I.

The Origin of Species without the Aid of Natural Selection.

A REPLY.

I AM much indebted to Mr. Wallace for his interesting paper (NATURAL SCIENCE, vol. v., p. 179). As he was the joint propounder of Natural Selection with Darwin, I could not hope for a weightier critic. Still, I am not in the least shaken in my opinion by it.

In reply, I would first observe that I take the terms "definite" and "indefinite"—which Mr. Wallace says he does not understand—from Darwin himself, who says: "The direct action of changed conditions leads to definite or indefinite results;"[1] while of the former he writes: "By the term definite action, I mean an action of such a nature that, when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all the individuals, are modified in the same manner. A new sub-variety would thus be produced without the aid of selection."[2]

These words really strike at the root of Darwin's theory; and, indeed, the whole of my contention, if it were not founded on facts and observations, might be based on this passage; for Darwinism may be compared to an inverted pyramid, the apex being the *mistake* Darwin made in supposing variations in any seedlings of a plant (or variety) in *nature* being "indefinite." *They are always definite.* Though hundreds may perish, *the survivors all vary in the same direction*, viz., towards adaptation to the environment.[3]

In a correspondence with the late Professor Romanes last spring on this subject, he wrote me as follows: "Of course, if you could prove that indiscriminate [*i.e.*, indefinite] variations have not occurred in wild plants, but only under cultivation, you would destroy Darwinism *in toto*." (Hyères, March 12, 1894.)

Having stated my case thus briefly, I will proceed to remark upon Mr. Wallace's criticisms.

[1] "Origin of Species," 6th ed., p. 106.

[2] "Animals and Plants under Domestication," ii., p. 271.

[3] See, *e.g.*, "Origin of Species," pp. 72, 175, 176.

Mr. Wallace writes: "It is, of course, admitted that direct proof of the action of Natural Selection is at present wanting." "At present"—why is it still wanting if it really exist? Has not one of the many biologists who have studied nature all over the world, during the last five-and-thirty years, been able yet to find one single proof?

On the other hand, I venture to say and to prove, *in the strictest sense of the term*, that Natural Selection is not wanted as an "aid" or a "means" in originating species.[4]

In the *elimination* of superfluous weaklings, in the *delimitation* of specific forms, and in the *distribution* of plants, Natural Selection may be largely credited with the results, but in the *origin* of species it is not wanted.

Darwin says that "Natural Selection has no relation whatever to the primary cause of any modification of structure"[5]; and the question with which I am solely concerned is to try and find out how and by what means variations in structure originate in plants; for new sub-varieties, varieties, sub-species, species, and genera are all based upon morphological variations; these being the only things systematic botanists trouble themselves with at all. *Then*, whether Natural Selection exists as a "means" or an "aid" in establishing these differences is a separate question altogether, as Darwin insists. To answer *this*, one looks to see, not only if Nature supplies those data upon which Natural Selection is supposed to act, but if they are of any use in the process. Mr. Wallace tells us what they are, for he says: "Offspring resemble their parents very much, but not wholly—each being possesses its individuality. This 'variation' itself varies in amount, but it is always present, not only in the whole being, but in every part of every being. Every organ, every character . . . . is individual; that is to say, *varies* from the same organ, character . . . in every other individual."[6] Now, is there any evidence, direct or indirect, that any such slight morphological differences as are here alluded to are of the slightest consequence to a *seedling* so as to enable it to survive in the struggle for life? What attempts have been made experimentally to test the truth or the reverse of this hypothesis?

Let it not be forgotten, too, that specific and generic characters are more often taken from the flowers and fruits, organs which are totally undeveloped when the "slaughter of the innocents" takes place, and, therefore, must be all put out of court so far as Natural Selection is concerned in bringing about the survival of the fittest. It has been suggested that a plant survives because, say, of some superiority in the structure of the flower, this feature being correlated with a more vigorous constitution than that of the other seedlings, which die in a premature state. I reply this simply begs the question,

[4] The title of Darwin's book is "The Origin of Species by means of Natural Selection."

[5] "Animals and Plants under Domestication, ii., p. 272.

[6] "On Natural Selection," p. 266.

or is putting the cart before the horse. *A seedling survives solely because it is vigorous.* This is capable of proof, and whatever flowers it may subsequently bear, it must be contented with them, whether they be the "best" or not for fertilisation or otherwise. In corroboration of the above, I would add my own experience with small and large seeds. These show that the better nourished have a much greater chance of starting and crowding out the rest by growing into larger plants, and that if small seeds be selected for some years, they either die out altogether or a tiny race of beings is for a time procured. Hence, for the word "fittest," *i.e.*, morphologically, I would substitute "strongest," *i.e.*, constitutionally.

I note here that Mr. Willis says (NATURAL SCIENCE, v., p. 240) that "Natural Selection has to be *dis*proved." No one, however, can be called upon to "prove a negative." It is for Darwinists to prove that the Origin of Species *does* really require the aid of Natural Selection.

On the other hand, it is for me to prove that the Origin of Species can take care of itself; in other words, to establish the truth of Mr. H. Spencer's observation: "Under new conditions the organism immediately begins to undergo certain changes in structure, fitting it for its new conditions,"[7] and that what is true for the individual is true for its offspring, the result being, to adopt Darwin's words, a new sub-variety without the aid of Natural Selection is produced.

I will now give illustrations of "definite" and "indefinite" variations. In 1847, Professor J. Buckman sowed seed of the wild parsnip in the garden of the Agricultural College at Cirencester. The seedlings began to vary, but in *the same way*, though in different degrees. By selecting seed from the best rooted plants, the acquired "somatic" characters of an enlarged root, glabrous leaves, etc., became fixed and hereditary; and "The Student," as he called it, having been "improved" by Messrs. Sutton & Sons, is still regarded as "the best in the trade." This is *definite variation*, according to Darwin's definition, for those weeded out did not differ from the selected, morphologically, except in degree, the variations towards improvement not being quite fast enough to entitle them to survive.

M. Carrière raised the radish of cultivation, *Raphanus sativus*, L., from the wild species *R. Raphanistum*, L., and moreover found that the turnip-rooted form resulted from growing it in a heavy soil, and the long-rooted one in a light soil.[8] Pliny records the same fact as practised in Greece in his day, saying that the "male" (turnip form) could be produced from the "female" (long form), by growing it in "a cloggy soil." Both forms are now, of course, hereditary by seed.

When a plant has been *long cultivated*, the relatively fixed nature, characteristic of most wild forms, generally breaks down; and the seeds from one and the same individual plant cannot always be

[7] "Factors of Evolution."

[8] This has been corroborated by M. Languet with the carrot. *Soc. Roy. et. Cent. d'Agricult*, 2nd ser., vol. ii., 1846–7, p. 539.

depended upon "to come true." Thus, an eminent agriculturist once said to me (a trifle hyperbolically, of course) speaking of the varieties of wheat: "You can almost get a different variety from every grain in a single ear."

Sir J. D. Hooker records no wild variety of the cabbage (*Brassica oleracea*, L.). Theophrastus (300 B.C.) only knew three cultivated forms. Pliny speaks of six, but who will count them now? It would seem as if plants underwent two courses of variation. First, in adaptation to it, by responding at once to a new environment, *i.e.*, definite variation. Then, when this has been thoroughly established, as with all of our ordinary vegetables, they may vary indefinitely, but why they do so no one can tell. Still, taking a broad view of the whole process, it is obvious that all such variations were primarily due to the environment of cultivation; because *they never occur in the wild state.*

Hence, to test the reality of specific characters of wild plants, as Mr. Wallace describes, by their degree of stability under cultivation in a garden, cannot possibly give but the most untrustworthy results. Some may resist for a time the influences of the new artificial environment, others may succumb to them; but it will be *the very best means of forcing them to change*; for, as Darwin and Weismann assert, cultivation induces variability. Suppose this test had been supplied to the wild and tall *Cineraria cruenta* with its small flowers; what would a systematist now say if he had never known the origin of the modern dwarf kind with large flowers of innumerable colours? He would undoubtedly call it a new species.

The rule may be laid down that *a species may be constant as long as its environment is constant, but no longer.* I have changed the spiny *Ononis spinosa*, L., the Rest-harrow, both by cuttings and by seed, into a spineless form undistinguishable from the species *O. repens*, L., in two years; but it would have, I doubt not, at once reverted to *O. spinosa* if I had replanted it in the poor soil from which I took it. It seems, therefore, to be a very hazardous and fallacious method of testing the value of specific or other characters by cultivation. A wild plant may or not change at once. Thus the carrot, *Daucus Carota*, L., proved refractory with Buckman, but not with Vilmorin, who converted this *annual* to a hereditary *biennial*, by sowing the seed late in the season, till the character of flowering in the second season became fixed.

Indeed, the proposed test is not unlike trying a man's guilt by making him eat an ordeal bean!

Mr. Wallace illustrates his remarks by the case of species of *Arabis*, but quite fails to perceive that it goes to prove my contention altogether. He says: "*A. anachoretica* has tissue-papery leaves—*due to its growth in hollows of the rock*" (my itals.). "Seeds of this plant, when cultivated at Kew, produced the common species *A. alpina*. The same thing occurs with many plants, as every cultivator knows."

If the rocky environment is to be credited with species-making in the one case, so must Kew be in the other. In both cases there is neither mention made nor need of any selection at all. Mr. Elwes told me that the various bulbous plants he introduced from the East into his garden at Preston, Cirencester, changed so greatly in a few years in all their parts that he could scarcely recognise them again.

Mr. Wallace adds: "Other forms, with no greater peculiarities externally, preserve their characters under cultivation, though exposed to the most varied conditions."

This is equally and quite true; but any investigation into the causes of the origin of species by *variation* has nothing to do with any other question of the causes of preservation of the type-characters, or *heredity*. Evolution accounts for all living beings by variation; but it does not attempt to offer any explanation of the existence of "survivals." *E.g.*, *Nautilus* and *Lingula* have lived on from the Silurian days till now; *Equisetum* has flourished from, at least, the Carboniferous epoch till to-day. Therefore *change is not absolutely necessary in organisms under changed conditions;* but when it does occur, then I maintain, with Dr. Weismann, that all changes are primarily due to external influences. He says: "We are driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism."[9]

Mr. Wallace is good enough to call attention to my book, "The Origin of Floral Structures by Insect and Other Agencies,"[10] and attacks, very rightly, what I fully admit may be regarded as a weak point in it; *i.e.*, I can bring but few positive illustrations to demonstrate my view that irregular flowers have been formed through the direct action of insects from regular ones; but he quite ignores the whole line of argument running through the book in support of the probability. It is one which Dr. Weismann recommends in support of evolution, which "may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation."[11] It is the well known argument of the accumulation of coincidences which can furnish probabilities of so high an order that they may be regarded as an equivalent to a demonstration. Thus, physicists tell us that they *know* the composition of the sun, but their knowledge is solely based on the coincidences between the lines of the solar spectrum and those of vapourised substances.[12] Similarly with flowers: when we find innumerable coincidences all tending in one direction, coupled with an indefinite capacity for varying in response to forces in all parts of

9 "Essays on Heredity," etc. Eng. trans., p. 279.

10 *International Scientific Series*, vol. lxiv.

11 "Essays on Heredity," etc., p. 255.

12 The "fact" that udders have become enlarged by hand-milking is based on a similar accumulation of probabilities.

plants, I still maintain that "Mr. Henslow's theory [does not] utterly break down." Mr. Wallace contends that the negative evidence derived from "regular" flowers, as gentians, tells against me, as they ought to have long ago become irregular, since their "lower petals have been always subject to irritation and have never developed irregular flowers." This is scarcely fair; for not only do all botanists believe—on precisely the same grounds of probabilities—that all irregular flowers have descended (somehow) from regular ones; but that, if he will refer to the chapter on "Peloria," he will see that existing regular flowers, being mostly "terminal," have no "lower" petals at all, but are so situated as to offer access to insects from all points of the compass. Moreover, whenever a plant with normally irregular flowers (which are always situated close to the axis, so that insects can only enter them in one way) produces a blossom in a terminal position (as foxglove, larkspur, horse-chestnut, etc., often do), it at once becomes quite regular. These differences between regular and irregular flowers represent two of those groups of coincidences respectively, to which I referred.

Mr. Wallace adds: "The very first essential to this theory is to prove that modifications produced by such irritations are hereditary." Quite so. But this proves itself, if my contention be right; for plants with irregular flowers *are* all hereditary. So that there is no need to prove this point, provided the "previous question" as to the origin of irregular flowers themselves be answered. But the converse change can be readily shown; for flowers, normally irregular in nature, often revert to their ancestral regular form under cultivation in the absence of insects, and then come true from seed, as do Gloxinias. Unfortunately, one cannot make a regular flower become irregular. How long it required in nature to do so no one can tell; but all the innumerable minute details of structure coincide to one end; a multitude of correlations all fit together for one effect; so that we may put the alternative thus—Which is more likely, that some one common cause has set up these minute, often microscopic, details in unison together; or that they have arisen by selection out of innumerable wasted variations, which no one ever saw in nature, nor can even ever see a trace of under cultivation?

When, however, we come to variations in the vegetative system of plants, there is nothing easier than to prove, first, the direct action of the environment, and secondly, the hereditary persistence of the result. I need go no further than to take Buckman's parsnip, Carrière's radish, Vilmorin's carrot, or anybody's variety of cabbage. What are all these and many other instances but experimental verifications.

Mr. Wallace alludes to my last paper on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert and Xerophilous Plants,"[13] and attacks my inferences with

[13] *Journ. Linn. Soc. Bot.*, xxx., p. 218.

regard to spinescent processes of desert plants; but he again ignores the primary argument of innumerable coincidences; while in the case of vegetative organs this argument has been in many cases "verified by experiment." When, however, Mr. Wallace calls in question my statement that spines are correlated with a dry soil and atmosphere, he controverts those of Belt, Aitchison, Scott Elliott, Grisebach and others, for he says: "There is no such general coincidence of aridity of soil and atmosphere with abundance of spiny plants, as very little enquiry will show." Having seen and gathered them myself in the Libyan desert and even on our own sandy heaths, I cannot accept this statement; and if those eminent travellers I have named are misleading us, where are we? He then mentions the Galapagos and other islands, where, though of a desert character, plants are *not* spinescent. Here, again, I am not concerned with what does *not* occur, but with what *does*. Moreover, any cause that may tend to arrest an axis likewise may tend to render it spinescent, and more than one cause may produce the same result,[14] so that it is not altogether strange to find spinescent processes away from deserts; but I *do* maintain that spinescence is one and an important element in the *facies* of hot and arid deserts with a barren soil.

Mr. Wallace advances the well-worn theory of the interaction of mammals and spines. In the first place, if I may still believe in the prevalence of spines in deserts, they occur where no herbivorous quadrupeds live. Secondly, if a mammal wishes to eat a spiny plant, it somehow often gets over the difficulty; thus donkeys knock off the spines of *Opuntia*; horses eat gorse. I had a cow which was partial to holly, another rejoiced in netties! But all this is beside the question. It seems to me that there is a lurking element of teleology in this view: for any structure which arises *in anticipation of its use* savours of natural theology[15] rather than of evolution by natural processes alone. I fully admit that plants, when once they have got their spines, may be able to keep animals more or less at bay; but they originate, I maintain, as a mere accidental and inevitable result of an arrest of the organ in question, such arrest being mainly due to drought.

If teleology in its old dress of *Design in anticipation of Use* is, and ought to be, extinct, we may accept Darwin's form of it, that Evolution is the Deity's method of creation. Let us, then, recognise protoplasm as having been impressed with the power of self-adaptation—such being the inference from direct observation of its behaviour; and, consequently, enabled to build up structures in an automatic response to the environmental forces, whenever it is necessary to bring about a better degree of equilibrium between the internal and external forces.

[14] I observe Mr. Osborne makes a corresponding statement. NAT. SCI., p. 223.

[15] Indeed, such anticipation is absolutely necessary for the theory of Natural Selection in general.

On the last page but one of his paper, Mr. Wallace alludes to the case of the hard shells of nuts, and asks if the direct agency of birds, monkeys, etc., has anything to do with them. He admits the question is absurd. I do not therefore know why he asks it. I have not myself written a line on this branch of the subject, but will suggest, from what one knows of all other parts of plants having the capacity of varying, that I see no reason for inferring that hard coats of fruits should be subject to any different law. Soft fruits vary readily enough, as melons, pea-pods, apples, as well as pears in their degrees of "stoniness." Moreover, under cultivation, varieties of forms of nuts and walnuts have arisen, as well as of olives, almonds, and dates, and other hard-coated or hard-seeded fruits. The fact seems to be that cultivation affects the whole organisation of the plant; for the environment is not always solely concerned with an isolated bit of a plant, as a nut or a root. Many visible changes are due to secondary causes within the individual; but in all cases, as I believe with Dr. Weismann, they are *primarily* attributable to the direct action of the environment, simply because *they never occur unless the environment itself is changed.*

Finally, to return to my starting point. The whole question lies within a very small compass. Thus, first, no one disputes the fact that the environmental forces can act upon an organism. Secondly, that the organism can respond to those forces. But now follow two views. Darwinites say that the resulting variations are indefinite in Nature, just as they so often are in cultivation; and that the environment *selects* the best fitted *to survive.* I say that they are always definite in Nature: and not only exceptionally so, as Darwin thought; and that the environment *induces* the best fitted *to arise.*[16] Therefore, Natural Selection has nothing to do in aiding the Origin of Species.

For additional facts I would refer the reader to a paper entitled, "A Theoretical Origin of Endogens from Exogens, through Self-Adaptation to an Aquatic Habit"[17]; and to a companion volume to the "Origin of Floral Structures," which I hope will be shortly published in the "International Scientific Series," and entitled "The Origin of Plant Structures by Self-Adaptation, in Response to the direct Action of the Environment." In this, similar lines of argument, with illustrations, will be applied to Desert, Aquatic, Maritime, Alpine, and Arctic, as well as Climbing Plants, and to the Origin of Peculiarities of Roots, Stems, and Leaves.

George Henslow.

[16] See "Animals and Plants under Domestication," ii., p. 272.

[17] *Journ. Linn. Soc. Bot.*, xxix., p. 485.