26 January 1932.

Professor C.G. Darwin, F.R.S.,
14 Heriot Row,
EDINBURGH.

Dear Prof. Darwin:

Naturally I have thought a good deal, from time to time, on your query about dominance, and I am very glad you have developed it, because it has made me think more about it from a rather fundamental aspect. I fancy one might put it this way.

Jones is a character of whom we know something only because Smith tells a story about him in his (Smith's) autobiography. One might say that Smith's autobiography is what it is because of people like Jones. Seeing that I have introduced a new class of people who exist, as far as we are concerned, only as characters in other peoples' autobiographies, you invite me to raise veil after veil of the transformation scene, and reveal a further class who exist only in the autobiographies of the Jones's, and so ad infinitum; but I can stop short at Jones because I do not know that he ever wrote an autobiography, Smith's yarn only involves Jones himself. Of course he may have, and I should be

much interested if I could meet him in propria persona, and find out if he has, but the fact with which we state that Smith's autobiography consists largely of anecdotes about Jones does not itself imply the like fact about Jones.

Now Harland in Trinidad is now in a position to say that Sea Island differs from the other New World cottons (i) in mutating to Crinkled Dwarf, and (ii) in a bunch of modifying factors which conspire to make Crinkled Dwarf recessive. He might now go further and isolate one or more of these modifying factors. I am afraid he will not, as it would probably be a very tedious job, and he is apparently all on fire to use the differences in dominance which his backcrosses have brought out, as a new taxonomic character for the classification of the New World cottons. Supposing, however, that he did so, and found that they all displayed complete dominance, i.e. that in every case either the gene in Crinkled Dwarf or its opposite number in Upland was dominant, then I should be taken aback, and have to admit that your suspicion was well grounded, and that there was something more behind. But it might well be that in most or all of these factors there was no dominance, the heterozygote being intermediate (in its effect upon the dominance of Crinkled Dwarf) between the two homozygotes. Thus if the whole effect were due to one modifier (which it is not) we might designate Crinkled Dwarf by C^a and its normal allelomorph by C, while M and m stand for the two

allelomorphs of the modifier, and get the scheme for the 9 possible kinds of plants

	CC	Cc	cc
MM	Normal	Normal	Dwarfed
Mm	Normal	¼ Dwarfed	Dwarfed
mm	Normal	½ Dwarfed	Dwarfed

The modifier would in fact behave as in the blue Andulusian fowl, or the English white rabbit, where the heterozygote has been cultivated, and doubtless modified by the fanciers, and differs greatly from both the homozygotes.

Some years ago one of the American geneticists called attention to Lancefield's Semi-forked mutant in Drosophila, which abolishes the recessiveness of the previously known mutant Forked, and which is itself a complete recessive. He thought it strongly supported my theory, but I was really embarassed by the recessiveness of the modifier, which showed on my view that it also had a history, i.e. had been modified. I am inclined to guess in such a case that Semi-forked had once had a considerable effect in its own right, had gradually become recessive, and then had gradually had its visible effect obliterated even in the homozygote, but that its developmental effect could still be brought to light by the simultaneous presence of the mutant Forked, which perhaps intensifies some chemical difference which Semi-forked normally produces.

I mention this case to show that I do not want to be dogmatic in denying that these modifiers may not sometimes

have acquired dominance, while sticking to it that I am under no obligation to admit that they have.

Thanks for telling me about the book. I shall be glad to get a copy while I can.

Yours sincerely,