

## DARWINISM *VERSUS* WALLACEISM.

**D**R. ALFRED RUSSEL WALLACE contributed a most interesting article to the August number of this REVIEW, the concluding lines of which invite us to form [our] conclusions as to whether Darwinism is or is not "an unsuccessful hypothesis."

I for one am firmly convinced that not only is Darwinism a successful hypothesis, but that it is the hypothesis which is influencing modern thought and modern society to an extent which even its most devoted adepts fail to realise. The old creeds are being shaken to their foundations, and that venerable and mighty body which has already weathered so many centuries, the Church of Rome, is wisely intent upon widening its bases so that the perturbations caused by Darwinian views may be rendered innocuous when once Darwinism will no longer be anathematised, but will be placed side by side with Galileo's teachings and Copernicus' system, which were originally so fiercely condemned, but are now accepted by the "Holy Office."\*

As Dr. Wallace (on p. 133) calls me to account as one who pretends that "new species are produced *per saltum*, and not by the slower "process of variation and selection as maintained by Darwin," I may be allowed to correct this mis-statement, and at the same time to point out the difference which we continental naturalists have long since been making between that "variety" of Darwinism which may be termed "Wallaceism" and the real foundation of Darwinism, which is the Selection Theory as it was formulated fifty years ago by Darwin and Wallace simultaneously.

Paradoxical as it may sound, I am willing to show that my colleague, Hugo de Vries, of Amsterdam, who a few years ago grafted his *Mutations-theorie* on the very thriving and very healthy plant of Darwinism, is a much more staunch Darwinian than either Dr. Wallace

\* That such is the case is evident from the teaching of an eminent zoologist, Father Wasmann, S.J., whose conclusions, drawn from very extensive studies upon diverse species of ants, have led him to the conviction that species originate by a gradual process of development out of other species. Wasmann's papers have not been placed on the Index, and this learned Jesuit is undoubtedly the pilot who is sagaciously and convincingly conducting the idea of evolution to a position inside the Roman Catholic interpretation of nature. Evolution will no longer be challenged by the highest authorities of that Church, who are undoubtedly aware of Father Wasmann's pioneer work.

himself or the two great authorities in biological science whom he mentions, Sir William Thiselton Dyer and Prof. Poulton.

For those who have really read, perused and digested de Vries' wonderful book, as also his earlier "Intracelluläre Pangenesis," there can be no doubt of this; and now that the semi-centennial celebration of the publication of the "Origin of Species" is approaching, it should, in justice, be loudly proclaimed that de Vries, an early Darwinian of the purest water, is closer to the original fountain from which Darwinism has sprung than is even Darwin's associate, Dr. Wallace, who has drifted far away in his personal interpretations of variation.

Variations are the material on which Natural Selection acts, the outcome of this action being the consecutive appearance of new and modified and beautifully adapted species. It is consequently of the utmost importance to define closely what is meant by the word variation. For Darwin this was an object of the most careful study, and he knew from the very beginning that under this name very different phenomena were understood, the crucial difference being, to a great extent, obliterated by the fact that the same name was applied to diverse phenomena. Variations for Darwin were, in the first place, the spontaneous or single variations, sometimes (when very catching to the eye) called sports. But, in the second place, he repeatedly calls our attention to the fact that "under the term of "variations mere individual differences are included,"\* and this, the so-called individual or fluctuating variability, is always with us, and is perpetually demonstrable throughout the whole animal and vegetable kingdom.

De Vries' great and all-important contribution to the Darwinian theory has been a series of the most painstaking investigations into the nature of these different processes of variation. He has not confined himself to reasoning about them: he has experimented on a very extensive scale. As regards fluctuating (or individual) variations, he found that they obey the laws which Quetelet laid down when working out anthropological problems on a statistical basis, following whose footsteps Galton established the curves to which his name has become attached.

All this was unknown, or imperfectly known, to Darwin, and everywhere in his works—especially when comparing the earlier with the later editions—we find that for many long years he struggled to find out which of the two, the fluctuating or the chance variations produced the actual material with which selection dealt. It is only natural that at the end he should have felt inclined to decide for the first-named: (1) because we find these fluctuating variations everywhere present in nature; (2) because the reluctance which Wallace and many others showed in recognising the significance of sports for the formation of new species was so particularly strong

\* "Origin of Species," 6th Edition, p. 64, p. 80, and others.

that this had a natural counter-effect on Darwin's own mind, and perhaps made him over-rate the strength of their objections.

An article by Fleeming Jenkin, which appeared in the *North British Review* for June, 1867, seems to have had a particularly strong effect in shaping Darwin's mind with respect to this most important question. Darwin writes to Wallace (Jan. 22, 1869; *Life and Letters*, iii., p. 107): "I have always thought individual differences more important than single variations, but now I have come to the conclusion that they are of paramount importance. . . . Fleeming Jenkin's arguments have convinced me." And in a later letter to Wallace: "I . . . thought that single variations might be preserved much oftener than I now see is possible or probable. . . . I believe I was mainly deceived by single variations offering such simple illustrations as when man selects."

After the writing of that letter it was all plain sailing for what I have above designated by the name of Wallaceism. It coincided with the preparation for the press of the fifth edition of the "Origin of Species," and this and the sixth have been the editions from which the majority of biologists have drawn their views concerning the theory of Natural Selection. The younger men have not themselves passed through that phase of doubt and wavering which we of the older generation, who worked with the earlier editions of the Origin, have all the time shared with its illustrious author, when we felt inclined, on his example, to leave a due share to single variations in the production of new species. An investigator of the eminence of Wallace has found no difficulty in convincing his readers that the time has come to stop that wavering; that single variations should be dropped overboard, and that henceforth Natural Selection should be looked upon as limited in its action to those individual variations that are so marked and so numerous and that have been of so eminent importance to breeders and to agriculturists whenever they have attempted to improve their crops and their breeds.

This may explain how "shipshape" Wallaceism has gradually come to substitute itself for the originally "undecided and expectant" Darwinism, however much the latter attitude was scientifically better justified. It should here be noted that in that wonderful piece of work which Darwin himself cherished as a favourite achievement (*Life and Letters*, Vol. III.), the theory of Pangenesis, he has actually attempted to trace back variability in its different aspects to the "gemmules" as they are present in the generative substance of each species ("Animals and Plants under Domestication," 2 ed., 1875, II., p. 390). He says:

"Variability depends on at least two distinct groups of causes. Firstly, the deficiency, superabundance and transposition of gemmules and the redevelopment of those which have long been dormant, the gemmules themselves not having undergone any modification: and such change will amply account for much fluctuating variability."

As to the variability which gives rise to the single or chance variations, he writes :

“In this case the gemmules from the modified units will be themselves modified and, when sufficiently multiplied, will supplant the old gemmules and be developed into new structures.”

De Vries has called attention (*Mutations-theorie*, I., p. 27) to these remarkable and important expressions of opinion. He and Weismann—another staunch Continental Darwinian, who, however, should be classed in the Wallacean school—have independently expressed their very high admiration for the Pangenesis hypothesis, and have borne testimony to the wonderful fertilising effect it has exercised on other minds; whereas in other quarters this very portion of Darwin's work has been misunderstood and judged of lightly and superficially.

The phrases here cited will have shown that the theoretical difference between fluctuating and single variations was, in Darwin's own mind, of the most fundamental importance. In the single variations *something new was added*, and we can well understand that originally he felt inclined to look in this direction when musing about the origin of new species out of old and existing ones.

It may be said that de Vries' numerous and extended experiments, first on the scope and the potentiality of fluctuating variations, and secondly, on that other phenomenon of which he was the first to understand the full value, and which he called mutability, have been a considerable stride in advance of what is in reality the Darwinian theory.

In his article above quoted, Wallace entirely missed the mark when he assumed that de Vries claims his Mutation-theory to be (l.c., p. 130) a substitute for Darwin's explanation of organic evolution by means of natural selection. De Vries might, with the same right, accuse Wallace of having deviated from the original teaching. And even if Darwin himself in his later years—partly, perhaps, owing to his extreme modesty—allowed himself to side with Wallace and others to an increasing extent, this does not diminish the right of modern biologists to stick to Darwin's earlier views, claiming at the same time that these views should stand as parts of the Darwinian theory quite as much as the later changes, and that a theory which offers an experimental further development of the idea of species formation by mutations is just as much a healthy outgrowth of true Darwinism as is the narrower limitation which derives new species only from the fluctuating variability.

However repeatedly and however depreciatingly Wallace may say (l.c., p. 133), that de Vries' mutations (of *Oenothera Lamarckiana*) are “phenomena presented by *one* species of plant;” the fact *that these phenomena have actually been observed* is more than can be said of any single species that is said to have developed by slow individual

(fluctuating) selection, a development which has *never* been observed, but only presumed, surmised, and accepted without the confirmation of actual experiment.

Wherever our agriculturist succeeds by the most careful artificial selection in producing (*e.g.*) a beetroot of which the percentage of sugar has been raised, say, to 15 per cent. out of roots which originally stood at 7 to 8 per cent., he knows that the fluctuating variation of the beetroot has permitted him to attain this end; but he knows, at the same time, that what he has obtained is not a new species of beetroot, richer in sugar, but a product of nature which the moment it is left to itself and freed from the bonds of artificial selection goes back to an inferior sugar-producing root again.

And this is the insurmountable difference between the *mutants* which de Vries has seen originating out of *Oenothera Lamarckiana* and any favourable variety which man has reared by taking fluctuating variation as his basis of operations. The *mutants* are henceforth a fixed type, the "keimplasma" has become something different from that of the mutating species, "the gemmules have "developed into new structures," in the wording of Darwin's pangenesis.

Now in many cases both agriculturists and horticulturists have raised important varieties which are constant and which do not fall back to the parent form. But with those new "varieties" it can be proved in most cases—and in many other cases the proof is gradually being unearthed—that they started not from fluctuating but from "single" variations, for which "mutations" is only another name. The enormous confusion caused by mixing under one head the two processes here alluded to can be easily understood. Some may object to apply the name "new species" to the creations just referred to, many of which have meant a fortune to some horticulturists; but here again we are confronted by the formidable question: What *is* a species?

And on that question de Vries' book has undoubtedly thrown new light. Going back to Linnæus, he finds that this celebrated legislator had to be satisfied with creating *Sammelarten*, collective species. Much later we have become aware that the 200 species of *Draba verna*, only recognisable by trained specialists, have full right to be looked upon as true species, because of the perfect fixity of their characters. And we surmise that these 200 are so many mutants which have arisen not so very long ago, and have found occasion to thrive simultaneously; whereas in other cases of mutation natural selection very soon kills off most, if not all, of the mutants in the fierce struggle for existence, which is not waged between individuals but between species and their altered offspring, the fixed mutations. Here, too, we notice that it is not natural selection which is denied by de Vries. He believes fully in the high significance

of natural selection, only he does not accept its significance for species formation out of fluctuating variations; he does not admit the selection by nature of the individual variations.

It is the different mutations (which, when they appear, are admittedly present in all directions, and thus not at all of directive significance from the first) that do battle with each other for obtaining food and light, and that in this battle make use of those new properties (sometimes hardly perceptible and never a real *saltus*) which the as yet unexplained change in their idioplasm has called into existence.

It is to some extent idle to say so, but a repeated perusal of the *Life and Letters* has called forth in me the very strong feeling that if de Vries' German book in its two volumes had appeared, say, in 1868, Darwin himself would have welcomed it with that particular warmth which he so often expressed when he found that somebody had succeeded in understanding fully the trend of his own magnificent pioneer work. And that, recognising by his personal experience, the immense value of an important series of accurate experiments in this most intricate field of the evolution of life on the globe, he would have examined de Vries' results with infinite patience, and would have found that they were of ever so much higher importance than the reflections and calculations of Mr. Fleeming Jenkin, the gifted engineer mentioned above.

Now that we cannot have Charles Darwin's personal utterance upon the matter, and now that Wallace, the other godfather of Darwinism has taken a different line of thought and anathematises the mutation theory, we find ourselves under the obligation—unless we want to follow the far easier method of "going by authority"—to weigh the evidence concerning these all-important problems with all due care. I cannot put enough emphasis into my advice to commence this by reading most closely and critically the first volume of the German edition (Leipzig, 1901) of de Vries' *Mutations-theorie*. I have good reason to believe that neither Dr. Wallace nor many leading biologists in England have done so. And I feel sure that if, after having done so, they do not feel inclined to accept de Vries' conclusions, none of them will, at all events, side with a large number of silly antagonists of Darwinism and of Evolution, who have thought fit to proclaim with a loudness that is in an inverse ratio to the square of their accuracy that Darwinism has been played out since the appearance of de Vries' *Mutations-theorie*.

On the contrary, they will by that time recognise that de Vries, who has always honestly believed that he was working hard in the straight path which Darwin has traced for all of us, is indeed among living naturalists the one who has by his "experimental" work come closest to the inimitable example set by his immortal predecessor.