

This experiment I have repeated since reading Wiesner's book, and have found the results to be the same. The conclusion is inevitable and is in this case absolutely destructive of Wiesner's theory of "Zugwachsthum."

This theory he grounds on the following experiment (p. 69), in which he makes use of Sachs's method of observing heliotropism:—Seedlings growing in small vessels are fixed in the place of the minute-hand of a large clock, so that each seedling is at right angles to the axis of rotation, and rotates like the hand of the clock; they are then illuminated by light which is parallel to the axis of rotation, and therefore each seedling has one side constantly illuminated by light striking it at right angles. Owing to the constant rotation the effect of weight is eliminated, and thus any curvature which occurs cannot be due to "Zugwachsthum." Wiesner states that whereas the seedlings on the klinostat (Sachs' name for this instrument), were only curved in their upper parts; plants growing normally without being subjected to rotation were curved down to the ground. This seems at first a conclusive argument against our view, but I shall show that in the case of two plants, cabbage and Phalaris, it is not so.

We expressly stated (p. 479) that our experiments on cabbages were made on young seedlings "about half an inch or rather less in height," because when the plants have grown to an inch and upwards in height the lower part ceases to bend heliotropically. Now Wiesner's experiments were made confessedly on seedlings whose lower part was growing slowly, and which were therefore probably older than those which we employed for our experiments. When Wiesner made his rotating experiment with *young* cabbage seedlings they became curved down to the ground. This proves that the curvature which occurs near the ground in young cabbage seedlings is not due to weight; and this is the very curvature which we have shown not to occur unless the upper part is illuminated. I do not attempt to explain Wiesner's experiments on old cabbage seedlings, but those made with young ones are alone of importance for us, and they are conclusively on our side.

With regard to Phalaris I regret that I cannot confirm Wiesner's results, who states that these seedlings behaved like the dicotyledons experimented on; *i.e.* that when grown on the rotating apparatus they do not become bent down to the ground. I have experimented with young seedlings such as we should have used for the experiments on transmission of the light-stimulus, and found that many of them became well bent down to the ground. But it should be remarked that in some cases a certain amount of difference in this respect was observable between the plants on the klinostat and normal ones.

FRANCIS DARWIN

(To be continued.)

OUR BOOK SHELF

Through Siberia. By Henry Lansdell. Two volumes, with illustrations and maps. (London: Sampson Low and Co., 1882.)

It is obvious that much scientific information cannot be expected from a traveller who was, to use his own expression, "flying across Europe and Asia," and who crossed Siberia from Ekaterinburg, in the Ural Mountains, to Tobolsk in the North, Barnaoul in the Altai,

and Nikolaevsk on the Pacific, a distance of 6600 miles, in seventy-eight days, and whose aim was, during this very short time, to investigate the situation of Russian prisons. The author has, however, supplemented his own somewhat superficial observations by information obtained from good sources. The book is provided with many illustrations, partly taken from other works (without quoting the source from which they are taken), and partly from new photographs. These are sometimes very good, but sometimes they convey quite false ideas, as, for instance, the photograph of a "Buriat girl," who obviously is a metis, having very little in common with true Buriats.

P. K.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Limulus

IN a criticism published in the *American Naturalist* for April, 1882, on Prof. Ray Lankester's recent most able memoir, entitled "Limulus an Arachnid," Mr. A. S. Packard, whose most important researches on Limulus are familiar to all zoologists, and to whose courtesy I am indebted for a copy of his criticism, after stating other grounds which lead him to differ in opinion from Prof. Lankester as to the close relationship of the King Crab and the Scorpion, quotes in his final paragraphs extracts from published letters written by my late lamented friend and shipmate, R. von Willemoes-Suhm, from on board H.M.S. *Challenger*, at the Philippine Islands and Japan in February and May, 1875, concerning certain Arthropod embryos which he had had under observation at Zamboangan, and which he then supposed to be the larvæ of *Limulus rotundicauda*. As Von Suhm and I worked together for more than two years daily with our microscopes within two feet of one another, we naturally discussed all that we did and observed in common, and we frequently talked about these supposed Limulus embryos, and looked at them together. It is as well, therefore, since the statements concerning them are being made use of to assist in disproving the position assumed by Prof. E. van Beneden, Prof. Lankester, and others as to the Arachnid nature of Limulus, a position of the strength of which I am myself persuaded, that I should state in print, that long before his death Von Willemoes-Suhm was completely convinced that he had been misled as to the larvæ, and told me that he felt sure they were not those of Limulus at all, but belonged to a Cirripede of some sort. I some time ago told my friend, Prof. E. van Beneden, who inquired on the matter, that such was Von Suhm's final conclusion. And I also long ago told Prof. Lankester, and this is no doubt the reason why no reference to Von Suhm's letters was made by the latter in his memoir.

It must be remembered that the only evidence in favour of Von Suhm's Nauplius larvæ being those of Limulus, lay in their general appearance, which simulated to some extent that of an adult Limulus, and in the fact that they were caught with the tow-net in Zamboangan harbour, a locality at which *Limulus rotundicauda* occurs.

H. N. MOSELEY

Oxford, April 15

Silurian Fossils in the North-West Highlands

THE publication of Dr. Heddle's geological and mineralogical map of Sutherland, which was noticed in NATURE, vol. xxv. p. 526, calls to mind some curious points with reference to that region—points on which we should like to have some further and more definite information.

Dr. Heddle quite acquiesces in the general accuracy of the stratigraphical conclusions arrived at by Murchison and his colleagues, and, as may be gathered both from his map and writings, has seen no cause whatever to induce him to believe either in the great fault of Prof. Nicol, or in the unconformity alleged by Dr. Hicks to exist in the adjacent county.