Co-adaptation.

THERE is one point in Prof. Meldola's review of Mr. Pascoe's book on the origin of species touching which it seems desirable that I should say a few words. The matter is introduced by the following passage:—

"Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—'On the converting "an ordinary hoofed quadruped" into a giraffe, Mr. Romanes observes: "Thousands and thousands of changes will be necessary." . . . "The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters." The chances of such changes are "infinity to one" against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident.' I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it."

The "dissected" sentences here referred to have been taken from an article on Mr. Wallace's "Darwinism," which I published in the *Contemporary Review* for August 1889. It is, perhaps, needless to say that the "pleonasm" does not occur in the original, and that I do not there hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, but likewise entertained such aid to natural selection as of "importance" in other cases where the phenomena of "co-adaptation" are concerned. Whether or not he underrated the power of natural selection in regard to such cases, it is in my opinion too early to dogmatize. But I am quite sure that "the well-known difficulty" in question cannot be met by the "Neo-Darwinians" with any appeal—explicitly or implicitly—to what is here the false analogy supplied by artificial selection. For example, suppose that there are n different parts which are required to vary, each in one particular way, but all to vary together in the same individual, if any of the variations is to confer an advantage in the struggle for existence. Suppose, further, that there is nothing but "chance" to lead to the simultaneous variation of all these parts in the same individual. Upon these data it is sufficiently evident that the happy combination would not occur with sufficient frequency to admit of being perpetuated in progeny—even if n be only equal to 4 or 5. Now I say that this "difficulty," be it great or small, cannot be met by what Mr. Wallace has called "the best answer"—namely, "the very thing said to be impossible by variation and natural selection has been again and again effected by artificial selection." For there is no "difficulty" at all in understanding how artificial selection is able to choose the separate congenital variations A, B, C, D, &c., as they severally occur in different individuals, and, by suitable mating, to blend

them all in a single individual. Here the "selection" is *intentional*; and therefore the whole ground on which the "difficulty" stands is absent. This ground is the supposition of *fortuity*, with regard (*a*) to *all* the variations A, B, C, D, &c., happening to occur in any one individual to begin with, or (*b*) being afterwards preserved (by suitable mating) from obliteration by free intercrossing. Therefore, thus to appeal explicitly from natural selection to the analogy of artificial selection is to be cheated by a metaphor.

How, then, does it fare if the appeal be made implicitly, as in Prof. Meldola's review, by supplying *utility* in the one case as corresponding to *intelligence* in the other? Obviously, here again, the element of *fortuity* is ignored, and therefore, as previously, the "difficulty" is not met, but evaded. For no one who believes in natural selection could deny, that if *each* of the variations, A, B, C, D, &c., is of advantage *per se*, they would all be preserved as they severally happened to arise in this, that, and the other individual, till, by general intercrossing, they would eventually coalesce in single individuals—as in the case of artificial selection. But all this is quite wide of the mark. Indeed, intercrossing is here a necessary condition to, instead of a fatal impediment against, the blending of co-operative modifications; and therefore Mr. Spencer would have been a fool had he brought his "difficulty" to bear upon this case. This case, however, is not that which is meant by "co-*adaptation*": it is the case of a confluence of *adaptations*. Or, otherwise stated, it is not the case where adaptation is *first initiated in spite of intercrossing*, by means of a fortuitous concurrence of variations, each in itself being without any adaptive value; it is the case where adaptation is *afterwards increased by means of intercrossing*, on account of the blending of variations each of which has always been of adaptive value in itself.

The "difficulty," therefore, remains just where it was before; and the only way of meeting it is to show that the phenomenon of co-adaptation does not occur in nature. In other words, it must be shown that the difficulty is fictitious, by showing that, as a matter of fact, there are no cases to be found where *n* modifications, each being useless in itself, become useful in association. Whether or not the difficulty does admit of this the only rational solution, I will not occupy space by discussing; but I have thought it desirable to state what I have always understood to be the real nature of Mr. Spencer's well-known objection."

Oxford, March 10. GEORGE J. ROMANES.