Letters to the Editor.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Biological Fact and Theory.

"Who is this that darkeneth counsel by words without knowledge?" (Job xxxviii. 2).

Prof. J. S. Huxley (NATURE, Mar. 5, p. 350) writes: "Prof. Walker says . . . that Dobell 'proved' that hereditary characters could not be controlled by chromosomes in certain Protozoa." What I did say is quite different. Here it is: "There is a number of organisms in which the distribution of the chromosomes is such that they could not possibly convey a Mendelian character." I did not use the word 'proved,' which Prof. Huxley places in inverted commas, from beginning to end of my letter.

He goes on to say, "The main reason advanced by Dobell concerned sex," and "if Prof. Walker had been better acquainted with genetical literature he would have remembered that almost simultaneously with Dobell's 'proof,' Wettstein was demonstrating experimentally, and conclusively, . . ."

Mr. Clifford Dobell in the paper referred to, having given a detailed description of the distribution of the chromosomes in certain organisms, begins his argument as follows: "Let us now select a particular character and a particular chromosome, and consider their relations to one another. It is really immaterial which character or which chromosome we take; but for the sake of argument let us take sex as the character to be studied."

Any one reading the paper will find that it was written with the object of confronting with actual facts certain sweeping generalisations of which Prof. Huxley so constantly provides us with examples. He will also see that sex has nothing to do with the argument. So far as I can see, Wettstein's work on mosses, which Prof. Huxley quotes but to which he gives no reference, has no direct bearing on the particular point at issue, and I must place his use of it in the same category as his misquotation of me and his misunderstanding of Mr. Dobell.

Referring to Prof. Johnstone and myself, Prof. Huxley says, "But has Liverpool never heard of Boveri's experiments on disperm sea-urchin eggs, published exactly twenty years ago?" This work was published too late for me to refer to it in my little book on cytology (1907), but Prof. Huxley will find sufficient evidence to show that I had read it in my "Hereditary Characters" (1910, Edward Arnold, London, pp. 37-8 and 222).

I would bring a remarkably apposite passage from another work to his notice.

"Now, from the purely artistic point of view, . . . I consider it an axiom that one should never appear to doubt that the other side has performed the elementary duty of acquiring proper elementary information, unless there is demonstrative evidence to the contrary" (Thomas H. Huxley, "Collected Essays," vol. 5, p. 373. Macmillan, London, 1909).

As, however, the main object of all of us is the ascertainment of truth, I must ask to be forgiven for wasting so much space on trivial self-justification and return to my protest against the constant presentation of doubtful theories as proven facts. I have watched the development of 'neo-Mendelism' from its inception. (Having regard to Prof. Huxley's methods

of controversy I must be precise. I do not mean from the time of the first publication by Mendel in 1865, but from the time of the 'rediscovery' very many years later.) I have watched, often with amazement, sometimes with amusement, assumption piled upon assumption as occasion arose to make the 'neo-Mendelian' 'laws' agree with the results of breeding experiments. The attitude of Prof. Huxley and others of the same school seems to me to approximate to that of the advertiser who is convinced that the more often he repeats a statement the greater will be the number of people who will believe it. Out of the vast accumulation of assumptions has arisen the jargon in which Prof. Huxley sets forth what he claims to be a statement of the fundamentals of genetics. This statement of fundamentals represents, not proven facts, but assumptions for which a varying amount of evidence is available. May I be allowed one final quotation:

The meaning doesn't matter if it's only idle chatter of a transcendental kind,

And every one will say

As you walk your mystic way,

"If this young man expresses himself in words too deep for me,

Why, what a very singularly deep young man this deep young man must be!"

(W. S. Gilbert, "Patience," Opéra Comique, 1881.)

CHARLES WALKER.

The University, Liverpool, Mar. 8.

IN NATURE of Feb. 26, Prof. Johnstone asks the question, "Is breeding cats, and cocks and hens, and flies, and so on, such fundamental research?" He might equally well ask, "Is reading galvanometers, or making organic compounds, or cutting sections, or stimulating nerves, such fundamental research?" The answer is quite clear. These operations, like breeding animals, are part of the technique of research, which may or may not be fundamental in any given case. For example, cat-breeding was part of a fundamental research when it showed that the spermatozoon determined the sex of the young, and that some of the differences between the wild species (or sub-species) of cat were inherited in a Mendelian manner.

After admitting his inability to understand the results which geneticists have so far reached, Prof. Johnstone then proceeds to tell them their real function, which is to determine how genes grow. Here I would very respectfully beg leave to differ from him. To-day the atomic theory in genetics is about as well established as was the atomic theory in chemistry a hundred years ago. But for a century after Dalton chemists concerned themselves not quite unprofitably with the mutual relations of atoms, without knowing very much about their nature. This is what geneticists are now doing with regard to genes; and just as the internal structure of atoms was elucidated by physicists rather than chemists, it is probable that the inner nature of the gene is a problem for the biochemist rather than the geneticist.

Prof. Johnstone's second fundamental problem, of how the genes co-operate in development, is analogous to the problem of how atoms co-operate in a molecule. The most obvious preliminary to its solution is to combine genes in as many different ways as possible and see what happens; and this is just what the despised breeders of cats are doing, to the extent of their financial resources. If they are better than Prof. Johnstone at solving crossword