

archipelagoes are treated with equal thoroughness, and the work is provided with a map of the Indian Ocean, an index, and numerous well-executed woodcuts.

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.]

Physiological Selection and the Origin of Species

IN the *Journal* of the Linnean Society (Zoology, No. 115, 1886, p. 350, footnote) Mr. Romanes says: "I cannot find that any previous writer has alluded to the principle which it is the object of the present paper to enunciate, and which is explained in the succeeding paragraphs."

But in the fourth edition of the "Origin of Species" (1866), p. 311, the following passage occurs, in which the main idea of "physiological selection" is clearly alluded to.

"It may be admitted, on the principle above explained, that it would profit an incipient species if it were rendered in some slight degree sterile when crossed with its parent-form or with some other variety; for thus fewer bastardised and deteriorated offspring would be produced to commingle their blood with the newly-forming variety."

The author then goes on to show that, as he believed, this kind of sterility could not be increased by natural selection—a discussion with which I am not now concerned. I have other evidence to show that my father was familiar with the principle of physiological selection, and, moreover, that he did not regard it with any great favour.

In Mr. Belt's "Naturalist in Nicaragua" (1874), a suggestion is made, identical with that of Mr. Romanes in the *Linnean Journal*. Mr. Belt says (p. 207):—"The varieties that arise can seldom be separated from the parent form and from other varieties until they vary also in the elements of reproduction. . . . As long as varieties interbreed together and with the parent form, it does not seem possible that a new species could be formed by natural selection, excepting in cases of geographical isolation. All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty. Before a variety can become permanent, it must either be separated from the others or have acquired some disinclination or inability to interbreed with them. As long as they interbreed together, the possible divergence is kept within narrow limits, but whenever a variety is produced the individuals of which have a partiality for interbreeding, and some amount of sterility when crossed with another form, the tie that bound it to the central stock is loosened, and the foundation is laid for the formation of a new species. Further divergence would be unchecked, or only slightly checked, and the elements of reproduction having begun to vary, would probably continue to diverge from the parent form, for Darwin has shown that any organ in which a species has begun to vary is liable to further change in the same direction. Thus one of the best tests of the specific difference of two allied forms living together is their sterility when crossed, and nearly allied species separated by geographical barriers are more likely to interbreed than those inhabiting the same area."

In my copy of Belt's book the words "No, No," are pencilled in my father's handwriting on the margin, opposite the sentence "All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty."

Cambridge, August 27

FRANCIS DARWIN

NEITHER Mr. Galton nor Mr. Meldola have had time or opportunity to consult my original paper before writing their comments on the *NATURE* abstract. I will, therefore, consider

¹ A corresponding but not identical passage occurs in the sixth edition, p. 247.

those of their remarks which have been anticipated in the paper.

Mr. Galton writes:—"It has long seemed to me that the primary characteristic of a variety resides in the fact that the individuals who compose it do not, as a rule, *care to mate* with those who are outside their pale, but form through their own sexual inclinations a caste by themselves." Now, I have fully recognised this principle as one among several others which is accessory to, although independent of, physiological selection: see *L.S. paper*, p. 377, where also reference is given to the "Origin of Species," showing that this factor was likewise recognised by Mr. Darwin as one of importance in the prevention of intercrossing. But, inasmuch as this factor—which may be called psychological selection—can only apply to the case of the Vertebrata,¹ I am disposed to think that it is of much less general importance than the other factors which I have mentioned as accessory to physiological selection, and which, taken altogether, furnish a complete theoretical explanation of the fact that sterility between natural species is not invariably absolute, but occurs in all degrees. For, "in all these cases where the principles of physiological selection have been in any degree accidentally assisted by other conditions, a correspondingly less degree of variation in the reproductive system would have been needed to differentiate the species" (p. 377).

Thus far, therefore, Mr. Galton is really in full agreement with me. But he goes on to say:—"If a variety should arise in the way supposed by Mr. Romanes, merely because its members were more or less infertile with others sprung from the same stock, we should find numerous cases in which members of the variety consorted with outsiders." But how can we possibly know that such is not the case? If my theory is true, it must follow, as Mr. Galton says, that such unions would be more or less sterile, and, as this sterility is itself the only variation which my theory supposes to have arisen in the first instance, *ex hypothesi* we can have no means of observing whether or not the individuals which present this variation "consort with outsiders," or with those individuals which do not present it. Lastly, in as far as it is true that "we hardly ever observe pairings between animals of different varieties when living at large in the same or contiguous districts," the fact in no way makes against my theory of physiological selection: it only serves to supplement this theory, in the case of higher animals, by what I regard with Mr. Galton as the proved facts of psychological selection.

The letter by Mr. Meldola is a masterpiece of Darwinian thinking, and on this account I am glad to find myself much more in agreement with him than he appears to suppose. For when he reads my full paper he will see that I have taken precisely the same view upon natural selection as a possible cause—or, rather, accessory promoter—of specific sterility as that to the statement of which the larger part of his letter is devoted. I may remark, however, that of all parts of my paper I regard this as the most speculative and least secure. And this, first, because Mr. Darwin himself, after profound meditation upon the subject, came to the conclusion that natural selection could not operate so as to induce sterility; and, next, because the supposition that it does so operate involves one of the most difficult and complex questions in the whole philosophy of evolution—namely, whether it is possible for natural selection to modify an entire *type* without reference to benefit of its constituent *individuals*. Now, although for reasons which need not here be detailed, I have been led, like Mr. Meldola, to take a different view from that of Mr. Darwin, and to conclude that natural selection may benefit the type without reference to the individual, still I regard this conclusion as so highly speculative that I am glad to think the much more certain theory of physiological selection is not vitally affected either by its acceptance or its rejection. If it is true that natural selection may be able to modify an organic type (as my critic and myself agree in arguing, the type in this case being a variety) by conferring on it the benefit of sterility with its parent form, notwithstanding that this cannot be effected through benefit conferred on any of the constituent individuals, then all we have to say in the present connection is that natural selection is probably one of the many other causes which lead to physiological selection.

¹ This, at least, is what I state in the paper. Mr. Galton, however, suggests that the principle may be extended even to plants, through "the selective appetites of the insects which carry the pollen." This suggestion is unquestionably original, and bears the stamp of its author's ingenious mind. Moreover, considerable probability is, I think, lent to the suggestion by the observations of Mr. Bennett and others on individual insects selecting similarly coloured flowers on which to feed (see *Journ. L.S.*, 1883).