

February 5, 1942

Dear Mother,

I am very glad you have taken up the discussion started by Espinasse, for you are one of the few people capable of doing it properly and setting the present position of genetics against its proper background.

Reading your article from the point of view of the general scientist who has not gone into the details of genetic facts or genetic history, I doubt if it is really as good as you could make it; I seen that it leaves such a man no sufficiently emphasised conclusions for him to hang on to and retain as his lodestone when any similar controversy blows along. Having decided actually what points or points, you want to make in this way, I think you should indicate them near the beginning, and state them explicitly near the end, if your article is to have any real effect.

As a tradition, though of course not as a science, genetics is exposed more indefensibly than you seem to admit to the criticism of being anti-Darwinian, not in the Russian sense of theological heresy, but in the equally damning sense of factiously attacking and trying to discredit the far-reaching and penetrating ideas on the means of organic evolution which Darwin had originated. It was

not only Bateson and De Vries, but almost the whole sect of geneticists in the first quarter of this century, who discredited themselves in this way. The ideas of this period are permanently embalmed in amber in Morgan's mind. Writer after writer asserted, or implied, as though it were a demonstrable fact, that species arose by single mutations, and that selection of small continuous variations within the species was known to be inoperative pending the arrival of an appropriate mutation. Continuous or normal distributions were identified by ~~degrees~~^{De Vries} with non-heritable fluctuation. The idea of polygenic Mendelism was frowned upon by both the biometricians and the geneticists when I published the paper you cite, in 1916. It would not have been published had not the cost of publication been reimbursed to the Royal Society of Edinburgh by my friends.

I am very glad that Dobzhansky has grasped, as you mention, the fact that particulate inheritance, ^{so} though far from being antagonistic to Darwin's main theory, actually removed the principal difficulty with which it was encumbered. This assertion was entirely new when I put it forward in 1930. Indeed, before that time I doubt if anyone had taken the trouble to understand why Darwin should have concerned himself so much with Lamarckian effects of changed conditions and increased food as the causes of variation, although, as he shows in many passages, he was clear that, as regards evolutionary effect, such factors were quite subsidiary. The whole distinction between mutation and evolution latent in Darwin's thought was ignored by De Vries and Bateson, and entirely obscured throughout the infancy of genetics.

That Bateson's attitude was determined by jealousy of Poulton, and that De Vries tried to steal Mendel's work, though probably true, are not, of course, facts appropriate to the discussion. I do think, however, that one of the claims of modern genetics to respect as a science must be that it now clearly repudiates the pettiness and prejudices of some of its earlier exponents.

About my Edinburgh paper, did I ever tell you that it was first submitted to the Royal, and that the two referees who were unanimous in its rejection were Pearson and Punnett - it must have been the only subject on which they ever agreed:

Yours sincerely,