## 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.]*

### Utility of Specific Characters.

PROF. LANKESTER's lucid statement in NATURE for August 20, shows that a part of his objection to my position is due to my own want of skill in stating clearly what I mean.

I am far from wishing to reject the method of imaginative hypothesis and subsequent experiment or observation. I respect that method as sincerely as Prof. Lankester himself, and although I cannot pretend to his measure of skill in using it, yet, so far as I can see, I have, in my work on the frontal breadth of crabs, employed this very method to the best of my ability. The hypothesis with which I started was, that if natural selection acted upon the frontal breadth of crabs at all, there ought to be a demonstrable difference between the percentage of abnormal frontal breadth in young crabs, and the percentage of the same abnormalities in older crabs; and I proceeded to test this hypothesis by measurement of crabs of different sizes. The result showed that a change in the frequency of abnormal frontal breadth could, in fact, be observed. The effort of imagination was here small enough, but, such as it was, it served to guide my first step; and, having made this first step, I had to formulate a second hypothesis. A diminution in the frequency of abnormal frontal breadth, with increasing size of crabs, might be due either to a selective destruction of abnormal crabs during growth, or to a modification of these crabs, by which abnormal individuals lose their abnormality as they grow. In order to decide which of these imaginative hypotheses should be adopted, I have spent a great part of the last two years in ascertaining the law of growth of crabs, so far as their frontal breadth is concerned. Setting the question of skill on one side, the only difference I can perceive between the method of this whole investigation and that of any research conducted by Prof. Lankester, is a difference in the tools employed in verification of hypotheses. The only tool which I have used has been some kind of measuring scale; and, although this kind of tool is more unpleasant to work with than those used by more fortunate persons, it does not imply any difference in the method of work.

Further, assuming the law of growth to yield evidence of selective destruction, so that change in frontal breadth is correlated with change in death-rate, I heartily agree with Prof. Lankester that a further hypothesis ought to be formulated as to the whole process connecting change in frontal breadth (and the whole group of characters correlated with it) with change in death-rate. The only step taken by Prof. Lankester, which I cannot follow, is the admission of hypotheses in which some of the factors of the problem are neglected. I should like to explain what I mean by this.

In *Carcinus mænas* I have shown that change of frontal breadth is correlated with change in several other dimensions of the exoskeleton; and I have no doubt that it is correlated also with change in the size and shape of several internal organs, such as the brain, liver, kidneys, and others. I have not measured such an oxyrhynchous crab as *Stenorhynchus;* but it is probable that the changes among internal organs correlated with change in frontal breadth, will prove to be very different in such a crab as *Stenorhynchus* from the corresponding changes in *Carcinus.*

Let us suppose, therefore, that the liver is shown to vary when the frontal breadth of *Carcinus* varies, but not when the frontal breadth of *Stenorhynchus* varies; and suppose, further, that an hypothesis is submitted as to the process by which change in the liver of *Carcinus* leads to change in the death-rate. It seems to me that, unless one of the steps in this process involves a change in frontal breadth, the hypothesis must be rejected, because one of the properties of the liver of *Carcinus* is not accounted for. The hypothesis submitted may be true of *Stenorhynchus;* but, since it neglects one of the differences between that animal and *Carcinus*, it cannot be true of both.

To put the matter in another form: suppose I wish to obtain hydrogen from sulphuric acid, I can do so by adding to the sulphuric acid a certain quantity of zinc. From a known quantity of sulphuric acid I can obtain a definite quantity of hydrogen, and I shall, in so doing, dissolve a definite quantity of zinc with the formation of a definite quantity of zinc sulphate. If, instead of dissolving zinc, I dissolve iron in my sulphuric acid, I can still obtain from it the same quantity of hydrogen, but the quantity of iron required will not be the same as the quantity of zinc used in the previous experiment, and the resulting sulphate will be different. It is, of course, impossible to form an exact hypothesis of what occurs in either of these cases, if I pay attention only to the evolution of hydrogen, and regard the formation of sulphate as an unimportant concomitant. I must in each case form a theory of the behaviour of the metal, the hydrogen, and the acid radicle; and, so far as it fails to account for any fact concerning any one of these bodies, my theory is imperfect.

In precisely the same way, it seems to me that we ought not to rest content with any theory of an animal structure which does not account for all the phenomena associated with it; so that a theory of the function of frontal breadth in a crab should, I think, involve every organ correlated with it. It may be said that such a theory is unattainable because of its complexity; and this is certainly at present true; but the habit of regarding one or other of the properties of an organ as unimportant, would for ever prevent the formation of such a theory even if it were otherwise possible.

It is this sense of the necessary complication of such hypotheses which makes me glad to assert that they are unnecessary to a knowledge of the factors of evolution. It is possible to know that change in frontal breadth in a *Carcinus*, for example, is associated with change in death-rate under the conditions of Plymouth Sound; so that those crabs in which the frontal breadth has a particular magnitude, can be known to have a greater chance of living and breeding than those of different frontal breadth. A complete knowledge of the processes associated with this relation between frontal breadth and death-rate is a thing of very great interest, and I believe, as firmly as Prof. Lankester, that every effort should be made to attain to it; but, desirable as it is, it is still not necessary in order to know that a crab's chance of living and breeding may be known by measuring its frontal breadth. It is not necessary in order that the change in mean frontal breadth may be measured from generation to generation, and the direction and rate of evolution by this means ascertained.

W. F. R. WELDON.

Marine Biological Laboratory, Plymouth, August 26.

### The Death of Lilienthal.

I HAVE received this authentic report of Mr. O. Lilienthal's death. If you think the letter worth publishing in NATURE, it is at your service.

C. RUNGE.

Hannover, Technische Hochschule.

YOU are right in presuming that I can give you details referring to Otto Lilienthal's death, authentic as far as they can be obtained.

As early as the beginning of last spring, Lilienthal's experiments had taken a new departure. He had gradually come to the conclusion that the surfaces employed by him were not sufficient.

With a surface of twelve to fourteen square metres he could take sufficiently long flights to serve his purpose of observation and practice in strong, gusty wind, but he very rightly considered experimenting in a strong wind to be too dangerous, and with a light breeze about twenty square metres were found necessary. This enormous surface, however, could not be handled with the same certainty and exactness as the older wings, and as his system of steering consisted in shifting his weight within the surface upon which it was suspended, he had hit upon the simple expedient of placing two surfaces one above the other.

This system promised from the beginning to be a very marked advance. In former days Lilienthal had tried, over and over again, to make small paper models that would soar like birds, and had always been disappointed. Now this problem seemed to be solved. These two-story models, which resembled beetles rather than birds, soared in the most astonishing manner. He would let them off from the top of the artificial cone which he had erected at Lichterfelde, and they would take long and sometimes circuitous flights into the surrounding fields, and never showed the slightest tendency to take "headers"—a peculiarity very frequently hitherto observed in soaring models.

These experiments, therefore, seemed to prove that not only would a two-story surface be more easily steered, because a