

Transcription, June 2015:

Nature 43(1102) (11 Dec. 1890): 127-128.

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‘Mr. Wallace on Physiological Selection.’

I will endeavour, as briefly as possible, to justify what I have said elsewhere touching “the peculiar position to which Mr. Wallace has eventually gravitated with reference to my views.” For a fuller statement I must refer to the *Monist*, vol. i. pp. 1-20.

It is quite true, as he says, that “in his original paper, and in the summary of it published in *Nature*, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection.” And this is exactly what Mr. Wallace himself has done in his “alternative theory.” Taking it for granted that these variations must always occur—as in my “original paper” I said they probably “most frequently” occur—in a whole race or strain, his theory seeks to explain, (1) the *causes* of such variations, and (2) their *effects* in furnishing an important condition to the origination of specific types, and this exactly in the manner that the theory of physiological selection had previously exhibited. Space forbids any long quotations, and therefore on the present occasion I will confine myself to transcribing two sentences from the paragraph in which he very correctly summarizes his theory, viz. the sentences which deal with the *effects* of these “variations in regard to fertility and sterility.”

“The preceding argument, it will be seen, depends entirely upon the assumption that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species...It is by no means necessary that all varieties should exhibit incipient infertility, but only some varieties; for we know that, of the innumerable varieties that occur, but few become developed into distinct species; and it may be that the absence of infertility, to obviate the effects of intercrossing, is one of the usual causes of their failure.”

Here we have the whole essence of “physiological selection” in a nutshell. The first sentence conveys the “fundamental fact;” the second indicates its possibly important consequence in permitting the origination of species by preventing the effects of intercrossing. Why, then, does Mr. Wallace not perceive that this is the whole essence of physiological selection? As far as I can understand, the reason appears to be that he deems his variations in the direction of cross-infertility to differ from mine, in that while his may be associated with other conditions or causes of modification, mine, as he now repeats in *Nature*, are supposed always to act “alone in an otherwise undifferentiated species.” Now, not only did I expressly guard against this interpretation in my “original paper,” but in all my answers to Mr. Wallace’s criticisms which have since appeared I have over and over again corrected the mis-statement on his part; and I am the more surprised that he should again reproduce it as the sole basis of his present reply, inasmuch as some of the passages which he quotes for this purpose from the paper in question themselves furnish the needful correction in their own immediate context.¹ Moreover, not only have I thus from the first fully recognized the sundry other causes of specific change with which the physiological variations may be associated; but Mr. Gulick has gone into this side of our common theory much more fully, and elaborately calculated out the high ratio in which the differentiating agency of any of these other causes must be increased when assisted by—*i.e.* associated with—even a moderate degree of the selective fertility, and *vice versa*. Therefore, it is simply impossible for Mr. Wallace to show that “our theory” differs from his in this respect. Yet it is the only respect in which his reply alleges any difference.

I do indeed myself believe that in many cases the physiological variation may arise alone, in an otherwise undifferentiated species; but from the first I have always maintained that it makes no essential difference to the theory in what proportional number of cases it has done so. And in a forthcoming treatise I shall be able very completely to dispose of Mr. Wallace's "two carefully considered cases," whereby he claims to have proved that the possibility of physiological selection ever working alone is "absolutely unfounded."² At present, however, the point is that, even if I am wrong in supposing that physiological selection can ever act alone, the principle of physiological selection, as I have stated it, is not thereby affected. And this principle is, as Mr. Wallace has re-stated it, "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species"—infertility, whose absence, "to obviate the effects of intercrossing, may be one of the

¹For instance, to take only the first of his "few quotations," he reproduces by itself the following sentence:—"It becomes almost impossible to doubt that the primary specific distinction [meaning sterility] is, as a general rule, the primordial distinction." Now this, be it remembered, is quoted for the expressed purpose of "making it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, *was founded on the supposition of sterility alone, arising in an otherwise undifferentiated species.*" Yet, if Mr. Wallace had but read the context, he would have seen that this statement is directly contradicted. For the very next sentence is as follows:—"I say as a general rule, because the next point which I wish to present is, that it constitutes no part of my argument to deny that in some—possibly in many—cases *the primary distinction may have been superinduced by the secondary distinctions.*" By the "primary distinction" Mr. Wallace correctly understands me to mean cross-infertility between allied species, while by "secondary distinctions" it is explained in the same place that I mean specific characters of all other kinds. The passage then goes on, through a number of pages, to show "how natural selection, or any other cause [which is concerned in the differentiation of species], may have induced this particular kind of variation in the reproductive system by its operations on other parts of the organism," and the ultimate conclusion of this lengthy argument is: "Thus, we see, it really makes no essential difference to my theory whether it be supposed, in any given case, that the primary distinction was prior, or subsequent to the secondary distinctions." Comment appears needless.

²These cases consist in some exceedingly simple arithmetical computations, which, as I shall hereafter show, rest upon erroneous data. Mr. Fletcher Moulton, who has kindly gone into the matter in a really efficient manner from the mathematical point of view, reports, to use his own words, "an enormous difference from Mr. Wallace's results."

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usual causes of their failure to become developed into distinct species."

This, I repeat, is the essence of physiological selection; and any "originality" which my views upon the subject present consists in recognizing the "fundamental fact" set forth in the first of the two sentences above quoted, together with its consequence as set forth in the second. Before Mr. Catchpool published the theory in these columns, no one—with the partial exception of Mr. Belt—had perceived this factor of organic evolution; and while, for about the sixth time, repudiating the grotesque "originality" which Mr. Wallace continues to ascribe, I may conclude by observing that—personalities apart—it is to me a matter of satisfaction that he has now himself begun to perceive the existence and the importance of the factor in question.¹

George J. Romanes.

Oxford, December 1.

¹ It is, perhaps, desirable to add, as already stated elsewhere, that I entertain no doubt at all touching the unconscious or unintentional nature of the “adoption.” Nevertheless, I may further add, the adoption itself is so manifest, that several eminent men of science wrote to me on the subject when first his work on “Darwinism” appeared. Among the mildest of their comments are:—“Mr. Wallace has treated you very badly. After having set up a caricature of your theory, he adopts the theory itself, pure and simple.” But of more importance is Mr. Gulick’s opinion, seeing that he was the first to conceive, though the last to publish, the theory of physiological selection. As soon as he had read “Darwinism,” he wrote me from Japan a long letter, the substance of which may be gathered from the first two sentences, as follows:—“Mr. Wallace has most certainly adopted the fundamental principles of our theory. He takes our principles, which in the previous chapter he has combated: but he makes such disjointed use of them that I am not willing to recognize his statement as an intelligible exposition of our theory.” More recently he sent to the *American Journal of Science* a paper, which he summarizes thus:—“Mr. Wallace’s criticism of the theory of physiological selection is unsatisfactory: (1) because he accepts the fundamental principles of that theory on pp. 173-79, in that he maintains that without the cross-infertility the incipient species there considered would be swamped; (2) because he assumes that physiological selection pertains simply to the infertility of first crosses, and has nothing to do with the infertility of mongrels or hybrids; (3) because he assumes that infertility between first crosses is of rare occurrence between species of the same genus, ignoring the fact that, in many species of plants, the pollen of the species is prepotent on the stigma of the same species when it has to compete with the pollen of other species of the same genus; (4) because he not only ignores Mr. Romanes’ statement that cross-infertility often affects ‘a whole race or strain,’ but gratuitously assumes that the theory of physiological selection excludes this ‘racial incompatibility’ (which Mr. Romanes maintains is the ‘more probable form’), and bases his computation on the assumption that the cross-infertility cannot be associated with any other form of segregation.

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The Alfred Russel Wallace Page, Charles H. Smith, 2015.