

*DARWINISM.*

*Darwinism.* By Alfred Russel Wallace, LL.D., F.R.S.  
(London: Macmillan and Co., 1889.)

THE object of Mr. Wallace in writing the admirable work which he has published with the title of "Darwinism" has been "to give such an account of the theory of natural selection as may enable any intelligent reader to obtain a clear conception of Darwin's work, and to understand something of the power and range of his great principle." No one has so strong a claim as Mr. Wallace to be heard as an exponent of the theory of the origin of species, of which he is—with Darwin—the joint author. He has produced a thoroughly readable book, condensing into an octavo volume much of the speculation and description of important facts which are contained in the numerous volumes published by Darwin himself, and in the essays and occasional contributions of subsequent writers. Besides this, Mr. Wallace's book contains an exposition of highly important and interesting views of his own on subsidiary matters, which have either not been published previously or have appeared in a scattered and more or less inaccessible form. Consequently, the book is one which has interest not only for the general reader, to whom it is primarily addressed, but also for the more special student of natural history. The latter will find in its pages an abundance of new facts and arguments which, whether they prove convincing or not, are of extreme value and full of interest. If we attempt here to point out some of the shortcomings of Mr. Wallace's treatise, it is not from any desire to minimize its value and interest, but rather an acknowledgment of the weight and significance of a work on so important a subject by so specially competent an author.

Mr. Wallace's book necessarily suffers, in comparison with the works of Darwin himself, by the limitation of space. It is in consequence of this compression that we miss in the new statement by Mr. Wallace that extraordinary cogency or power of convincing which so distinguished the writings of Darwin. With Darwin one becomes accustomed to see no speculation put forward, no step of an argument advanced, unless there is an overwhelming weight of testimony in its favour: facts are cited in astonishing abundance, and at the same time the conviction establishes itself that the author has reserves of fact as rich as those of which he makes use, and further that he is so scrupulous and so modest that he will never ask his reader to accept a conclusion, however trivial, without stating fairly the amount of evidence for and against such conclusion. Mr. Wallace is prevented by the scope of his work from such treatment of his subject. As a result, his conclusions often appear to be (when they may not be so) based on very insufficient evidence, and his statement meagre. "Darwinism" can never take the place of the "Origin of Species," but may

well serve as an introduction to the study of that and the other works of Darwin—the value of which, not only as storehouses of fact and suggestion, but as classical models of scientific discussion, cannot be over-estimated, and will probably never be surpassed.

In his preface, Mr. Wallace, through a misconception which is perhaps explained by the retired life which he enjoys—makes an attack upon what he calls “the modern school of laboratory naturalists.” He states that these persons seek to minimize the agency of natural selection and to subordinate it to laws of variation, of use and disuse, of intelligence and heredity. He commends, as leading to truer views, the study of the external and vital relations of species to species in a state of nature—a study which Semper has called “the physiology of organisms,” and I have proposed in the article “Zoology” in the “Encyclopædia Britannica” to call “bionomics.” Now though there is no doubt an increasing number of younger students who have little or no interest in natural history beyond what is derived from the contemplation of ribbons of sections dyed like Joseph’s coat, yet it is going too far to say that they have in any sense formed a school. And further, if we endeavour to estimate the influence on naturalists of a considerable devotion of time to the study in the laboratory of histology and embryology, physiology and morphology, we shall be led to the conclusion that this study has been associated with exactly opposite results from those attributed to it by Mr. Wallace. Who are they who seek to minimize natural selection and to set up the false gods of variation, use and disuse, &c.? Certainly not laboratory men. Is the Duke of Argyll a laboratory naturalist? Is Dr. George Romanes? Is Prof. Cope? Are Mr. Herbert Spencer and Prof. Patrick Geddes? I venture to say they are not; yet they are the authors with whom Mr. Wallace has subsequently to contend when he maintains that the selection of congenital variations by natural selection is an adequate theory of the origin of species, and requires no aid from Lamarckism, Copism, or other interlopers. Who are they who agree with Mr. Wallace in this contention? Precisely “laboratory men,” who are, however, by no means *only* laboratory men, but, like Darwin himself, search for their material in the garden, the field, the seashore, or the sea-bottom; and as a part—but only a part—of their study of it eventually bring it to the laboratory. Such a “laboratory naturalist” is Weismann, whose essays and memoirs in favour of the identical view maintained by Mr. Wallace, appear to have escaped his attention until very recently. I presume also that I may claim to be a laboratory naturalist; and yet four years ago I found it necessary, in lectures delivered at the London Institution, to discard even that tincture of Lamarckism which Darwin had admitted, and to advocate “pure Darwinism,” on the ground that the Lamarckian hypothesis is still devoid of experimental basis, and in view of the logical principal *Entia non sunt multiplicanda præter necessitatem*. It is true, as I have elsewhere insisted, that there are not at present such facilities for the study of bionomics as are provided in our laboratories for the study of histology, embryology, morphography, and the physics and chemistry of living bodies. But it is not right to identify the class of speculations, to which Mr. Wallace is opposed, with laboratory training. This,

indeed, in virtue of its tending to bring speculation to the test of fact, is favourable, and often directly conducive, to the study of “the external and vital relations of species to species in a state of nature,” or in one word “bionomics.” I will only cite as instances Bateson’s researches in Tartary, Caldwell’s in Australia, Poulton’s experiments on insects, and Moseley’s “Notes of a Naturalist on the *Challenger*.”

Mr. Wallace’s plan of treatment of his subject is an excellent one. After a brief statement of what naturalists have understood by the word “species,” and a lucid exposition of the views of the earlier transmutationists, he enunciates Darwin’s theory. He then proceeds to show, by citing a wide and comprehensive array of facts, that the foundations of the theory are secure. In one chapter he describes the rapid multiplication of organisms and the consequent struggle for existence; in further chapters the fact of variability is shown, by an appeal to instances, to be one of the widest and most general character; in another chapter the facts of heredity and selection are brought forward. Then follow discussions of “difficulties and objections,” hybridity, the origin and use of colour in animals and in plants, geographical distribution, the geological evidences of evolution, the fundamental problems of variation and heredity, and, lastly, Darwinism applied to man.

The chapter on “Variability of Species in a State of Nature” is one on which considerable pains has been expended. It presents some of the facts of variation in a very striking manner, and provides us with a number of well-studied instances which have not before been accessible to naturalists. A method followed by Mr. Wallace is to take any large collection of a single species and to measure various parts, such as length of head, tail, limb, &c. As he observes, it is very important to convince ourselves that variation does occur in a state of nature, so that natural selection has the material to act upon. He considers that the instances which he brings forward show that the range of variation is larger and more general in a state of nature than is usually assumed, and that “it is clear that Mr. Darwin himself did not fully recognize the enormous amount of variability that actually exists.” Whilst admitting the interest of Mr. Wallace’s present contribution to this subject, I think it is clear that he has failed to make a distinction which is desirable and important, viz. that between *variations* exhibited by adult specimens and the *variability* presented by the young of any given species. After all, the specimens of lizards and birds, of which the measurements are given to us by Mr. Wallace, only comprise such individuals as were *not too widely divergent* from the parent form to survive to maturity under conditions which select more or less closely a given specific set of characters. What one would like to know is the actual range of variability as shown by the artificial rearing of *all* the offspring of a single pair. With plants such a study of variation is practicable, but less so with animals. Variation includes those extreme cases which are called “monstrosities,” and it is by no means certain that natural selection would *always* exclude these extreme cases from survival. The facts of variation under domestication are more to the point, in so far as the range of congenital variability is concerned, since in regard to a limited number of animals and plants we have

removed the primary sifting of young forms. This sifting must occur under natural conditions, so as to allow only a limited range of variations to reach the collector in his museum. Clearly enough, this primary sifting, and all later operations of the same kind due to natural conditions, may under new circumstances be vastly modified in their nature, and variations may be allowed to pass the sieve which at another time are excluded. The range of variation, therefore, in even a very large museum series of a wild species, can afford but an inadequate notion of the variability of animals. We may, however, justly conclude that, if the former is so large as Mr. Wallace shows it to be, the congenital variations which occur, but never in given conditions reach maturity, must comprise instances which are very much more marked, and would furnish abundant material for natural selection were the natural conditions of the species to change. An attempt to determine by experimental rearing, the range of congenital variation (that is, of *possible* adult variation) in such animals and plants as are fitted for the inquiry, seems to be well worth making.

Mr. Wallace, who must have watched the early criticism of Darwin's theory with special keenness, makes a good point when he insists that the objection that it is difficult "to imagine a reason why variations tending in an infinitesimal degree in any special direction should be preserved" is a quibble. Darwin never used the word "infinitesimal," but spoke of variations being "slight" or of "small amount," and we agree with Mr. Wallace that even those terms are open to the objection that they may seem to imply that congenital variation is of less range and frequency than it really is.

Naturally enough, Mr. Wallace is not equally thorough in his treatment of each of the various "difficulties and objections" which he discusses, but the chapter thus headed gives an interesting summary of the present state of opinion. Among the matters discussed are the supposed smallness of variations, the doubt as to the right variations occurring when required, the beginnings of important organs, useless or non-adaptive characters, the instability of non-adaptive characters, the swamping effects of intercrossing, and the effects of isolation. In some of these instances Mr. Wallace's reasoning is very clear and forcible; in other cases it is much less so. Mr. Cunningham has already pointed out, in a letter to NATURE (July 25, p. 297) a curious slip on Mr. Wallace's part in his explanation of the gradual development of the twisted condition of the head and eyes of flat-fish. Mr. Wallace declines to admit the transmission of acquired characters as a cause of variation and progressive development; yet, apparently without being conscious of it, he attributes the movement of the eye of flat-fish from one side of the head to the other, to the transmission of a series of slight shiftings of the eye acquired in successive generations by the muscular effort of the ancestors of our present flat-fish, which is (to use an expression already known to the readers of NATURE) "flat Lamarckism." In relation to this, I may mention that the asymmetry of the Gastropod Mollusca, the forward position of the anus, and the twisted condition of the nerve-loop in the Streptoneurous division of that class, had been similarly attributed by myself to the cumulative effect of a mechanical cause—the one-sided lopping of the shell—

operating in successive generations. Like Mr. Wallace, I had failed to notice that the explanation adopted was an admission of Lamarckism. It seems to me possible to explain the position of the flat-fish's eye by the selection of congenital variations, since there is no doubt of the advantage to the animal of having its two eyes on the one side of the body. But I confess that the Gastropods at present have not been satisfactorily explained. I have not been able at present (and I say at present advisedly) to find any evidence of advantage to the Gastropod in the torsion of its visceral hump, such as would justify the supposition that a monstrosity presenting this condition in full development was favoured by natural selection; still less does it appear how the steps of a gradual torsion—that is, a series of approximations to complete torsion—could be advantageous. It does not follow that we must admit Lamarckism; but merely that we must further examine Gastropod habits, structure, and development with this problem in mind.

Mr. Wallace does not, in my judgment, give sufficient grounds for rejecting the proposition which he indicates as the main point of Mr. Gulick's valuable essay on "Divergent Evolution through Cumulative Segregation." By the bye, Mr. Gulick is one of the heretics who attribute some part in the production of species to other causes than natural selection, yet he is not a laboratory naturalist, but one who, substituting land-shells for butterflies, has precisely the same foundations and training as Mr. Wallace himself. Mr. Gulick's idea is that there is an inherent tendency to variation in certain divergent lines, and that when one portion of a species is isolated, even though under identical conditions, that tendency sets up a divergence, which carries that portion further and further away from the original species; or, in other words, no two portions of a species possess exactly the same average character, and the initial differences will, if the individuals of the two groups are kept from intercrossing, assert themselves continuously by heredity in such a way as to insure an increasing divergence of the forms belonging to the two groups, amounting to what is recognized as specific distinction. Mr. Gulick's idea is simply the recognition of a permanence or persistency in heredity, which, *ceteris paribus*, gives a twist or direction to the variations of the descendants of one individual as compared with the descendants of another. Ireland is cited by Mr. Wallace as an evidence that isolation has not been effective in modifying specific character of plants and animals. If, however, unlike Mr. Wallace, we may look upon mankind as subject to the same developmental causes, and only to the same causes, as animals, then Ireland would seem to be a very interesting case of the production of divergent character by isolation. All parties are agreed that, whatever value is to be assigned to the fact, the human inhabitants of Ireland, whether of Celtic or Teutonic ancestry, exhibit characters which are "divergent" from those of the inhabitants of Great Britain, and, without going into details, we may say that the isolation and persistence of an original tendency seem to be the only explanation of the divergence.

The subject of "correlated variations" is but lightly touched on by Mr. Wallace, and its immense importance in relation to the whole question of "useless organs" and useless characters of growth and structure is not sufficiently

put forward, as it was by Mr. Darwin. It is true that we know little about the physical basis of correlated variation, and are therefore open to hostile criticism when we take refuge in an appeal to it as an explanation of phenomena. The truth is that correlated variation is as important a property of living matter as heredity and variability themselves. It may be formulated thus: "Every departure from the parental form of any given part of an animal or plant is accompanied by a definitely correlated and often a commensurate departure in other parts remote from it." The possibilities thus introduced are simply gigantic—a new factor is brought in which extends the results of simple variation and selection indefinitely. In the future the laws and limitations of correlated variation will no doubt be determined. At present our knowledge of them rests where Mr. Darwin himself placed it. Both Mr. Gulick's doctrine of persistent hereditary tendency, and that of the immense capacities of correlation in variation, commend themselves to the mind of a laboratory naturalist who is accustomed to conceive of vital phenomena as mechanico-physical affections of a living substance, viz. protoplasm. They are, on the other hand, less valued—perhaps insufficiently—by Mr. Wallace.

In his chapter on the infertility of crosses, Mr. Wallace treats at length and with admirable effect a very important subject, as to which he is full of ingenious novel suggestions and apposite facts. His criticism of Mr. Romanes's essay, entitled "Physiological Selection," appears to me to be entirely destructive of what was novel in that laborious attack upon Darwin's theory of the origin of species.

The chapter on the origin and uses of colour in animals is that which will be most interesting to the general reader, and is indeed a charming essay, illustrated by numerous woodcuts. Here Mr. Wallace sets forth at length his convincing argument as to the use of colour as a means of recognition among animals, giving many examples—amongst others, that of the white patch on the rabbit's tail. In conjunction with his theory of the importance of the principle "like to like" in the segregation of varieties and the consequent development of new species, great significance must be attached both to the nervous organization, which makes recognition possible, and to the markings or other characters which are recognized. A very interesting discussion of Mr. Darwin's theory of sexual selection occurs in a subsequent chapter. Mr. Wallace, whilst admitting some of the effects of sexual selection recognized by Darwin, is not able to follow him in attributing to it the brilliant colours of birds and butterflies. Mr. Wallace attributes the deeper or more intense colouring of the male, which often occurs, to his "greater vigour and excitability." The female in many groups retains the primitive and more sober colours of the group for purposes of protection. The occurrence of colour itself in patches and lines is attributed by Mr. Wallace (following the late Mr. Alfred Tylor) to the distribution of subjacent nerves and blood-vessels, which follow, like the colour-patches, in the main, certain lines determined by the general structure. Mr. Wallace seems scarcely to have succeeded in showing that Darwin's theory of sexual selection is inapplicable to the explanation of special developments of colour and ornament, although he has suggested additional causes which

influence the primary distribution and development of colour.

We have not space to speak of subsequent chapters on colour in plants and on geographical distribution, concerning the latter of which subjects Mr. Wallace speaks with every title to respect, and suggests some novel views. On the "Geological Evidences of Evolution" as well as on the "Fundamental Problems" of variation and heredity, he is less satisfactory. In regard to the latter, one chapter is altogether an inadequate space in which to deal with such an array of antagonists as Mr. Herbert Spencer, Dr. Cope, Dr. Karl Semper, and Mr. Patrick Geddes. Mr. Wallace has barely space to do more than state his opponents' views, and to give a rapid summary of reasons for his dissent, without sufficiently establishing those reasons. This will be especially regretted by those who, like myself, agree with Mr. Wallace in his rejection of Spencer's and Semper's Lamarckism, and are unable to attach any serious value to the speculations put forward on this matter by Dr. Cope and Mr. Geddes. The translation of Weismann's "Essays," which appeared coincidentally with Mr. Wallace's book—although many of the essays have been for some years familiar to readers of German—supplies that more solid treatment of the subject which is desirable. It is satisfactory to find that justice is done by Mr. Wallace to Mr. Francis Galton, whose views on heredity, arrived at by a special method of inquiry, are closely similar to those arrived at on other grounds by Weismann.

Prof. Semper's work "On the Natural Conditions of Existence as they affect Animal Life" is duly mentioned by Mr. Wallace, and he does not fail to notice the striking fact that in this interesting volume the author entirely fails—as I pointed out in NATURE when it appeared—to adduce a single fact in proof of the Lamarckian theory which he sets out to champion.

Of the American evolutionists Mr. Wallace justly says: "In place of the well-established and admitted laws to which Mr. Darwin appeals, they have introduced theoretical conceptions which have not yet been tested by experiments or facts, as well as metaphysical conceptions which are incapable of proof." They have, in fact, conspicuously abandoned the "scientific method."

The words which Mr. Wallace has applied to the American evolutionists are, in the opinion of many, strangely applicable to portions of his own concluding chapter on "Darwinism applied to Man." He here introduces us to a "spiritual world" and to "different degrees of spiritual influx." Mr. Wallace is in the peculiar position of one who believes that he has experimental evidence of the remarkable theoretical and metaphysical conceptions which he introduces. He boldly takes up this position, and we may be sure that he would not wish attention to be diverted from it. It remains an interesting problem for the future student of human faculty to reconcile Mr. Wallace's wonderful ingenuity and skill as a reasoner and observer concerning animal life, with his views as to the so-called "manifestations" of spiritualists.

Mr. Wallace's contention that the mathematical, musical, and artistic faculties of man have not been developed under the law of natural selection

must in large part be conceded. Whilst the earlier development of these faculties may be explained as due to natural selection since some amount of each may well have been an advantage to the primitive man in his struggle for existence, it is yet true that their sudden and rapid development to a very much higher level in civilized communities cannot be traced to the struggle between man and man. It does not, however, follow that, because natural selection will not account for these extraordinary developments of the human brain, therefore we must have recourse to the assumption of supernatural agencies. Mr. Wallace seems so much convinced of the importance and capability of the principle of natural selection, that when it breaks down as an explanation he loses faith in all natural cause, and has recourse to metaphysical assumption. On the other hand, it must be contended that we know very little of the development, either in the individual or in various races, of these and other faculties of the mind. The formation of civilized communities has had the result of withdrawing the individual man almost entirely from the operation of natural selection. Such selection as still obtains operates by the struggle of communities rather than by that of individuals. Accordingly there is a possibility of the most useless "sports" making their appearance, and even establishing themselves in human communities as hereditary qualities. Mr. Gulick's notion that an initial tendency due to accidental variation can increase and develop in succeeding generations, without reference to the advantage or disadvantage of the species, would assuredly be applicable, if anywhere, to the human mind in communities where individuals are no longer subject to natural selection, or only to a minimal extent, and in relation to a few points of structure. Does the luxuriant development of some Professor's mathematical faculty, as compared with the poor numerical conceptions of an Australian black, offer really any greater difficulty of transition than do the 9-foot-long tail feathers of some Japanese barn-door fowls, as compared with the shorter feathers of other varieties? That is a question which can only be answered by a more elaborate analysis of the nature of the qualities compared than has, so far as I know, been hitherto accomplished.

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