sure about the spectroscopic purity of the material used. These have been the chief reasons why for many years, before Moseley's discovery of the possible number of elements, it was believed that new elements had been discovered only from the fact of having seen new spectral lines. Some of these elements have been really found afterwards, but in this case the discovery did not belong only to those who had noticed the spectral anomalies; the non-existence of other of these supposed elements was proved on further examination.

Indeed, about the homogeneity of the elements composing the old didymium there have been for many years numerous discussions after Auer v. Welsbach succeeded, in 1885, in separating neodymium from praseodymium; many experimenters tried to show the heterogeneity of the new elements. In 1886, Crookes (*Proc. Roy. Soc.*, 40, 502; 1886) affirms that the two didymia are complex, and so too Demarçay (*C. R. Acad. Sci.*, 102, 1551; 1886; 105, 276; 1887), Becquerel (*C. R. Acad. Sci.*, 104, 777; 1887; 104, 1691; 1887), G. M. Tomson (*Chem. News*, 55, 227; 1887), Ksewetter (*Ber. Chem. Ges.*, 21, 2310; 1888). Crookes and many others, starting from the anomalous behaviour of emission and absorption spectra, believed that didymium should contain at least one other new element. Krüss and Nilson (*Ber. Chem. Ges.*, 20, 2134; 1887) even stated that didymium and praseodymium were composed of at least nine elements.

In 1913, Moseley's rule removed any uncertainty from these researches, establishing that one and one element only ought really to be found in the didymia earths, and that to this element belonged the ordinal number 61. The merit for this improved prediction should be credited only to Moseley, who defined and circumscribed the field of research.

We believe, then, that the priority in the discovery of element No. 61 belongs instead to those who first had sure data as to its existence, and in similar cases sure evidence cannot be obtained except by means of X-ray investigations. While L. F. Yntema published the negative results of his research in this field, we obtained (X-ray measurements were made by Prof. R. Brunetti) the first photographs of *K*-absorption spectra showing the characteristic band of element 61 and, a few months after, we collected our results in two papers deposited, as *plico suggellato*, at the Accademia dei Lincei. These contained the first certain data, therefore we believe that we should be credited with priority for the discovery.

As regards Prof. Noyes' remark upon the priority of name, we would point out that the name Florentitim was given by us in June 1924, a year and eight months before the paper referring to the name Illinium appeared. We are, on the other hand, perfectly in agreement with Prof. Noyes that much additional work has still to be done on the subject, and we hope that the combined effort of researches will, in a short time, bring about the undisputed acceptance of the new element.

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Mar. 21.

Transmission of Stimuli in Plants.

FOR two writers living in the East to carry on a correspondence in the columns of NATURE is a somewhat protracted process, but perhaps I may be permitted to reply to the remarks of Sir J. C. Bose (NATURE, Jan. 8) regarding a previous letter of mine (NATURE, Oct. 23, 1926).

It is interesting to note that Sir J. C. Bose now apparently accepts the fact, originally demonstrated by Dr. Ricca, that in Mimosa the excitation induced by a flame can be transmitted across a water-gap.

The importance of this fundamental experiment is quite unaffected by any amount of experimental evidence which may appear to show that the phenomena associated with the transmission of stimuli are closely similar in the plant and in the animal. It is not really essential to show that the excitation induced by every kind of stimulus can similarly be transmitted across a water-gap, although, provided that the stimulus can be made sufficiently intense, there should not be any difficulty in affording the necessary proof. It must be remembered, however, that if only a small amount of hormone is set free, it would suffer so much dilution in passing through the water-gap that its stimulating power would be seriously diminished.

Some of the arguments used by Sir J. C. Bose against the transpiration current theory have little force. For example, he states that the impulse should only travel upwards in the same direction as the ascent of sap. Movement in either direction is readily explained by the generally accepted theory of the ascent of sap put forward by Dixon and Joly, and can easily be demonstrated by cutting the plant under stain. At the same time it must be admitted that, although Dr. Ricca's theory undoubtedly explains many of the normal instances of conduction in Mimosa, it does not afford a complete explanation in every case. In submerged shoots, for example, where the transpiration current is almost negligible, stimuli may be conducted through long distances in the stem at a rate of more than 200 cm. per minute. In a paper which I hope will be published early this year, I have suggested that in such cases the transport of the stimulus takes place as a result of the contraction of highly turgid cells. At the point where the stimulus is applied, certain of the cells contract and liberate a stimulating substance. This affects neighbouring cells, which in turn liberate more of this substance, and so the process goes on. For the experimental evidence on which this theory is based, I must refer Sir J. C. Bose to the complete paper, but I may mention that it appears to afford an explanation for those cases where, as already pointed out by Mr. R. Snow, the transpiration current theory is inadequate.

The evidence which Sir J. C. Bose has brought forward in favour of his own theory is well known and is extremely interesting. Even if one holds the view that the conduction of stimuli in Mimosa takes place apart from any nervous mechanism, it is still necessary to consider certain of the phenomena associated with electrical stimulation. It seems probable that these may be explained in other ways, although, at the present state of our knowledge of the factors involved, this may be difficult. If, however, it is finally proved that there are certain facts which cannot be explained by any other hypothesis, then, and then only, shall we be justified in accepting a nervous mechanism as *one* of the methods by which stimuli are transmitted in plants.

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Feb. 3.

River Pollution and the Acidity of Natural Waters.

IN "Fundamental Problems relating to River Pollution" (NATURE, Mar. 26, p. 463), Mr. H. W. Harvey mentions the probable effect of the hydrogen ion concentration on river flora and fauna, and suggests that possibly *p*H 5·5 is a critical value.

It seems probable that, as regards fish life, acclimation may be an important factor and that