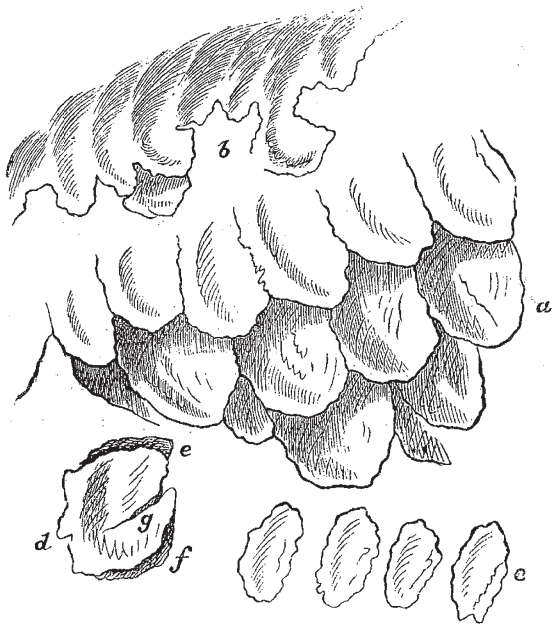
a north-west storm passing slowly north of the city without bursting, and disappearing in the south-east. Great branched masses of cloud appeared suspended from a sheet of Pallio-Cirrus. Some resembled bunches of grapes (*a*), others stalactites (*b*) in a striking manner, and still others formed round balls (*c*) separated by the azure of the sky. These balls seemed to be formed of snow flakes, and approached the form of Cirro-Cumulus; one might say of masses of snow rolled upon themselves by the effect of electric currents developed during the storm. This was accompanied by thunder and lightning at Washington, and by lightning only at Beloit. *d* represents one of these balls detached, with two sorts of penumbra, darker in *e* and *f*, and a streak at *g*, the rest whitish. Somebody at Beloit told me he had seen this form of cloud two or three times. A slightly brilliant aurora borealis was seen at Beloit the same evening. The night of its appearance at Washington no aurora was visible, but I do not know whether there may not have been one in other parts of




the United States. The same evening and the next day at Beloit the temperature fell several degrees. It is a general belief that the aurora borealis is followed by a decrease of temperature. We know that in higher strata of the air vapour of water floats constantly in the form of frozen needles, especially in the polar regions. It is not impossible that these ice needles may be drifted by the electric current which engenders the aurora borealis* into lower latitudes, and thence towards lower strata of the atmosphere by the winds and storms. Hence the cooling of the air which is said to attend the aurora.

ANDRÉ POËY

## *EXOGENOUS STRUCTURES AMONGST THE STEMS OF THE COAL MEASURES*

THE perusal of Dr. M'Nab's reply to my short article on the existence of an exogenous process of growth amongst the cryptogamic stems of the coal measures, confirms my previous conviction that the discussion of *the details* of my proposition can lead to no beneficial results until the publication of my large store of new facts has been completed. Dr. M'Nab's article convinces me, as indeed is necessarily the case, that he has no conception either of the nature or of the extent of those facts. Were it otherwise, he would see at a glance how far his explanations are from accounting for them. He has given an exposition of a common process of exogenous growth, which is true as far as it goes; but I can assure him that the modifications of that process, so far as we can infer from peculiarities of structure, have been much more varied in past geological ages than he is aware of. He is pleased to affirm two things which require proof: (1) that I have "been led away by the mere superficial resemblance of the parts;" and (2) that I have "never tried to understand the homologies of these stems." To the first of these charges I plead not guilty; to the second I reply that I was *trying* to understand these things when he was a child at school. Whether or not I have succeeded remains to be seen, but as yet he has told me nothing new to me.

In studying the relations of the several parts of a plant, we have to consider three things, of which Dr. M'Nab has mainly dwelt upon one. These are—

1. The relative positions of the tissues.
2. The mode of their development.
3. The functions they have to perform.

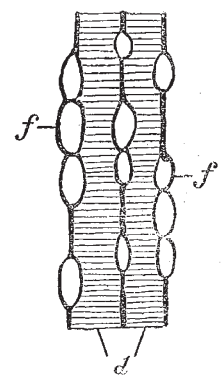



FIG. 1

The first point where I shall differ from Dr. M'Nab is in supposing that a correspondence on the first of these clauses invariably pre-supposes a similar correspondence on the second. I shall have to show on a future occasion that Nature has attained the same end in more ways than one; and that she refuses to be shut up to that dichotomous arrangement pre-supposed by Dr. M'Nab; but for the present I will limit my illustration to the particular mode of growth upon which he rests his case.

If we take a perfect Stigmaria, we find its centre (*a*, Fig. 2, p. 491) to be occupied by an axis of ordinary cellular parenchyma unmixed with any vascular tissue. This is surrounded by a ligneous or vascular cylinder (*b*) which, in its turn, is invested by a thick bark (*c*) consisting of a mixture of parenchyma and prosenchyma arranged in definite positions. The central axis differs in no respect whatever from the cellular piths of ordinary exogenous stems. The woody cylinder consists of vessels which, in the transverse section, are arranged in radiating lines (*d*) running from the pith to the bark; these lines are separated by intervening cellular tracts (*e*), which I, in common with Brongniart and Dr. Hooker, designate medullary rays. The radiating lines of vessels exhibit proofs of distinct interruptions to the process of growth, and afford clear evidence that the cylinder began as a thin ring of vessels surrounding the pith, and which grew, by successive concentric additions of vessels, to its peripheral surface where the cambium layer is found in ordinary exogens. We have here no trace of the limiting tissues of which Dr. M'Nab speaks; the growth has been free and prac-

* See my Memoir on the Development of Electricity during the Aurora Borealis in the "Annuaire de la Société Météorologique de France," 1861, vol. ix. p. 42.

tically continuous, in an outward direction, by the addition of layer after layer. The materials for the new vessels have obviously been furnished by some protoplasmic element which, whether we call it cambium, or choose to give it some other name, was located at the line of junction between the wood and the bark. The additions effected by its agency have gone on through successive ages until the thin vascular cylinder became a large hard-wooded stem capable of upholding a gigantic forest tree.

If we turn to the medullary rays, we find that they consist of vertical laminæ of cells. In the tangential section they appear as vertical lines of cells (*f*, Fig. 1, p. 490), undistinguishable from those seen in the corresponding sections of most conifers. In radial sections made in the plane of the medullary rays, we find that the latter proceed continuously from the pith to the investing bark, with each of which tissues they become intimately blended at their corresponding extremities. The component cells further exhibit, in this radial section, the mural arrangement so characteristic of ordinary medullary rays. As the vascular cylinder increased in diameter by additions to its exterior, so these medullary rays became lengthened by the similar addition of new cells to their outer extremities, such cells being supplied from the same source (cambium) as the corresponding new vessels.

Now, in all these processes of growth, I re-affirm that we have nothing which can, in any plain sense of the word, be termed *Acrogenous*. I can discern no material difference between what I have just described and what occurs in a *Cycad* or in a *Conifer*. In all these cases the additions are equally made to the exterior of a gradually enlarging cylinder, new cells being added to the outer extremities of the medullary rays, and vessels to the intermediate lines of vascular tissues; the raw material for both having been furnished, as in exogens, by some protoplasmic layer located between the vascular cylinder and the bark. I do not very clearly understand what Dr. M'Nab means when he speaks of a "pseudo-exogenous" growth, or of an "increase which takes place in the wood cells of the primitive tissues, not, as in Dicotyledons, by additions to the wood-cells of the fibro-vascular bundles." I detect no such difference as he seems to imply in the example which I have given.

If I rightly understand his meaning, Dr. M'Nab considers me to affirm that in all these cryptogamic plants of the coal-measures, there has been exactly the same process of growth, corresponding in each minute detail, as takes place during the growth of an oak tree. I have never affirmed this. On the contrary, I shall have to show that, amongst these coa plants, there are indications of many remarkable combinations and varied modifications of the process of growth.

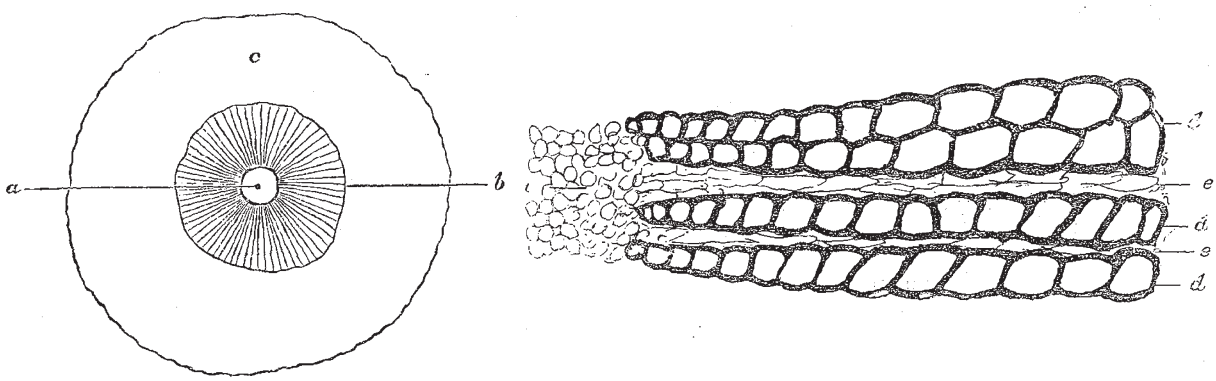



FIG. 2

Whether we do or do not accept the doctrine of evolution, we should expect to find such generalised combination amongst these primæval forms of vegetable life. I once more repeat, however, that these matters are scarcely capable of further discussion until my series of detailed memoirs has been published. When this takes place, I think Dr. M'Nab will see that I have not made the two "fatal errors" which he imagines I have done, and that there is more in my proposed classification than he, at present, has any idea of. At the same time I may remind him that the recognition of an exogenous process of growth amongst cryptogams is not now propounded for the first time. Dr. Hofmeister has given us most detailed accounts of such a process in his history of the development of Isoetes—itself a Lycopodiaceous plant. I merely propose to show that a mode of increment which now lingers in this one dwarfed genus amongst living Lycopodiaceæ, was once widely diffused, not only throughout this group of plants, but equally presented itself amongst the Calamitaceæ.

Prof. Dyer's temperate and intelligent reply to my article on the above subject resolves itself into two parts, the first of which deals with facts and the second with opinions. As to his facts he is in the same position as Dr. M'Nab. He is not acquainted with the materials for forming an opinion which I have in my hands, and upon which my views are based, consequently he has taken one extreme type of Lycopodiaceous stem, and made its supposed characters representative of the entire group. No. 129 of the Proceedings of the Royal Society which contains an abstract of my last memoir on the subject, would have shown him that I do materially differ from Mr. Carruthers in my interpretation of *Lepidodendron selaginoides*, the plant to which he refers, which difference of opinion I also expressed at the Edinburgh meeting of the British Association. I there showed that the central axis does not, as Prof. Dyer affirms, "consist wholly of scalariform vessels," but that these vessels are largely intermingled with true scalariform cells. But this is not all. The plant in question is but one of a large variety of forms. It occupies one end of a linear series of types—the opposite extremity of which series exhibits a very different aspect. The medullary vessels, which, in *Lepidodendron selaginoides*, are thus intimately commingled with the medullary cellular tissue, in the other types gradually recede from the centre to the periphery of the central cellular axis; the latter thus assuming the condition of a purely cellular parenchymatous pith, the cells of which are not even scalariform. The medullary vessels, thus driven to the periphery, now assume the position of the medullary sheath of the higher exogens. The vascular tissues for which I claim an exogenous origin are superadded to the exterior of this vascular medullary cylinder. We thus see that the central axis of these plants, instead of consisting of two parts, as Prof. Dyer affirms, really consists of three,* viz, a central cel-

* Stigmaria is an exception. In it the medullary vessels are altogether absent, as stated in my reply to Dr. M'Nab.

lular pith, an inner ring of vessels belonging to the medullary portion of the axis, and an external vascular cylinder, which grows by additions to its exterior, and which no more belongs to the central medulla than do the ordinary wood layers of an exogenous phanerogam. It has unquestionably been the product, as Prof. Dyer admits to be probable, of a cambium layer.

Speaking of the Lycopodiaceous stems of the coal measures, Prof. Dyer says, "I am inclined to think, with Prof. Williamson, that the stem increased in thickness." This point is not one to be thought about as if it was uncertain. We have in our museum accurate casts of the Dixonfold trees, and the base of the stem of the largest of these, above the point whence the huge roots are given off, is twelve feet in circumference! Higher up it is eight feet. There is surely no room for questioning an increase of thickness here, and this instance is but one example of what is sufficiently common in the coal measures. When we turn to the interior of these large trees, we find, as I have abundant evidence to prove, that they were enabled to sustain their huge bulk by an exogenous development of their outer cylinder of vessels, which were not mere modifications of the medullary vessels, but something superadded. This woody structure was amply provided with medullary rays, and each of the several layers of the thick bark increased *pari passu* with an increase of the ligneous zones, whilst a large cellular pith occupied the centre of the stem. So much for the facts, which are very different from those recognised by either of my two opponents. Now as to opinions, Prof. Dyer says he thinks that this increase was "nothing more than an incident in the life-history of a particular race of plants, nothing more than an adjustment to an arborescent habit dropped when the arborescent habit was lost." I am not sure that I understand all that Prof. Dyer means in this passage. He appears, however, to imply that these exogenous conditions were merely adventitious growths assumed for a season, and thrown off at the earliest opportunity; that they had no true affinity with the plants in which they were found. I confess I see no grounds for so remarkable a conclusion, especially remembering that, at least, these conditions lasted throughout the vast duration of the Devonian and Carboniferous ages. That one object of the exogenous growth was to enable these trees to sustain a huge superstructure, is doubtless true, though we find that growth in myriads of plants that have no such ponderous superstructures; but must we not say the same thing of the oak and the beech, as well as of the Lepidodendra? I see no difference between the cases. We have no more reason for regarding these conditions as merely an incident in the life-history of a particular race in the one instance than in the other.

I will not now discuss the value of the terms exogen and endogen, since the question has little importance in reference to the present object. I will only say that the mode of growth of a plant appears to me to have equal value with the mode of reproduction. There is a fashion in these matters—and in some circles there is now a tendency to elevate the reproductive at the expense of the vegetative, with which I do not agree, but I repeat this is not a question essentially important at present. My two great objects have been, first to demonstrate the existence of the exogenous structure in the trees in question; and second, to show the absurdity of applying the term acrogen to trees so constructed.

The value of my proposed classification is an independent question. I attach but a limited importance to the artificial boundary-lines introduced by systematisers, and do not wish to assign more to my own than to those of others; nevertheless, such divisions are useful so far as they indicate affinities, and it is because I find such affinities in the plants before us, *unrecognised by existing classifications*, that I have suggested a new one. Whatever value different minds may attach to the fact, there exists the great vegetative difference upon which I have dwelt between the Lycopodiaceæ and the Calamites on the one hand, and the Ferns on the other. There is certainly something more involved in this fact than "the old division of plants into trees and shrubs," with which Prof. Dyer compares it. Such a division is merely one of size and duration, not of organisation. Herbs, if they belong to the exogenous group, are as truly exogenous in their type as the most gigantic trees of the same class. Size has nothing to do with the matter. The same uniformity of type, apart from size, exists amongst my fossil cryptogams. True, the exogenous growth attains the fullest development amongst the large trees—but all the rudiments of this growth are equally to be found in the small ones, as my forthcoming memoirs will demonstrate.

The outer exogenous growth must be distinguished from the primitive vessels of the central medullary axis. I have yet to publish a remarkable series of facts illustrating this point. I have stated in a previous article that, in one sense, the exogenous vessels are a development of the vascular bundles of the living Lycopods. This is teleologically true rather than morphologically. Viewed in the latter aspect the two groups of vessels are independent of each other. The medullary vessels may be, and often are, primitive tissues formed at the first growth of the plant or of its young branches. The exogenous ones are something added, furnished by a cambium layer. The two groups retain their independent positions permanently, just as in living exogens the medullary sheath remains distinct from the woody cylinder which encloses it.

W. C. WILLIAMSON

## NOTES

WE believe that the arrangements of the Eclipse Expedition are nearly all made, and that the numbers are now complete. The Expedition sails on Thursday next in the *Mirzapore*, arriving at Point de Galle on the 27th November, if all goes well. M. Janssen, we believe, is already *en voyage*. Prof. Respighi, of Rome, will accompany the English Expedition.

BOTH Mr. Hind and M. Stephan at Marseilles have obtained observations of Encke's comet. Mr. Hind thus writes: "It is a large, faint, and very diffused nebulosity—a different-looking object from what I remember it in one or two former returns, when it has been drawing just within reach of the telescope. The last observation on the 12th of October gives the following place:—At $9^h\ 16^m\ 18^s$ mean time at Twickenham, right ascension, $1^h\ 7^m\ 37{\cdot}8^s$; north declination, 36° 47′ 38″. The ephemeris for this appearance, published in *Mélanges Mathématiques*, of the Academy of Sciences of St. Petersburg, and calculated by Herr von Glasenapp, of the Russian National Observatory at Pulkowa, required, according to the above observation, corrections of 36 seconds in right ascension and ten minutes in declination, subtractive in both elements. The comet's positions for the next few days will be nearly as follows:—

*For Midnight at Greenwich.*

| | R.A. h. | m. | Decl. N. Deg. | Min. |
|---|---|---|---|---|
| October 19 | 0 | 30·7 | 38 | 27 |
| ,, 21 | 0 | 17·8 | 38 | 45 |
| ,, 23 | 0 | 3·6 | 38 | 56 |
| ,, 25 | 23 | 48·3 | 38 | 59 |

THE Expedition to Moab, which has been organised by Dr. Ginsburg, and goes out under the auspices of the British Association, will leave England in January. Its object is to explore the geography, antiquities, and natural history of the region. Canon Tristram will accompany Dr. Ginsburg.

*Bulletin Astronomique de l'Observatoire de Paris* is the title of an official circular, containing meridional observations of the sun,