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WEISMANN AND GALTON.*

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THE "stirp" of Mr. Galton resembles both the "germ-plasm" and "gemmules" of Professor Weismann. But it differs from gemmules and further resembles germ-plasm in all the following particulars. It is derived from the stirp of proceeding generations, and constitutes the sole basis of heredity. Only a part of it, however, is consumed in each ontogeny—the residue being handed over to "contribute to form the stirps of the offspring," where it undergoes self-multiplication at the expense of the nutriment supplied to it from the somatic system of the offspring, and so on through successive generations. Again, stirp is concerned in all processes of regeneration and repair in the same centrifugal manner as germ-plasm is so concerned. Furthermore, the importance of sexual propagation in the blending of hereditary qualities of the stirp is recognisable, while the principle of panmixia, or the cessation of selection, is entertained, and shown to invalidate the evidence of pangenesis which Darwin derived from the apparently transmitted effects of use and disuse in our domesticated animals. Lastly, it is clearly stated that on the basis supplied by this "theory of heredity," it becomes logically possible to dispense with the Lamarckian principles *in toto*, leaving natural selection as the sole known cause of organic evolution through a perpetual continuity of stirp, together with individual variations of the same, whether by sexual admixture or otherwise.

So far, then, there is not merely resemblance, but virtual identity, between the theories of stirp and germ-plasm. Disregarding certain speculative details, the coincidence is as complete as that between a die and its impress. But although the two theories are thus similar in *logical construction*, they differ in their interpretations of *biological fact*. That is to say, although Galton anticipated by some ten years all the main features of Weismann's theory of heredity,† and showed that, as a matter of form, it was logically intact, he refrained from concluding on this account that it must be the true theory of heredity. He argued, indeed, that in the main it was probably the true theory; but he guarded his presentation of it by not undertaking to deny that there might still be some degree of intercommunication between the material basis of heredity in stirp, and the somatic tissues of successive organisms. The construction of a theory, which, as a matter of theory, could dispense with the Lamarckian principles *in toto*, was seen to be a very different thing from proving, as a matter of fact, that these principles are non-existent—and this, even though it was seen that a recognition of the principle of panmixia must be taken to have considerably attenuated the *degree* of their operation as previously estimated by Darwin in the theory of pangenesis. In short, after pointing out that the doctrine of stirp might very well adopt the position which about a decade later was adopted by the doctrine of germ-plasm—namely, that of altogether *supplanting* the doctrine of gemmules—Galton allowed that this could be done only as a matter of formal speculation; and that, as a matter of real interpretation of the facts of nature, it seemed more judicious to stop at *modifying* the doctrine of gemmules, by provisionally retaining the hypothesis of gemmules, but assigning to their agency a greatly subordinate *rôle*. Or to quote his own words:—

"The conclusion to be drawn from the foregoing arguments is, that we might almost reserve our belief that the structural [i. e., 'somatic'] cells can react on the sexual elements at all, and we may be confident that at the most they do so in a very faint degree; in other words, that acquired modifications are barely, if at all, inherited, in the correct sense of that word. If they were not heritable, then the second group of cases [i. e., those of acquired as distinguished from congenital characters] would vanish, and we should be absolved from all further trouble; if they exist, in however faint a degree, a complete theory of heredity must account for them. I propose, as already stated, to accept the supposition of their being faintly heritable, and to account for them by a modification of Pangenesis." (*Journ. Anthropol. Inst.*, p. 346.)

Seeing, then, that Galton did not undertake to deny a possibly slight influence of somatic tissues on the hereditary qualities of stirp, it follows that he did not have to proceed to those drastic modifications of the general theory of descent which Weismann has attempted. Stirp, like germ-plasm, is *continuous*; but, unlike germ-plasm, it is not *necessarily* or *absolutely* so. Again, stirp, like germ-plasm, is *stable*; yet, unlike germ-plasm, it is not *perpetually* or *unalterably* so. Hence we hear nothing from Galton about our having to explain the unlikeness of our children to ourselves by variations in our protozoan ancestors; nor do we meet with any of those other immense reaches of deductive speculation, which, in my opinion, merely disfigure the republication of stirp under the name of germ-plasm.

Now, I allude to these, the only important points of difference between stirp and germ-plasm, for the sake of drawing prominent attention to the fact that it makes a literally immeasurable difference whether we

† Galton first published his theory in 1872 (*Proc. R. S.*, No. 136), but presented it in a more complete form three years later (*Contemporary Review*, Dec. 1875, and *Journl. Anthropol. Inst.*, 1875).

suppose the material basis of heredity to be *perpetually* continuous and *unalterably* stable, or whether we suppose that it is but *largely* continuous and *highly* stable. In the former case, all the far-reaching deductions which Weismann draws with reference to the general theory of descent—or apart from the more special problem of heredity—follow by way of logical consequence. In the latter case, there is no justification for any such deductions. For, no matter how faintly or how fitfully the hereditary qualities of the material in question may be modified by the somatic tissues in which it resides, or by the external conditions of life to which it is exposed, these disturbances of its absolute stability, and these interruptions of its perpetual continuity, must cause more or less frequent changes on the part of its hereditary qualities—with the result that specific or other modifications of organic types need not have been solely due to the varying admixture of such material in sexual unions on the one hand, or to the unassisted power of natural selection on the other. Numberless additional causes of individual variation are admitted, while the Lamarckian principles are still allowed some degree of play. And although this is a lower degree than Darwin supposed, their influence in determining the course of organic evolution may still have been enormous; seeing that their action, in whatever measure it may be supposed to obtain, must always have been *cumulative* on the one hand, and *directive* of variations in adaptive lines on the other. Or, as Galton himself observes, in the passage already quoted, "if they exist, *in however faint a degree*, a complete theory of heredity must account for them." He saw, indeed, that a most inviting *logical* system could be framed by denying that they can ever exist in any degree—or, in other words, by supposing that stirp was *exactly* the same as what was afterwards called germ-plasm, in that it always occupied a separate "sphere" of its own, where its continuity has been uninterrupted "since the first origin of life." But Galton was not seduced by the temptation to construct an ideally logical system; and he had what I regard as the sound judgment to abstain from carrying his theory of stirp into any such transcendental "sphere" as that which is occupied by Weismann's theory of germ-plasm, in relation to the general doctrine of descent.