

1st January 1934.

Dr. J. Rasmusson,  
Sveriges Utsadesforening,  
Svalof,  
Sweden.

My dear Rasmusson,

I have just received your very welcome letter of December the 21st, but have scarcely considered all the good points in it. I am very glad that we do not really disagree as to the possible influence of duplicate factors, and its relation to interaction, which term I have been inclined to think of rather physiologically than genetically. If such a distinction may be permitted. I mean that the effect on the gene might be expressible to a good approximation in terms of some pheno-typical quantity, such as the height of plant. At different heights then, the gene would have different effects, but at the same height much the same effect by whatever complex of other genes that height is determined. This would be what I think of a physiological kind of interaction, but it might be also that the effect of a gene is expressible to a good approximation in terms of the other genes present, or some few of them, and not simply related

to their aggregate pheno-typical expression, and this I would call a genetical interaction. Some day you and I must devise experimental procedures fit to disentangle these two possibilities.

I do not at all understand Haldane's remark about "Dominance Theory". I am in doubt, as I suppose all good men of science must be, in the sense that there is very little that I would wish to be dogmatic about, but I am more firmly convinced than I was when I wrote in 1928, and not less firmly so as to (1) the modifiability of dominance, (2) that most mutations now recessive have become so progressively since their first appearance. (3) That the dominants in Polymorphic species produce external effects, which are beneficial and balanced in nature by a lower viability in the homozygote. (4) That most of the so called dominants in poultry are really quite incompletely dominant. There is a great deal more that I should like to be sure of, especially in relation to the complex linkage systems in the Polymorphic species.

I was interested in re-reading East and Jones's "Inbreeding and outbreeding" to see what I had overlooked, that in 1919 they already felt the need of an evolutionary explanation for the great excess of

recessives among mutations, and suggests that natural selection has eliminated those types which would be most inclined to dominant mutations. They do not however, discuss numerically the selective intensity available to alter the mutation rates, and indeed such a selective action would really be trifling in magnitude for mutation rates not much higher than one in a million. It might, I think, be reasonably argued that the type of selection suggested by East and Jones provides the reason why mutation rates in general do not seem as high as one in a thousand, or one in ten.

With best wishes to Mrs Rasmusson and Karle for the New Year.

Yours sincerely,