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New Garden Plants.

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Since the time when Sir W. Hooker described his unique Dendrobium amboinense, no such remarkable and extraordinary gorgeous Dendrobe has been discovered. Imagine a dwarf stem three inches high, with shining internodes a little thicker at their upper ends, and then add a flower with linear sepals and petals, nearly two inches long when dried, and a long pandurate lip narrower towards its obtuse tip. All these organs appear, when dried, whitish-yellow, with a deep lilac blotch at the base of the lip. I have only a single flower, not cohering with the stem. To judge, however, from the little scars on the stem it must be a Eudendrobium. I believe a single plant of it has been found in Burmah by Mr. Boxall, who must have been filled with enthusiasm at the sight, since he dried it! One feels that the Burmese plants incline to the Malayan type, since it is impossible not to think of Renanthera flos-aëris (Arachnanthe moschifera). I was favoured with the flower and two stems by Mr. Low. Of course I can only describe the things as they are, and am unable to say that the stems may not become longer. *H. G. Rchb. f.*

PLEOCNEMIA LEUZEANA, *Presl, Tent. Pterid.* 183; *Id. Epim. Bot.* 50. POLYPODIUM LEUZEANUM, *Gaudichaud, Frey. Voy.* 371, t. 6. ASPIDIUM LEUZEANUM, *Kunze, Bot. Zeit.* xiv. 474. NEPHRODIUM LEUZEANUM, *Hooker, Sp. Fil.* iv. 61.

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CARNIVOROUS PLANTS.

DR. HOOKER'S address to the Department of Zoology and Botany of the British Association at Belfast is by far the best and clearest statement of the position of the believers in carnivorous plants that I have met with, and not only merits respectful consideration on account of the position and abilities of the author, but the evident sincerity of his search after the truth almost, in common fairness, requires that any one who thinks he can throw light on the subject should contribute his views and observations in reply. It is with this feeling that I shall note the result of one or two attempts that I made during the last six weeks to ascertain whether our common Pinguicula had carnivorous properties or not. It may be remembered that at the last meeting of the Scientific Committee of the Royal Horticultural Society (on which occasion Dr. Hooker was in the chair) Professor Thistleton Dyer brought before the meeting some experiments that had been made by Mr. Darwin upon the Pinguicula, which led him to believe that, like Dionæa, it, too, was a carnivorous plant, deriving sustenance from the insects which it captured on its leaves. I have been staying in the Ochil Hills in Kinross-shire, where I had an abundance of material to observe, and a fair proportion of both dry and wet weather so as to see the behaviour of the plant under both conditions. The first thing of which I convinced myself was that, whether it was carnivorous or not, Pinguicula was rightly regarded by Mr. Darwin as coming under the same category as Dionæa and Drosera. It was a fly-catcher and a fly-dissolver—whether it was a fly-digester is a different thing—but neither on that point, any more than on the other, can it be separated from them. If the one digests, the other will no doubt do so likewise. If the one does not, neither will the other. A comparison of the points on which the carnivorous theory rests, will show how far these different plants correspond, and will at the same time enable me to say how far my observations do, or do not, confirm or warrant the conclusion of Mr. Darwin, Dr. Hooker, and others. Dr. Hooker enables me to contrast them easily, for he puts the salient points in favour of carnivorous feeding very clearly.

In Dionæa the phenomena consist—1, of a certain amount of irritability by which insects are captured; 2, of electricity displayed during contraction, in the same way as during muscular action in animals; 3, of the exudation of an acid secretion by which the captured insects are dissolved; and 4, of the digestion of the insects and the assimilation of their substance by the plant.

Now, first, as to the irritability, I am bound to confess that my experiments have only a qualified value. Mr. Darwin and others have found that after a fly has been entangled in the sticky secretion formed on the surface of the leaf of Pinguicula vulgaris, the margins of the leaf slowly and gradually curl over it so that at the end of twenty-four hours the insect is completely covered by it. I could not get my Pinguiculas to do this. In fact my experience is wholly negative. As any one can verify, the leaves are found with their margins in all stages of curling over, some with no insect on them much more curled over than others with several. I do not mean to say that they have no irritability, but I think I can safely say that it is so slight and so slow that it cannot have any purpose in relation to the flies that may be entangled on the surface of the leaf. The idea is that from the curled-over dome of the margin the secretion that comes from the leaf drops down on the insect, thus enveloping it in the liquid. I could see nothing to support this view. The secretion is so far glutinous that it does not fall. Each drop stands on the summit of a small crystalline-like cone from which it has exuded, and the whole leaf is studded with rows of these cones which at the throat or base of the leaf become elongated with from one to three foolscap-like crystalline cells, one on the top of the other. When the insect alights, or is blown on to the leaf, it gets entangled in this sticky secretion, and it is killed, and speedily killed (long before the curving of the margin of the leaf could have any effect upon it), by the secretion adhering to and closing up the spiracles by which the insect breathes, just on the same principle that a drenching with oil is used in our modern hospitals, &c., to kill the vermin with which dirty patients or inmates may be swarming on their admission—a remedy for the same reason found most effectual in destroying the itch insect. So far as regards Pinguicula, therefore, any irritability it may possess does not appear to be capable of being exerted injuriously to the insect; but in Dionæa it does, and in the general argument we must admit the facts found in any one as an element in its support against all; and so far as the inferences drawn by Dr. Hooker and others in favour of these plants being carnivorous are derived from their irritability, its existence and relevancy must be admitted. This does not mean that irritability is essential to digestion. It is not essential, for we can very well conceive of a plant being carnivorous and yet not irritable; as, for example, the Pinguicula, which I may be quite right in considering as not irritable, and quite wrong in regarding as not carnivorous, or the reverse. The two phases are not necessarily nor mutually interdependent. In connection with their irritability, however, an additional argument beyond its mere existence has been drawn from some phenomena which have been observed in connection with it. At the meeting of the British Association at Bradford in 1873 Dr. Burdon Sanderson announced that irritability in plants is accompanied with electrical phenomena analogous in their nature to those which occur when nervous or muscular actions are induced in animals. The following are the words of the published report:—"Strange as it may seem, the question whether these contractile movements are accompanied with the same electrical changes as those which occur in the contraction of muscle and in the functional excitation of nerve has never yet been investigated by vegetable physiologists. By a most remarkable series of experiments (which will be published subsequently), made with the aid of Sir Wm. Thomson's galvanometer, Dr. Burdon Sanderson has shown that these currents are subject, in all respects in which they have been as yet investigated, to the same laws as those of muscle and nerve." Now, there was certainly nothing to surprise physiologists in this discovery. The phenomena of the Sensitive Plant, when subjected to the vapour of chloroform (identical with that on animals), and other curious facts, had prepared men's minds for it. But it would appear that either a great stride has been made on this subject since this time last year of which I have not heard, or Dr. Hooker has overestimated the meaning of Dr. Sanderson's discovery. He says in his address:—"Quite recently the subject has acquired a new interest from the researches of Mr. Darwin into the phenomena which accompany the placing albuminous substances on the leaves of Drosera and Pinguicula, and which, in the opinion of a very eminent physiologist, prove, in the case of Dionæa, that this plant digests exactly the same substances and in exactly the same way that the human stomach does." If this refers to new experiments not yet published, of course those of us who do not otherwise accept Mr. Darwin's and Dr. Hooker's conclusions must be allowed to reserve our judgment until we see what they are. But, if he alludes to Dr. Burdon Sanderson's last year's experiments, then, although it may be that the Dionæa exhibits electrical phenomena after contracting identical with those shown by the stomach of animals during the process of digestion, these will prove nothing unless it can be shown that they are both special and peculiar to digestion, and in some way different from those resulting from mere muscular action, and it will not be easy to separate the digestive from the muscular action, seeing that the coats of the stomach are in active muscular operation during the process of digestion.

I therefore admit the occurrence of irritability and of electrical action during its operation. I also admit the exudation of an acid secretion from the surface of the leaf which assists in dissolving the insects. In Dionæa it would appear, indeed, that this secretion only takes place after the insects have been captured. This, however, is certainly not the case with Pinguicula, which has the secretion always present, unless artificially removed, as by rain or otherwise. Dr. Hooker says—"To Ellis belongs the credit of divining the purpose of the capture of insects by the Dionæa, but Curtis made out the details of the mechanism by ascertaining the seat of the sensitiveness in the leaves; and he also pointed out that the secretion was not a lure exuded before the capture, *but a true digestive fluid poured out like our own gastric juice after the ingestion of food.*" I have not access here to the *Proceedings* of the Boston Natural History Society of 1834, in which Curtis' paper appears, so as to see how far the words that I have put in italics are matter of inference or of actual observation; but the following passage, quoted by Dr. Hooker from Dr. Curtis' paper, would rather lead me to think it is a point requiring more verification, viz.: "At other times I have found them (the insects) enveloped in a fluid of a mucilaginous consistence, which seems to act as a solvent, the insects being more or less con-

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sumed in it." The phrase "at other times" rather suggests the idea that there were times when he did not find them so, and as a great deal depends upon this point, it would be well that the matter should be put out of doubt by actual observation. This can surely easily be done. Whoever has a plant should be able to do it without trouble. All that he has to ascertain is—1, whether the secretion is never present until after an insect has been captured; and, 2, whether it is always present after one has. If it is not so, then it leaves Dionæa in the same category as Pinguicula; but if it should be so it is a strong argument on the carnivorous side, but still by no means conclusive. The fact of the pouring out of the secretion after a fly has been taken leaves us still a long way from digestion; but it seems all but conclusive in favour of an intention to deal with the fly in some way and for some purpose or other. It may be a mechanical consequence of the movement of the leaf in catching the fly, but although it may be intended to dissolve the insect, it by no means follows that it is intended to digest it; still, such a sequence looks so like the operations of Nature in the adaptation of structure to purpose, that if it turns out that the result of the movement of the leaf is to press out a certain amount of fluid secretion, I do not see how it can apparatus of epithelial cells, the follicles, the villose coat, and all the means of assimilation? It is perfectly plain that Dr. Hooker only uses the comparison with the human stomach as analogous and not identical; and even here he will scarcely deny that, put it any way he likes, the analogy is of the feeblest.

Again, assuming that there is such an apparatus, although we cannot discover any trace of it, or anything different in the minute analogy of Dionæa and Pinguicula from that of any other plant, I think it is fair to assume that it will be a complicated apparatus—seeing that the supposed commencement of the operation is on the pattern of the human stomach, and the histological arrangement of the human stomach is extremely complicated. Would it not be an anomaly in the economy of Nature if a complicated apparatus should be provided to do something which is of no advantage to the plant, and which it seems to be able to do quite well without? Of course there are no flies for the Pinguicula to feed on in winter, and yet it grows as well then as in summer. For three weeks of the time I observed it we had a great deal of rain, and the leaves were washed free from all remains of flies—yet the plants seemed to thrive better and better. Drosera was in the same predicament, and I presume Dionæa must be so too.

sequently, propose to endow them with digestive powers. It may be that the action of both is the same, and produces the same result, viz., that of scavengers. If we reflect on the myriads upon myriads of flies and midges that we see disporting in the air, and if it be true that the duration of life of most of them is, as we are told, only a few hours, it must be subject of surprise that we see so little of their remains. Where do their dead bodies go? If they are thrown by the wind on acid-decaying vegetation or on plants whose secretions are acid, we can understand how they soon disappear, and why we do not see them except when they undergo the dissolution of their elements on something whose colour or form allows them to be easily seen,—not that I suppose for a moment that the primary purpose or intention of the secretion on the leaf of Pinguicula was for any such purpose, for I regard it as a rule without exception that the primary object of every structure or property with which a plant is endowed is for its own advantage, and only secondarily and indirectly for that of others, or for the general benefit. But there are an immense number of properties and structures whose advantage to the creature possessing them we cannot see. What advantage do any of the medicinal or poisonous properties of plants confer on the plants

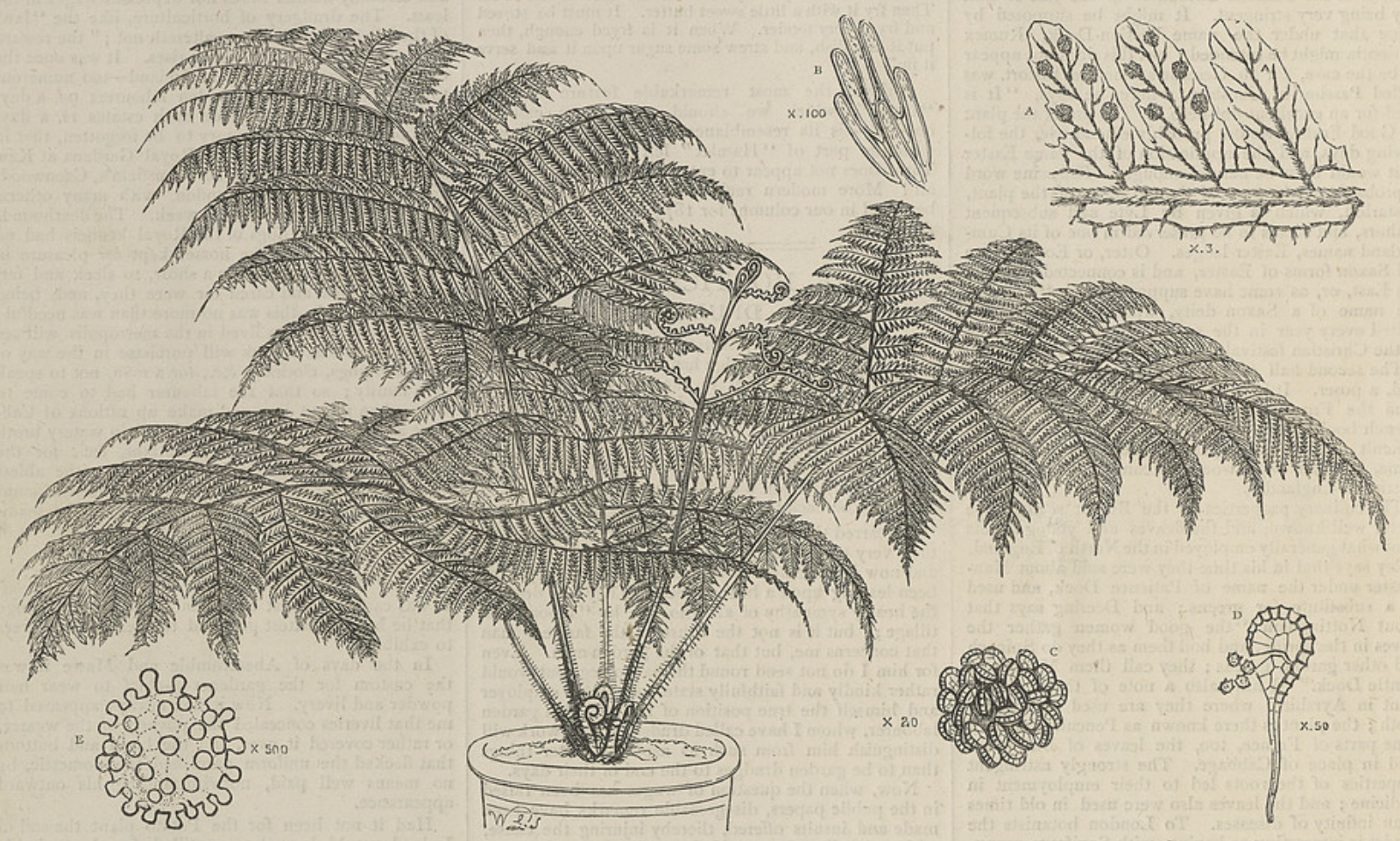

FIG. 74.—PLEOCNEMIA LEUZEANA.—A, Pinnulæ and sori × 3 dia.; B, Scales from veins × 100 dia.; C, Sorus × 20 dia.; D, Dehiscing sporangium × 50 dia.; E, Spore × 500 dia.

well be disputed that it is done for a purpose, and that purpose apparently the dissolution of the fly.

I now come to the last of the stages of the supposed carnivorous operation, the digestion of the fly or albuminous matter by the plant. I have held out with Dr. Hooker untired this length, but here I must leave him. I cannot believe in feeding without a mouth or digestion without a stomach. We are not dealing with an amœba but with an organism whose operations Dr. Hooker compares to those of the human stomach. His position is that it digests "the same substances and exactly in the same way that the human stomach does." As to the substances being the same it comes to nothing. There are two agents at universal work all over world, engaged continually in dissolving what can be dissolved—the stomachs of animals and the agents of natural decay; their action is similar and their powers limited by the same bounds. But when we come to the plant digesting in exactly the same way as the human stomach does I am entitled to ask, where is the apparatus by which it does so? Had he said in some way analogous to the operation of digestion in a sponge or in a foraminifer, I might have, from my ignorance of how they do it, been forced to hold my peace; but when the human stomach is given as the type, I am entitled to ask, where is the apparatus for digestion?—something more than gastric juice is needed for that purpose; where are the complicated

There is, however, a simulation of the process of digestion in Pinguicula, which doubtless also occurs in Dionæa, and which, I think, is probably the circumstance which has had most weight in leading Mr. Darwin and his supporters to the carnivorous view. The entrapped insects do melt away under the influence of the secretion, but no more, I apprehend, than they would do under the influence of any other feeble acid. Now the secretion is slightly acid—not quite so much as the juices of the plant itself, but still slightly so. The juices of most plants are acid. If you break a blade of grass, and apply a piece of litmus paper to it, you get a slightly acid reaction. It is the same with most other plants, with sometimes a surprising intensity, as in the case of Sorrel; so, if you break a leaf of Pinguicula, you find that the juice is acid. It is not an unnatural assumption that the secretion exuded will participate more or less (less rather than more) in its acidity; and so it does. It is feeble, and is not imbibed readily; but if a tiny morsel of litmus paper is laid gently on the leaf, and left there for some time, it becomes slightly tinged with red. Here is a means to dissolution which is applied universally all over the world to assist decay; but something more is needed to make digestion. If you apply the litmus paper to the moist or half rotten leaves of Sphagnum or Polytrichum growing alongside the Pinguicula, you will find them much more acid than the Pinguicula, but no one will, consequently, propose to endow them with digestive powers. It may be that the action of both is the same, and produces the same result, viz., that of scavengers. If we reflect on the myriads upon myriads of flies and midges that we see disporting in the air, and if it be true that the duration of life of most of them is, as we are told, only a few hours, it must be subject of surprise that we see so little of their remains. Where do their dead bodies go? If they are thrown by the wind on acid-decaying vegetation or on plants whose secretions are acid, we can understand how they soon disappear, and why we do not see them except when they undergo the dissolution of their elements on something whose colour or form allows them to be easily seen,—not that I suppose for a moment that the primary purpose or intention of the secretion on the leaf of Pinguicula was for any such purpose, for I regard it as a rule without exception that the primary object of every structure or property with which a plant is endowed is for its own advantage, and only secondarily and indirectly for that of others, or for the general benefit. But there are an immense number of properties and structures whose advantage to the creature possessing them we cannot see. What advantage do any of the medicinal or poisonous properties of plants confer on the plants themselves? Why should it seem more extraordinary to us that we cannot define the benefit conferred on Pinguicula by the liquid secretion of its leaf than that on the Rose by the fragrant gaseous secretion of its petals?—and I do not believe that, had the question not been complicated by the curious machinery of Dionæa, for which it is so difficult to find a purpose, we should ever have heard of carnivorous plants or digesting vegetables; and of course the solitary character of the example aggravates the difficulty of believing that a special digesting apparatus would be provided for the doubtful advantage of one species at one season. But all this is *jus alieni* to me. I advance no theory on the subject. All that I have to do is to give my reasons why I cannot accept the theory propounded by Mr. Darwin. *Andrew Murray.*

EASTER MANGIANTS AND HERB PUDDINGS.

CERTAINLY a puzzling-looking name, and one which it is difficult to explain satisfactorily, but not so uncommon as the Editors seem to think, although it has seldom appeared in print. Throughout Cumberland it is in general use, though, as far as I know, it does not occur in other counties. The third syllable is short—at least, so I am informed by a friend who is familiar with the name in Cumberland—and it is

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same length as the tomb. In 1858 some skeletons were discovered at Tremblay, Department of the Eure, some of which had a stick lying by the side of them. Two coffins in tufa were found in All Saints' Church, Angers, containing bodies of abbots, as their vestments indicated. In one was found a stick, about 5 feet long. In some tombs of the thirteenth century, at the church of Ste. Genevieve, Paris, were found in 1807 wands which, to all appearance, had been placed in the hands of deceased ecclesiastics. Montfaucon speaks of a Hazel stick, discovered in 1645, in the coffin of Queen Bilichilde, wife of Childeric II., in the choir of St. Germain des Prés. In the Swabian graves at Oberflacht were found Hazel wands 7 feet long. (*Proceedings of the Society of Antiquaries*, second series, vol. i., p. 257.) Bishop Pilkington, in his *Exposition upon Nehemiah*, alludes to 'the laying of the mete-yard in the grave with the dead man,' but he does not explain the usage. Mr. Bloxam concurred in the remark that the presence of the wand indicated a pilgrim, which Dean Howson said was borne out in Ralph Higden's case, who made a pilgrimage to Compostella."

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CARNIVOROUS PLANTS.

DR. HOOKER'S address to the Department of Zoology and Botany of the British Association at Belfast is by far the best and clearest statement of the position of the believers in carnivorous plants that I have met with, and not only merits respectful consideration on account of the position and abilities of the author, but the evident sincerity of his search after the truth almost, in common fairness, requires that any one who thinks he can throw light on the subject should contribute his views and observations in reply. It is with this feeling that I shall note the result of one or two attempts that I made during the last six weeks to ascertain whether our common Pinguicula had carnivorous properties or not. It may be remembered that at the last meeting of the Scientific Committee of the Royal Horticultural Society (on which occasion Dr. Hooker was in the chair) Professor Thistleton Dyer brought before the meeting some experiments that had been made by Mr. Darwin upon the Pinguicula, which led him to believe that, like Dionæa, it, too, was a carnivorous plant, deriving sustenance from the insects which it captured on its leaves. I have been staying in the Ochil Hills in Kinross-shire, where I had an abundance of material to observe, and a fair proportion of both dry and wet weather so as to see the behaviour of the plant under both conditions. The first thing of which I convinced myself was that, whether it was carnivorous or not, Pinguicula was rightly regarded by Mr. Darwin as coming under the same category as Dionæa and Drosera. It was a fly-catcher and a fly-dissolver—whether it was a fly-digester is a different thing—but neither on that point, any more than on the other, can it be separated from them. If the one digests, the other will no doubt do so likewise. If the one does not, neither will the other. A comparison of the points on which the carnivorous theory rests, will show how far these different plants correspond, and will at the same time enable me to say how far my observations do, or do not, confirm or warrant the conclusion of Mr. Darwin, Dr. Hooker, and others. Dr. Hooker enables me to contrast them easily, for he puts the salient points in favour of carnivorous feeding very clearly.

In Dionæa the phenomena consist—1, of a certain amount of irritability by which insects are captured; 2, of electricity displayed during contraction, in the same way as during muscular action in animals; 3, of the exudation of an acid secretion by which the captured insects are dissolved; and 4, of the digestion of the insects and the assimilation of their substance by the plant.

Now, first, as to the irritability, I am bound to confess that my experiments have only a qualified value. Mr. Darwin and others have found that after a fly has been entangled in the sticky secretion formed on the surface of the leaf of Pinguicula vulgaris, the margins of the leaf slowly and gradually curl over it so that at the end of twenty-four hours the insect is completely covered by it. I could not get my Pinguiculas to do this. In fact my experience is wholly negative. As any one can verify, the leaves are found with their margins in all stages of curling over, some with no insect on them much more curled over than others with several. I do not mean to say that they have no irritability, but I think I can safely say that it is so slight and so slow that it cannot have any purpose in relation to the flies that may be entangled on the surface of the leaf. The idea is that from the curled-over dome of the margin the secretion that comes from the leaf drops down on the insect, thus enveloping it in the liquid. I could see nothing to support this view. The secretion is so far glutinous that it does not fall. Each drop stands on the summit of a small crystalline-like cone from which it has exuded, and the whole leaf is studded with rows of these cones which at the throat or base of the leaf become elongated with from one to three foolscap-like crystalline cells, one on the top of the other. When the insect alights, or is blown on to the leaf, it gets entangled in this sticky secretion, and it is killed, and speedily killed (long before the curving of the margin of the leaf could have any effect upon it), by the secretion adhering to and closing up the spiracles by which the insect breathes, just on the same principle that a drenching with oil is used in our modern hospitals, &c., to kill the vermin with which dirty patients or inmates may be swarming on their admission—a remedy for the same reason found most effectual in destroying the itch insect. So far as regards Pinguicula, therefore, any irritability it may possess does not appear to be capable of being exerted injuriously to the insect; but in Dionæa it does, and in the general argument we must admit the facts found in any one as an element in its support against all; and so far as the inferences drawn by Dr. Hooker and others in favour of these plants being carnivorous are derived from their irritability, its existence and relevancy must be admitted. This does not mean that irritability is essential to digestion. It is not essential, for we can very well conceive of a plant being carnivorous and yet not irritable; as, for example, the Pinguicula, which I may be quite right in considering as not irritable, and quite wrong in regarding as not carnivorous, or the reverse. The two phases are not necessarily nor mutually interdependent. In connection with their irritability, however, an additional argument beyond its mere existence has been drawn from some phenomena which have been observed in connection with it. At the meeting of the British Association at Bradford in 1873 Dr. Burdon Sanderson announced that irritability in plants is accompanied with electrical phenomena analogous in their nature to those which occur when nervous or muscular actions are induced in animals. The following are the words of the published report:—"Strange as it may seem, the question whether these contractile movements are accompanied with the same electrical changes as those which occur in the contraction of muscle and in the functional excitation of nerve has never yet been investigated by vegetable physiologists. By a most remarkable series of experiments (which will be published subsequently), made with the aid of Sir Wm. Thomson's galvanometer, Dr. Burdon Sanderson has shown that these currents are subject, in all respects in which they have been as yet investigated, to the same laws as those of muscle and nerve." Now, there was certainly nothing to surprise physiologists in this discovery. The phenomena of the Sensitive Plant, when subjected to the vapour of chloroform (identical with that on animals), and other curious facts, had prepared men's minds for it. But it would appear that either a great stride has been made on this subject since this time last year of which I have not heard, or Dr. Hooker has overestimated the meaning of Dr. Sanderson's discovery. He says in his address:—"Quite recently the subject has acquired a new interest from the researches of Mr. Darwin into the phenomena which accompany the placing albuminous substances on the leaves of Drosera and Pinguicula, and which, in the opinion of a very eminent physiologist, prove, in the case of Dionæa, that this plant digests exactly the same substances and in exactly the same way that the human stomach does." If this refers to new experiments not yet published, of course those of us who do not otherwise accept Mr. Darwin's and Dr. Hooker's conclusions must be allowed to reserve our judgment until we see what they are. But, if he alludes to Dr. Burdon Sanderson's last year's experiments, then, although it may be that the Dionæa exhibits electrical phenomena after contracting identical with those shown by the stomach of animals during the process of digestion, these will prove nothing unless it can be shown that they are both special and peculiar to digestion, and in some way different from those resulting from mere muscular action, and it will not be easy to separate the digestive from the muscular action, seeing that the coats of the stomach are in active muscular operation during the process of digestion.

I therefore admit the occurrence of irritability and of electrical action during its operation. I also admit the exudation of an acid secretion from the surface of the leaf which assists in dissolving the insects. In Dionæa it would appear, indeed, that this secretion only takes place after the insects have been captured. This, however, is certainly not the case with Pinguicula, which has the secretion always present, unless artificially removed, as by rain or otherwise. Dr. Hooker says—"To Ellis belongs the credit of divining the purpose of the capture of insects by the Dionæa, but Curtis made out the details of the mechanism by ascertaining the seat of the sensitiveness in the leaves; and he also pointed out that the secretion was not a lure exuded before the capture, *but a true digestive fluid poured out like our own gastric juice after the ingestion of food.*" I have not access here to the *Proceedings* of the Boston Natural History Society of 1834, in which Curtis' paper appears, so as to see how far the words that I have put in italics are matter of inference or of actual observation; but the following passage, quoted by Dr. Hooker from Dr. Curtis' paper, would rather lead me to think it is a point requiring more verification, viz.: "At other times I have found them (the insects) enveloped in a fluid of a mucilaginous consistence, which seems to act as a solvent, the insects being more or less con-

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sumed in it." The phrase "at other times" rather suggests the idea that there were times when he did not find them so, and as a great deal depends upon this point, it would be well that the matter should be put out of doubt by actual observation. This can surely easily be done. Whoever has a plant should be able to do it without trouble. All that he has to ascertain is—1, whether the secretion is never present until after an insect has been captured; and, 2, whether it is always present after one has. If it is not so, then it leaves Dionæa in the same category as Pinguicula; but if it should be so it is a strong argument on the carnivorous side, but still by no means conclusive. The fact of the pouring out of the secretion after a fly has been taken leaves us still a long way from digestion; but it seems all but conclusive in favour of an intention to deal with the fly in some way and for some purpose or other. It may be a mechanical consequence of the movement of the leaf in catching the fly, but although it may be intended to dissolve the insect, it by no means follows that it is intended to digest it; still, such a sequence looks so like the operations of Nature in the adaptation of structure to purpose, that if it turns out that the result of the movement of the leaf is to press out a certain amount of fluid secretion, I do not see how it can apparatus of epithelial cells, the follicles, the villose coat, and all the means of assimilation? It is perfectly plain that Dr. Hooker only uses the comparison with the human stomach as analogous and not identical; and even here he will scarcely deny that, put it any way he likes, the analogy is of the feeblest.

Again, assuming that there is such an apparatus, although we cannot discover any trace of it, or anything different in the minute analogy of Dionæa and Pinguicula from that of any other plant, I think it is fair to assume that it will be a complicated apparatus—seeing that the supposed commencement of the operation is on the pattern of the human stomach, and the histological arrangement of the human stomach is extremely complicated. Would it not be an anomaly in the economy of Nature if a complicated apparatus should be provided to do something which is of no advantage to the plant, and which it seems to be able to do quite well without? Of course there are no flies for the Pinguicula to feed on in winter, and yet it grows as well then as in summer. For three weeks of the time I observed it we had a great deal of rain, and the leaves were washed free from all remains of flies—yet the plants seemed to thrive better and better. Drosera was in the same predicament, and I presume Dionæa must be so too.

sequently, propose to endow them with digestive powers. It may be that the action of both is the same, and produces the same result, viz., that of scavengers. If we reflect on the myriads upon myriads of flies and midges that we see disporting in the air, and if it be true that the duration of life of most of them is, as we are told, only a few hours, it must be subject of surprise that we see so little of their remains. Where do their dead bodies go? If they are thrown by the wind on acid-decaying vegetation or on plants whose secretions are acid, we can understand how they soon disappear, and why we do not see them except when they undergo the dissolution of their elements on something whose colour or form allows them to be easily seen,—not that I suppose for a moment that the primary purpose or intention of the secretion on the leaf of Pinguicula was for any such purpose, for I regard it as a rule without exception that the primary object of every structure or property with which a plant is endowed is for its own advantage, and only secondarily and indirectly for that of others, or for the general benefit. But there are an immense number of properties and structures whose advantage to the creature possessing them we cannot see. What advantage do any of the medicinal or poisonous properties of plants confer on the plants

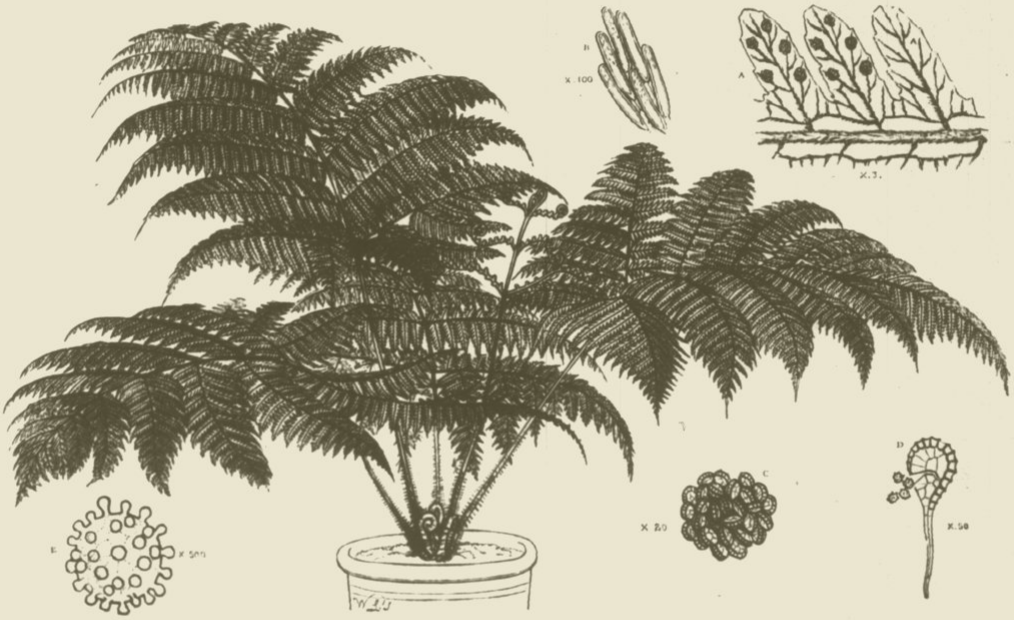

FIG. 74.—PLEOCNEMIA LEUZEANA.—A, Pinnulæ and sori × 3 dia.; B, Scales from veins × 100 dia.; C, Sorus × 20 dia.; D, Dehiscing sporangium × 50 dia.; E, Spore × 500 dia.

well be disputed that it is done for a purpose, and that purpose apparently the dissolution of the fly.

I now come to the last of the stages of the supposed carnivorous operation, the digestion of the fly or albuminous matter by the plant. I have held out with Dr. Hooker untired this length, but here I must leave him. I cannot believe in feeding without a mouth or digestion without a stomach. We are not dealing with an amœba but with an organism whose operations Dr. Hooker compares to those of the human stomach. His position is that it digests "the same substances and exactly in the same way that the human stomach does." As to the substances being the same it comes to nothing. There are two agents at universal work all over world, engaged continually in dissolving what can be dissolved—the stomachs of animals and the agents of natural decay; their action is similar and their powers limited by the same bounds. But when we come to the plant digesting in exactly the same way as the human stomach does I am entitled to ask, where is the apparatus by which it does so? Had he said in some way analogous to the operation of digestion in a sponge or in a foraminifer, I might have, from my ignorance of how they do it, been forced to hold my peace; but when the human stomach is given as the type, I am entitled to ask, where is the apparatus for digestion?—something more than gastric juice is needed for that purpose; where are the complicated

There is, however, a simulation of the process of digestion in Pinguicula, which doubtless also occurs in Dionæa, and which, I think, is probably the circumstance which has had most weight in leading Mr. Darwin and his supporters to the carnivorous view. The entrapped insects do melt away under the influence of the secretion, but no more, I apprehend, than they would do under the influence of any other feeble acid. Now the secretion is slightly acid—not quite so much as the juices of the plant itself, but still slightly so. The juices of most plants are acid. If you break a blade of grass, and apply a piece of litmus paper to it, you get a slightly acid reaction. It is the same with most other plants, with sometimes a surprising intensity, as in the case of Sorrel; so, if you break a leaf of Pinguicula, you find that the juice is acid. It is not an unnatural assumption that the secretion exuded will participate more or less (less rather than more) in its acidity; and so it does. It is feeble, and is not imbibed readily; but if a tiny morsel of litmus paper is laid gently on the leaf, and left there for some time, it becomes slightly tinged with red. Here is a means to dissolution which is applied universally all over the world to assist decay; but something more is needed to make digestion. If you apply the litmus paper to the moist or half rotten leaves of Sphagnum or Polytrichum growing alongside the Pinguicula, you will find them much more acid than the Pinguicula, but no one will, con-

themselves? Why should it seem more extraordinary to us that we cannot define the benefit conferred on Pinguicula by the liquid secretion of its leaf than that on the Rose by the fragrant gaseous secretion of its petals?—and I do not believe that, had the question not been complicated by the curious machinery of Dionæa, for which it is so difficult to find a purpose, we should ever have heard of carnivorous plants or digesting vegetables; and of course the solitary character of the example aggravates the difficulty of believing that a special digesting apparatus would be provided for the doubtful advantage of one species at one season. But all this is *jus alieni* to me. I advance no theory on the subject. All that I have to do is to give my reasons why I cannot accept the theory propounded by Mr. Darwin. *Andrew Murray.*

EASTER MANGIANTS AND HERB PUDDINGS.

CERTAINLY a puzzling-looking name, and one which it is difficult to explain satisfactorily, but not so uncommon as the Editors seem to think, although it has seldom appeared in print. Throughout Cumberland it is in general use, though, as far as I know, it does not occur in other counties. The third syllable is short—at least, so I am informed by a friend who is familiar with the name in Cumberland—and it is

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