

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 intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Early Arrival of the Swift.

A FEW minutes before six o'clock in the afternoon of March 26, my brother, Sir Edward Newton, saw, from the cliff near the high lighthouse at Lowestoft, what, at the first glance, looked like a Swallow, flying over the trees in the garden of the house known to many as that occupied more than thirty years ago by the late Dr. Whewell. As the bird turned and gave us a better view of it, we perceived it to be a Swift. Crossing the foot-bridge and getting to the edge of the Park overlooking the garden, we watched it pass backwards and forwards for about a couple of minutes, when it flew away to the northward, and, though we waited for some little while, it did not reappear. I may add that we were favourably placed as regards light, the sun being behind us. I do not recollect any record of the occurrence of the Swift in England so early as this by some weeks, and it would be interesting to know if the bird should have been observed elsewhere.

ALFRED NEWTON.

March 27.

Red Dust of Doubtful Origin.

ON Tuesday morning, March 22, I noticed on the glass of our greenhouses, and on many of the shrubs, a sort of red dust. On making inquiries I found the same thing existed about two miles off, due west. I collected some, and, by the kindness of one of the directors of Messrs. Brunner, Mond, and Co., it was examined in the laboratory connected with their works. To-day I got the report, which is as follows:—

"The dust, under high magnification, shows minute fragments of clayey matter mixed with quartz. Organic matter such as pollen grains are absent. The particles are about 0.0001 millimetres in diameter, many of them less.

"The chemical examination shows clay mixed with a little carbonate of lime and a fair amount of fine sand. The reddish colour is due to oxide of iron."

We are surrounded by grass; the soil is a clayey loam without oxide of iron or quartz.

Could any of your readers suggest where the dust can have come from?

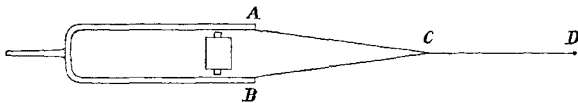
J. M. YATES.

Davenham, Cheshire.

Experiment on Interference.

I HAVE successfully performed the following experiment on interference:—

To the prongs of an electrically-driven fork are fastened the ends A and B of an elastic string; at the middle point, c, of this string another string, C D, is fastened. D is held in a clip, and the whole stretched. By properly adjusting the lengths and



tension, A C and B C will vibrate in unison, while C D remains motionless; but if A C or B C be damped, C D immediately vibrates.

This is a very pretty experiment when projected, and by flashing the light on it is easily demonstrated that A C and B C are always in opposite phases.

JOHN WYLIE.

101 University-street, Belfast, March 8.

The Additional Colouring Matter of "Fucus vesiculosus."

I DO not know whether the following is likely to be of any interest to your readers.

Having prepared a solution of the brown additional colouring matter of the common Bladder wrack, I placed it in the line of a beam of sunlight that had passed through a prism, with the result that the violet indigo and blue rays were entirely intercepted, whilst the yellow orange and red rays were practically unaffected. I repeated this test with vessels that, owing to their flatter form, presented varying quantities of the solution, and in one in which the rays had only to pass through about a quarter of an inch of

the liquid the green rays were slightly visible, but neither the blue nor the violet. To make sure that the vessels had nothing to do with the result, I filled them with plain water and passed the beam through them.

Is it justifiable to conclude from this that the action of this additional colouring matter is to protect the chlorophyll from the relative increase of the blue rays, and not to heighten the effect, save indirectly, of the others?

Could any of your readers refer me to any papers upon the chemical nature of this additional colouring matter, &c.?

CLARENCE WATERER.

Ingleside, Northdown Road, Margate, March 8.

Chinese Yeast.

IN answer to Mr. C. E. Stromeyer's query in NATURE, March 18, p. 463.

An account of *Léuvre chinoise*, with details on the manner in which it is prepared, and on the moulds, yeasts, and bacteria it contains, is given by Calmette in the *Annales de l'Inst. Pasteur*, t. vi., 1892, p. 604. A lengthy review of Calmette's paper appeared in *Centralblatt für Bakteriologie*, vol. xiii., 1893, p. 273.

ITALO GIGLIOLI.

Agricult. Chem. Laboratory, Portici, near Naples.

The Electric Eel.

ON a recent expedition to the N.W. district of British Guiana, I was able to secure a specimen of the electric eel, which I believe to be the largest on record. The fish measured 7 feet 2 inches in length. It was caught with hook and line in a very shallow and unfrequented branch of the Waini River. The skin is now in the local museum.

J. J. QUELCH.

British Guiana Museum, March 3.

The Utility of Specific Characters.

I HAVE followed the discussion on this subject with great interest; and though I am at such a distance that my thoughts may come a little late, I wish to call attention to a few points. In NATURE for October 22, 1896, p. 605, mention is made of a discussion on Neo-Lamarckism at the British Association. In opening the discussion, Prof. Lloyd Morgan referred to the importance of noting the bearing of certain cases that may be considered as crucial, or as nearly crucial as any that we are at present able to obtain, on the process by which specific instincts are built up. As illustrating this class of cases, he refers to the drinking instinct in newly-hatched chickens, where the instinctive response begins at the point where the teaching of the parent bird would naturally be inadequate.

The question I wish to raise is, whether such observations as this can do more than justify the conclusion that life-saving instincts are strengthened and established by natural selection. Are they sufficient to show that all permanently inheritable specific characters are wholly due to natural selection, or even that natural selection is always one of the factors by which any and every permanent character has been built up? It seems to me that there are large classes of facts, some of which may be found in almost every species we examine, which throw doubt upon there being any such inseparable connection between natural selection and the inheritance of characters.

The majority of the human species inherit right-handedness. Does this prove that right-handedness is better for the race than left-handedness? The shells of most molluscs are coiled in a way that is called dextral; but some groups of species are as constantly sinistral as most groups are dextral; and of the dextral groups there are certain species that are persistently sinistral; others that are nearly equally divided between dextral and sinistral forms. Is it necessary to believe that for each species that is usually either dextral or sinistral, there is some vital necessity that would exterminate, or even diminish, the species if the character was reversed? A similar class of cases is found amongst the different species of flat-fish. One species persistently lies on the right side, another on the left, and I think it is Mr. Cunningham who has told the readers of NATURE that there are some species in which both forms may occur. In twining plants similar persistence is observed in the direction in which the vine encompasses the support. In each of these classes of cases I am unable to conceive of any advantage gained by the species that would not be equally gained, if the character under discussion was reversed. If the adaptation to the environment of a flat-fish that now lies upon the right side would be equally