27 January 1932.

Dr. S.C. Harland,
Cotton Research Station,
TRINIDAD, B.W.I.

Dear Dr. Harland:

Many thanks for sending me your paper which I return herewith. I was very glad to have a full account of your experiments with Crinkled Dwarf; as I was not previously clear as to how extensive they were on the different points involved.

It would be most interesting to have further data, when these become possible, on back-crosses, carried out as in Upland, with other sub-species of the Peruvian group; and, in the case of Upland, also on the characteristics of homozygotes derived, say, from the fourth back-cross. The increasing constancy which you stress in the case of the heterozygotes, combined probably with some increase in constancy of the homozygotes, would probably make the two types clearly distinguishable at this stage, and enable one to speak confidently as to whether there is any evidence as to dominance or not.

By the way, homozygous on page 12 should, I suppose, be homogeneous. On the same page I have missed the basis

of your remark that "we have seen that all Uplands are not alike in their reactions to Crinkled", for I think you report only one Upland cross with its subsequent back-crosses. A priori I should think it very probable that Upland, and other species in which Crinkled Dwarf had not occurred, were already segregating for genes capable of influencing its manifestation, but it would be most interesting if you had different parallel crosses with different Upland varieties.

Is there any other sub-species of the Peruvian group in which sufficient inbred material has been grown and examined for one to say, as in the case of Upland, that the mutation does not occur?

I see in your discussion that you are inclined to be cautious, and, no doubt, it is still wise to be so. Wright's calculations, however, as he now fully realises, added nothing to those on which the theory was originally based, and his argument that the modifying factors must be very insensible to selection misses the point that exactly the same effects would be brought about, and at the same speed, with modifiers selected on their own account, merely by accelerating the progress of some and retarding that of others. Probably however the fact in this connection which needs emphasizing most is that with very moderate cross-fertilization the ancestry of every individual of a species (apart from well separated sub-species)

is practically identical beyond the last hundred generations. So that to say that 1 in 50,000 has been heterozygous is to say that every single line of the ramifying ancestry of existing plants has passed through about 20 heterozygotes in the last million generations. From this point of view there is no mystery about species having become modified independently in their reaction to any number of different mutants; without normals descended recently from heterozygotes being specially favoured.

I hope you will give us lots more of the same sort. Is there any chance of your striking another taxonomically delimited mutation?

Yours sincerely,