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 insure the appearance even of communications containing interesting and novel facts.*]

On the Connection between Chemical Constitution and Physiological Action

IN his letter to NATURE last week (p. 594), Dr. Blake considers that I have not only misunderstood the scope of his experiments, but have been led into error on account of my having no definite idea of the meaning of the term chemical constitution, which he thinks I have evidently confounded with that of chemical composition.

In regard to the first of these points, I shall be very sorry if, by mishap, I have not rightly understood, or have failed to appreciate at their true value, Dr. Blake's experiments (most of which were published before I was born), for I regard him as a true pioneer in the field of pharmacology.

The scope of Dr. Blake's researches, as defined by himself in the Report of the British Association for the Advancement of Science for 1846, was "fully to establish the law of the analogous action of isomorphous substances."

I should no doubt have described Dr. Blake's researches more correctly had I used the word "isomorphous" instead of translating it into popular language, for my translation undoubtedly does not give the full meaning of the word; but my whole address was an attempt to make a difficult subject as popular as I could, and I thought that I had sufficiently acknowledged Dr. Blake's priority by observing that the present epoch of pharmacology might be dated from his researches, although it was those of Crum Brown and Fraser which fairly started pharmacological investigation in a new direction. Perhaps Dr. Blake will be inclined to regard my shortcomings in regard to him more leniently if he will read over my address again, for, if he does so, I think he will see that if on my part I have failed to give him due credit, he on his part has completely misunderstood the whole drift of my address, which was to show the importance of chemical constitution as distinguished from chemical composition.

T. LAUDER BRUNTON

The Origin of Species

IT has already been pointed out by Mr. Evershed that the "physiological selection" of Dr. Romanes is identical with the theory outlined by me nearly two years ago in these pages (vol. xxxi. p. 4). As all the objections which have been raised apply equally to my theory, I may perhaps be allowed to give my answer to some of them; it will probably differ in some points from that promised by Dr. Romanes in the *Fortnightly*.

I quite agree with Mr. Wallace (in the *Fortnightly*) that it is only among the group of animals which have at least one common parent that the corresponding variations of the sexual organs which are required for physiological insularity would be likely to occur. But when he maintains that not more than two or three of such a group would reach maturity, and that therefore they would soon die out, he seems to me to forget that it is only on the average that the number would be so small. Many groups would be small, and would die out; exceptional families would be more numerous and more lucky; just as we can all point to human families where twelve or more children have reached maturity, though the average number of those who do so is under three in a family.

The survivors, more or less numerous, would generally not be scattered far from their common birthplace, so that their chance of finding one another would not be very small, especially if the sexual instinct was correspondingly modified, and this might well be the case from what we know of the connection between the psychical and physiological parts of the reproductive function. This presupposes some difference of smell, form, colour, &c., to enable an animal to distinguish those of its own family from the rest of the species, but this probably exists between any two animals.

They might thus be under no great disadvantage compared with the parent species, and they would have a counterbalancing advantage in the much greater adaptability to circumstances which a small group possesses. Any useful variation occurring in a large group, if not swamped by the effect of interbreeding with a large number of unimproved forms, must take many generations to modify the whole mass; while a similar useful variation occurring in one member of a small and physiologically isolated group could modify the whole group in a few generations. The existence of a six-fingered man in England would not appreciably modify the inhabitants in a thousand years, even if it was a slight advantage to have six fingers; while if a six-fingered man was introduced into an island with five other inhabitants, a fair proportion of the population would probably be six-fingered in three generations.

It is perhaps worth pointing out that the curious connection between colour and fertility, in which Mr. Wallace seeks for the explanation of the sterility of species, follows at once as a corollary from the doctrine of physiological selection. For, apart from any special modification of the sexual instinct, all animals seem to prefer to breed with those of their own colour, and hence *any* change of colour in the isolated family would be an advantage, and would indeed remove the one disadvantage under which such a family lies. So a change of colour, otherwise useless, would in such cases be preserved, and be found accompanying sterility with the parent species.

Another of Mr. Wallace's objections seems to me a strong argument in favour of my (and Dr. Romanes's) theory. He says that some animals, not only of different species but of different genera, can produce hybrids, and he instances the pheasant and black grouse. Now this is just what we ought to find on our theory, and ought not to find on any other. If either structural divergence or divergence in colour produces infertility, then the pheasant and the black grouse should be sterile, since they differ more, both in structure and colour, than many sterile pairs. But if species are produced sometimes by physiological isolation, but sometimes by other causes, such as geographical isolation, spontaneous distaste (not disability) for pairing, or even unaided natural selection, then those species which have been produced by aid of any of these latter processes will be fertile in spite of any ordinary amount of divergence, since nothing has occurred to render them otherwise; while those which have been formed by physiological isolation will be sterile even though they have hardly diverged at all. We cannot tell, without assuming what I am trying to prove, what form of isolation has been at work, except in the case of island species; but we can tell that there ought to be both very divergent fertile forms and slightly divergent non-fertile forms, and this is the case.

It has also been objected that the gradual increase of sterility, as we pass from different species to different genera and families, proves that divergence produces sterility. But it would exist on my theory; for if physiological isolation, more or less complete, occurs before each species is formed, it will have occurred at least twice between the members of two genera, and more often between those of two families. If B is separated from A by being nearly infertile, and C from B in the same way, C is likely to be still more infertile with A. But in some cases geographical or other isolation takes the place of physiological isolation, and then any number of successive divergences may occur without any accompanying infertility.

It has been said (I have lost the reference) that a certain amount of sterility has resulted in some cases from the divergence produced by artificial selection. It may be so. But on my theory, physiological isolation, the spontaneous occurrence of a fertility circumscribed by the boundary of common parentage, must be of very common occurrence, since it must have occurred not only once for each of most of the recognised species, but many more times when the resulting species has died out, and in some cases where the two species, though still existing, have not diverged in any way so as to suggest to observers that they are not one, (just as many island species do not differ perceptibly from those on the mainland). If spontaneous physiological isolation is so common, it would be certain to occur, at any rate in its commoner partial form, among the great variety of our domesticated animals, even if, as I believe, ordinary variation has no tendency to produce it.

EDMUND CATCHPOOL

Friends' Institute, E.C., October 13

Note upon the Habits of Testacella

BETWEEN four and five months ago I found eleven specimens of this slug upon a low wall surrounding the garden of a house