

I WROTE in good faith when in my last brief communication I expressed the intention of allowing the subject to drop, because I considered that the discussion had arrived at a stage when

those who were interested in the matter would be able to form their own opinion as to the value of the arguments adduced on either side of the question. I very much regret to find, however, that Dr. Romanes—whose amount of spare time appears to be most enviably inexhaustible—still finds it necessary to prolong the correspondence. I am compelled, therefore, to enter the field once more, if only for the purpose of presenting my own case in its true light. What Dr. Romanes's position may now be I must confess is becoming distinctly less clear with each of his contributions to the subject, but I am not the first who has lost his way in attempting to thread the mazes of this writer's productions. As far as I am concerned it will suffice to say that the case is not "simply" as he presents it in the foregoing communication. In the review of Mr. Pascoe's book, from which this discussion originated, I did not merely reproduce "Mr. Wallace's argument against Mr. Spencer's defence of 'use-inheritance.'" I accepted that argument as valid, but I extended it by emphasizing the importance of the factor of superimposed useful characters accumulated during successive periods of the phylogeny. I pointed out that large numbers of cases of co-adaptation might be thus accounted for, and I used Mr. Spencer's own illustration by way of example. In summing up his own conclusion, Mr. Wallace says: "The difficulty as to co-adaptation of parts by variation and natural selection appears to me, therefore, to be a wholly imaginary difficulty which has no place whatever in the operations of Nature" ("Darwinism," p. 418). Not only, therefore, has Dr. Romanes misrepresented my view, but he has gone further. The other "argument on the same side" referred to in the above communication is this very denial of co-adaptation as a fact in Nature. This, with most amazing sangfroid, is now claimed by my correspondent, who speaks of it as "the one which I had stated"! I must leave it to others to decide what value can be attached to the statements of a writer who adopts the principle of appropriating an argument, and putting it forward in a manner which would lead most readers to consider that he had been the first to elaborate it simply because he has expressed the same idea in abstract symbols instead of in concrete terms.

The next phase in the discussion is the admission by Dr. Romanes that Mr. Wallace's conclusion is correct, *i.e.* that co-adaptation is non-existent: "As it appears to me, from his reply, that Prof. Meldola's views on the subject of 'co-adaptation' are really the same as my own, I write once more in order to point out the identity" (NATURE, vol. xliii. p. 582). Mr. Romanes did more, therefore, than simply point out that we were agreed that this was "the only argument which could be properly brought against Mr. Spencer's position." He said that our views were "really the same," and this after I had accepted Mr. Wallace's conclusion as to the non-existence of co-adaptation. To crown all, he now tells us that he has no fully-formed opinion to express, but that he is in a condition of "suspended judgment"! I must really leave the case as it stands. If "neo-Darwinians" have a language of their own, at any rate it appears to be intelligible among themselves, if only from the circumstance that they have been enabled to stereotype a phrase which conveys their views with respect to the difficulty of following my correspondent's reasoning. I have been no more fortunate than other "neo-Darwinians" in this attempt, but in the endeavour to carry on the discussion of a biological question with a writer who stops short as soon as the subject assumes a truly biological aspect (see NATURE, vol. xliii. p. 582), I have become keenly impressed with the utter sterility of Dr. Romanes's method, which not only fails to advance our knowledge of the origin of species by any substantial contribution of fact, but which degrades the theoretical side of the subject into mere verbiage. If this is "palæo-Darwinism," I am rejoiced to think that I am grouped with those who are outside the pale.

In conclusion, to prevent further misunderstanding, let me add that, in admitting that the chances are "infinity to one" against a number of independent *useful* variations occurring when required in the same individual, I merely quoted the expression as given by Mr. Herbert Spencer and repeated by Dr. Romanes. I do not for a moment suppose that Mr. Spencer used the words in any more than a colloquial sense as indicating that there were "heavy odds" against such a combination, and in this sense only is my admission made. That the phrase has no exact mathematical significance is, I imagine, sufficiently obvious, but I have thought it desirable to make this qualification.

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