scalariform vessels, and that there is in fact nothing medullary or medulla-like about it.

Outside this central structure is what Mr. Carruthers terms the investing, and Prof. Williamson the vascular woody cylinder. I believe that Mr. Carruthers is right in looking upon this as belonging to the central axis, which is therefore composed of two parts.* I find, which I did not sufficiently appreciate at the time, that Prof. McNab regards this investing cylinder as homologous with the cylinder of wood cells surrounding the central axis of fibro-vascular bundles which is met with in many recent Lycopodiaceæ. From this I certainly dissent for two reasons; (1) because I think its equivalent is to be found in the central axis itself, and not outside it; (2) because it is not composed of wood cells but of scalariform vessels.

Secondly, as to opinions. The terms Exogen and Endogen, as is pretty well known, were founded upon a mistake. A great deal too much has been made of the difference implied by them; in fact, if we compare a one-year-old dicotyledonous shoot with a monocotyledonous stem, we find that it does not exist. If Prof. Williamson will look at the stem of the common artichoke, he will find it difficult to convince himself that he is examining an "exogenous" plant at all.

The imagined characters which were implied by these terms are, nevertheless, as everyone knows, correlated with others, which in the aggregate enable phanerogamic plants to be divided into two satisfactory groups; but this is certainly not equally the case with the groups into which Prof. Williamson would divide the vascular cryptogams. These groups, I think, most botanists will agree in considering in the highest degree unnatural, inasmuch as, assuming the vegetative distinction upon which they are founded to exist, it is a wholly artificial ground for classificatory purposes. Nor is it any argument that one vegetative character must be good because others are in use, since the simple answer is that these coincide with natural divisions, while Prof. Williamson's does not.

I shall not dispute Prof. Williamson's position that our living Lycopodiaceæ should be interpreted by the more complete extinct types. To do this, however, the extinct types must be thoroughly understood; when we are dealing with imperfect material, comparison with the more perfect but less highly developed existing plants is not only justifiable but necessary.

It is obvious that the great development of the stem in the Lycopodiaceæ of the Coal Measures was correlated with their arborescent habit. I am inclined to think with Prof. Williamson that the stem increased in thickness; it is certain that *Lepidodendron* was branched, and not improbably also *Sigillaria*. The branches as they were gradually developed must have been the cause of an increasing strain upon the stem; it seems to me more congruous with known laws of the response of structure to circumstances, to conclude that the stem was proportionately developed as the strain increased, than that the stem should have been produced once for all of its maximum thickness without reference to the crown of branches that was finally to surmount it.

I am quite prepared therefore to admit that the investing cylinder may have increased by external additions, and probably did do so; this would of course imply the existence of a cambium layer outside it. There is some analogy for this in the recent *Isoëtes*, where we have a "slight woody mass which occupies the longitudinal axis of the stem, but encloses *no pith*." † Outside this we have a "bark-forming" cambium (which also adds, but more sparingly, to the wood mass); in *Sigillaria* and *Lepidodendron* we might have had a cambium not merely renewing the bark but adding to the central axis.

In whatever way the increase took place, it was, as I think, 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, but showing a lingering ancestral tendency in *Isoëtes*. Comparing a simple stemmed palm with *Dracæna*, we have a parallel instance of the strengthening of the stem *pari passu* with the continued development of a system of branches; only in *Dracæna* it is the circumferential part of the stem alone which developes.

If I am right in regarding a stem gradually developing in size as the necessary correlate of a large system of branches, Prof. Williamson's view practically amounts to the old division of plants into trees and herbs. I cannot see how it can afford any safe ground for a re-arrangement of the vascular cryptogams.

W. T. THISELTON DYER

London, Sept. 26

* *Monthly Micro. Journ.*, 1869, p. 169.

† Hofmeister, Higher Cryptogamia, pp. 356, 361

The Solar Spectrum

MAY I venture to suggest that quite possibly something of value might be obtained by observing the sun during totality with a spectroscope of reasonable dispersive power (say four or five prisms) *without a collimator*, or even simply with one of the so-called meteor spectroscopes.

If the bright rays and rifts are really and simply (or even mainly) composed of the green-line-giving substance, they will give a well-defined green image; if they are formed by reflection (either at the sun or in our atmosphere) of ordinary sunlight, they would be so dispersed as to be invisible or nearly so, and if formed by the reflection of chromosphere light they would give several images, the red (C) and blue-green (F) being most conspicuous.

C. A. YOUNG

Hanover, N.H., U.S., Sept. 13

*** Arrangements have already been made for carrying out a similar suggestion to this by the Eclipse Committee; and the corona will also be observed with an open slit.—ED. N.

Eclipse Photography and the Spectroscope

THE endeavour of the Eclipse Committee to secure some uniformity in the photographs from different stations next December does not appear to be duly appreciated, it being contended that immense "personality" shown in various photographers' manipulation must frustrate the good intention. I submit that in this case the personality is greatly over-estimated; that a number of competent photographers taking the same subject would probably produce, under any ordinary circumstances, pictures bearing considerable resemblance; while by using like apparatus and giving exposure of the same duration, we might safely predict a similarity of result amply sufficient for comparative purposes, and for the identification of structural peculiarity should it exist.

Among others there is a possible advantage to accrue from uniform work by the philosophers which I have not seen or heard noticed. Supposing the outer corona, rays, streamers, or any portion of the apparently luminous matter be terrestrial, is it unreasonable to expect that photographs, taken at stations more or less widely separated, will, when properly combined in the stereoscope, give clear ocular proof of the sublunary situation of such luminous matter?

HENRY DAVIS

Phenomena of Contact

MR. STONE can safely be left to meet the arguments specially addressed to him in Prof. Newcomb's letter; but as the subject relates to the only point of importance touched on in Prof. Newcomb's criticism of my chapter on the sun's distance, I crave permission to meet his general argument.

I submit that he tries to prove too much.

He admits that the phenomenon of irradiation exists in the case of a disc. The sun's disc, then, must be to some extent enlarged, and the dark disc of Venus must be to some extent reduced by the effects of irradiation. Now this being so, *what becomes of the cusps*, when Venus is all but wholly on the sun's disc? Either the irradiation is diminished near the cusps or it is not. If it is diminished there must be distortion, because the disc of Venus is then not uniformly reduced: if the irradiation is not diminished a ligament must appear.

Let any one draw a large circle (say a foot in diameter) on paper, and a small one (say an inch in diameter) extending very slightly (say by the twentieth of an inch) beyond the boundary of the first; and let him blacken the smaller circle as well as all the space outside the larger one. He has then a space representing the disc of the sun with a very large Venus upon it near the time of internal contact. Now let him conceive the whole of this space (a sort of exaggerated crescent) slightly enlarged as by irradiation, the enlargement-fringe extending outside the boundary of the large disc and inside the boundary of the small black (incomplete) disc. He will find the conception of this enlargement exceedingly easy everywhere save near the cusps; but here there is a difficulty in determining how the fringe outside the larger disc is to be joined on to the fringe inside the smaller disc. If he can conceive these two fringes meeting in such sort as to leave the reduced outline of the small disc completely circular up to the very points in which it meets the enlarged outline of the large disc, he will have done what Prof. Newcomb's theory requires. But note, this must be done for the case when the fringe of enlargement is wider than the twentieth of an inch, by which the small disc overlaps the large one. When this is the