to the question whether there is blaze and germination. As regards the lower degrees and the smaller differences of vitality, the chances of disagreement between the two tests are obviously greater. As regards the electrical test, it is more difficult to take the measure upon the entire seed than upon its isolated radicle. As regards the germination test, it is not always easy to ensure identical and optimum conditions.

Fresh and vigorous seeds manifest a large blaze response (0.0500 volt or more), and germinate strongly. Older and less vigorous seeds manifest a smaller blaze (0.0100 volt or less), and a less active germination. Still older seeds, incapable of germination under even the most favourable conditions, manifest still smaller blaze (0.0010 volt or less), and finally none at all, or the small counter-effect due to polarization (0.0005 volt more or less).

THE QUADRIPOLE SPINDLE IN THE SPORE-MOTHER-CELL OF PELLIA EPIPHYLLA.—I have read with interest the account of nuclear division in *Pellia* as given by Prof. Davis in the last number of this journal¹, and it is a source of gratification to find that his description of the processes accords so nearly, at any rate as regards matters of fact, with that given by me in 1894 and 1895². But in dealing with the first mitosis in the spore-mother-cell, he has arrived at conclusions respecting the peculiar spindle so characteristic of the earlier stages, which diverge considerably from those which I expressed in the second paper above cited. As the matter is not merely one of detail only, but is of great cytological interest, it seems desirable briefly to examine the evidence adduced by Prof. Davis in support of his own contention.

He apparently regards the quadripolar spindle, described by me, as non-existent ('there is no four-poled spindle,' p. 174), and he was not able to trace any structures in the cell of the nature of centrosomes in connexion with the mitosis under consideration. The first of these objections seems to depend chiefly on the mode of interpretation adopted, the latter more nearly touches upon fact.

Now, as regards the failure to identify the centrospheres at this stage, I can only assert that they are certainly to be distinguished in appropriately prepared sections, although they are by no means

¹ Annals of Botany, xv, No. LVII.

² Ibid., viii and ix.

so obvious as those of *Aneura* or, especially, of *Fossombronia*, at the corresponding stage. One reason for this is perhaps to be sought for in the shape of the spore-mother-cell of *Pellia*. The four lobes are much elongated tetrahedrally, and hence only one of the four spindle-rays can possibly, even in the most favourable case, be cut radially, i.e. in such a manner that it can be followed out, in the same section, to the distal end. The rays in the three remaining lobes will obviously present a more or less truncated appearance, such as is actually represented in the Figures 8 and 9 in Plate X. A further obstacle in the way of the recognition of centrospheres lies in the extreme difficulty of properly fixing the cell-contents of the spore-mother-cell of *Pellia* at this stage, and in this respect again it compares unfavourably with the other examples above mentioned. An inspection of some of Prof. Davis' figures, notably 1, 2, and 7, sufficiently proves that he has himself experienced this difficulty.

When discussing the significance of the quadripolar spindles, and their relation with the achromatic figures commonly met with in ordinary cells, I expressly endeavoured to guard myself against the possible misconstruction of having attributed them to any circumstances other than those involved in the unusual form of the cells concerned. Indeed, the chief interest for me in these spindles lay precisely in the possibility of so correlating them. And thus it appeared that the extreme diversity met with in the different genera of the Hepaticae investigated by me was susceptible of an explanation sufficiently general to cover them all (vol. ix, p. 150).

But notwithstanding the evidence furnished by his own figures, Prof. Davis seeks to explain away the existence of the quadripolar spindles by calling them a condition of prophase only, and by implying that the existence of a kinoplasmic web enveloping the nucleus is sufficient to disprove their existence, or at any rate to rob them of all importance. Such *a priori* arguments appear to me to be futile, even were they not sufficiently answered by actual observation of the very definite structures themselves. Quite apart from the existence of centrospheres, it is impossible to disregard the evidence presented by the character of the nucleus and the kinoplasm. Thus a nucleus is shown in Fig. 5, distorted and pulled out tetrahedrally, and the proximal parts of the spindle arms are also represented. The suggestion is put forward that the 'accumulations of kinoplasm most naturally then take up positions of least resistance, and extend

into the lobes of the spore-mother-cells.' But this can hardly be accepted as an adequate explanation, for it implies that the force which causes the nucleus to assume the peculiar shape in question, and which further impels the outward development of the fibrillae, acts from the centre of the nucleus. The evidence, however, here, as in other cases, points not to a centrifugal push, but to a pull acting from without upon the nucleus, at four points corresponding severally to the aggregations of protoplasm situated in the four lobes of the cell.

The further assertion that 'the cones of fibrillae are not spindles' obviously involves a revision of terminology, and is at complete variance with accepted usage. It would be of interest to learn the precise stage at which Prof. Davis considers the fibrillae of the cones, which he figures as running out into the several lobes of the cell, to be transformed into actual spindle-fibres. But the mode of origin of 'the true spindle' is not given, although this is obviously a point of supreme importance in connexion with the views put forward by Prof. Davis.

I may conclude this note by remarking that I see no good reason at present for abandoning the views as to the origin and relations of the spindle structures advanced in the memoirs already cited, and I may be permitted further to express the opinion that, in dealing with complex matters such as those raised by Prof. Davis, the value attaching to general conclusions is very small unless they are founded on a wide and comparative basis.

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LOXSOMA CUNNINGHAMII—A CORRECTION. In vol. xv, No. LVII, *Annals of Botany*, Mr. D. T. Gwynne-Vaughan gives a short review of previous literature upon the anatomy of *Loxsoma*, in which he says (p. 75): 'Finally, Giesenhagen confuses the correct opinion hitherto held regarding the vascular system, by speaking of it as a central collateral or concentric bundle; he also refers incorrectly to the structure of the cortex.'

In view of the fact that the author has given in his paper a most careful description of the anatomy of *Loxsoma*, which adds greatly

Farmer, J. B. 1901. "The quadripolar spindle in the spore-mother-cell of *Pellia epiphylla*." *Annals of botany* 15, 431–433.

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