatum*, on the other hand, reproduce with the greatest constancy, and, from the earliest stages, their characteristic, abnormal, sterile and fertile fronds. And the plants cultivated in the garden were perfectly abnormal when found, so that the early appearance of the abnormality in the young plants is readily explained, by supposing that the causes of variability have been called into action at an early period of the development of their original progenitors. These, then, are a few illustrations of the laws governing the inheritance of peculiarities in plants, as taken from our own observations, and we hope that others will be forthcoming, as we fully admit that there is an extreme paucity of evidence in support of these views; nevertheless, so far as we are aware, there is no evidence of a negative nature that has been brought to bear against them.

Having now briefly considered the relations of the fern-spore and plant, and indicated the probable cause of that peculiar power which the spore possesses for the reproduction of accidental variations, we will next proceed to consider the spore as a product of parthenogenesis. To substantiate our claim, however, for regarding it as such, it is necessary to show what grounds there are for the belief in parthenogenesis in plants, as of late the doctrine has been doubted, and even denied, by many botanists. It therefore becomes necessary, in hypothetically regarding the fern-spore as such, to furnish some positive evidence of parthenogenesis amongst plants. In using the expression "positive evidence of parthenogenesis," it is of course only in the sense that a plant may be developed from a germ without any direct action of the male element. Professor Owen's theory of parthenogenesis may, or it may not, be true; it is sufficient for our purpose to give examples of the phenomena. And, indeed, by so doing, we substantiate a claim for the acceptance of the theory, as it can scarcely be denied that the inherited portion of the generative force—at least in the lower forms of vegetative life—is of too subjective a nature to exhibit any objective existence.

Accepting, then, the supposition alluded to in a preceding part of the paper, we regard the *spore* and its *immediate products* as parthenogenetic, satisfied that in so doing we shall have the most philosophically accordant explanation of the phenomena under consideration. And, by thus limiting the influence of the preceding generative act to the spore and its immediate products, *i.e.* the prothallus, antheridial, and archegonial cells, we invalidate an objection to the theory in its present application which has been thus expressed: "If the impregnation of the spore influences the whole future individual and its produce, would it be reasonable to expect the spores from an abnormal frond to reproduce in their offspring that abnormality?" The tenability, however, of this objection to the doctrine, even as originally applied, is very questionable, on a fair consideration of the results of parthenogenesis. It is well known that the offspring of parthenogenesis do not represent all the characteristics of the species, the male individual being always, so far as known, absent. Hence, the fact that female individuals alone are produced, demonstrates a modification in the inherited portion of the generative force, otherwise both would have been reproduced. And why, it may well be asked, may not the inherited portion of the generative elements in ferns also undergo a modification similar to the organs through which they have been transmitted, and thus admit of the reproduction of a similarly modified individual? In fine, we consider the latter view as much less gratuitous, though, as we before showed, insufficient for all the requirements of the case, than that which, regarding the *spore* as a *flower-bud*, must necessarily assume that the reproductive elements generated upon the prothallus, and which consequently have had no objective existence in the parent, will nevertheless faithfully reproduce the characters of that organ of the parent they never had any immediate connection with.

The opinion, then, that the spore and its immediate products are the resultants of that portion of the generative force inherited from the preceding generative act, seems to be more fully accordant with observed phenomena than any other yet proposed. This hypothetical accordance will, however, be of small avail, if, as has been averred, parthenogenesis has no existence amongst plants. Such is the position assumed by Dr H. Karsten in his paper "On the Sexual Life of Plants and Parthenogenesis," in the Ann. and Mag. of Nat. Hist. vol. viii., from which we extract the following, being the conclusions which Karsten considers his investigations justify: "That all known species of plants possess, besides an asexual multiplication of individuals by cell-division or gemmation, a means of preserving the species by sexually developed germs, and that in these special reproductive organs a normal germ is never formed without the operation of a fertilising material; that, consequently, parthenogenesis never occurs in plants." Generally sweeping, however, as this conclusion is, it is far from being based on a general examination of all the known instances of parthenogenesis in plants. Dr Karsten undoubtedly proves that certain supposed cases of parthenogenesis were in fact true products of sexual generation, as instanced by Cœlebogyne, which produces at times hermaphrodite flowers; but there are other cases in which no such error of observation has yet been detected. Braun, in treating this subject, gives, besides the Cœlebogyne, a Chara in illustration of the doctrine. Karsten, however, in examining this paper, satisfies himself by exposing the errors of Braun's observations in the former instance, the latter being entirely ignored, which certainly is not a fair procedure in one who avers an anxiety for the discovery of the truth, and eschews the fallacious evidence of a preconceived interpretation. The example alluded to is, nevertheless, perfectly satisfactory, as may be seen by the following extract from the notice of Braun's paper in the Trans. Ed. Bot. Soc. vol. vi.:—"The *Chara crinita* is said to be diœcious, yet we seldom can detect antheridia. Braun has in vain attempted to find these organs; he therefore considers it as usually represented by female individuals, and that, nevertheless, it produces in abundance sporangia and fertile spores. M. Requiem has recently found male plants at Courtheson, near Orange, bearing antheridia. . . . . Braun considers himself justified in attributing to *Chara crinita* the power of producing, at least in certain localities, without the action of male organs, perfectly formed spores fit for germination, and that, consequently, it is a true case of parthenogenesis." Another example has come under our own observation in the Botanic Garden here, where we have been successful in raising the Nardoo, *Marsilea quadrifolia*, var. *macropus*, from archegonial spores separated carefully from the antheridial, after the bursting of the primary receptacle, and before they had been acted upon by the spermatozoa of the latter, as the antheridial cells do not burst for several hours after the dehiscence of the primary receptacle. It is worthy of remark, however, that the germination of the archegonial spores, so far as our observations go, is much retarded by their separation from the other organs of the receptacle. This may partly be attributed, perhaps, to the mere chemical action, arising from the decomposition of the contents of the receptacle, which will no doubt have a tendency to accelerate the development of the unseparated spores, independently of the fertilising influence of the antheridial. Again, it has been shown by Duverney, that the archegonial spores of Salvinia are also capable of development, when separated from the antheridial. And this affords even a more conclusive instance of parthenogenesis than Marsilea, as the antheridial and archegonial spores of the former are contained in distinct receptacles, while in the latter they are contained in one.

These few examples, then, will, we trust, be sufficient for our present purpose, *i.e.*, to substantiate a claim for the existence of parthenogenesis in plants. And, at a subsequent period, we hope to lay before you the results of a series of experiments instituted for the purpose of testing more satisfactorily the validity of this doctrine, which, according to Karsten, has lost its last insecure prop; and who states "that the doctrine of parthenogenesis in plants is thrust aside, and it is established beyond doubt, that the production of a normal germ in the female organs is dependent upon the co-operation of the male organs of plants."

We will now proceed to examine the phenomena presented by the prothallus after the abortion of the archegonial cells. In a normal condition, the prothallus, after attaining a certain size, ceases to develop cells externally, and shortly after, from a point on its under surface, near the sinus, the primary frond is in general produced. At times, however, the prothallus, instead of producing the young plant, recommences a merely vegetative development, in a variety of ways. Thus, it at times increases by the development of cells at its circumference, attaining a large size, or it may produce a number of upright lobes, while in another these lobes are recumbent, the one overlapping the other. These, then, are the more common modes in which prothalli develop, the lobes in all these cases being very irregular, and indefinite in form. Instead of producing these indefinite forms, however, the prothalli at times exhibit a tendency to a "vegetative repetition of similar parts;" an example of which occurred under our own observation in the Botanic Garden here, where the whole surface of a prothallus became covered with minute granulations of green cellular tissue. These, on examination, were found to originate from the individual cells of the prothallus; each, however, retains its individuality, and now present miniature fac-similes of *normal prothalli.* In this instance we found no less than *forty* of these *secondary prothalli* developed from the surface of the single primary prothallus. In another example of this kind, which occurred on a prothallus of the *Asplenium obtusatum*, these secondary prothalli were confined to the circumference, forming a fringe around its margin. Another very anomalous appearance presented itself in the *Gymnogramma leptophylla*, of which we raised a fine healthy pot of prothalli in March 1861. These, however, having been inadvertently exposed to the direct action of the sun, were nearly shrivelled up before they were observed. Indulging a hope that they might possibly recover, we did not turn them out, but placed them in a moist, shady situation, and found that, to a certain degree, this had the desired effect of restoring them. They, however, never regained their former appearance, and in a short time a gradual decay commenced from the circumference inwards, till at last we thought they had entirely died off. On examining a few of the dried-up and apparently lifeless remains of these prothalli, we were surprised to find that they had each produced a small, somewhat ovoid, cellular nodule. These nodules, or gemmoid bodies, appear to perform the functions of hybernacula, as, after a short period of rest, they each produced, in varying numbers, small tufts of narrow, erect, forked, marchantia-like processes. Whether these will ultimately produce young plants, or continue for a time a merely vegetative development, and ultimately decay, it is impossible, in their present condition, to say; and so far as our examination goes, there are as yet neither antheridial nor archegonial cells developed upon them. The late Professor Henfrey, in treating upon these points, remarks, " that after the abortion of the archegonial cells, the prothallus may again commence a vegetative development, but *will never produce young plants.* The latter opinion, however, does not agree with observed facts, as we have at present prothalli which recommenced this vegetative development some time ago, and from which young plants are now produced; and, we may also remark, that these are produced upon newly developed portions of the prothalli—thus demonstrating that the power of reproducing the plant *is* retained after the abortion of the primary archegonial cells. It must be borne in mind, however, that this has reference to those kinds of development alone where the original prothallus is increased by the development of cells around its circumference. As to the results of the other modes of development we are as yet ignorant, though it does not appear at all improbable that even these more anomalous prothalloid developments may likewise produce young plants.

Before concluding our remarks, we may briefly examine two other points connected with the subject we have been considering. First, the supposed incapability of the early spores of tree-ferns to germinate; and, second, the power which the fern-spore possesses of retaining for lengthened periods its vitality. In regard to the first of these opinions, then, it has been observed by Dr Lowe, "that the early spores of tree-ferns are asexual, and incapable of germination." Following out this somewhat gratuitous opinion, Dr Lowe completes the fanciful analogy he has drawn between the organogenetic powers of the higher plants and ferns, by regarding these so-called non-fertile spores as similar to the *leaf-buds* of the higher plants. Thus, he remarks, "it is well known that young trees have a greater tendency to produce leaf-buds than flowers, and this may serve to explain why the earlier spores of tree-ferns are asexual, if we regard them as *leaf-buds*." "In later life," he continues, "the plant would naturally produce floral-buds, and these, of course, would be provided with sexual organs." This opinion, however, of the infertility of the early spores of tree-ferns, though thus given as an acknowledged fact, we cannot but regard as very doubtful, inasmuch as we have found no difficulty in raising various species of tree-ferns from the first spores produced by young plants. We may state, that we have succeeded in raising the following tree-ferns from these early spores, viz., the *Alsophila excelsa*, *A. Miquelii*, and *Hemitelia grandifolia*, all of which germinated freely. Again, the existence of those so-called "asexual spores" is disproved by the fact, that the first spores of the dwarfer kinds of ferns exhibit no such phenomenon, which they certainly ought to do were the view under consideration correct. And we have experimented on spores from very young plants. Amongst others which we have tried, we may mention *Asplenium Shepherdi*, *Athyrium Filix-fœmina*, and *Lastrea dilatata*, all of which, as is well known to the fern-cultivator, exhibit a tendency, even in the youngest fronds, to produce spores. These, then, like those produced on the early fronds of the more noble representatives of the order, are theoretically asexual, as they must be regarded by the supporters of the preceding analogy as occupying the same position amongst ferns that the perennial shrubby plant does amongst phænogams.

These experiments, however, have been considered as inconclusive, on the ground, that as restriction of the roots has a tendency to produce the more early production of floral-buds, so in the case of ferns grown and restricted in pots, they, as a natural consequence, will produce the homologue of the floral-bud—the perfect spore. This, so far as regards the mere production of spores—in tree-ferns at least—is quite correct, as we observe Professor Balfour, in his "Class-Book of Botany," in remarking on the rare occurrence of fructification in fossil-ferns, directs attention to the tree-ferns of the present era, whose fronds rarely exhibit fructification, as affording, in all probability, an explanation for the rare occurrence of fructification in fossil ferns. In admitting this, however, we do not at all invalidate our experiments, seeing they were made upon the *earliest spores* produced by the young plants, consequently they ought to have been the "asexual" productions, if such are ever produced, and of which we have already expressed our doubts. In fine, the non-germination of the spores in the experiments—which we suppose have been made—may be due to unsuitable physical conditions, as has been well shown by an experiment in point made in the Botanic Garden here. Mr M'Nab had been repeatedly unsuccessful in his attempts to raise the *Cyathea arborea* from spores taken from a fine old plant in the stoves. These experiments were tried in a mean temperature of 60° Fahr. From the success attending other experiments which Mr M'Nab had been repeating in higher temperatures, he was induced to try the spores of the *Cyathea arborea* with an increase of temperature likewise. They were accordingly placed in a moist, shady situation, in a mean temperature of 75° Fahr. The experiment proved perfectly successful, as in fourteen days, by the aid of the lens, the spores were all seen to be in a germinative state, and at present there are plants in the stoves raised from them. Thus we have an instance which might have induced the less demonstrative theorist to conclude at once that these spores were imperfect "asexual" productions, without for a moment considering that their so-called asexuality might be a consequence of uncongenial physical conditions. And therefore we consider ourselves justified in regarding the non-germination of the early spores of tree-ferns, in the experiments we presume to have been made, as due to accidental causes, such as unsuitable physical conditions, immaturity of the spores; or, perhaps, the bursting of the spore-cases, and escape of the spores before the former were committed to the soil. These, then, are a few of the probable causes that may afford an explanation of the opinions we have been considering. But whether they do so or not, our experiments nevertheless demonstrate that there is no generally recognised law in the economy of the tree-ferns warranting the acceptance of the view, that *asexual spores* precede the *sexual* in the life of the individual.

2. In regard to the capabilities of spores retaining for lengthened periods their germinative powers, little information is at present to be found. Indeed, so far as we are aware, the only instance on record is that mentioned by Professor Balfour, in his "Class-Book," p. 628, where it is stated, that "two plants of *Gymnogramme calomelanos* were obtained from spores which were taken from the Herbarium of Forster, and were about fifty years old." We, however, have been less fortunate in our attempts to raise ferns from spores taken from Herbaria, though we have experimented on a large scale. Having taken spores from upwards of two hundred species, from the University Herbarium here, the dates of which varied from 1827 to 1859. Amongst these we observe the *Gymnogramme calomelanos* occurs twice, bearing respectively the dates 1827 and 1844. From neither of these spores, however, did we succeed in raising a single plant of the *G. calomelanos*, though a few other ferns came up in the pots. This was, indeed, the case in the majority of our experiments; but not a single instance occurred of plants of the species labelled being produced in their respective places. The growth of other species of ferns in the pots took place, though the greatest care was taken to exclude all other spores, each pot being covered immediately after the spores had been sprinkled on the surface of the soil. May this not have been the case in the example of the *G. calomelanos* raised from old spores? We are much inclined to suspect that the *Gymnogramme* may have been produced from the accidental introduction of recent spores amongst the old ones; more especially because *it*, and other species, are perfect weeds in

the stoves where they are grown, springing up wherever a suitable *nidus* presents itself. Indeed, it is difficult otherwise to understand the complete failure in the numerous experiments we have made for the purpose of determining the vitality of fern spores, as these were taken from fronds in an excellent state of preservation, and the conditions they were placed in were fully sufficient to have called into action their dormant vitality, had they possessed the power of retaining it. In fine, the numerous unsuccessful experiments we have made on this point certainly justify us in concluding, that the spores of ferns do not retain their germinative power for any length of time in the ordinary conditions of Herbaria. What their capabilities may be for retaining *it* when buried in the soil, we cannot at present positively say, though it would appear, from experiments now pending, that they, like certain phanerogamic seeds, may retain this power for long periods.

On the Origin of Aerolites. By Mrs G. S. SILLIMAN. Communicated by the Authoress.

Fragments of stone of a peculiar character, not identical with any telluric masses, are known to have fallen from the atmosphere upon the earth for more than two thousand years. They have come from fire-balls careering in the clear sky both in the day and by night, and also from small, very dark clouds, which suddenly threw down showers of stones like hail. These were dissimilar in size, appearing to have been shattered and crushed in the fall, but possessing a thorough identity of constituents with those which fell from the meteor or fire-ball. In their original condition they are invested or coated by a crust or rind peculiarly characteristic of these bodies, being only a few lines in thickness, often glossy and pitch-like, and sometimes veined. This black crust, though adhering closely, is divided from the inner light-grey mass by a sharply defined line of separation.*

Biot states that we are indebted to the all-registering

* Humboldt, Kos. i. p. 118. The peculiar colour of the crust was observed as early as the time of Pliny.