

HARVARD UNIVERSITY  
BUSSEY INSTITUTION FOR RESEARCH IN  
APPLIED BIOLOGY  
FOREST HILLS, BOSTON, MASSACHUSETTS

January 17, 1936

Professor R. A. Fisher  
Galton Laboratory  
University of London  
Gower Street  
London, W. C. 1

Dear Professor Fisher:

I am very glad if we are not very far apart. I want to assure you that if I have misapprehended your views, it is not through a lack of study of your papers. I have read each one at least twice and your book twice, and after receiving your letter, I have re-read certain essential parts. I have the feeling that you have shifted your position materially without making published statements of your changes. In saying this, I have not relied wholly on my own impressions but have presented the material to several of my colleagues, all of whom have replied that they got the same impression that I had.

Perhaps it is true that you have restricted the scope of your theory to major deleterious mutations having no particular evolutionary significance, but certainly your statement in your 1928 paper clearly leads one to believe that your theory was, to a large extent, formulated because you believed the *Drosophila* mutations were a fair sample of all mutations. The first sentence of your '28 paper says: "1. Gene mutations: The very extensive series of gene mutations observed in *Drosophila melanogaster* and allied species is at present by far the most ample basis available for the study of these changes; nevertheless the nature of the mutant genes found in other groups may be cited as indicating that the outstanding characteristics of the *Drosophila* mutations may be characteristic of gene mutations in general." Now, since this paper and certain other paragraphs in your writings are susceptible of misinterpretation of your present views, why don't you write a paper bringing them more up to date? I should be glad to see it. I don't see how I could "correct the false impression which you have given of my views," because unless I published your letter - which I should be glad to do, if you wish - there is nothing I could say. I am sure that a prepared statement would be better than the publication of the letter. I have made

1/17/36

such a statement concerning what I conceive to be a mis-statement of my views about heterostylism. You will find it in the current number of the AMERICAN NATURALIST. The fact that I did not withdraw the so-called balanced lethal hypothesis as an interpretation of the results obtained from the plants descending from Mrs. Barlow's aberrant mids, was not important enough to need a note; but your suggestion gave me the opportunity to think about the straight genetics of linkage changes, and I thought it worth while to note the conclusions I drew.

I understand that we shall have the opportunity of welcoming you at Harvard next summer. I hope that the rumour is true, and that we can get together for some discussions.

Sincerely yours,

*Em. East*

EM:E:R