

but this latter translocation only occurs when the partial starvation of the cell has rendered possible the dissolution of starch by enzyme-action.

From the invert-sugar, derived from the cane-sugar, the dextrose is more readily used up for the respiratory processes, and possibly also for new tissue-building, than is the levulose: hence in a given time more levulose than dextrose must pass out of the leaf into the stem.

Knowing as we do how enormous is the resistance which living protoplasm affords to the ordinary physical processes of diffusion, it seems highly improbable that the wandering of the sugars in living plant-tissue is altogether dependent upon osmosis. It is no doubt to the continuity of the protoplasm from cell to cell, which may now be regarded as an established fact, that we must look for a full explanation of those rapid translocations of certain substances which we know take place. That diffusibility is however a determining factor of importance cannot, we think, be doubted when we regard the nature of the substances which up to the present time have been recognized as wandering metabolites.

THE GENUS TREMATOCARPUS.—With reference to my note on this genus in 'Annals of Botany,' vol. vi, no. 21, April, 1892, a letter has been received from Dr. Zahlbruckner, of which the following is a translation.

'In the "Annalen des k. k. naturhist. Hofmuseums in Wien," vol. vi, I described my genus *Trematocarpus*, basing it on *Lobelia macrostachys*, Hook. et Arn., which differs absolutely from all species of *Lobelia* in the structure of its fruit. Mr. W. B. Hemsley raised objections, in the 'Annals of Botany' of April, 1892, to my proposed new genus on the ground either that the genus *Trematocarpus* does not refer to *Lobelia macrostachys*, Hook. et Arn.; or—and this seems to be regarded by Mr. Hemsley as more probable—that the capsules in my possession were not normal, but had been eaten by insects. As to the latter objection, I can only state that the capsules on which my description is based are quite normal, and that my description is an accurate account of the facts. Were the holes in the wall of the capsule due to insects, the margins of the holes would consist merely of the tissue of the wall, or possibly there might have been some development of cork from callus. But this is not the case. On the contrary, the holes are bounded by a raised ring consisting of fibrous

sclerenchyma. Moreover, the apex of the woody capsule is and remains completely closed: how then can the dissemination of the ripe seeds be effected? I hope that Mr. Hemsley—to whom I have since sent a photograph of Wawra's plant and a ripe capsule—will be able to confirm my description of the capsule in every detail.

The first objection demanded careful reconsideration of the question whether or not the plant were really identical with Hooker and Arnott's species: for, in view of the inadequate descriptions of the authors, there was a possibility that I might have been misled in spite of conscientious investigation. I addressed myself to Mr. Hemsley for further information, and he was so good as to send me a capsule of the original species. This material made the matter quite clear to me. It was apparent to me in a moment that Mr. Hemsley had before him only quite young unripe fruits of the original species. Capsules in this stage of development are present also in the upper part of the inflorescence of Wawra's plant, and I have also seen them on a plant collected by Hillebrand. This is the cause of the error into which Mr. Hemsley, like Hillebrand, has fallen. Mr. Hemsley, since seeing the photograph which I sent him, has written to me saying that Wawra's plant is undoubtedly identical with *Lobelia macrostachys*, Hook. et Arn.

I conclude, therefore, that there can no longer be any doubt that (1) the creation of the genus *Trematocarpus* was fully justified; and that (2) *Trematocarpus* refers to *Lobelia macrostachys*, Hook. et Arn.

In conclusion, I would merely point out that *Trematocarpus* is not at all closely allied to the genus *Lobelia*, and that I propose to state the reasons for this assertion in a paper which will appear shortly.'

Vienna, Jan. 18, 1893.

DR. A. ZAHLBRUCKNER.

In reply, I have only time at present to state that I have again superficially examined the Kew specimens of *Lobelia macrostachys*, but I am still of opinion that the orifices in the ripe capsule are not pores of dehiscence in the ordinary acceptance of the term, because they either appear irregularly on any part of the capsule and vary in number from one to several, or, what is more frequent, there may be none at all. However, my colleague, Dr. Stapf, has undertaken to investigate the anatomy of the capsule: the conclusions at which he may arrive will be published in the next number of the 'Annals of Botany.'

Kew.

W. BOTTING HEMSLEY.