Transmission of Stimuli in Plants.[1]

MR. R. SNOW has conferred a real benefit on botanists by reviewing Ricca's important experimental investigation on the conduction of stimuli in Mimosa, and by discussing the evidence for the conclusions of that Italian investigator which originated such an unexpectedly simple theory for the conduction of stimuli in plants. In the paper before us Mr. Snow describes how the experiments which Ricca performed on the conduction in the stem of *Mimosa spegazzinii* were repeated on *M. pudica*, and states that Ricca's explanation of the conduction, as depending on the transference of a hormone in the transpiration-current in the wood of the stem, has been tested and may be accepted as correct.

It will come, however, as a surprise to most physiologists that Mr. Snow does not consider this mechanism adequate to explain all the forms of conduction in *Mimosa pudica*. He believes that there are three types of transmission, namely, (1) The "normal" conduction in the transpiration-stream, elucidated by Ricca; (2) "high speed" conduction in the cambium or inner layers of the phloem, and (3) leaf-conduction in the phloem. Such a point of view demanding a special hypothesis to meet each type of conduction is not likely to find favour unless there is evidence to show conclusively that the original theory alone is unable to explain the experimental results. It may be stated at once that such evidence does not appear to be forthcoming.

First, with regard to the "high speed" conduction: This type is observed when a small lesion is inflicted on the inner layers of the phloem of the stem, or, occasionally, when an internode of the stem is cut through. In these cases the nearest one or two leaves above the cut are seen to fall practically simultaneously with the administration of the stimulus, while in "normal" conduction several seconds are required to traverse an internode. The reaction in the cases of "high speed" conduction also differs from that of "normal" conduction in that it involves only the main pulvinus and not the pinnules of the leaf. From the evidence, however, which Mr. Snow presents it does not seem probable that these two types differ essentially from one another. Assuming the liquid in the xylem is in tension, the rapidity of the motion of a hormone introduced into the xylem will depend on the relation of the cross-section of the conduit, in which the hormone moves, to the volume of the liquid moved by transpiration and by the elastic recovery of the tissues. Thus, if the motion upwards is confined to a slender linear series of tracheæ, the velocity will be high, while if a woody conduit having a large cross-section can be utilised, then the velocity will be small.

The method of experimentation described by Mr. Snow on p. 352 of his paper is just such as would produce a rupture of the tensile sap in one or two tracheæ and so allow the tensile liquid, having dragged in the hormone from the wounded cells, to hurry it upward in an instantaneously shortening filament of sap. The volume of this rising fluid is necessarily very small. Hence it is found able to affect only the pulvini of the reacting leaves and does not extend to the pinnules. Similarly it is evident that, where an internode is cut across, the velocity of the transfer of the hormone will depend upon the relation just pointed out. Consequently, the accident as to how exactly the cut is made, together with its chance relation to the transverse septa in the vessels, etc., will decide whether the conduction will be "high speed" or "normal." In this connexion Bode's observation should be borne in mind. He found that the tensile sap in tracheæ is ruptured by the pressure of a metallic point on the cells adjacent to the tracheæ. Thus we must infer that a rupture will take place in the sap just as the pressure of the blade producing the stimulus injures the semi-permeable protoplasm of the cells.

In the second place, Mr. Snow's reasons for assuming that the mechanism for the transmission of stimuli in leaves is fundamentally different from that in stems do not appear convincing. His first argument for this conclusion is that the rapid conduction observed in submerged leaves, or in those in a nearly saturated space, would be impossible if conduction depended solely on the movement of the transpiration-stream. It must be remembered, however, that, even in leaves in these conditions, there is evidence that tension exists, and that the turgescence (suction force) of the uninjured cells will draw liquid from the permeable injured cells back along the xylem. The very small cross-section of the xylem in the leaves will render the velocity of this motion comparatively great. Mr. Snow also believes that the fact, that in the leaf basipetal is as fast as acropetal conduction, shows that the conduction-mechanism of the leaf differs from that of the stem. This is just what we would expect if the hormone travels in the tensile sap; for the tension is hydrostatic and the resistance is the same in both directions.

Mr. Snow further finds support for his contention in Herbert's observation, that the velocity of conduction in the leaf is dependent on the intensity of the stimulus, while in the stem it is not so. Mr. Snow's own observations on "normal" and "high speed" conduction show that this rule is not without its exceptions. The fact that the volume of the tracheæ of the leaf is small compared with that of the stimulated cells furnishes a rational explanation of those cases where this difference is observed. This also accounts for the greater rapidity and greater certainty of the conduction in the leaf.

Again, the observation that an eosin solution is not always drawn back into the tracheæ of a responsive leaf cut under the stain, does not show, as Mr. Snow suggests, that in these cases there is no basipetal current in the tracheæ available for transport. Rather the failure of the stain to enter and pass down the xylem is almost certainly due to the unwettable surface of the leaf, which secures that the air adhering to the surface enters the tracheæ and prevents the eosin following the retreating sap. Mr. Snow himself instances a case where the eosin was drawn in 1·25 cm. in 60 seconds. In my own experiments I have observed a downward motion of the dye of 2·75 cm. in 40 seconds in the same circumstances. Lastly, in Mr. Snow's experiments, where transmission was observed in leaves after the continuity of the xylem had been broken by a cut not involving the phloem, there is no evidence produced that the hormone did not pass in the liquid filling the cut. Such transference has been shown to take place in stems and it is hard to see why it should not do so in leaves.

With regard to the nature of the hormone responsible for evoking the reactions, Mr. Snow has made some interesting observations. The hormone may be rendered inactive by dilution, and probably by boiling. Ricca found that of *M. spegazzinii* was not thermolabile. It is not precipitated by a protein-precipitant. It diffuses through a collodion shell. It is not injured by desiccation, nor by treatment by strong alcohol.

HENRY H. DIXON.

[1] R. Snow, "Conduction of Excitation in Stem and Leaf of *Mimosa pudica*," Proc. R.S., Series B, vol. 96, No. B 678.