Hence, when a small scatterer is used the modified band shows great intensity at 0·0242 Å.U. The centre of this intense portion and its width depend, however, on ϕ. Hence for a large scatterer the intense portion becomes indistinct. Further, for a large variation of ϕ, the theory shows that the most intense part of the band due to the *L III* electrons is displaced to the short wave-length side of the value obtained by putting the average value of ϕ in (1). Large scatterers, therefore, tend to obscure the existence of the Compton wave-length change.

Still further, a spectrometer of low resolving power will show the Compton wave-length change better than one of high resolving power. With low resolving power the most intense part of the band comes at 0·0242 Å.U. for $\phi=90°$, whereas with high resolving power the band character of the modified rays is emphasised. For example, with high resolving power the $K\alpha_1$ and $K\alpha_2$ lines of molybdenum can be separated in the unmodified line, while the modified band will show no such separation, as each modified band overlaps the other due to its width. The absence of separation in the modified band might cause an experimenter using high resolving power to doubt the existence of the Compton effect.

G. E. M. JAUNCEY.

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February 14.

Transmission of Stimuli in Plants.

NATURE of January 17 has just reached me and I hasten to send a reply to Mr. Snow's letter on this subject. I repeat once more that in the numerous experiments which I carried out with the stem of *Mimosa pudica*, the stimulus was never transmitted across the water gap. Mr. Snow repudiates the idea that he has disagreed with my conclusions in regard to the conduction in the petiole of Mimosa. "Actually, however," he says in his letter, "in agreement with him [Sir J. C. Bose] I have produced evidence to show that in the leaf, excitation is conducted in the phloem and has nothing to do with the transpiration current. I agree also that this conduction in the leaf is, in all probability, a true physiological process, and consider that Sir J. C. Bose's experiments on the petiole, which so strongly support this view, are of very great value. In the stem, however, as I found, this conduction in the phloem either fails completely, or at least is regularly too weak to cause the leaves to fall."

Since it is admitted that conduction does not take place across a water gap in the petiole, Mr. Snow will note that there is a serious breach of continuity in the assumption that the mechanism of conduction in the stem is widely different from that in the petiole. We stimulate the leaf; excitation is transmitted along the phloem in the petiole and overflows into the stem, causing the successive fall of the leaves. Where does the hiatus in the conducting mechanism come in? The supposed conduction across a water gap in the stem has led to the theory that the transpiration current in the wood is the means of conduction of excitation. But the observation of Mr. Snow himself does not support this theory, for he finds, in opposition to Prof. Dixon, that Dr. Ricca's theory of the transpiration current is inadequate "to cover *all* the phenomena of conduction in Mimosa, including conduction in the leaf and the subordinate phenomenon of 'high-speed' conduction in the stem."

In my "Physiology of the Ascent of Sap" (1923), p. 269, I have clearly explained the modes of inter-communication and interaction between more or less distant organs of a plant. "This is accomplished in two different ways, by *transfer of matter* and by *transmission of motion*. The first is exemplified by hydraulic convection of liquids carrying chemical substances in solution, such as occurs in the circulation of sap; the second, in the conduction of excitatory change along nerves." These two can be easily distinguished from each other from the fact that the conduction of excitation is from a hundred to a thousand times quicker than the sap-movement of the transpiration current. A very simple experiment which requires no apparatus is to put a drop of hydrochloric acid on the tip of a leaf of *Mimosa pudica*: the acid remains practically localised, but the protoplasmic excitation induced by it is transmitted with considerable velocity, causing the fall of numerous leaves both above and below. This experiment, which can be repeated without any difficulty, completely demolishes the theory of the transpiration current.

I have, as stated in my previous letter, carried out numerous crucial experiments which fully establish the nervous character of the transmitted impulse in Mimosa. It has been a matter of surprise to me that reference to my work has been omitted in all letters and articles on transmission of stimulus in Mimosa that have appeared in NATURE during the last year. This could not have been due to the obscurity of the journal in which my results appeared, for they were published in the Proceedings and Transactions of the Royal Society. The omission appears to me to be very curious; it cannot but obscure truth and thus divert the energy of workers in wrong directions which lead nowhere.

In this short communication I can make but bare mention of only a few of the results which I have recently obtained. The anatomical structure of the nervous tissue, made clear by selective staining of microscopic sections, has been found to be in every way the same in the petiole and in the stem. The distribution of the nervous tissue in the whole plant has been clearly traced, affording the fullest explanation of the transmission of excitatory impulse up and down and across the stem. The innervation of the contractile pulvinus, which functions as a muscle, has also been fully investigated, making possible a rational explanation of the purposeful movements by which the leaf places itself so as to absorb the largest amount of radiant energy. Still more striking is the success of my efforts to obtain, by means of selective staining, a definite outline of the rapidly contracting tissue in the pulvinus of *Mimosa pudica*. No such staining occurs in the slowly reacting pulvini of other plants. This characteristic denotes the possession of a specific catabolic substance, and is highly significant. These and other results prove that a very high and quite unexpected degree of differentiation has been reached in the nervous system of Mimosa. I am in hopes of early publication of these results. In case of unforeseen delay, I shall take the opportunity of sending a short account of them for publication in NATURE.

J. C. BOSE.

Bose Institute, Calcutta,
February 12.

The Behaviour of Crystals and Lenses of Fats on the Surface of Water.

DURING the last two years we have been interested in examining the behaviour of crystals and liquid lenses of fatty acids and esters when placed upon the surface of water.

Sir W. B. Hardy was the first to observe that the magnitudes of the surface tensions of the various