

26th October 1928.

Dr L.C. Dunn,
Columbia,
N.Y.C.

Dear Dunn,

Many thanks for your letter, which I was particularly glad to receive, as I was beginning to think that you did not see much in my suggestion as to dominance, and I was rather eager to have your judgment upon it. I very much agree with you that we have to do with the dependence of gene expression on the whole hereditary gene background; so much so that I can scarcely find a meaning to put to the phrase "dominance per se."

I should be glad of a reference to Lancefield's case of forked, and to hear more about it in other ways. From the facts you mention, the heterozygote is modified (in the direction of the homozygote mutant) by another factor, but is this factor without effect on the two homozygotes? To determine this would seem to need careful experimentation, though this should be fairly easy in Drosophila. You will see that I want more evidence as to whether the heterozygote is in fact more modifiable than the homozygotes, as is reasonable on biochemical grounds. I hope to get Crinkled Dwarf bred back to (say) Peruvian, so as to determine what its homozygote looks like on a Peruvian background. Has it been modified too, and if so how much compared to the heterozygote?

I wonder how confidently you ought to say that dominance is practically never complete. Nothing is easier than to get some evidence of intermediacy, if the crossed forms differ in more than one factor, as is clearly apparent in my mice, and it is not easy to devise an experiment which excludes such a bias. The best cases available seem to be provided by mutants at their first appearance, and with these is not the heterozygote very often indistinguishable from the wild? How does black and tan react with the other 4 allelomorphs?

As to my hen experiment, I doubt if I have put the problem sufficiently clearly. To obtain a strain, in which the ^{white} heterozygote was clearly intermediate between the homozygotes, would, indeed, support my theory in general, by showing that dominance could be modified by selection, but what would clinch my special assumption in respect to Gallus, would be to show that the wild ^{an} ^v Bankira is already such a strain. I should be tremendously interested if it were possible on your side to perform the crucial experiment.

My light line have also thrown a lighter line, but not, as I judge, so extreme as yours. Mine all have one or more small coloured patches near the tails. You know that I have stuff clean enough to show the head factor quite clearly, which is at any time at your service. I am putting the extra lightener through into dominant pied, where it will have, I suspect, a better chance of showing up; what do you think of this on principle?

How do centres of depigmentation fit with white areas

this shape



and dark areas like this



I should like to see Iljin's paper, or your abstract rather, when you have a spare.

Yours sincerely,