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THE TRANSMISSION OF ACQUIRED CHARACTERS.*

MAX MORSE.

I shall invite your attention this evening to a theme which like the poor, "Ye have always with you." It is the old question whether the changes in the growing organism, or the adult, produced by the direct action of the environment about it, are carried, through heredity, to the offspring. Jean Lamark first used the term "acquired character" to designate characters such as these and to him are we to look for the first clear statement of the case. By this it is not to be understood that the idea of the transmission of acquired characters arose with Lamark. No great generalization ever arose or ever can arise with one man alone. The attribution of the idea of the transmission of acquired characters to Lamark falls in the same category as attributing evolution to Darwin. And as Darwin first attempted to *answer* the question how organisms change, Lamark first *raised* the question how they change at all. The Greeks in the dawn of history accounted for diversity in living forms by the direct effect of environment. Indeed, not until the time of Darwin was there a rival theory advanced. And we can easily see the reason for this when we consider the directness and naïveté of the transmission theory as against the negative action of selection. The history of science shows that hypotheses created as explanations of natural phenomena are at first simple and that it is only when the phenomena are better understood that the hypotheses become more complex. The Corpuscular Theory of light in Newton's sense sufficed for a

* Presidential Address, Biological Club, Nov. 2, 1903.

long time to explain that phenomenon and it has been revived in a refined, augmented and complex form to stand as the modern theory of light. And so, had selection been advanced at first as an explanation of diversity in plants and animals, it would have meant a far deeper insight into the ways of Nature than the Greeks had at that time.

What, we may ask, is an acquired character? That it is a difficult task to answer this question one may infer from the fact that in the periodical *Nature* for 1895, a discussion, ranging over six or seven numbers and led by some of the greatest workers in biology was carried on, each contributor offering a different definition of varying length and complexity. And it is doubtful whether the discussion ended because a conclusion had been reached or whether no more space could be given by the publishers. The most comprehensive definition of the term is that an acquired character is a modification of an organism in its ontogeny, produced by reactions to external stimuli. Its opposite is the congenital character which arises from the genital cell irrespective of external conditions. Now, obviously, these definitions involve severe difficulties, if not in themselves, at least in their application. For the sake of clearness, let us consider the development of an organism in ontogeny and phylogeny.

The Protozoa or Protophyta cannot be said to have an ontogeny. Whatever may be said to be the method of reproduction in them, we may reduce it to its simplest terms—binary fission. Consequently, we cannot speak of palingenesis or cenogenesis in the protozoa or protophyta. Since there is no division of labor whereby one portion of the organism is set apart to perform the function of nutrition, another for reproduction, etc., we can say that the environment exerts a direct effect on the reproductive element and the transmission of acquired characters in unicellular forms is a reality. But when we pass the line between the unicellular forms and multicellular forms, our problem is different. Here we have division of labor. One cell has as its special function the elimination of waste; another, movement, while the third reproduces the animal or plant in its entirety. The question arises, is the method here the same as in the unicellular forms? Or is there a modification necessary to meet the new conditions? In the case of the one celled forms, the *environment* of the reproductive element is the environment of the organism as a whole, while in the multicellular forms the environment of the germinal cell is the group of cells surrounding it—the *environment* of the multicellular organism being the medium outside the body which rarely or never comes in contact with the germ cell, at least until that cell is mature. Hence the case is different. In the latter case—i. e., the multicellular organisms, the generative cell would react to such stimuli as are furnished by the surrounding body.

We may sum up these stimuli as nutritive, respiratory, mechanical, thermic, perhaps electrical and finally, what some will have—a stimulus due to irritability, a virtual vital force. Now one school holds that there is no connection or direct communication between germ cell and body cell,* while another says there is and has shown that there is a possible means of communication by certain protoplasmic bridges that are known to occur at least in some cases. It is obvious what application this has to the subject in hand. The germ-cell in the multicellular forms, located as it is deep in the tissues of the body and away from the surroundings of the organism to which it belongs, may react in one of two ways: it may react to simply the stimuli given by the cells immediately surrounding it or to this *plus* an effect induced by something such as a nervous force, as was mentioned as a possible means of communication between more distant cells. The existence of such a force is not countenanced by modern biologists and it is useless to follow the theme longer. This leaves us with but the hypothesis of Darwin which he termed that of Pangenesis. Darwin early saw the necessity of some such hypothesis, if acquired characters are inherited, in accounting for a means of communication between the body-cells and the germ-cells. In place of a subtle force, Darwin postulated an actual material transmission of a portion of the body-cell to the germ-cell. He assumed protoplasm to be composed of pangens or corpuscles and that these might pass from cell to cell carrying with them the characters, hereditary and acquired, of the cell from which they came. The pangens migrate from the body-cell to the germ-cell and becoming resident there, are transmitted to the offspring, in which they pass to the several parts of the body, thus reproducing the form of the parent. An acquired character could thus be inherited. From other considerations Darwin was led to believe strongly in the transmission of acquired characters and it is a mark of farsightedness on his part when he saw the necessity of some such hypothesis, and met it. It is well to note in passing that the so-called Neo-Darwinians are more Darwinian than the man himself, paradoxical as it may seem. Darwin believed, and that strongly, in the transmission of the direct effects of environment and attempted to explain it, and it is only his followers that have dropped it from the creed.

So much, then, for the *à priori* condition of the subject. We have seen that in unicellular forms, acquired characters are inherited and that in so far, in multicellular forms, as we can treat the germ-cell as a single cell, and apart from the somatic

*The term "germ-cell" is meant to designate such cells as reproduce the parent form—all other cells being "body-cells." Obviously the argument which was originally applied to sex-cells will apply to cases of vegetative reproduction equally well, as in cases of budding, spores, polyembryony, etc.

cells, its acquired characters are inherited; but when we begin to consider that it may be affected in a larger way by remoter portions of the body, either through pangens or some other means, the question takes another turn. Is it not difficult to imagine how some specific change in a remote portion of the body can be registered on the germ-cell with the result that the offspring has reproduced in it the same specific modification? Of course, inconceivability can never be advanced as an argument, pro or con, unless an easier explanation is at hand, and in this case many think there is.

Let us turn now to another phase of the subject. Breeders and fanciers have long insisted that their produce show case after case of the inheritance of acquired modifications. Nay, indeed are not our social institutions themselves built on this assumption? Educate the father and the child will profit thereby. Raise the man of the slums and thereby better his offspring. What teacher that will not on first thought answer that the child of an educated parent learns more easily than that of an ignorant and illiterate father? And so we may read in the stock journals and the fanciers journals of the transmission of acquired traits and an outbreak of discussion is probable at any time. Of discussions on this topic the most noteworthy is the Spencer-Weismann controversy that was carried on in the pages of the *Contemporary Review* in 1893. The discussion arose from an article by Herbert Spencer entitled "The Inadequacy of Natural Selection." In it he attempted to show that coadaptation of the various parts of the body of an organism could be explained far easier by admitting the transmission of functional changes than by the theory of Natural Selection. From the law of probability he attempted to show that the chance of two characters that were mutually adapted arising in the same individual was almost infinite. As a concrete example he took the case of the stag with its antlers weighing pounds. Now in an adult stag we find the most beautiful coadaptation of parts to parts. The shoulder muscles are immense, the front legs are much stronger than the hinder pair, there is an increased blood supply to these parts, etc. How, he asks, can we assume that all these adaptations arose simultaneously in the same individual as variations, so that from the other less favorable conditions these were selected by natural selection? How much easier, he says, is the transmission hypothesis to be applied here!

In answering this and admitting the force of the argument, Weismann submits that if one case could be shown whereby there is no possibility of the transmission of acquired characters the burden of proof would fall to the transmissionists. As such a case he brings forward that of the worker bee. It is well known that the worker bee as well as the soldier termite produce no offspring, as in their development the organs of generation atrophy.

Obviously, selection of favorable variations is the only explanation here. If, then, we must assume that, for instance, the immense jaws with the corresponding muscles of the termite soldier are produced by selection, why must we assume a different cause in the case of the antlers of the stag? When all evidence is weighed, it must be admitted that here is a solution of the problem.

The problem has been attacked from other points of view. Thus, Henry Fairfield Osborn, in an article in the *American Naturalist*,* shows the plausibility of the transmission of functional changes being the method of evolution in organic life. It is too much to assume, he says, that the tubercles in the teeth of mammals have been formed in any way other than by the transmission of mechanical mouldings. Eimer, the friend of Weismann, is the author of an elaborate volume in which he presents an array of facts in support of the transmission theory. He lays special stress on the matter of the pigmentation of the races of man. He finds that in the Nile valley there is a gradation, as one passes from Alexandria southward, in the color of the native races from an intense black to lighter complexions through various intermediate shades. How, he asks, are we to account for such gradations by the preservation of favorable variations? Is it not more logical to assume that they have been the direct effect of environment from generation to generation? Eimer's work is written in German and J. T. Cunningham of England has translated it. This author himself is a firm believer in the transmission hypothesis and is a frequent contributor to the subject. To him is due partly the prominence that the question occupies at the present time.

We have considered thus far proofs from the *à priori* point of view and also deductive proofs. There remains but one class of evidence—experimental. The classic experiments of Brown-Sequard on the guinea pig, in which he attempted to show that epilepsy, caused by the severance of the spinal cord in adults was transmitted to the offspring, are now considered invalid since germs of disease may be transmitted in the germ-cells as syphilis is known to be. There have been thousands of cases reported of the so-called transmission of mutilations. Absolutely no dependence can be put on the large majority of these because of insufficient data. Moreover, regeneration is so general that it is *à priori* improbable that amputations and the like are ever transmitted.

The acme of attempts at experimental proof is found in the work of John Cossar Ewart, the Scotchman. The experiments in breeding zebras, horses, sheep, dogs, rabbits, etc., that he has carried out are of the highest type of scientific work. Environed

* *American Naturalist*, 23 : 561.

as he is by transmissionists, both as men of science on the one hand and with fanciers on the other, one would expect him to follow. But he does not, and as a conclusion to these remarks and as an expression of what the speaker deems the sentiment of those biologists who have worked more especially in this field, the following summary of his experiments, given by himself before the British Association, is appended: "In my experiments I have never seen anything that would point to the transmission of an acquired character."

NOTE.—Since the above was prepared, a volume from Macmillan & Co., written by Thomas Hunt Morgan and entitled "Evolution and Adaptation" has appeared. In this book is found a treatment of the general subject in the light of recent research. It may be said that the transmission hypothesis is not countenanced by this author.
