

CHAPTER XI.

COMPARISON OF DARWIN'S AND WALLACE'S SECTIONS
OF THE JOINT MEMOIR—RECEPTION OF THEIR
VIEWS—THEIR FRIENDSHIP.

COMPARING the essays of these two naturalists, we observe that Darwin here first makes public the phrase "natural selection," Wallace the "struggle for existence"; although so closely do their lines of thought converge that Darwin, using practically the same words, speaks of the "struggle for life." Both show, by examples, the tendency of all animals to multiply at an enormous rate, and both show that their tolerably constant numbers are due to the constant supply of food.

Both treat of domesticated animals, but in very different ways. Darwin uses them as the practical illustration of selection, and argues that if man by selection can make such forms, Nature can make her species by the same means. Wallace disposes of the argument that the reversion of domesticated varieties to the wild form is a proof of the permanent distinctness of species, by showing in some detail that the former are "abnormal, irregular, artificial."

Neither of them draws any distinction between instinct and other qualities, but assumes that the

former is, like the latter, operated upon by natural selection.

Wallace makes a special point of protective resemblances in the colours of insects, etc.

The important principle of "divergence of character," and the relatively unimportant one of "sexual selection," are both clearly explained by Darwin.

Neither writer speaks of the direct effect of external conditions—except as a cause of plasticity by Darwin—or the inherited effects of use and disuse. Lamarck is mentioned only to be dismissed by Wallace. The evolution of the giraffe's long neck is explained by Wallace on the principle of natural selection, which is contrasted with Lamarck's original explanation of the same character. This contrast, which has been so often drawn, was therefore originally contained in the first public statement of natural selection.

As has been indicated above, Darwin suggested a cause of variation in the direct effect of changed external conditions on the reproductive system.

In comparing the two essays it is not unnatural to conclude, as Professor Osborn has done ("From the Greeks to Darwin," 1894, p. 245), that the two writers held different views upon the material utilised by natural selection in the production of new species, Darwin relying upon the usual slight differences which separate individuals and upon variations in single characters, Wallace upon fully formed varieties—viz. individuals which departed

conspicuously from the type of the species, and which may exist singly or in considerable numbers side by side with the parent form.

Professor Osborn's actual words are as follows:—

“Darwin dwells upon *variations in single characters*, as taken hold of by Selection; Wallace mentions variations, but dwells upon *full-formed varieties*, as favourably or unfavourably adapted. It is perfectly clear that with Darwin the struggle is so intense that the chance of survival of each individual turns upon a single and even slight variation. With Wallace, varieties are already presupposed by causes which he does not discuss, a change in the environment occurs, and those varieties which happen to be adapted to it survive. There is really a wide gap between these two statements and applications of the theory.”

Further consideration tends to obliterate this supposed distinction. Although Wallace used the term “variety” as contrasted with “species,” the whole context proves that he, equally with Darwin, recognised the importance of individual variations and of variations in single characters. This becomes clear when we remember his argument about the neck of the giraffe, the changes of colour and hairiness, the shorter legs of the antelope, and the less powerful wings of the passenger pigeon. Wallace has kindly written to me (May 12th, 1896) stating the case as I have given it, and he further explains—

“I used the term ‘varieties’ because ‘varieties’ were alone recognised at that time, individ^l variability being ignored or thought of *no importance*. My ‘varieties’ therefore included ‘individual variations.’”

On the other hand, Darwin certainly included large single variations (in other words, "varieties") as well as ordinary individual differences, among the material for natural selection, and he did not abandon the former until he was convinced by the powerful reasoning of Fleeming Jenkin (*North British Review*, June, 1867), who argued that single large differences of a sudden and conspicuous kind (Darwin's "variations") would certainly be swamped by intercrossing. Upon this review of the "Origin" Francis Darwin says ("Life and Letters")—

"It is not a little remarkable that the criticisms which my father, as I believe, felt to be the most valuable ever made on his views should have come, not from a professed naturalist but from a Professor of Engineering."

After reading this review, Darwin wrote to Wallace (January 22nd, 1869):—

"I always thought individual differences more important than single variations, but now I have come to the conclusion that they are of paramount importance, and in this I believe I agree with you. Fleeming Jenkin's arguments have convinced me."

The ambiguity of this sentence evidently misled Wallace into believing that the single variations were considered of paramount importance. Darwin therefore wrote again (February 2nd):—

"I must have expressed myself atrociously ; I meant to say exactly the reverse of what you have understood. F. Jenkin argued in the 'North British Review' (June 1867) against

single variations ever being perpetuated, and has convinced me, though not in quite so broad a manner as here put. I always thought individual differences more important; but I was blind and thought single variations might be preserved much oftener than I now see is possible or probable. I mentioned this in my former note merely because I believed that you had come to a similar conclusion, and I like much to be in accord with you. I believe I was mainly deceived by single variations offering such simple illustrations, as when man selects."

From these two letters to Wallace we see that the latter was the first to give up the larger variations in favour of ordinary individual differences.

Darwin also wrote to Victor Carus on May 4th, 1869:—

"I have been led to . . . infer that single variations are even of less importance, in comparison with individual differences, than I formerly thought."

There has been much misconception on this point, and a theory of evolution by the selection of large single variations—a view held by many, but not by Darwin—has been passed off as the Darwinian theory of natural selection. It is surprising that this old mistake should have been repeated at so recent a date, and on so important an occasion as the Presidential Address to the British Association at Oxford on August 8th, 1894, and that so ill-aimed a criticism should have been quoted with approval in a leading article in the *Times* of the following day. The following extracts from Lord Salisbury's address unfortunately leave no doubt on the matter:

“What is to secure that the two individuals of opposite sexes in the primeval forest, who have been both accidentally blessed with the same advantageous variation shall meet, and transmit by inheritance that variation to their successors? . . . The biologists do well to ask for an immeasurable expanse of time, if the occasional meetings of advantageously varied couples from age to age are to provide the pedigree of modifications which unite us to our ancestor the jelly-fish. . . . There would be nothing but mere chance to secure that the advantageously varied bridegroom at one end of the wood should meet the bride, who by a happy contingency had been advantageously varied in the same direction at the same time at the other end of the wood. It would be a mere chance if they ever knew of each other's existence—a still more unlikely chance that they should resist on both sides all temptations to a less advantageous alliance. But unless they did so, the new breed would never even begin, let alone the question of its perpetuation after it had begun.”

It is of interest to reproduce Lord Salisbury's words in close proximity to Darwin's real statements on the subject, as shown in the letters to his friends—statements which are also expressed in many places in his published works.

The joint paper was read before the Linnean Society on July 1st, 1858, about a fortnight after Wallace's essay had been received by Darwin. There was no discussion, but the interest and excitement at the meeting were very great, owing in large part to the influential support with which the new theory came before the scientific world. Darwin appreciated the importance of this support at its true value, for he wrote to Hooker, July 5th:—

“ You must know that I look at it, as very important, for the reception of the view of species not being immutable, the fact of the greatest Geologist and Botanist in England taking *any sort of interest* in the subject : I am sure it will do much to break down prejudices.”

In the following January Darwin received a letter from Wallace, and his reply (on the 25th) shows how much relieved and pleased he was at its generous spirit. Alluding to Lyell's and Hooker's action in his “ Autobiography ” Darwin says :—“ I was at first very unwilling to consent, as I thought Mr. Wallace might consider my doing so unjustifiable, for I did not then know how generous and noble was his disposition.” It was this letter which conveyed the knowledge to him and set his mind at rest on the subject.

Thus ended one of the most interesting and memorable episodes in the history of science. It was sufficiently remarkable that two naturalists in widely-separated lands should have independently arrived at the theory which was to be the turning-point in the history of biology and of many other sciences—although such simultaneous discoveries have been known before ; it was still more remarkable that one of the two should unknowingly have chosen the other to advise him upon the theory which was to be for ever associated with both their names. It was a magnificent answer to those who believed that the progress of scientific discovery implies continual jealousy and bitterness, that the conditions attending

the first publication of the theory of natural selection were the beginning of a life-long friendship and of mutual confidence and esteem.*

It is justifiable to speak of this episode as the *beginning* of Darwin's and Wallace's friendship, for the latter writes (February, 1895):—

“I had met him *once* only for a few minutes at the Brit. Mus. before I went to the East.”

Later on Darwin, in his letters to Wallace, more than once alluded to the simultaneous publication of their essays. Thus he wrote, April 18th, 1869, congratulating Wallace on his article in the *Quarterly Review* for that month:—

“I was also much pleased at your discussing the difference between our views and Lamarck's. One sometimes sees the odious expression, ‘Justice to myself compels me to say,’ &c., but you are the only man I ever heard of who persistently does himself an injustice, and never demands justice. Indeed, you ought in the review to have alluded to your paper in the ‘Linnean Journal,’ and I feel sure all our friends will agree in this. But you cannot ‘Burke’ yourself however much you may try, as may be seen in half the articles which appear.”

* Since the above paragraph was written I have again read Professor Newton's eloquent Address to the Biological Section of the British Association at Manchester in 1887, and find that he says on the same subject—“If in future you should meet with any cynic who may point the finger of scorn at the petty quarrels in which naturalists unfortunately at times engage, particularly in regard to the priority of their discoveries, you can always refer him to this greatest of all cases, where scientific rivalry not only did not interfere with, but even strengthened, the good-feeling which existed between two of the most original investigators” (Report of Meeting, p. 731).

And again, on April 20th of the following year, he wrote:—

“I hope it is a satisfaction to you to reflect—and very few things in my life have been more satisfactory to me—that we have never felt any jealousy towards each other, though in one sense rivals. I believe that I can say this of myself with truth, and I am absolutely sure that it is true of you.”

CHAPTER XII.

THE GROWTH OF WALLACE'S CONVICTIONS ON EVOLUTION AND DISCOVERY OF NATURAL SELECTION—BORNEO 1855—TERNATE 1858.

WE have already seen in the earlier part of this volume, the gradual development of the theory of Natural Selection in the mind of Darwin, and the long succession of experiments and observations which he undertook before he could bring himself to publish anything upon the subject, as well as the conditions which forced him to a hurried publication in the end. It is of the deepest interest to compare with this the account which Wallace has given us of the mental process by which he arrived at the same conclusions.

This deeply interesting personal history has only been known during the last few years ; in 1891 Wallace republished his "Essays on Natural Selection" in one volume, combined with "Tropical Nature," and he has added (on pp. 20, 21) the following introductory note to Chapter II., viz. the reprint of his Linnean Society Memoir "On the Tendencies of Varieties to depart indefinitely from the Original Type." The note is here reprinted in full:—

"As this chapter sets forth the main features of a theory identical with that discovered by Mr. Darwin many years

before but not then published, and as it has thus an historical interest, a few words of personal statement may be permissible. After writing the preceding paper ["On the Law which has Regulated the Introduction of New Species"] the question of *how* changes of species could have been brought about was rarely out of my mind, but no satisfactory conclusion was reached till February 1858. At that time I was suffering from a rather severe attack of intermittent fever at Ternate in the Moluccas, and one day, while lying on my bed during the cold fit, wrapped in blankets, though the thermometer was at 88° Fahr., the problem again presented itself to me, and something led me to think of the 'positive checks' described by Malthus in his 'Essay on Population,' a work I had read several years before, and which had made a deep and permanent impression on my mind. These checks—war, disease, famine and the like—must, it occurred to me, act on animals as well as man. Then I thought of the enormously rapid multiplication of animals, causing these checks to be much more effective in them than in the case of man; and while pondering vaguely on this fact there suddenly flashed upon me the *idea* of the survival of the fittest—that the individuals removed by these checks must be on the whole inferior to those that survived. In the two hours that elapsed before my ague fit was over I had thought out almost the whole of the theory, and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin. Up to this time the only letters I had received from him were those printed in the second volume of his *Life and Letters* (vol. ii., pp. 95 and 108), in which he speaks of its being the twentieth year since he 'opened his first note-book on the question how and what way do species and varieties differ from each other,' and after referring to oceanic islands, the means of distribution of land-shells, &c., added: 'My work, on which I have now been at work more or less for twenty years, *will not fix or settle anything*; but I hope it will aid by giving a large collection of facts, with one definite end.' The words I have italicised, and the whole tone of his letters, led me to conclude that he had arrived at no definite view as to the origin of species, and I fully anticipated that

my theory would be new to him, because it seemed to me to settle a great deal. The immediate result of my paper was that Darwin was induced at once to prepare for publication his book on the *Origin of Species* in the condensed form in which it appeared, instead of waiting an indefinite number of years to complete a work on a much larger scale which he had partly written, but which in all probability would not have carried conviction to so many persons in so short a time. I feel much satisfaction in having thus aided in bringing about the publication of this celebrated book, and with the ample recognition by Darwin himself of my independent discovery of 'natural selection.' (See *Origin of Species*, 6th ed., introduction, p. 1, and *Life and Letters*, vol. ii., chap. iv., pp. 115-129 and 145)."

A very similar account, differing in a few unimportant details from that quoted above, was written December 3rd, 1887, by Wallace to Professor Newton, and is published in the abridged "Life and Letters of Charles Darwin" (1892; pp. 189, 190). At the conclusion Wallace says:—

"... I *had* the idea of working it out, so far as I was able, when I returned home, not at all expecting that Darwin had so long anticipated me. I can truly say *now*, as I said many years ago, that I am glad it was so; for I have not the love of *work, experiment* and *detail* that was so pre-eminent in Darwin, and without which anything I could have written would never have convinced the world."

It is of great interest to learn that Wallace as well as Darwin was directed to natural selection by Malthus' Essay. Hence, as the late Professor Milnes Marshall has pointed out (*Lectures on the Darwinian Theory*, pp. 212, 213), the laws of the multiplication and extinction of man suggested to both naturalists

those more general laws by which it was possible to understand the development of the whole animal and vegetable worlds.

There is a tremendous contrast between these two discoverers, in the speed with which they respectively developed their ideas on the subject into a shape which satisfied them as suitable for publication. Wallace, after the inspiration which followed his reflections upon Malthus, had "thought out almost the whole of the theory" in two hours, and in three evenings had completed his essay. Darwin, receiving the same inspiration from the same source, in October 1838, wrote a brief account of it after four years' reflection and work, and finished a longer account two years later, but was not prepared to give anything to the public until he was compelled to do so fourteen years later in 1858. All this delay was of the greatest advantage when a full exposition of the theory finally came before the world in the "Origin of Species"; for all difficulties had been fully considered and answered beforehand, while the wealth of new facts by which it was supported compelled a respectful hearing for the theory itself.

Wallace, like Darwin, was convinced of evolution before he discovered any principle which supplied a motive cause for the process. This conviction is expressed very clearly in his interesting essay already alluded to "On the Law which has regulated the Introduction of New Species" (*Ann. and Mag., Nat. Hist.*, 1855, p. 184; reprinted without alteration in his

Essays on Natural Selection). The law he states in these words:—

“Every species has come into existence coincident both in time and space with a pre-existing closely allied species,”

a law which, as he justly claims for it,

“connects together and renders intelligible a vast number of independent and hitherto unexplained facts. The natural system of arrangement of organic beings, their geographical distribution, their geological sequence, the phenomena of representative and substituted groups in all their modifications, and the most singular peculiarities of anatomical structure, are all explained and illustrated by it, in perfect accordance with the vast mass of facts which the researches of modern naturalists have brought together, and, it is believed, not materially opposed to any of them. It also claims a superiority over previous hypotheses, on the ground that it not merely explains, but necessitates what exists. Granted the law, and many of the most important facts in Nature could not have been otherwise, but are almost as necessary deductions from it, as are the elliptic orbits of the planets from the law of gravitation.”

This important essay is dated by Wallace from Sarawak, Borneo, February, 1855.

The conclusions remind us of the words Darwin wrote in his note-book in 1837. “Led to comprehend true affinities. My theory would give zest to recent and Fossil comparative Anatomy.” By his theory Darwin here means evolution and not natural selection, which was not discovered by him until the end of 1838.