

MR. WALLACE ON DARWINISM.

TO all who have read the life and letters of the late Mr. Darwin it must appear that, over and above the personal and scientific interest which attaches in so high a degree to that admirable biography, there is what may be termed a dramatic interest. The antecedents of Charles Darwin, the Sir Isaac Newton of biology, in Charles Darwin, the undergraduate at Cambridge—hitherto unconscious of his own powers, and waking up to a love of science under the guiding influence of a beautiful friendship; the delight and the diffidence which attended his nomination by Professor Henslow as a suitable naturalist for the *Beagle* expedition; the uncertainty which afterwards marked the course of negotiations between his family on the one hand, and the Admiralty on the other, wherein issues of incalculable importance were turning and re-turning in the balance of chance, determined this way and that by the merest featherweights of circumstance; the eventual suddenness of a decision which was destined to end not only, as his father anticipated, in an “unsettling” of his own views, but also, and to a never paralleled degree, in the unsettling of the views of all mankind; the subsequent dawning upon his mind of the truth of evolution in the light of his theory of natural selection, and the working out of that theory during twenty years of patient devotion in the quiet retirement of an English country life; the bursting of the storm in 1859, and all the history of the great transformations which have followed;—these in their broadest outlines are some of what I have ventured to call the dramatic elements in the records of Mr. Darwin’s life. Now, not least among these dramatic elements is the relation in which Mr. Darwin’s work stood to that of Mr. Wallace. For assuredly it was in the highest degree dramatic, that the great idea of natural selection should have occurred inde-

pendently and in precisely the same form to two working naturalists ; that these naturalists should have been countrymen ; that they should have agreed to publish their theory on the same day ; and last, but not least, that, through the many years of strife and turmoil which followed, these two English naturalists consistently maintained towards each other such feelings of magnanimous recognition, that it is hard to say whether we should most admire the intellectual or the moral qualities which, in relation to their common labours, they have displayed.

Now, I have sought to lay emphasis on this the dramatic side of "Darwinism," because in the work which under this title I am about to review, it appears to me that Mr. Wallace has added yet another scene, or episode, which, in the respects we are considering, is quite worthy of all that has gone before. I do not allude merely to the fact that in this work we have the matured conclusions of the joint-originator of Darwinian doctrine, published most opportunely at a time when biological science is especially anxious to learn his views upon certain questions of the highest importance which have been raised since the death of Darwin ; nor do I allude merely to the further fact that in now speaking out, after nearly a decade of virtual silence on scientific topics, the veteran naturalist has displayed an energy of investigation as well as a force of thought which is everywhere equal to, and in many places surpasses, anything that is to be met with in all the solid array of his previous works. That these facts present what I call a dramatic side I fully allow ; but the point which in this connection I desire to bring into special prominence is the following.

It is notorious that, from the time when they published their joint theory of evolution by natural selection, Darwin and Wallace failed to agree upon certain points of doctrine, which, although of comparatively small importance in relation to any question of evolution considered as a *fact*, were, and still continue to be, of the highest possible importance in relation to the question of evolution considered as a *method*—*i.e.*, in relation to the causes or factors which have been concerned in the process. It was the opinion of Mr. Darwin that natural selection has been the chief, but not the only, cause of organic evolution ; while, in the opinion of Mr. Wallace, natural selection has been the all and in all of such evolution—virtually the sole and only principle which has been concerned in the development both of life and of mind from the amoeba to the ape—although he further and curiously differs from Darwin in an opposite direction, by holding that natural selection can have had absolutely no part at all in the development of faculties distinctively human. Disregarding the latter and subordinate point of difference (a re-presentation of which in the concluding chapters of his present work I may however remark appears to me sadly like the feet of clay in a figure of iron, marring by its manifest weakness what

would otherwise have been a completed and self-consistent monument of strength), let us first clearly understand to what it is that the major point of difference amounts. This may best be done by quoting from each of the authors in question parallel passages, which occur in the concluding paragraphs of their latest works.

Mr. Darwin writes:—

“I have now recapitulated the facts and considerations which have thoroughly convinced me that species have been modified during a long course of descent. This has been effected chiefly through the natural selection of numerous successive, slight, favourable variations, aided in an important manner by the inherited effects of the use and disuse of parts; and in an unimportant manner, that is in relation to adaptive structures, whether past or present, by the direct action of external conditions, and by variations which seem to us in our ignorance to arise spontaneously. It appears that I formerly underrated the frequency and value of these latter forms of variation, as leading to permanent modifications of structure independently of natural selection. But as my conclusions have lately been much misrepresented, and it has been stated that I attribute the modification of species exclusively to natural selection, I may be permitted to remark that in the first edition of this work, and subsequently, I placed in a most conspicuous position—namely, at the close of the Introduction—the following words:—‘I am convinced that natural selection has been the main, but not the exclusive, means of modification.’ This has been of no avail. Great is the power of steady misrepresentation; but the history of science shows that fortunately this power does not long endure.”

Mr. Wallace writes:—

“While admitting, as Darwin always admitted, the co-operation of the fundamental laws of growth and variation, of correlation and heredity, in determining the direction of lines of variation or in the initiation of peculiar organs, we find that variation and natural selection are ever-present agencies, which take possession, as it were, of every minute change originated by these fundamental causes, check or favour their further development, or modify them in countless varied ways according to the varying needs of the organism. Whatever other causes have been at work, natural selection is supreme, to an extent which even Darwin himself hesitated to claim for it. The more we study it the more we are convinced of its overpowering importance, and the more confidently we claim, in Darwin’s own words, that it has been the most important, but not the exclusive, means of modification.”

Now, in the latter quotation it is manifest that the “co-operation” which is spoken of takes cognizance only of factors which are themselves either necessary conditions to, or integral parts of, the process of natural selection; and, therefore, the approval which Mr. Wallace bestows upon Mr. Darwin’s emphatic reservation (“but not exclusive means of modification”) can only be understood to have reference to the development of those distinctively human faculties which he immediately proceeds to consider, and touching which, as already indicated, Mr. Darwin’s reservation was certainly not intended to apply. Thus, in brief, at the time of Mr. Darwin’s death the state of matters was this: while Mr. Wallace held persistently to his original belief in natural

selection as virtually the sole and only cause of organic evolution, the whole body of scientific opinion, both in this country and abroad, had followed Mr. Darwin in holding that, while natural selection was "the main" factor of such evolution, nevertheless it was largely supplemented in its work by certain other subordinate factors, of which the most important were taken to be the inherited effects of use and disuse, together with the influence of the environment in directly producing alterations both of structure and of instinct.

Shortly after Mr. Darwin's death, however, this state of matters underwent a very serious change. For it was shortly after Mr. Darwin's death that Professor Weismann began to publish a remarkable series of papers, the effect of which has been to create a new literature of such large and rapidly increasing proportions that, with the single exception of Mr. Darwin's own works, it does not appear that any publications in modern times have given so great a stimulus to speculative science, or succeeded in gaining so influential a following. The primary object of these papers is to establish a new theory of heredity, which has for one of its consequences a denial of the inherited effects of use and disuse, or, indeed, of any other characters which are acquired during the lifetime of individuals; according to this theory, the only kind of variations that can be transmitted to progeny are those which are called congenital. For instance, there is no doubt that in his individual lifetime the arms of a blacksmith have their muscular power increased by constant exercise (or use) of the muscles in hammering; and therefore, if there were a thousand generations of blacksmiths, it seems reasonable to suppose that the children of the last of them would inherit somewhat stronger arms than those of average children—or, *à fortiori*, than those of children born of a similarly long line, say, of watchmakers. This was the supposition that constituted the basis of Lamarck's theory of evolution, and, as we have seen, it was sanctioned by Darwin—although, of course, he differed from Lamarck in not regarding this supposed transmission of the effects of use and disuse as the sole factor of evolution, but merely as a factor greatly subordinate to that which he had himself discovered in survival of the fittest. Nevertheless, he unquestionably did regard this subordinate factor as one of high importance in co-operation with survival of the fittest, and, as Mr. Herbert Spencer has shown in detail, he apparently attributed more and more importance to it the longer that he considered its relation to the greater principle. But, as we have just seen, according to the school of Weismann it is only variations of a congenital kind that *can* be inherited: no matter what adaptive changes may be induced in the individual by suitable use and disuse of its several parts, and no matter what adaptive changes may be directly caused by environing agencies, these all count for nothing in the process of evolution: the

only adaptive changes that can count for anything in this process are those which can be transmitted to progeny—*i.e.*, according to this school, those which arise fortuitously as congenital variations, for the accidental occurrence of which natural selection is always, so to speak, waiting and watching. The human hand, for example, considered as a mechanism, owes nothing to its continued use through numberless generations as an instrument for the performance of functions which it is now so admirably adapted to discharge; on the contrary, its evolution has throughout been exclusively dependent on the occurrence of fortuitous variations, which, whenever they happen to occur in a profitable direction, were preserved by natural selection, and passed on to the next generation. Now, it is evident that, according to this theory, natural selection is constituted the one and only cause of organic evolution; and for this reason the followers of Weismann are in the habit of calling his doctrine “pure Darwinism,” inasmuch as without invoking any aid from the Lamarckian principles above described, it constitutes the Darwinian principle of natural selection the sole, and not merely as he said the “main, means of modification.”

Obviously, without going further than this quotation (which I have already made from the last edition of the “Origin of Species”) it is a misnomer to designate the doctrine in question “pure Darwinism.” That quotation presents the only note of bitterness which is to be met with in the whole range of Mr. Darwin’s writings, and it is a note which has express reference to this very point: notwithstanding the multifarious directions in which his doctrines were abused, the only protest against “steady misrepresentation” that he has ever allowed himself to lodge, he lodged against those who imputed to him this so-called doctrine of “pure Darwinism.” On the other hand, it is no less manifest that this doctrine, although not pure Darwinism, assuredly is, and always has been, pure *Wallaceism*. In point of fact, it is with reference to this very doctrine of natural selection as the sole cause of organic evolution that the opinion of these two renovators of biology has been from the first divided: it is upon this point, and upon this point alone, that there has ever been any serious difference between them—for, as we shall presently find, every other point in which they failed to agree (save with respect to the origin of man) has a direct logical reference to this one, or grows out of this one by way of logical consequence.

And here we arrive at what seems to me the dramatic interest attaching to Mr. Wallace’s latest work. On the present occasion I am not going to consider the pros and the cons of the momentous question which has always divided his teaching from that of his great compatriot. But, whether he is right or whether he is wrong, he has lived to see a most extraordinary revolution of biological thought in the direction of opinions which have always been distinctively his own,

and which for a large part of a lifetime he has been virtually alone in maintaining.

Yet, notwithstanding the gratification with which Mr. Wallace must have watched this remarkable change within the last few years, there is in his recently published book no sound of exultation. On the contrary, his aim everywhere appears to be that of concealing his personal interest in this matter; and so well does he succeed that, after having finished his book, not one in a hundred of his readers will be in a position to surmise that for more than a quarter of a century their author has steadily maintained the opinions which are now being adopted by an influential and rapidly increasing body of evolutionists. Therefore, it is partly for the sake of drawing attention to a claim which Mr. Wallace characteristically abstains from making on his own behalf that I have ventured to write this review of his latest work. If ever there was an occasion when a man of science might have felt himself justified in expressing a personal gratification at the turning of a tide of scientific opinion, assuredly such an occasion is the present; and in whichever direction the truth may eventually be found to lie, historians of science should not omit to notice that in the very hour when his lifelong belief is gaining so large a measure of support Mr. Wallace quietly accepts the fact without one word of triumph.

To me individually it does not appear that the recent movement of scientific opinion in the direction of "Wallaceism" is scientifically justifiable; and therefore I remain an adherent of "Darwinism," as this was left by the matured judgment of Darwin. For, on the one hand, I cannot find that the school of Weismann has added anything of importance to the body of facts previously known; while, on the other hand, I do find that Professor Weismann himself is put to the sorest straits while trying to maintain his theory in the presence of some of these facts. So that, while fully recognizing the extraordinary ability with which he has marshalled his evidence—and also, it may be added, the great service which he has rendered to biological science in raising certain questions of the highest possible importance in the acutest possible form—I must still confess that to my mind there does not seem to have been hitherto shown any adequate reason to pass from the theory of evolution as this was always held by Darwin, to the theory of evolution as it has always been held by Wallace. Therefore I am free to conclude this article by briefly considering the points upon which Wallace, in his matured publication on "Darwinism," expressly differs from the teachings of Darwin.

As already stated, all these points of difference (with the one exception as to the origin of man) arise by way of logical necessity from the great or radical difference which we have hitherto been considering—viz., as to whether natural selection is only the "main" or actually

“the exclusive means of modification.” Nevertheless, it is desirable to consider what Mr. Wallace has to say upon these secondary or sequent points of difference, because, by examining them in the light of the diverse facts which they severally involve, we may obtain valuable material for guiding our judgment upon the larger issue.

SEXUAL SELECTION.

Against Mr. Darwin's theory of sexual selection—*i.e.*, selection which depends on the superior power which males may be supposed to present in the way of charming their females—Mr. Wallace urges the following objections, which, in his opinion, are sufficient to dispose of the theory *in toto*.

In the first place, he argues that the principal cause of the greater brilliancy of male animals in general, and of male birds in particular, is that they do not so much stand in need of protection arising from concealment as is the case with their respective females. Consequently natural selection is not so active in repressing brilliancy of colour in the males, or, which amounts to the same thing, is more active in “repressing in the female those bright colours which are normally produced in both sexes by general laws.”

Next, he argues that not only does natural selection thus exercise a negative influence in passively permitting more heightened colour to appear in the males, but even exercises a positive influence in actively promoting its development in the males, while, at the same time, actively repressing its appearance in the females. For heightened colour, he says, is correlated with health and vigour; and as there can be no doubt that healthy and vigorous birds best provide for their young, natural selection, by always placing its premium on health and vigour in the males, thus also incidentally promotes, through correlated growth, their superior colouration.

Again, with regard to the display which is practised by male birds, and which constitutes the strongest of all Mr. Darwin's arguments in favour of sexual selection, Mr. Wallace points out that there is no evidence at all of the females being in any way affected thereby. On the other hand, he argues that this display may be due merely to general excitement; and he lays stress upon the more special fact that moveable feathers are habitually erected under the influence of anger and rivalry, in order to make the bird look more formidable in the eyes of his antagonists.

Furthermore, he adduces the consideration that, even if the females are in any way affected by colour and its display on the part of the males, and if, therefore, sexual selection be conceded a true principle in theory, still we must remember that, as a matter of fact, it can only operate in so far as it is allowed to operate by natural selection. Now,

according to Mr. Wallace, natural selection must wholly neutralize any such supposed influence of sexual selection. For, unless the survivors in the general struggle for existence happen to be those which are also the most highly ornamented, natural selection must neutralize and destroy any influence that may be exerted by female selection. But obviously the chances against the otherwise best fitted males happening to be likewise the most highly ornamented must be many to one, unless, as Wallace supposes, there is some correlation between embellishment and general perfection, in which case, as he points out, the theory of sexual selection lapses altogether, and becomes but a special case of natural selection.

Once more, Mr. Wallace argues that the evidence collected by Mr. Darwin himself proves that each bird finds a mate under any circumstances—a general fact which in itself must quite neutralize any effect of sexual selection of colour or ornament, since the less highly coloured birds would be at no disadvantage as regards the leaving of healthy progeny.

Lastly, he urges the high improbability that through thousands of generations all the females of any particular species—possibly spread over an enormous area—should uniformly and always have displayed exactly the same taste with respect to every detail of colour to be presented by the males.

Now, without any question, we have here a most powerful array of objections against the theory of sexual selection. Each of them is ably developed by Mr. Wallace himself in his work on *Tropical Nature*; and although I have here space only to state them in the most abbreviated of possible forms, I think it will be apparent how formidable these objections appear. Unfortunately the work in which they are mainly presented was published several years after the second edition of the "*Descent of Man*," so that Mr. Darwin never had a suitable opportunity of replying. But, if he had had such an opportunity, as far as I can judge it seems that his reply would have been more or less as follows.

In the first place, Mr. Wallace fails to distinguish between brilliancy and ornamentation—or between colour as merely "heightened," and as distinctively decorative. Yet there is obviously the greatest possible difference between these two things. We may readily enough admit that a mere heightening of already existing colouration is likely enough—at all events in many cases—to accompany a general increase of vigour, and therefore that natural selection, by promoting the latter, may also incidentally promote the former, in cases where brilliancy is not a source of danger. But clearly this is a widely different thing from showing that not only a *general brilliancy of colour*, but also the *particular disposition of colours* in the form of ornamental patterns, can thus be accounted for by natural selection.

Indeed, it is expressly in order to account for the occurrence of such ornamental patterns that Mr. Darwin constructed his theory of sexual selection; and therefore, by thus virtually ignoring the only facts which that theory endeavours to explain, Mr. Wallace is not really criticizing the theory at all. By representing that the theory has to do only with brilliancy of colour, as distinguished from disposition of colours, he is going off upon a false issue which has never really been raised.* Look, for example, at a peacock's tail. No doubt it is sufficiently brilliant; but far more remarkable than its brilliancy is its elaborate pattern on the one hand, and its enormous size on the other. There is no conceivable reason why mere brilliancy of colour, as an accidental concomitant of general vigour, should have run into so extraordinary, so elaborate, and so beautiful a pattern of colours. Moreover, this pattern is only unfolded when the tail is erected, and the tail is not erected in battle (as Mr. Wallace's theory of the erectile function in feathers would require), but in courtship; obviously, therefore, the design of the pattern, so to speak, is correlated with the act of courtship—it being only then, in fact, that the general design of the whole structure, as well as the more special design of the pattern, becomes revealed. Lastly, the fact of this whole structure being so large, entailing not only a great amount of physiological material in its production, but also of physiological energy in carrying about such a weight, as well as of increased danger from impeding locomotion and inviting capture—all this is obviously incompatible with the supposition of the peacock's tail having been produced by natural selection. And such a case does not stand alone. There are multitudes of other instances of ornamental structures imposing a drain upon the vital energies of their possessors, without conferring any compensating benefit from a utilitarian point of view. Now, in all these cases, without any exception, such structures are ornamental structures which present a plain and obvious reference to the relationship of the sexes. Therefore it becomes almost impossible to doubt—first, that they exist for the sake of ornament; and next, that the ornament exists on account of that relationship. If such structures were due merely to a superabundance of energy, as Mr. Wallace supposes, not only ought they to have been kept down by the econo-

* The only remarks he has to offer on disposition as distinguished from brilliancy of colours are offered as an after-thought suggested to him by the late Mr. Alfred Tylor's book on "Colouration in Animals and Plants" (1886). In this paper Mr. Tylor sought to show "that diversified colouration follows the chief lines of structure, and changes at points, such as the joints, where function changes." Now, while agreeing with Mr. Wallace that this posthumous work is "most interesting and suggestive," I certainly cannot agree with him in regarding the material which it presents as in any degree subversive of the theory of sexual selection. Even if it be granted that Mr. Tylor has satisfactorily established his principles, these principles do not in any way apply to sexual colouration; they apply only to colouration as affected by physiological functions common to both sexes. Moreover, even these functions are of a kind which cannot be supposed to affect, in either sex, those details in the colour of feathers, &c., which it is the object of Mr. Darwin's theory to explain.

mizing influence of natural selection ; but we can see no reason, either why they should be so highly ornamental on the one hand, or so exclusively connected with the sexual relationship on the other.

For these reasons I think that Mr. Wallace's main objection falls to the ground. Passing on to his subsidiary objections, I do not see much weight in his merely negative difficulty as to there being an absence of evidence upon hen birds being charmed by the plumage or the voice of their consorts. For, on the one hand, it is not very safe to infer what sentiments may be in the mind of a hen ; and, on the other hand, it is impossible to conceive what motive can be in the mind of a cock, other than that of making himself attractive, when he performs his various antics, displays his ornamental plumes, or sings his melodious songs. Considerations somewhat analogous apply to the difficulty of supposing so much similarity and constancy of taste on the part of female animals as Mr. Darwin's theory undoubtedly requires. Although we know very little about the psychology of the lower animals, we do observe in many cases that small details of mental organization are often wonderfully constant and uniform throughout all members of a species, even where it is impossible to suggest any utility as a cause.

Again, as regards the objection that each bird finds a mate under any circumstances, we have here an obvious begging of the whole question. That every feathered Jack should find a feathered Jill is perhaps what we might have antecedently expected ; but when we meet with innumerable instances of ornamental plumes, melodious songs, and the rest, as so many witnesses to a process of sexual selection having always been in operation, it becomes irrational to exclude such evidence on account of our antecedent prepossessions.

There remains the objection that the principles of natural selection must necessarily swallow up those of sexual selection, as the fat king swallowed up the lean in the dream of Pharaoh. And this consideration, I doubt not, lies at the root of all Mr. Wallace's opposition to the supplementary theory of sexual selection. He is self-consistent in refusing to entertain the evidence of sexual selection, on the ground of his antecedent persuasion that in the great drama of evolution there is no possible standing-ground for any other actor than that which appears in the person of natural selection. But here, again, we must refuse to allow any merely antecedent presumption to blind our eyes to the actual evidence of other agencies having co-operated with natural selection in producing the observed results. And, as regards the particular case now before us, I think I have shown, as far as space will permit, that in the phenomena of decorative colouring (as distinguished from merely brilliant colouring), of melodious song (as distinguished from merely tuneless cries), of enormous arborescent antlers (as distinguished from merely offensive weapons), and so forth—I say that in all these phenomena we have phenomena which cannot possibly

be explained by the theory of natural selection; and, further, that if they are to be explained at all, this can only be done, so far as we can at present see, by Mr. Darwin's supplementary theory of sexual selection.

I have now briefly answered all Mr. Wallace's objections to this supplementary theory, and, as previously remarked, I feel pretty confident that, at all events in the main, the answer is such as Mr. Darwin would himself have supplied, had there been a third edition of his work upon the subject. At all events, be this as it may, we are happily in possession of unquestionable evidence that he believed all Mr. Wallace's objections to admit of fully satisfactory answers. For his very last words to science—read only a few hours before his death at a meeting of the Zoological Society—were:—

“I may perhaps be here permitted to say that, after having carefully weighed, to the best of my ability, the various arguments which have been advanced against the principle of sexual selection, I remain firmly convinced of its truth.”

INHERITED EFFECTS OF USE, DISUSE, AND DIRECT ACTION OF ENVIRONMENT.

We have just seen that one of Mr. Wallace's strongest arguments against sexual selection consists in representing *à priori* that there can be no room for the operation of such a principle in the presence of natural selection: the greater principle must swallow up the less. This *à priori* argument he extends to all the other supplementary principles which have ever been suggested, and appears to regard it as “a short and easy method” with the Darwinists. He urges it with special vehemence against the so-called Lamarckian principles, and therefore it is suitable that under this head we should consider more carefully the value of such an argument.

In the present connection this argument is that, even admitting the abstract possibility of Lamarckian principles, in the presence of natural selection they could never have an opportunity of acting, inasmuch as the needful changes would be effected by a natural selection of fortuitous variations more rapidly than they could be by an inheritance of the effects of use and disuse, &c. Now this argument admits of two rejoinders. First, it is surely conceivable that in many cases where slight (because initial and afterwards finely graduated) improvements are concerned, such improvements need not have been, *in every stage of their progress*, matters of life and death to the organisms presenting them. Yet, unless at *every* stage of their progress they were matters of life and death, they could not have been produced by the unaided influence of natural selection. Now it is just in such cases that the supplementary or Lamarckian principles are supposed by Darwinists to

come in ; for to the operation of these principles it is not necessary that at each stage of the process every slight improvement should be a matter of life and death to the organisms presenting it. To me it appears that we have here a consideration of the highest importance. Nowadays no one disputes the supremacy of natural selection over all other principles of organic change hitherto suggested, or even, it may be predicted, suggestable. But this acceptance of natural selection as *supreme* by no means necessitates (as Mr. Wallace appears to imagine) acceptance of natural selection as *unique*. Nor is there any incompatibility between our acceptance of natural selection as supreme and a further acceptance of any other principles as subordinate or co-operative. What we all agree upon is, that no such other principles can act, save in so far as they are allowed to act by natural selection ; but to maintain that there can be no room for the action of any other principle hitherto suggested, or in the future suggestable, appears to me extravagant. At all events, the burden of proof must lie with any one who affirms that no adaptive improvement—or, indeed, change of any kind—can ever take place unless every stage in the gradual process has been a matter of life and death to the organisms presenting it—a burden of proof which it is obviously impossible that any one can ever be in a position to discharge.

In view of this consideration it seems to me that Mr. Wallace's *a priori* objection to the abstract possibility of Lamarckian principles falls to the ground, although of course the question remains whether there is any sufficient evidence *à posteriori* of their operation in actual fact. And a virtual answer to this question appears to me to be involved in the second consideration, which, as above stated, remains to be adduced.

Long ago Mr. Herbert Spencer pointed to the facts of co-adaptation within the limits of the same organism as presenting the strongest possible evidence of Lamarckian principles working in association with Darwinian. Thus, taking one of Lamarck's own illustrations, Mr. Spencer showed that there must be thousands and thousands of changes—extending to all the organs and even to all the tissues of the animal—which in the course of numberless generations have conspired to convert an antelope into a giraffe. Now the point is that, throughout the entire history of these changes, their utility must have always been dependent on their association. It would be useless that an incipient giraffe should present a tapering down of the hind-quarters, unless at the same time it presented a tapering up of the fore-quarters ; and as each of these modifications entails innumerable subordinate modifications throughout both halves of the creature concerned, the chances must be infinity to one against the required association of so many changes happening to arise by way of merely fortuitous variation. Yet, if we exclude the Lamarckian inter-

pretation as adopted by Darwin, which gives us an intelligible *cause* of co-adaptation, we are required to suppose that such a happy concurrence of innumerable co-adaptations must have occurred by mere accident, and this thousands and thousands of times in the bodies of as many successive ancestors of the existing species; for, at each successive stage of the improvement, natural selection (if working alone) must have needed all, or at any rate most, of the co-adaptations to occur in the same individual organisms.

Against this formidable consideration Mr. Wallace adduces the following rejoinder: "The best answer to this objection may, perhaps, be found in the fact that the very thing said to be impossible by variation and natural selection has been again and again effected by variation and artificial selection." This analogy he then enforces by special illustrations, &c.; but does not appear to perceive that it really misses the whole point of the difficulty against which it is brought.

The point of the difficulty is, not that the needful variations do not occur, but that they occur associated in the same individual, and that unless they do thus occur associated in the same individual they must be useless—*i.e.*, cannot fall under the sway of natural selection. Therefore the analogy of artificial selection is here irrelevant, seeing that it fails in respect of the very point which it is adduced to meet. The difference between natural selection and artificial selection is, that while the former acts with exclusive reference to the utility (or life preserving character) of variations, the latter acts without such reference. Hence, there is obviously no difficulty in understanding how artificial selection is able to choose this, that, and the other congenital variation as each happens to occur in so many different individuals, and, by suitable pairing, to blend them together in any required proportions. But artificial selection is able to do this simply because the selected individuals do not depend for their lives upon presenting the blended characters which it is the object of such selection to produce. Natural selection, on the other hand (if working alone), must wait until the blended characters happen to arise *fortuitously* in the same individuals—in all cases, that is, where utility depends on the co-adaptation of characters, which are the only cases now under consideration. Thus the two forms of selection present absolutely no point of analogy in the very respects where it is necessary that they should, if Mr. Wallace's appeal from one to the other is to be logically justified. In the one case the association of characters is purposely produced *by* the selection; in the other case it must arise by chance before its resulting utility can be offered *to* the selection.

NATURAL SELECTION AS A CAUSE OF STERILITY BETWEEN SPECIES.

After matured deliberation Mr. Darwin came to the conclusion that natural selection could not be a cause of sterility between species. Mr. Wallace now furnishes an argument to show that in this respect also Mr. Darwin "underrated" the powers of natural selection. The argument, however, is too abstruse to admit of reproduction here. On the present occasion, therefore, I will merely remark that it does not seem so much as to try to meet the considerations which determined Mr. Darwin's judgment in the opposite direction. Nevertheless the theory is profound as well as ingenious, and, although it fails to convince me, I am glad to note that in the course of its exposition Mr. Wallace appears to sanction the essential principle of my own hypothesis of "physiological selection"—viz., to quote his own words, "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." The words which I have italicized very tersely convey the whole gist of "physiological selection."

Later on, however, he criticizes adversely what I have written upon this subject, and also represents me as having misunderstood Mr. Darwin's views with respect to the utility and inutility of specific characters. On both these points I shall have an answer to make on some future and more suitable occasion. In this article I have confined attention to points wherein Mr. Wallace differs from Mr. Darwin; and although in so doing it has been necessary for me to express uniform disagreement with the author of "Darwinism," this has been due only to the limitations of my project, and in no way prevents my cordial appreciation of his work as a whole. Indeed, with the exception of those differences from Mr. Darwin, which it has been my object on the present occasion to consider, it appears to me that Mr. Wallace's latest work is one of the most interesting and suggestive in the whole range of Darwinian literature. And even these points of difference, it will be remembered, all arise out of the single difference before stated—namely, whether natural selection is to be regarded as the main, or as the exclusive, means of modification. Therefore, notwithstanding all that I have said on the Darwinian side of this momentous question, the fact that it still remains an open question compels us to recognize that Mr. Wallace's views with regard to it may eventually prove to be right; while, in any case, he is certainly to be congratulated on having lived to see the great movement which has recently taken place in the direction of those

views. But to many of us it still appears that Mr. Darwin's judgment on this matter is the sounder one to follow. When a great generalization has been fairly established, there is always a tendency to exaggerate its scope; and, perhaps, in no respect was the wonderful balance of Mr. Darwin's mind so well displayed as it was in the caution with which he abstained from assigning to his vast principle of natural selection a sole prerogative. Moreover, as previously stated, the longer that he pondered the question, the more he became persuaded that the problem of organic evolution as a whole was too complex and many-sided to admit of being resolved by the application of a single principle. This conclusion, I believe, will eventually be justified by the advance of biological science; and, therefore, until some better reason is shown than has yet been shown for departing from it, I cannot help feeling that naturalists will do well to suspend their judgments, even if they are not so sure as they used to be touching the doctrines of "Darwinism," as these were left by Darwin.

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