

the famous horse as the only known exception to the general rule, when Mr. Mullins at once interposed with the remark, "And that is no exception," and proceeded to inform me that it had been introduced surreptitiously into the cave, more than one person—as I understood—having been involved in this nefarious proceeding. He demurred, however, and as I thought very naturally, to my request for names, but assured me that he spoke of his own personal knowledge.

Any reflexion on the good faith of any of the explorers of the cavern—particularly my old and dear friend the Rev. Magens Mello, the actual finder of the engraving—would have at once aroused my indignant resentment; but there was no hint of this, and as Mr. Mullins's statement not only disposed finally of what I had always regarded as malicious gossip, but was also in general harmony with the state of knowledge at the time, I felt that I ought to make it public, even if only in a modest footnote.

It is to be regretted that Mr. Mullins is no longer with us to add his explanations to mine. All I can do now is to withdraw the controverted statement and to delete it from the footnote at the earliest opportunity.

Perhaps I may be permitted to refer to another discovery which was made too late for notice in my last edition. It is of great importance, since it affects not merely a locality but a whole industry. I allude to the finding by M. Peyrony in a Solutrean layer at Les Eyzies of a slab of limestone bearing a carving in high relief of two oxen (*Bos primigenius*). When I examined this last Christmas I was much impressed by its skilful modelling, fidelity to Nature, and artistic feeling. It recalls, though less bold, the famous bisons of Tuc d'Audoubert.

Hitherto the Solutrean age has afforded no objects of art, and this has always been regarded as a remarkable fact, for the Solutrean people were the first to introduce that new method in the working of flint which produced the most beautiful weapons of the Palaeolithic age, and afterwards found its culmination in the wonderful productions of Neolithic Egypt and recent North America. It was supposed in explanation that the Solutreans were a war-like invading race, who concentrated all their attention upon the perfection of their weapons, and had none left to bestow on purely artistic effort. We now see how far this was from the truth.

University College, Oxford,
March 10.

W. J. SOLLAS.

Transmission of Stimuli in Plants.

MR. SNOW'S letter on this subject in NATURE of January 17, p. 82, suggests the following considerations:

The fact that the velocity of the movement of coloured fluids in transpiring shoots and leaves is often much slower than the transmission of stimuli can scarcely be used as an effective argument against the rapid transport of hormones in the transpiration current. When a shoot, or leaf, is cut across and supplied with stain, the whole, or most, of the cross-section of its wood may be utilised by the current, and the velocity of flow may be comparatively slow; whereas, if the water transpired is made good through a conduit consisting of one or two tracheæ only, the velocity may be very great. It is significant that Mr. Snow himself attributes the transmission of a stimulus at a velocity of 52 cm. per minute to the movement of water in the vessels (Snow, R., Proc. R.S. B, vol. 96, p. 358).

Mr. Snow refers to his Experiment 11. In this

experiment the wood of the petiole of a "stem and leaf preparation" was severed by a single cut of a razor. The tension of the water in the wood was relieved by the submergence of the preparation in water, and yet, after a rest of several hours, a stimulus was observed to be transmitted across the cut to the pulvinus. It still seems to me possible that the movement of the motile tissue at the base of the pinnules forced water containing the hormone through the distal tracheæ and that some of this was drawn (really pressed by the atmosphere) into the tracheæ below the cut. The continued expansion of the cells adjacent to these tracheæ by turgor would give space for this movement. Thus relief of the tension might not preclude motion in the tracheæ.

In any case, the experiment requires careful confirmation. It appears from Mr. Snow's description that the experiment was attempted with 40 stem and leaf preparations. The exact depth of cut required, just severing the continuity of the wood, was attained in only 5 of these. Of these 5, 3 showed no transmission of the stimulus across the cut. The extreme difficulty of ascertaining with certainty, even with microscopic observation, that all the water-conducting elements were cut across, and remained severed during the experiment, may justify us in suspending our judgment as to the correct interpretation of this experiment.

Mr. Snow also invites attention to his Experiment 12, in which he observed that exposing a narrow zone of a petiole to steam prevented the passage of a stimulus. In his paper he points out that both Haberlandt and Fitting obtained the opposite result. Furthermore, it is quite probable that substances introduced into, or developed in, the tracheæ by the heat may have prevented rapid motion of fluids in these capillaries.

With some trouble I have succeeded in consulting, at Mr. Snow's advice, Prof. Herbert's paper in the *Philippine Agriculturist*. It has disappointed me. The details of the experimental arrangements are scanty throughout, and until more convincing evidence is produced I could scarcely agree with Mr. Snow's statement—"The xylem of the petiole cannot conduct excitation downward at all, as has been shown by Herbert." In fact the latter emphasises more than once that, if the downward transmission is to be prevented, "a good deal of the wood" must be removed. Further, when "dexylification" was practised by Herbert, he filled the cavity left by the removal of the wood with water. In this, of course, the hormone may have been transmitted, as in Ricca's experiments.

Mr. Snow quotes Bose as having showed that the phloem is the conducting tissue, by evidence derived from electrical changes. Unfortunately the same investigator produces the same evidence in favour of transmission in the parenchyma on the inner side of the xylem, mistaking it for phloem. To this tissue both Snow and Herbert deny the power of transmission.

HENRY H. DIXON.

School of Botany,
Trinity College,
Dublin.

Indian Kala Azar Commission.

IN the issue of NATURE dated December 6, 1924, page 840, under a notice of the anniversary meeting of the Royal Society, mention is made of an investigation to be carried out under the auspices of that Society upon kala azar in India. The steady spread of kala azar in India has for long been a most disquieting feature in certain provinces. Recently, as a