

Mr. Cope on the Causes of Variation.

MR. E. D. COPE'S letter in NATURE of November 28 (p. 79) is a fair sample of his writings on biological theory, in so far as I am acquainted with them.

Mr. Cope proposes to teach Mr. Wallace and others the first principles of both logic and biology. The tone of his letter encourages a similar frankness in reply. Mr. Cope must not take it amiss when he is charged with two of the gravest faults of which a critic can be guilty—namely, complete misapprehension of the matter which he is attempting to criticize, and no less complete ignorance of the recognized and elementary facts of the branch of science to which that particular matter relates. I do not hesitate to assert that Mr. Cope puts forward an argument which could not possibly be entertained by anyone who is acquainted with the most notorious and admitted facts of heredity and variation. I venture to express myself thus emphatically, because it is a matter for sincere regret that American biology should at this moment be identified with what is sometimes called "a school of philosophy" which owes its distinction to a deliberate ignoring of the writings of Mr. Darwin. By all means let us have discussion and criticism of Mr. Darwin's conclusions, but let it be understood that those who enter upon such discussion have at any rate an elementary acquaintance with the works of Mr. Darwin himself, if not with those of Weismann and Wallace; otherwise, much time and much of your valuable space will be wasted.

That Mr. Cope has not the necessary elementary acquaintance with the admitted facts of heredity and variation will appear from what follows. The discussion in which he has intervened is one as to whether certain structural peculiarities exhibited by flat-fish are due to the transmission to their offspring of a form and position of parts *acquired* by muscular efforts by the ancestors of flat-fish, or whether these given structural peculiarities suddenly appeared in the ancestors of flat-fish as a "congenital variation" having no adaptive relation to any efforts or experiences of a preceding generation, and were advantageous to their possessors, so that the individuals thus born were favoured in the struggle for existence, survived to maturity, and transmitted their peculiarity to some of their offspring with such intensification as is found experimentally to be the result of breeding from parents both of which possess a given congenital peculiarity.

The question raised is, in short, whether in this case Lamarck's *Hypothesis* of the transmission of acquired characters is the necessary explanation, or whether the case can be explained by the action of the *known* causes (not hypothetical causes) on which Mr. Darwin founded his theory of the origin of species, viz. the occurrence of congenital variations unrelated to any like variations in parents or ancestors, and the selection and intensification of such variations in subsequent breeding. There has been here no ambiguity—such as unfortunately arises sometimes when like questions are discussed—as to the sense in which the term "acquired characters" is used. It is clear enough that by the "acquired characters" of a parent we do not mean characters congenital in the parent, but expressly exclude them; it is clear that we refer on the contrary (as did Lamarck) to new characters acquired by the parent as the direct consequence of the action of the environment upon the parental structure, and exhibited by that parent as definite measurable features.

Now let us consider Mr. Cope's contribution to the discussion. He accuses Mr. Wallace—who is one of those who refuse to adopt Lamarck's gratuitous hypothesis of the transmission of acquired characters—of being guilty of the sin of "non-sequitur" and "paralogism." He then proceeds to make a general statement, the truth of which neo-Darwinians (or post-Darwinians, or anti-Lamarckians), in common with all men, recognize,

although Mr. Cope offensively implies that they do not, viz. "Selection cannot be the cause of those conditions which are prior to selection: in other words, a selection cannot explain the origin of anything." How can Mr. Cope presume to tell us this? Who has ignored it? when? and where? Mr. Cope does not seem to be aware of the fact that the anti-Lamarckians attach great importance to the existence of congenital variation, that Darwin himself has written at length on the subject, and that Weismann has developed a most ingenious theory as to the relation of fertilization and its precedent phenomena to this all-important factor in evolution.

Mr. Cope puts aside all that has been done on that subject, or else is ignorant of it, and calmly lays down the following proposition: "If whatever is acquired by one generation were not transmitted to the next, no progress in the evolution of a character could possibly occur. Each generation would start exactly where the preceding one did." The full significance of this sentence can only be apprehended when it is understood that Mr. Cope believes that progress in the evolution of a character *does* occur. The statement therefore amounts to this: (1) that whatever is acquired by one generation is transmitted to the next; and (2) that the only possible explanation of the fact that a new generation does not exactly resemble its parents at a corresponding age is that the parental generation has transmitted to its offspring particular features acquired by it between birth and maturity.

I doubt whether Mr. Cope will find any other naturalist—even the most ardent Lamarckian—to join him in these assertions.

With regard to the first, it is hardly necessary to say that it has never yet been shown experimentally that *anything* acquired by one generation is transmitted to the next (putting aside parasitic diseases); and as to *everything* ("whatever") being so transmitted, every layman knows the contrary to be true. Children are not born with the acquired knowledge of their parents. If there were no other explanation offered of offspring varying from their parents at a like age than the hypothesis of transmission of characters acquired by the parents on their way through life by the action of the environment, this hypothetical explanation would still be quite insufficient to account for the fact that the individuals of one brood vary enormously as compared with one another, a fact which points to the individual germs (egg-cells and sperm-cells) as the seat of the processes which result in variation, and not to the parental body which is the common carrier of them all. Assuredly these broods demonstrate that *all* the acquired characters are not transmitted to *all* the offspring.

With regard to the second proposition which Mr. Cope's statement contains, experimental fact is directly opposed to its truth. As cited by Darwin on p. 8 of the first edition of the "Origin of Species," Geoffroy St. Hilaire showed that "unnatural treatment of the embryo causes monstrosities; and monstrosities cannot be separated by any clear line of distinction from mere variations." Mr. Darwin himself was "strongly inclined to suspect that the most frequent cause of variability may be attributed to the male and female reproductive elements having been affected prior to the act of conception." What he meant by "being affected" is explained at greater length in the "Animals and Plants under Domestication," where, in chap. xxii., there is a long discussion of the causes of variability, the conclusions of which are supported by an array of observed facts which Mr. Cope cannot be permitted to ignore at his pleasure. Mr. Darwin there gives solid reasons (as was his wont) for holding that variability results from the conditions to which the parents have been exposed: changes of any kind in the conditions of life, even extremely slight changes, often suffice to cause variability. But Mr. Darwin's examination of the facts did not lead him to conclude that the bodily characters acquired by the parents as the result of changes were those which manifested themselves as variations in the offspring. On the contrary he showed that the effect of changed conditions, of excess of nutriment, and of the crossing of distinct forms, is a "breaking down," as it were, of the hitherto fixed characters of the race, leading to the reappearance of long-lost characters and to the appearance of absolutely new characters, the new characters having no more (and perhaps not less) relation to the exciting cause which acted through the parent than has the newly-formed pattern in a kaleidoscope to the tap on the kaleidoscope tube which initiated the rearrangement.

For Mr. Cope to complain of the methods of reasoning of

post-Darwinians, and at the same time without any reasoning at all to assert (as he does, not directly but by implication) that there is no such thing as "congenital variation" or "sporting," is not quite satisfactory. When it is asserted that every feature by which a young animal differs from the structure of its parents at a corresponding age must have been acquired by one or other of the parents as actual structural features, and so transmitted as an acquired character to the offspring, the whole world of fanciers, horticulturalists, farmers, and breeders, is ready with its unanimous testimony to contradict the assertion.

Let me say, in conclusion, that, as Mr. Wallace has pointed out, Mr. Darwin did not consider that variability in a state of nature was either so general or so wide in its range as later observations and reflections lead us to believe it to be. Mr. Darwin studied those causes which are found by practical gardeners and breeders to be favourable to excessive variation in animals and plants under domestication. He showed clearly that the resulting variations had no adaptive relation to the exciting causes, and were manifested in the structure at birth of a new generation, and not in that of the generation subjected to the exciting cause. No one has yet been able to give an adequate account of the frequency and range of variation of any number of animals or plants in a state of nature, because natural conditions destroy, on the average, all individuals born of two parents—except two—before maturity is reached, and those two are naturally selected in consequence of their adhesion to the specific type.

There can be no doubt from a consideration of the facts cited by Darwin that, whilst variation often is reduced to a *minimum* in natural conditions which remain constant, natural variations of conditions can and do occur, which excite the germ-cell and sperm-cell, or their united product, to vary as in conditions of domestication. There can be no doubt that there was in Mr. Darwin's mind the conception of a definite relation between two effects arising from changed conditions: the one being the disturbance of the equilibrium of the organism and its consequent production of variations; the other being the new requirements for survival; in fact, there seems to be, as it were, at once a new deal and new rules of the game. It is not difficult to suggest possible ways in which the changed conditions shown to be important by Darwin could act through the parental body upon the nuclear matter of egg-cell and sperm-cell, with its immensely complex and therefore unstable molecular constitution, so as to bring about *variations* (arbitrary, kaleidoscopic variations) in the ultimate product of the union of the remnant of the twice-divided threads of the egg-nucleus with the nuclear head of a spermatozoon. The wonder is, not that variation occurs, but that it is not excessive and monstrous in every product of fertilization. And yet Mr. Cope writes from the other side of the Atlantic to assert that there is no possible cause of departure from parental type in offspring, excepting that assumed in Lamarck's unproved, improbable speculation!

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