1 December 1931.

E.B. Ford, Esq., M.A.,
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My dear Ford:

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Biol. Rev

[CP 93]
JHB

I am sending herewith an offprint of my dominance paper from Fox's journal, which pulls the thing together a good deal better than I had managed to do before, though without extending the discussion to the much bigger subject of dominance theory as affording a logical explanation of dominance bias (heterosis), and hence of the separation of the sexes in motile animals, devices for cross-pollination in plants, unisexual families in the midges, etc.

I have from time to time been pondering over the contents of your book and trying to imagine how you will fill in the sections, a very fascinating and difficult occupation. I can see that it will be a book of very exceptional interest, not only to me but to many others in this country and America, where the younger geneticists, a very numerous body, are showing distinct signs of impatience at the unenterprising imitativeness of genetic text-books, and I think they will appreciate the originality of your outlook.

I have ventured upon a suggestion with respect to chapters 7 and 8, partly, perhaps, because I have not properly grasped the leading thought of your chapter 8. If so, take no notice of ~~any~~ my suggestion. It involves the intrusion into this section of the book of the main facts, relative to genetic theory, about the quantitative characters, which, if ~~it~~ they could be included here, would free you a good deal when you come to statistical methods under "Practical Applications" in Chapter 20. There are, too, a great many misapprehensions about these, which need rubbing in. Only the other day I heard Dr. Dale, of the Hampstead Medical Research place, explaining to his audience that the method by which our domesticated animals and plants had been improved was by an exact genetical analysis of the effect of each factor, followed by matings by which the desired combination was achieved, and intensive inbreeding and ruthless elimination of all other combinations. Having given this account, which is, I believe, entirely untrue of any one character of any one species, he was at pains to add that the mere suggestion of the application of such methods to man — the only methods as far as one could judge of which he thought a Eugenic programme could consist — would antagonise society against all science. Obviously writers on genetics have been a good deal to blame for the misapprehensions under which he was suffering, and you will perhaps agree with me that it is rather in the interest of genetics than other-wise to emphasise the fact that characters like the speed of race-horses, the milk-yield of cows, the fecundity of swine or of poultry, have never been, and, as far as one can judge, never will be analysed into the effects of single factors for the purpose of testing all the possible genetic combinations and establishing the most desirable one.

Another fallacy which has been repeated by I don't know how many writers on genetics is that the normal distribution of a heritable ~~factor~~ character can be explained by the cumulative action of many factors, only if these factors show no dominance. This is a pure fallacy which originated, I think, in the circumstance that the binomial $(1+1)^n$ is symmetrical, while $(3+1)^n$ will be skew for small values of n. Of course, even with complete dominance, there is no reason to suppose that in different factors the dominant genes all have like effects e.g. in head form some dominants may broaden and some lengthen the skull, but even in the extreme case of dominance bias when all dominants have like effects, the distribution tends to become symmetrical as the number of factors is increased; so that an observation that the skewness does not exceed so much only shows, (assuming perfect dominance), that the factors cannot be so few. Also looking at F_3 families in characters showing much heterosis one does frequently find significant skewness in the right direction.

This recalls an even more primitive fallacy which still unquestionably survives, namely that it has been proved by de Vries, or Johannsen, or both, that normally distributed characters which, as such, may be called characters showing fluctuating variation, are purely somatic and not inherited. It is an evasive theory, but none the less quite influential. I suppose it is possible only because many people who really have seen a correlation table showing measurements of parents and offspring, have never acquired the faintest notion of what it means.

I suppose you are always asking for trouble if you consult a friend about a book not yet completely written and published, but I should really much rather you should set aside any suggestion you have doubts about, quite peremptorily, than that, in trying to follow it up, you should be delayed in your work, without feeling that it met its purpose any better than before.

Yours sincerely,

ENCLOSURE:
Offprint from Biological Reviews.