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Papers presented at the Boston meeting.

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PAPERS PRESENTED AT THE BOSTON MEETING

UNDER THE AUSPICES OF THE
LINNAEAN FERN CHAPTER

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ABNORMAL FORMS, AND HYBRIDITY IN FERNS.

By Geo. E. Davenport.

ONE of the most interesting forms of variation is found in the fronds of dimorphous ferns, *i. e.* ferns with two kinds of fronds, where we meet with many strange phenomena for which satisfactory explanations are not always readily available. In what we might call semi-dimorphous ferns, that is, in those ferns which like the *Anemias* and *Botrychiums*, and we might include here *Osmunda regalis*, have their fertile and sterile portion borne on separate stalks, but with one common footstalk, we find some curious transitional states occurring. Sometimes the fertile panicle is partially sterile with lamina-like divisions, and sometimes the sterile portion is sporangiferous with more or less scattered sporangia, in some cases even whole divisions being transformed into fruit.

Among the strictly dimorphous ferns some of the *Acrostichums* show remarkable transitional states in which the fertile and sterile portions become mixed up in all sorts of ways, and I take it that all dimorphous ferns are likely at some time or other to exhibit similar traits of character. The *frondosa* forms of *Osmunda cinnamomea* will no doubt suggest themselves to you at once and it is those forms, and the *obtusilobata* form of *Onoclea sensibilis*, that I am going to speak about especially, not only because of their furnishing examples near home with which you are likely to be most familiar, but because there are still some open questions as to the causes which produce them.

It is a little singular that while *Osmunda regalis* frequently produces abnormal variations, *Osmunda Claytoniana* seldom does so. But *O. cinnamomea* frequently, and, in some cases without any apparent cause, produces transitional fronds which are partly fertile and partly sterile; these transitional fronds being, as is the case with other ferns of this class, what should otherwise be fertile fronds.* Sometimes these transitional fronds simulate the fertile fronds of *O. Claytoniana*, having fertile pinnæ in the middle of the frond with sterile pinnæ above and below, although we never find *Claytoniana* simulating or approaching

* According to Rev. Dr. Campbell in *Canadian Horticultural Magazine* for Sept., 1898, the *frondosa* form is as common in Province of Quebec as the normal form of the species itself.

cinnamomea in any way. Sometimes they are fertile at the top only, and sometimes below, while all the rest of the frond will be sterile.

Now it so happens that such fronds apparently occur most frequently on plants growing where some change in the conditions of their environments has brought about through various causes an apparent loss of vitality, and the somewhat hasty conclusion has been formulated that the *frondosa* forms are due to this and similar causes. That this is, however, a somewhat superficial explanation is evident from the fact that *frondosa* forms have also occurred in situations where there had not been any change in the environments, and on plants that were normally healthy in every way with the usual proportion of both sterile and fertile fronds beside the abnormal *frondosa* form. It has been sometimes stated that the *frondosa* fronds are a later growth, but that has not been my own experience. I have generally found them growing with, and maturing at the same time as the normally fertile fronds, and last year two *frondosa* fronds that I was watching the development of, matured in advance of the normal fertile fronds on the same plant, as when I went expecting to gather them, they had drooped and withered, although the other fertile fronds were still erect and fresh.

Dr. Robert T. Jackson has told me of an experience which he has had with a plant that has been growing in his garden for a number of years and which this year for the first time produced a frond of the *frondosa* form. The plant is still growing under precisely the same conditions it always has been, and he could not discover any cause and knew of no good reason why it should have developed the form this year any more than in previous years. All of which goes to show that while a change of conditions may help to bring out some latent disposition on the part of the plant to vary, the real cause lies deeper, and I trust that some of you will seek for and find it out. In the case of the *obtusilobata* form of *Onoclea*, it is not so easy, notwithstanding the assurances we have received that it is the result of injury to the plant. I at one time held this view myself, but have since modified it. It is certainly supported by what appears to be conclusive evidence, and the evidence is conclusive so far as it goes, but it does not go far enough. For while it is true that the form is found most frequently under conditions where the sterile fronds have been cut away, and otherwise injured, it is also true that the

form has been found on plants that had not been injured and were growing under normal conditions.

There is an imperative law of nature which makes the perfection of fruit dependant on the perfection of foliage. We cannot expect perfect fruit from a tree denuded of its leaves, yet we know that a tree may have abundant foliage and yet produce imperfect fruit. So undoubtedly the sterile fronds of *Onoclea* are indispensible for the perfection of the fertile, and if the sterile fronds are destroyed we may expect immature fertile fronds, but there is no reason that I can see why we may not occasionally find imperfect fertile fronds, and even transitional conditions on plants where the sterile fronds have not been destroyed, and this I claim has been done.

In October, 1881, I published in the Torrey Club *Bulletin* some observations of my own, to supplement those published by Dr. Underwood in September. The facts stated by him I knew by my own experience, to be true and his conclusions were justified by them. I have myself had similar experiences, and adopted similar views; but subsequently different experiences caused me to modify my views, and it was these later experiences that I published, as it seemed to me that they suggested some different conclusions from those previously held by myself and adopted by Dr. Underwood, and that it was important that they should be published as a contribution toward supplying data for a proper solution of the whole problem.

As the facts which I then gave have never been controverted or explained, they still remain important factors in any solution that may be offered for explaining the causes which bring about the production of the *obtusilobata* form. The more important of the facts then stated, were: 1st. That while I had collected specimens in open meadow lands, where its appearance subsequent to mowing time suggests a probable cause and effect, I had also collected it plentifully in situations where no scythe had ventured, and where the plants were otherwise perfectly developed —the sterile fronds being well grown—and in some cases bearing normally developed fertile fronds as well as the variations. 2d. That I had found one season, near a rivulet, and on a stony patch left unmown at the edge of a meadow newly mown, some of the finest *obtusilobata* I had ever collected, obtaining four specimens from one plant alone. This plant I marked, and revisiting it the next season found that it had resumed its normal

form, although there had not been any change whatever in its surroundings. Now what caused this plant to deviate from its normal habit one season and return to it another under conditions exactly the same in both seasons, and where there was no preceptible outside disturbance to interrupt or interfere in any way with its free growth? 3d. I had in one season found sixty specimens of *obtusilobata* in various stages of development, in one locality, on plants, some of which had been injured in various ways, and some of which were well supplied with healthy sterile fronds, while the next season I was unable to find a single specimen there, although I searched diligently, and found plenty of plants injured as before, the locality being one that was exposed to all sorts of accidents. 4th. During the seasons of 1873-74, I made a series of experiments for the purpose of ascertaining, if possible, how far injuries to the plants might go toward the *obtusilobata* forms. These experiments were begun as early as May, and continued at intervals of a fortnight, up to the first of September. During that time some hundreds of plants in different localities were mutilated in every conceivable manner, but no *obtusilobata* was obtained in any instance. These experiments were continued during the next season (1875) with the same unfavorable result, while late in August, 1881, I found three specimens of *obtusilobata* on plants with well matured, uninjured sterile fronds, and the appearance of the plants indicated that they had not been disturbed since they first started to grow in the spring.

My conclusion, from those and other facts given, was that "the variation was due to some cause within the plant, rather than to any outside influence however much such influence might assist in developing an impulse already existing." It will be seen from these data which I have quoted that my experiments antedate the recent experiments of Prof. Geo. F. Atkinson by nearly twenty-five years. Now I have the greatest admiration for Prof. Atkinson's work, which is of the very highest order and value, and I know that the experiments which he has published an account of must be correct, and justify the deduction which he has made from them. But I do not think he could have known of these published experiments of my own, or he would have taken them into consideration, and modified his conclusion in the same direction that I have mine, at least so far as to see that while *obtusilobata* may sometimes follow mutilation of plants, it does not

always do so, and that its occurring under the conditions which I have described shows there is still another cause, lying deeper, which must be searched for before a final conclusion is reached, and that until the facts I have given are explained, the solution still remains an open question.

These facts do not rest upon my own evidence alone, but are well supported by other testimony. Dr. Torrey himself gives an instance where specimens were found growing on the same root-stock with the normal form. Prof. Eaton, in "Ferns of North America," mentions its occasional occurrence also. Mrs. M. L. Stevens, a member of the Fern Chapter, tells me that the only time she ever found *obtusilobata*, was on large plants that had not been injured ; and Mrs. P. D. Richards, of the Massachusetts Horticultural Society, tells me that it was only within a few years that she knew it was more likely to be found in the new mown meadows, and she always found it before on normal plants.

During the present season I have persistently cut away, in some only partially, and in others all of the sterile fronds from several plants on my own grounds, and also in several patches of plants growing in a convenient woodland, and have so far found but one imperfect specimen on a plant with half the sterile fronds remaining. Within a week I have been over several hundred plants that had been cut down, trampled on, and otherwise injured by the Gypsy Moth Destroyers—destroyers in more sense than one—but I could not find any variation, although there were hundreds of fertile fronds in all stages of development, some of which had clearly developed after the plants had been mutilated.

Now I do not wish to be understood as questioning the fact that *obtusilobata* often follows mutilation, because I know from my own experience in collecting it in newly mown meadows that in such cases it certainly does do so, and the cause appears to be so clear that one may almost certainly expect to find specimens in the wake of the scythe ; but it is a little singular, if mutilation is the real cause, that in all of my own efforts to obtain specimens by mutilation I have only partially succeeded once, and I insist, that, inasmuch as mutilation does not account for the facts which I published seventeen years ago, and here restate with additional corroborative testimony, there must be some other cause to be searched for.*

*This has been still further confirmed by Mrs. Britton, and Rev. James A. Bates, both of whom stated at the meeting of the Fern Chapter, that they had found the form growing on normal plants.

Leaving the subject of variation here, let us consider briefly the probabilities of hybridity among ferns in nature. That hybridity among ferns in nature does sometimes occur we have some convincing evidence, but the difficulties in the way of its accomplishment are so great, and the possibilities of overcoming those difficulties are so remote, that its occurrence must necessarily be exceedingly rare. That it may be, and has been brought about through the culture of mixed fern spores seems to have been fairly well proven by the experiments of Lowe, and others. But in sowing a mixture of fern spores for cultivation one is able to confine the field of operation within a very limited and closely guarded area, so that the resultant prothallia are brought into close contact and their intercrossing is rendered comparatively easy if not absolutely certain.

It is not so, however, in nature. Spores may, as they do, sow themselves by the millions, yet the various agencies by which they are scattered about and dissipated throughout the unlimited space of the open make the blending of the prothalli from different species altogether a thing of chance, and that chance is not likely to repeat itself very often. It is not with ferns as with flowering plants, where the pollen from one blossom blown about by the wind, or transported by insects, is almost sure to fall upon some other flower, where the organs of fertilization are exposed and fertilize it; the spores of ferns never fertilize anything, and if they do not germinate themselves, perish altogether. When fern spores do germinate they produce an entirely different plant organism from the fern plant by which they are themselves produced, and the plant organisms, which we call prothalli, develop from their own substance the special fertilizing organs by means of which the reproduction of the fern plant is brought about.

It is now very well known that fern prothalli sometimes produce proliferous buds from which young fern plants may be developed without the intervention of fertilization exactly as on the laminæ or apices of fern fronds—a fact first observed by Dr. Farlow; and now more recently it has been shown that under certain conditions, the prothalli of some species develop a cylindrical process that bears sporangia with spores, although it is not yet known whether such spores are capable of germination or not. But as a rule, fern prothalli are either monœcious in character, or diœcious, and fertilization is more likely to take place in the former than in the latter, which are not likely to be-

come fertilized unless different prothalli of the proper kinds are sufficiently near to enable the antherozoides to pass from one prothallium to another.

Lowe has shown the necessity for this, quite clearly, although he has apparently demonstrated the possibility of antherozoides being carried from one prothallium to another by insects. But all this will serve to show you how unlikely it is for the hybridization of ferns to occur very often in nature, That, however, it may sometimes—however rarely—be, and has been brought about in certain instances, we have some fairly good presumptive evidence, if not positive proof. I do not see how we can ever obtain absolute proof of hybridity in nature, because it is manifestedly impossible for us to observe the process through which it is brought about; nor do I see how we can obtain positive proof through mixed spore sowing, as it is equally impossible for us to determine from which species the germinating spores came. It seems to me that in all such cases there will always be an element of uncertainty that will compel us to rely altogether upon inductive reasoning for our conclusions which are not always likely to be safe.

There is, however, a way—suggested by Lowe's experiments in dividing prothalli—by which proof of hybridity may be obtained, if one has the necessary skill and patience to undertake it,—and that is by first sowing the spores from different species separately, and then afterward dividing the resultant prothalli so as to bring the antheridium portion of a prothallium of one species, and the archegonium part of a prothallium of another species togther, until they coalesce and fertilization takes place. If this method should succeed the resulting fern plant must necessarily be a hybrid.

I do not find any evidence of this having been tried in the way here suggested, but Lowe has recorded some successful experiment in dividing prothalli and raising fern plants by bringing portions of different prothalli together, and Prof. Bowen has recently given an account, in the *Annals of Botany* for December 1897, of *Polypodium Schneideri*, which he considers to be a good hybrid between *Polypodium aureum* and a form of *Polypodium vulgare*, called var. *elegantissimum*, and which was raised by first sowing spores of each species separately, and then putting the resultant prothalli together afterward, a method, however, which does not eliminate every possible chance for doubt as

would be the case if the prothalli were divided in the way I have suggested.

The most interesting feature of Lowe's work is the account of his experiments in dividing prothalli, but he seems to have overlooked the only certain way of proving the hybrid origin of the ferns which he raised, and yet if he could do what he has already recorded, and it is all very wonderful, he could certainly have accomplished this. It seems a pity that more of his efforts could not have been directed to the crossing of distinct species instead of abnormal varieties ; as it is, nearly all of his so-called hybrids are mere crosses between varieties of one species instead of hybrids between two distinct species.

The only exception that I can find in his work, entitled "Fern Growing," is his crossing of *Aspidium filis-mas*, var. *paleaceum*, which he considered a distinct species from *filix-mas*, with *Aspidium abbreviatum*. The two ferns represent two very distinct species, if not genera, one being a *Nephrodium*, and the other a *Cyrtomium*, or *Polystichum ;* yet of several hundred young ferns raised he seems to have found only three showing any trace of *paleaceum*, the others being all *abbreviatum*. His crossing of *Aspidium aculeatum* with *angulare*, which he considered as two distinct species, can only be received negatively, as Hooker, Baker, Prof. Eaton, and other eminent pteridologists have regarded *angulare* as only a good variety of *aculeatum*, and that is my own judgment. So that in summing up the result of his fifty years' labor, as published in the work quoted, we find only one instance in which hybridity can justly be claimed. This leaves us with very little evidence of hybridity from that source.

More recently, however, he has apparently succeeded in crossing *Scolopendrium vulgare* with *Ceterach officinarum*, two very distinct genera, and the resultant fern is said to combine in a very marked degree the essential features of both the parent species. This is vouched for by Druery (M.) in *Gardner's Chronicle*, for September, 1895, and is certainly most interesting, as it is not only a departure from Lowe's previous crossings, but furnishes an instance of bigeneric hybridity analogous to that of *Asplenium ebenoides*.

I must dwell for a few moments on the case of this latter, because Dr. Underwood has recently, and with much show of reason, seriously questioned its claim to be considered a hybrid

any longer. His argument is based on its abundance in the station where he found it growing in Alabama ; the assumption being that such abundance is inconsistent with the theory of its hybrid origin. It does not appear, however, from anything which he has published that either of its supposed parents were absent altogether, if I understand the report of his statements before the Torrey Club, in the *Bulletin* rightly. The situation, I take it, was about like this :—*Ebenoides* was growing under overhanging rocks in deep recesses, while above on the flat surface *Camptosoms* grew, and below grew *Asplenium ebeneum*,— and this seems to me to be an ideal situation for the reception and germination of windblown or fugitive spores. Once established, a plant endowed with the faculty of multiplying by means of proliferous buds, as *ebenoides* is capable of doing, would naturally increase rapidly, and with the added protection of a secluded spot seldom visited, the abundance of *ebenoides* there might be accounted for. Now all this may be wrong, but Dr. Underwood has not published anything to show that it is wrong, and it is both probable and possible Here then is a disturbing element that will prevent the acceptance of his opinion until it is supplemented by more convincing evidence or positive proof.

A recent writer in the *Linnæan Fern Bulletin* thinks that the burden of proof rests upon those who maintain the hybrid origin for this species. I do not agree with him. I have always supposed that the burden of proving a long accepted theory false rested on those who questioned it. Now in the light of recent investigations it seems possible for one properly equipped for the undertaking, to prove whether *ebenoides* is a hybrid or not. If it is not, but is really a good species, then its spores ought to germinate, and yet I know of but one effort in this direction, and as I never heard of any results from it, I supposed it failed; if it is a hybrid, being a bigeneric one, it ought by every presumption of reason, to be sterile and its spores would not germinate. But no attempt has ever been made that I know of to obtain *ebenoides* by crossing *Camptosoms* and *ebeneum*, as Prof. Eaton suggested long ago, and as Lowe's successful experiments show might be done.

Ebenoides certainly possesses in a marked degree characters belonging to both of its supposed parents, and if it could once be produced by crossing, the question would be definitely settled. But until this is done its pedigree will continue to remain an open

question. In looking over the literature of this subject so far as it is available I find that most of the ferns which have been described as hybrids are mere crosses between varietal forms of some one species, and therefore are not true hybrids. Much of this will depend of course upon one's judgment; some authors regarding as good species what others may consider as mere varieties. But I believe that the actual number of ferns that have apparently originated from the crossing of species whose distinctiveness is recognized by all can be counted upon one's fingers.

One of the most recent and best of these is the New England hybrid *Aspidium cristatum* × *marginale* (Davenport) discovered by Raynal Dodge, of Newburyport. I have had several plants of this fern growing under my immediate observation for the past four years and I can find no reason for changing the views which I expressed when I first published it; but I find as my plants become older they change considerably in their appearance. This year some of the plants look quite different from what they did last year, the lower portion of the largest fronds suggesting *Clintonianum* more than *cristatum*, while some Medford plants that resembled a lax form of *Clintonianum* when found have become more compact and like the others; but in all, the essential characters remain, and these are, the broad *marginale*-like upper portion of the frond with acuminate pinnæ and *cristatum* fruit; the narrower *cristatum*-like lower portion, the *marginale* scales and above all the erect candex with dense central crown.

One of the most striking characteristics is its proneness to develop abortive fronds. Some plants exhibit this tendency more than others, but the most perfect plant I have and which has matured perfect fronds up to August 1st, has during the past three weeks been developing secondary fronds of an abortive character, and this tendency I believe to be one of the strongest characteristics of hybrid ferns.

But I have already made my paper too long, and I will close by expressing the hope that those of you who can will experiment with spore sowing and record the results. It will not be necessary for you to go into any scientific microscopical work to do this, as the process is simple and there is plenty of material available. A shallow flower pot well drained, and filled with a light porous soil, well sterilized, and watered is all you need. Sow the spores on the damp surface, set the pot in a pan of water and

keep it in a warm partially shaded window. You can hardly fail to derive interest and pleasure from watching the development and growth of the prothalli and the young ferns, which you can do with the aid of a good hand lens, and well-grown prothalli can be watched with the unaided eye. The prothalli are variable, especially those that bear proliferous buds, and when they are not fertilized they live a long time. Lowe divided them into quarters and kept them alive for seven years, afterward by putting two parts together, bringing about fertilization and raising a fern plant.

Now if any of you have the facilities for such work it might repay you to try some of Lowe's experiments and in such an event, it may be well remembered that in a work on "Cultivated Plants" by F. W. Burbridge, it is stated that the antherozoides from one prothallium may be transferred on the tip of a sable pencil to the archegonia of another prothallium by means of a drop of water.

But the simpler experiments are easily within your reach, and if *Aspidium cristatum*×*marginale* is a true hybrid it might possibly be reproduced artificially by sowing a mixture of the spores from its supposed parents. If some one of you could accomplish this, and also reproduce *Asplenium ebenoides* in the same way, it would be a great achievement.

Medford, Mass., August, 1898.

AN INTERESTING VARIETY OF OSMUNDA CLAYTONIANA.

By A. J. Grout.

IN the summer of 1897, Mrs. Grout, while riding along a country road in Newfane, Vt., noticed a peculiar *Osmunda*, which she collected and attempted to identify. When my attention was called to it, I saw that it was like nothing of which I had ever seen or heard. A frond was sent to Prof. Underwood, who also declared that he had seen nothing like it. No fruit was collected at this time, but in the summer of 1898 the same form was again collected in the same spot and in fruit.

The two or three lower pairs of pinnæ are much like those of *O. Claytoniana*, except that they are rather more deeply cleft and are oblong and obtuse instead of oblong-lanceolate. The

upper pinnæ, instead of being lanceolate and tapering, are oblanceolate to oblong-oblanceolate, and instead of being cleft into oblong obtuse segments have at their inner ends oblong to oblong-ovate obtuse pinnules, which are alternate and entirely separated by a distance equal to one-half to two-thirds their breadth. The upper-middle pinnules are also entirely separate, but in addition are greatly elongated, oblong-lanceolate and cleft into oblong obtuse segments after the manner of the pinnæ in normal *O. Claytoniana*. In the fruiting specimens the fruit was borne as in *O. Claytoniana*, but the fertile and sterile pinnæ intergraded, as some of the pinnæ were wholly fertile and others bore both fertile and sterile pinnules. Some of the pinnules even were in shape and texture like the sterile pinnules, but have sporangia at their edges. This last peculiarity seems to indicate an abnormality of some sort.

The form and arrangement of the lower pinnules of each pinna suggest a relationship with *O. regalis*, but it is hardly probable, all things considered, that it is a hybrid of this species. It seems quite possible that it may be a partial reversion to an ancestral type.

There were two separate rootstalks growing side by side and evidently derived from the same parental rootstalk by growth and the dying away of the connecting portion. This fact, together with the fact that the same form has been collected for two successive seasons, is pretty strong evidence that the form is permanent and not a monstrosity caused by mutilation or peculiarities of nutrition. The plants grew by a shady roadside in close proximity to many plants of normal *O. Claytoniana*. They were perfectly healthy and there were no evidences or probabilities of mutilation, as the plants were in a position where they were not likely to be much disturbed. They were close to the ditch, but beside a large boulder that had not been moved, and would have protected the plants from all probable interference.

Plymouth, N. H.

NOTES ON THE FERNS OF THE URAL AND CAUCASUS MOUNTAINS.

By Mary A. Fleming.

WHEN, in 1897, I went to St. Petersburg to attend the International Congress of Geologists, and to take part in its excursions to the Ural and Caucasus mountains, I imagined I should gather many ferns. From Moscow to the Ural mountains we traveled over the black zone of Russia, which resembles very much the prairies of the western part of the United States. Like everything in that vast country, the plants are simply enormous. The rich black earth is very fertile, and the grasses and other herbage were very luxuriant. When we reached the European side of the mountains we found no ferns. The upper parts are destitute of vegetation, and though on the lower slopes there are forests of birch and pine trees, there were no ferns seen until we reached the Asiatic slope. We did not enter the woods to search for them, but on the edges of the forests, for miles, we saw immense plants of *Osmunda*. Its great fronds were the largest I ever saw. The French and Germans expressed great surprise when we found no other ferns. The tops of these not very high mountains are in great, bare rock-masses, quite difficult to ascend. Some of us contemplated running away for a botanical excursion into the woods, but found each day's program too fascinating, and there was danger of being lost in that sparsely-settled region, if we left the main party, guided by the Russians.

On our journey to the Caucasus we passed over immense steppes, covered with tall plants, each striving to stretch its stout stems above its neighbors. Among these plants, the legends of Urania say, the Cossack can with his horse hide from his foes. In this sea of flowers mallows predominate. Thousands of beautiful bright-colored flowers, six feet or more in height, carpet the valleys of the Caucasus, and on the limestone crags are found, in great masses, a few varieties of ferns, very like our own species. On the hillsides is a medley of European forest trees with the most wonderful undergrowth, of which all travelers speak, of Laburnums, Azalias, Rhododendrons and Laurels; farther up are the Crystalline Schists, quite bare, and above them gleam the white tops covered with eternal snows. The few ferns which I

found were *Pteris aquilina*, in immense quantities and very large, quite gigantic; *Polypodium vulgare*, *Adiantum capillus-veneris* and *Cystopteris fragilis*. All these except the last were quite large, and resembled very much those found in our own country, except the Adiantum, and all seemed like those found all over Europe. As we were constantly reminded that our excursion was geological and not botanical, we found few opportunities to gather even the ferns which we saw, and, when about to secure some, were often hurried to some other locality. I know that in the deep forests and in other portions of the chain other ferns, gigantic, it is said, are to be found. But those named were the only ones I was fortunate enough to gather. The upper limit of tree growth is 7,126 feet in some places, but magnificent Primulas are found at 12,000 feet, and very beautiful Asters and Blue Gentians grow on the moraines of the many great glaciers. Lilies and other flowers are abundant. The slopes of these lofty mountains are in the spring continually subject to avalanches, which cause the surface to vary constantly, and where one finds lovely flowers and great ferns, another a month later may find only bleak desolation. The combination of valley or glade carpeted with flowers, then lofty fruit and forest trees, fern and moss-covered cliffs, then bare rocks and glistening snow, is singularly beautiful, and the only scenery to compare with it is that of our own mountains in the northwest.

ON THE DISTRIBUTION OF SOME EASTERN AMERICAN FERNS.

By Willard N. Clute.

I THINK the announcement of the discovery of a new station for the Hart's-tongue fern in New York State will be news to most of you. Within a month it has been found growing abundantly at Perryville Falls in Madison county. The locality is of peculiar interest to me from the fact that I paid it a short visit on my way to Chittenango Falls for specimens of the Hart's-tongue, and at the time asserted my belief that the fern could be found there also. At Chittenango Falls it grows very plentifully, and is likely to continue to thrive, since the spot is one of particular beauty and is protected by an association formed for the purpose. To this place is often credited the honor of being the first in America in which the Hart's-tongue fern was discovered.

The true locality, however, in which it was found by Frederic Pursh is at Geddes, a small town further west. Until recently the fern was thought to be extinct in this station, but was rediscovered last summer, I believe.

Perryville Falls, at which place the most recent discovery was made, is only a few miles from Chittenango and is very much like it in appearance; a deep ravine shut in by limestone cliffs, a waterfall, making the air moist with its spray, dense shade and rich soil. In sunny nooks appear *Pellæa atropurpurea* and *Asplenium ruta-muraria*, while in the shade may be found *Dryopteris Goldieana* and the Walking-fern.

The question has often been raised as to the origin of *Scolopendrium* in America. Is it the remnant of a once greater distribution? Is it increasing, or has it existed in about the same numbers for an indefinite length of time? Its wide distribution in other lands is well known, especially in England, where it is one of the most abundant of ferns growing on old walls, along roadsides and in the mouth of old wells. But in America, wherever found, it is tucked away in some dark ravine—one of our rarest ferns. If it is a remnant, the question may well be asked: Why has it decreased here, since it thrives so well in other countries in about the same climate? And if it is of comparatively recent introduction, why has it not increased in the time we have known it? It is found in several widely different parts of the country. If its means of dispersion were as limited as those of the Trailing Arbutus, for instance, the distances of the localities apart might seem to indicate that it is a remnant, but its spores are so light and so numerous that there seems to be no reason why it should not be as common everywhere here as in England.

In regard to its ability to conquer new country, it ought at least to be on a par with the Dandelion. This is what Mr. Charles T. Druery says of the spore production of another species: "Recently I took a large species of *Athyrium*, the backs of whose fronds were literally brown with sori, and after somewhat elaborate calculation of the number of fronds, number of pinnæ, number of pinnules, number of sori per pinnule, number of capsules per sorus, and finally number of spores per capsule, I came out with a very mighty string of figures which read as eleven hundred millions." Those who have examined fruiting fronds of *Scolopendrium*, with their long parallel lines of sporangia, know well that this species would not be far behind

Athyrium in the production of spores; in fact the fertile fronds are noticeably heavier than the sterile ones.

My own opinion with regard to the distribution of *Scolopendrium* in America is that it is more plentiful than it has been thought to be, and this opinion I find is shared by one of our members, my friend, Mr. Maxon, who lives in the *Scolopendrium* region of New York and has proposed a special hunt for it. There are numerous deep ravines in the vicinity similar to those in which it grows, and I believe it will yet be found in many of them.

Another fern to which fully as much attention attaches as to *Scolopendrium* is the little curly grass (*Schizæa pusilla*). It, too, has been found at widely distant points. The Rev. Mr. Waghorne found it recently in Newfoundland; Mrs. Britton collected it in Nova Scotia, and many collectors before and since have found it growing in the marshes of its New Jersey haunts. Who will say that it does not grow in many intermediate places? Its small size is an added element of difficulty in its discovery, while those who have never seen it growing might easily pass it, not knowing exactly what to look for, or where to search. If you will call to mind some half-desiccated cranberry marsh, interspersed with little dryish knolls on which the blueberry grows, and imagine *Schizæa* growing at their bases just on the border of the damper places, in company with Lycopodiums, Sundews, Sedges and other marsh vegetation, you will have an idea of how I have seen this fern growing.

Schizæa has never been found far from the coast, I think, but this does not imply that it needs the vicinity of the sea, for its haunts are in fresh water marshes. There is a much larger margin for speculation regarding the distribution of this fern than that of *Scolopendrium*, and we may confidently predict that it will be found in many places where it is now overlooked. I have not given up hope of finding it in some Long Island marsh.

To give one other and still more striking instance of an American fern with a very limited distribution, which is likely always to remain so, I need only mention *Trichomanes Petersii*. As yet it is only known from one small ravine in northern Alabama, but while its range is certainly very limited, none of us will be much surprised when some day other stations for it are recorded. When it is remembered that the section in which it grows has been explored only superficially and that the whole

aspect of the plant is moss-like, the individual fronds so small that a penny will cover several, it is not surprising that it has not been more frequently found. Indeed, its discovery must always be a sort of "lucky accident." How such a mite of a fern ever got into its present place, what its ancestors were, and by what chain of missing links it has descended from the nearest related species, would make interesting reading, I am sure, if we could make it out.

To come to more familiar fern-worts, instances are multiplying of their occurrence much beyond what we usually regard as their geographical limits. So far as can be judged from sterile specimens, *Adiantum capillus-Veneris* has been found in New York State; Mr. Eaton has placed on record a station for *Lycopodium alopecuroides* on an island along the coast of Massachusetts; and *Polypodium polypodioides* has been reported from Staten Island and northern Pennsylvania. I leave it to wiser heads to settle whether these are recent and permanent extensions of ranges; whether they are remnants of what was once a much wider range or only adventitious plants. And however it may be decided, the fact remains that we need not cease expecting to find a plant because we may be somewhat out of its known range. Concerning *Lycopodium alopecuroides*, I may add that I have found what appears to be this species on Long Island. When collected it was too young for satisfactory identification, but, from its manner of growth, I think it may be *alopecuroides*. I hope to collect it later in the season and make sure. Until Mr. Eaton discovered it in Massachusetts, it was not known from farther north than New Jersey.

Before leaving this subject, it may be well to say a few words about our ferns, or what are supposed to be our ferns, in foreign lands. The fact is recognized that several of our eastern American ferns have been found in far-away places. In this category I shall not consider species of such world-wide distribution as the Polypody, Bracken, *Dryopteris aculeata*, etc., nor yet the species that are common to the far north in both hemispheres, as *Asplenium ruta-muraria*, *Asplenium viride*, *Phegopteris Phegopteris* and others. The latter might have been distributed from any point in the north, but how *Onoclea sensibilis* and other ferns have bridged the distance from here to Japan is not so easily conceived.

There are three theories which aim to explain how this and

similar occurrences may have come about. The first and in some ways the most plausible is that the ferns have spread from a common center, the intervening plants dying out meanwhile, leaving these distinct colonies to mark the extent of their range. The second is, that one or more of these distinct localities has been populated from spores blown from another over perhaps thousands of miles of country. The most interesting theory, however, is probably the one that gives to these widely separated stations a separate origin. If a certain set of conditions can make *Adiantum pedatum* in America, why cannot a similar set make the same fern in Japan and do away with any reasoning that attempts to connect the two with a common ancestor?

Some slight endorsement is given this latter theory by the attitude of those botanists who refuse to recognize some of these foreign species as identical with ours. This is true regarding our *Botrychium ternatum*, upon whose identity with the Japanese species it is likely the last word has not been said.* I have seen specimens labeled *Botrychium Virginianum* from Japan, which I should hesitate to call identical with our own. That these and others are not exactly like ours may not militate against the fact of their having a common ancestor, but it may against their being the same species at present. It is quite reasonable to suppose that slight variation in the climate and soil of places far apart would in time slightly change these plants were they the same in the beginning, but whether such change has proceeded far enough to warrant the separation of another species is a matter for further discussion.

In a paper of this length no attempt can be made to settle any of the questions I have raised. If I have suggested topics for further thought and discussion, or directed the attention of others to these problems of fern distribution, my purpose will have been served.

N. Y. Botanical Garden, N. Y. City.

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*Since this was written, Dr. Underwood has published a revision of this and related species, and decides that there is no true *B. ternatum* in America.

ON THE GENERA OF FERNS: A STUDY OF THE TRIBE ASPIDIEÆ.

By B. D. Gilbert.

AFTER a short introduction, in which the speaker referred to the many specific changes in nomenclature that have taken place in recent years, he said:

While all this revision in the species of ferns is going on, it has occurred to me that some change in the genera of ferns might very properly be made in the interest of a more rational and natural arrangement of that family of plants. One generic change has already been made, viz., the adoption of *Dryopteris* in place of *Aspidium* and *Nephrodium;* but this seems rather to complicate than to simplify the situation. As I intend to confine my observations at the present time to the tribe *Aspidieæ*, let me explain what I mean by this assertion.

The tribe *Aspidieæ* has always been a bothersome one for systematists to handle. It stands on the border-land between the indusiate and the non-indusiate ferns, and hence contains characteristics belonging to both. The natural affinities between the ferns placed in this tribe and those of the sub-genera *Phegopteris* and *Goniopteris* (according to Hooker), which were formerly placed in the tribe *Polypodieæ* through absence of an involucre, were so strong that it was actually impossible to say where one ended and the other began. Numerous instances could be cited from Hooker's "Species Filicum" to illustrate this fact. For instance, under *Nephrodium oligocarpum* Hooker says: "So difficult is it to see an involucre, that, but for Mettenius having so accurately described it, I should have preferred to retain it in *Polypodium* (*Phegopteris*)." Again under *Nephrodium pilosohispidum* he says: "The involucres are so minute that I feel doubtful whether they really exist." These are only examples of remarks that occur quite frequently. Then it is well known that a creeping fern of the West Indies has been placed by some authors in *Polypodium* as *P.* (*Goniopteris*) *reptans*, and by others in *Nephrodium*, as *N. reptans*. In the same way Mettenius named a plant *Aspidium scolopendrioides*, and Hooker in "Species Filicum" followed him, saying: "It is true our own specimens show no trace of involucres, but Mettenius describes them, and there is no question of his

accuracy." At the same time both Fée and Presl put the plant in *Polypodium* (*Goniopteris*), following Schwartz, who originally named the species. Again under *Phegopteris rigidum*, in the "Synopsis Filicum," it is stated that the species is "very variable, the forms differing only by the want of an involucre from the forms of *Aspidium aculeatum*, to which it shouid probably be joined." Also in Mr. Baker's "New Ferns," published as late as 1892, he says of *Nephrodium hastatum Jenmani:* "I believe it is an *indusiate form* of *Polypodium obliteratum* Swartz." And finally, at the beginning of *Nephrodium* in "Synopsis Filicum," it is stated that "it is perhaps scarcely needful to warn the young student to bear constantly in mind that an *Aspidium* with an abortive or obliterated involucre is not distinguishable from a Desmobryoid *Polypodium.*"

On June 22, 1898, I gathered a plant of *Dryopteris Noveboracensis*, on which there were two sterile and two fertile fronds. One of the latter is perfectly normal and possesses distinct sori, with beautifully developed indusia. The other has the sori grouped as thickly as they can lie, with *not an indusium on them.* The time of year when gathered and the green appearance of the sori testify to the youth of both fronds, so there can be no question raised as to the indusium having disappeared. Thus, on the same plant, there is an indusiate and a non-indusiate frond. Here is an example right from nature to show how unreliable is the indusium as a broad distinction between genera. A generic description should certainly be composed of the most stable and uniform characters belonging to a certain group of plants, and this is what cannot be predicated of the organ called an indusium.

When we come to the distinction drawn by Hooker, both in his "Species Filicum" and in the "Synopsis," between *Aspidium* and *Nephrodium*, viz., a peltate involucre for *Aspidium* and a kidney-shaped one for *Nephrodium*, we find even Mr. Baker in his "New Ferns," declaring, "I should not in a new book keep up *Aspidinm* and *Nephrodium* as genera." The change which has been made from these genera to *Dryopteris*, therefore, seems to have the sanction of the best authority in England, at least in a negative manner.

All this goes to show that *Phegopteris* can only be told from *Nephrodium* (*Lastræa*), and *Goniopteris* from *Eu-Nephrodium* by the presence or absence of an involucre; and that in a number of cases where it is doubtful whether an involucre exists, the

fern has been placed in one genus by one author and in another genus by a second author, The fact is that if a person looks over the "Synopsis Filicum" with a view to investigate this question, he will be surprised at the number of *Nephrodium* species of which it is said that the involucre is "fugacious," or "very fugacious," which means that the involucre is never seen on them; or that the capsules are entirely "naked." Any one who has had occasion to study long suites of specimens in an herbarium knows to his vexation that this fugacious character is no mere figure of speech.

Thus we have at the present time two genera taking the place of three previous ones, and under *Dryopteris* a still greater aggregation of species than before. Meanwhile new species of this genus are being published from time to time, and its number is being increased and becoming more and more unwieldy. The last edition of "Synopsis Filicum" described 360 species of *Aspidium* and *Nephrodium*, while Baker's "New Ferns" gives 166 more for the two species. This makes 526 species for *Dryopteris;* but as Baker's book was published in 1892, the probability is that the genus by this time numbers 550 species. This is exclusive of a large number of forms which may be varieties, or may prove to be good species.

It may be urged that the genus *Polypodium*, after taking out *Phegopteris*, *Goniopteris* and *Dictyopteris*, will still aggregate about the same number as I have calculated for *Dryopteris*. That is correct; but it must be remembered that the Eremobryoid *Polypodia* are divided into five very distinct sub-genera by their venation, which is so distinct and constant as almost, if not fully to warrant the establishment of genera upon their different forms. In *Dryopteris* this is not the case. Leaving out of sight for the present the sub-genera given in "Synopsis Filicum" as *Eu-Aspidium* and *Sagenia*, there remain only two distinctions in the way of venation to divide the entire mass, viz., the free-veined and the pinnate-veined, with opposite groups of veins more or less united. *Polypodium*, therefore, stands on an entirely different footing, with regard to its sub-genera, from the equally large genus *Drypoteris*, because the former has such distinctive zones of venation.

I now come to the main purpose for which this paper is written, viz., some suggestions as to the rearrangement of the 600 to 700 species about which we have already been talking. It is per-

fectly evident, from what has been said, that between *Lastræa* and *Phegopteris*, *Nephrodium* and *Goniopteris*, there is absolutely no distinction beyond the presence or absence of an involucre; and that in a large number of instances it is utterly impossible to decide whether the sori originally have involucres or not. Is it worth while to maintain a distinction that is honored almost as often in the breach as in the observance? By the readoption of *Dryopteris* the distinction between peltate indusia and kidney-shaped indusia has been practically broken down as a generic distinction. Why not, then, do away with the barrier created by an indusium, when it prevents the formation of natural groups? For, as I understand it, this is what modern botany is striving for continually—the grouping together of plants whose natural modes of growth and natural affinities show that they are more or less closely related. In order to bring this about among the ferns, I believe that we must lay aside some of the ancient traditions of the order, ignore to a certain extent the old divisions of "involucrate" and "ex-involucrate"—ignore possibly the shape of the sorus itself, as in a certain section of *Gymnogramme*—and rearrange the species according to their natural affinities. Let us see to what result that would bring us in the tribe *Aspidieæ*.

In the first place, there is no more distinct and natural group among the ferns than that which was designated by Roth, Schott and Presl as *Polystichum*. So true is this that the "polystichoid habit" has come to be as well known among pteridologists as the "lomarioid habit," or the "pteroid habit," and I cannot see why it is not as much entitled to generic rank as any other strong, natural, distinctive growth. Here also, as in *Dryopteris*, we find a few species classed with *Phegopteris* simply because no indusia are evident with their sori, but the natural habit is so strongly polystichoid that no one would hesitate to place them in *Polystichum* who was not hampered with the idea that the presence or absence of an indusium is a fundamental principle of the science. I would make *Polystichum* a full-fledged genus, and would remove to it the polystichoid species of *Phegopteris*. As thus constituted, the genus would contain about sixty-three species.

Next I would make the genus *Dryopteris* cover all those species included by Hooker in the free-veined section of *Nephrodium* and designated as the sub-genus *Lastræa;* and would re-

move to it all the species of *Phegopteris* proper, except such as had already been placed in *Polystichum*. This would make a genus of the first rank, comprising about 390 species.

Third, the genus *Nephrodium* should include the species ranked in "Synopsis Filicum" under *Eu-Nephrodium* and *Goniopteris*, forming a genus of the second rank, with about 125 species.

Finally, I would retain Swartz's old genus *Aspidium* and make it include all the species placed, in "Synopsis Filicum," under the sub-genera *Eu-Aspidium*, *Cyrtomium*, *Sagenia*, *Pleœnemia* and *Dictyopteris*. By obliterating the distinction between peltate and kidney-shaped indusia, the four most important of these sub-genera are already united; by ignoring the absence of indusia, *Dryopteris* would be united with the others, and a very natural single group would be formed of the whole. This would constitute another genus of the second rank, having about 113 species. The first rank includes only three genera, viz., *Asplenium*, *Dryopteris* and *Polypodium*, each having 300 species or more.

This arrangement has, in my opinion, one great advantage over and above its usefulness as a means of classification; it retains the old and familiar generic names of *Aspidium* and *Nephrodium*, with much of their former significance. Probably this suggestion will be regarded by many as purely sentimental, and I am free to allow that it may be so. But if you desire to bring about unity and harmony among the students of ferns, it is wiser to cater to such a "sentiment" than to override it by laying down arbitrary rules which ignore it altogether. No one can complain that *Dryopteris* has not been treated fairly, since much the largest group of species is placed under this title, and it thus receives the merit due to it by reason of its priority in time. By the addition of *Phegopteris* to this group, it would be considerably increased beyond the old limits of *Lastræa*. But it would be divisible into several sections, just as it is now, some with simple veins, others with branched veins, some with reduced lower pinnæ, others truncate at the base, some deltoid, some spinulose, and so on. A statement would have to be made with each species, as indeed in many cases it has to be made even now, as to whether an indusium was known or not; but there seems to me to be no doubt that a more natural and satisfactory arrangement of species could be made under this plan than by any

method at present existing. It would also do away with the uncertainty which now assails the botanist whenever he receives a new fern belonging to this tribe, which has not an indusium evident Where shall it be placed? Is it a *Lastræa*, or a *Phegopteris*, a *Nephrodium* or a *Goniopteris?* Such are the questions that arise, and time is wasted, as well as patience, in searching first in one place, then in another, to determine the identity of the new acquisition.

These suggestions are such as have occurred to me in my own study of this great family of plants. I do not claim for them anything especially original. John Smith, the former curator of Kew Gardens, a most careful and painstaking observer, with an immense collection of living species open to his inspection, in his "Ferns, British and Foreign," published in 1866, was the first to bring forward the two grand differences in the growth of the stipes from the rhizome, which in his belief divided the true ferns into two sections somewhat analagous to the Exogens and Endogens in flowering plants, although far from being of the same value as the latter distinctions. At the same time the Desmobryous section, with stipes adherent to the stem, and the Eremobryous section, having the stipes articulated with rhizome, do undoubtedly divide the *Polypodiaceæ* into very natural groups, in which the indusium is ignored as a distinguishing feature of the tribes, but is retained as a feature of the genera. The only feature which I have proposed, that does not seem to have been proposed previously, is to do away with the indusium even as a generic distinction, and to arrange the species according to their natural affinities in growth, without reference to the old classification by indusia, except as the indusium is mentioned in the specific description. I have tried to take up the question from the view-point of simplicity and orderliness, and have made an effort to look at it as if there were no traditions behind us to hamper our views, or to make us feel that we must in a measure be guided by them. Something of the same kind will have to be done with the large genus *Gymnogramme*, a dozen or more species of which should also be transferred to *Dryopteris*. In fact Kuhn has already monographed what he calls the *Gymnogrammeæ* and separated them into eight genera. German pteridologists are fond of proposing new genera, which American and English botanists, in most cases, hardly feel willing to accept, but we ought not to be too conservative.

My suggestions are not presented as a finality, for many of the details have not yet been worked out; but it seems to me that it is along this line that future improvement in classification of the ferns will have to be made. These and other changes are destined to come soon, for the reason that they affect the natural affinities and relationships of the ferns, and because by doing away with artificial distinctions they will help to bring about a more natural classification.

NOTES ON A PECULIAR BOTRYCHIUM.

By Alvah A. Eaton.

WHILE hunting for capsules on Sphagnum in a maple swamp at the foot of the north slope of a hill, in June, 1895, I observed numerous specimens of a small *Botrychium* just appearing above the moss. It was at first supposed to be *matricariæfolium* belated by its cool position, but that species was prime June 20th, and situation seems to make little difference in its date. These specimens did not mature until July 20th, a full month later. I have since found it in nine localities, in the towns of Kingston, Kensington, Hampton Falls and Seabrook, N. H., and Salisbury, Byfield and Amesbury, Mass. Mr. Dodge adds West Newbury, and has found it in several places in the other towns named. In some localities it appears to be abundant, as I have collected 250 specimens in a small space (less than two square rods), in about an hour. I find it usually among maples, on the borders of the depressions where the leaves accumulate and soak in the water which fills them in winter and spring.

Usually they are in close proximity to a sluggish brook, which percolates through the soil rather than runs, keeping it in a state of constant saturation. Given these conditions on the north slope of a hill, and it will almost certainly be found in the deep shade at the border of the swamp among dwarf *Rhus toxidodendron*, *Onoclea sensibilis*, or other plants which may be scattered about, but having a hard time of it on account of the shade.

Often *B. matricarœfolium* and *lanceolatum* are found with it, where the area extends into a more open space. The width of the patch is usually circumscribed to from three to six feet.

The soil is always the richest leaf-mould, in which the bud is

sunk to the depth of three or four inches in the largest plants, one-third to one-half the stem being below the surface.

There is a peculiar provision for keeping the bud from being covered too deeply by the accumulation of leaves. The bud of the following year, after the development of the current year's growth, grows up into the stem of the present plant, which is hollow for the purpose, to the height of ¼-inch or more. There it puts forth a set of roots and lies dormant till the following spring. This provision is common to the Botrychia, and it is not rare to find three sets of roots on a specimen.

I have two plants of this species which have divided, evidently by two buds instead of one being formed, thus having two plants from the same root. I have noticed the same phenomenon in *matricariæfolium*, *Virginianum* and *obliquum*. In the first two named I have examples showing three well-developed plants from one root.

Sunlight apparently is fatal to this plant. Two good localities, one of them the original, have been destroyed by cutting the wood which protected it. In localities where there is an open place in the center, it is found in the deep shade of the south side, but is absent from the opposite side, where the sun's rays strike under the trees. I know of but one locality where it grows on the north side of a run, and there the trees are dense way across. Usually the maple leaves are pressed compactly down by the lingering snows of winter, and are held there by the dampness. Many small plants, and often quite large ones, get lost beneath them and fail to reach the surface; but they may be found by stirring the leaves, and fruit as well, and are apparently as healthy as those which come to the air.

In size the plants vary from a tiny thread-like stem one inch high, with minute sterile frond and a fertile frond of one or two sporangia, to a slender, usually decumbent specimen nine inches long, of which three inches is below ground. The average height above ground is two inches, and most commonly the sterile lamina is sessile or slightly stalked, less than ¼-inch long, the edges inflexed and top bent down, just as it covered the fertile division. It often covers the top in fruit, but usually the fertile is a little longer.

In this state the sterile division bears one lobe or notch on each side, and the apex is emarginate. Often it bears a sporangium, and may even bear one or two on each lobe. From this

there may be found a regular series up to the full developed form, 1¾ inches long, of which ¾-inch is petiole. There are in this two or three approximate pairs of semi-lunate lobes, the lower of which are alternate, and all decurrent. The lower lobes are usually separated from the next pair by three times their diameter, giving the frond a peculiarly attenuate appearance. The upper pair of lobes is near the terminal lobe. This is emarginate, as are those of the smaller plants, or it may bear in the sinus a rudiment of a continuation in the form of a triangular process, corresponding to the rachis and pair of lobes.

In only one specimen have I seen a segment incised; then there was only one split on one of the lower lobes. There is no midvein in the segment, but the main rachis sends out three or four veins to each, which dichotomously fork twice, once near the base and again near the margin. In small specimens the fertile division is overtopped by the sterile, but in the larger plants the sterile division reaches only the base of the fertile. The latter vary in size from a short-stalked division bearing one or two sporangia to a spike two inches in length. The spikes are usually simple, but occasionally send out one or two short branches near the base, and at times a group of 3–6 sporangia grow on a small elevation of the main rachis. The sporangia are sessile, or even sunk in the tissue of the rachis. After dehiscence one can look into the interior with a lens, and there is nothing to obstruct the view but the outer wall of the back of the rachis. The sporangia are set in groups, usually in pairs or fours, with an interval of several times the diameter of the group before the next appears. Sometimes they are an inch apart. The normal number of sporangia is five to eight, but large plants may have as many as fifty. The spores are larger than any of *simplex* I have seen, being 40–60 μ.

The color of the plant is at first a lively green, but rapidly becomes yellowish, even before fructification; and after that event are decidedly stramineous. They are rarely erect when the plant is of considerable size, and are usually decumbent after maturity. When dried under a moderate pressure the stems are transparent.

The vernation suggests *lunaria*. The fertile division is erect or the top is a little inflexed. The sterile division grasps the fertile at the sides, the segments are imbricated, and the terminal segment is bent to cap the fertile. Often, as remarked, the sterile

segment never opens out, but keeps the form assumed in the bud.

The time of fruiting is about the same as with *lanceolatum*. I have found a few ready to disseminate spores the first of July, but most are ripe about the 20th, and it is well into August before the last attain maturity. The largest specimens resemble *matricariæfolium* in the position of the sterile frond, three-quarters the way up the stem, but this is always simply pinnate and that is incised and lobed, even in small plants. The smaller specimens remind one of *lanceolatum* from the position of the sterile frond being sessile at the top of the stem; but its simple character again distinguishes it, as does the simple spike. *Botrychium lunaria* is the only species having a similar arrangement of the sterile segments, and, strangely enough, the vernation agrees also. A glance at the two will convince one that they are distinct. The fact that *lunaria* is so rare and grows on dry knolls, besides having the sterile segment sessile, is enough to cause one to hesitate long before ascribing to that species a plant of so different aspect and habitat, and which is so common, because of the vernation and other minor similarities. The vernation may be diagnostic for the species now known, but that is no argument that other species of precisely the same vernation may not exist.

A prominent pteridologist to whom some of this was sent called it *simplex*, "but unsatisfactory, as *simplex* always is when growing in the shade." In view of this I have tabulated the differences between them as observed in four years' study.

Simplex has, to my knowledge, been found but once east of the Connecticut river in Massachusetts, Mr. Sears having found a few at Salem. Last June I found four specimens on a gravelly hill of 150 feet elevation in full sun, among *Antennaria plantaginæfolia*, as directed by Mr. Eggleston, of Rutland, Vt. This is the only New Hampshire station I am aware of. The species under discussion is apparently very common, but overlooked because so small.

Simplex has the sterile frond from base or below the middle, is very fleshy throughout, and in the fresh state has a bluish cast similar to *matricariæfolium*. This plant has the sterile frond above the middle, the texture is not much fleshy, the color is yellowish. (The variety *fallax* of *simplex* has the frond above the middle, but it appears to be accidental, occurring amongst the true form. I have seen only very small plants so constructed, and the appearance is doubtless abnormal.)

The larger specimens of *simplex* have ternate fronds, which are again pinnate, the ultimate segments again incised, being twice and a half pinnate. All plants of lesser size show a tendency to the same state, although they rarely attain it save in the far west. The fertile spike is usually compound, even in the small plants. The variety in question, on the other hand, usually has a simple frond, never more than once pinnate, and but for the fewer number of segments and the distance apart, might be taken for *lunaria*. The sterile lamina of *simplex* has the terminal segment rounded, while in this variety it is emarginate. The fertile division is normally simple and rarely bears more than one or two branches below. The bases of the old stalks persist in *simplex*, giving a bulbous appearance. They die away in this plant. *Simplex* fruits about June 1st; this from July 1st to August 20, mostly at July 20. *Simplex* grows in full sun on gravelly hills, where there is little competition on account of aridity and sterility; this in richest woods, where there is little competition on account of shade and moisture. *Simplex* is a stout, fleshy, stiffly-erect plant; this is slender and inclined, even becoming prostrate in the later stages.

I might mention that whereas most authorities agree in ascribing *simplex* to dry knolls in the sun, Britton and Brown give its habitat as "moist woods, meadows or swamps," a statement which needs investigation.

That this is a new species there can be little doubt. Though not in sympathy with the tendency shown at various periods in botanical history to make a species of every form when it is known it cannot reproduce its like, neither am I in sympathy with the other extreme, which lumps all specimens which have a few points in common into one species cover. The more rational way is the one happily on the increase—to give each form sufficient study to see if it be entitled to specific rank, and then rank it accordingly.

Simplex, *matricariæfolium* and *lunaria* have at various times been confounded and variously combined. No one who has ever collected them is in any danger of making a mistake, even in the most poorly developed forms. Personally this form is as easily recognized as any other species, as it is also by Mr. Dodge, who has seen it in several localities in which I have not.

This is apparently well distributed in New England, as I have seen specimens from Tamworth and Jackson, among the

White Mountains, and from Charlotte, Vt. The conditions for its growth seem to be presented in many places in western Massachusetts and in Ontario, and along the wooded belt to Minnesota, and I have no doubt it will be found over most of this area as soon as collectors begin to search for it.

Seabrook, N. H.

STUDY OF OPHIOGLOSSUM.

By Mrs. E. G. Britton.

MRS. BRITTON stated that her observations on *Ophioglossum* were already in print in several journals, and as the session had been a long one and it was already late, she would omit any formal presentation of the subject, being content to show the specimens belonging to the herbarium of Columbia University to Mr. Gilbert and compare them with Mr. Davenport's. Mr. Alvah A. Eaton had brought some very interesting small forms, closely resembling the specimens from Italy collected by Rigo, preserved in the Gray Herbarium, and had duplicates for distribution. Mrs. Britton stated that the modern tendency of ecological studies in botany as illustrated by Kerner von Marilaun's "Pflanzen-leben" and the "Plant Geography of Nebraska" by Clements and Pound, showed that field observations of living species, to study their adaptations to their environment, and the recent awakening of interest in nature study in all the public schools, was bringing up a race of young students who did not care so much for the name of a plant as for what it did. In that light she looked on *Ophioglossum arenarium* not so much as a "new species" as a form of *Ophioglossum* which had originated from *O. vulgatum*, but from living in sandy soil near the sea had acquired a fleshy tissue, a stunted growth, and a gregarious habit, which differed considerably from its relatives in moist meadows and woods. She said it did not matter so much to the younger generation whether plants were ranked as species, varieties, races or forms, when it was remembered what different conceptions of these terms are held in classifying living things from man down to the bacteria. But what did matter was that all variations and adaptations to environment should be recognized. The aim of all modern teachers is to train the eye to see differences and the mind to recognize the truth, in all things. If the definition of a species as given by Dr. Farlow in his address

before the Section of Botany at the recent meeting of the American Association for the Advancement of Science be remembered, "a species is a perennial succession of like individuals," then either *Ophioglossum vulgatum*, as understood by Mr. Baker and Mr. Davenport, including as it did *O. Engelmanni*, *O. Alaskanum*, *O. Californicum* and *O. arenarium*, is either a very variable species, with marked differences in habitat and geographical distribution, or the modern view is more correct, and the subdivision of such a species is advisable. Mrs. Britton offered to show the differences and made sketches and determinations after the meeting from material which had been brought in to show her.