Thursby.
Carlisle.
Cumberland.

January 3rd 1936.

My dear Fisher,

I am *most* grateful for your kindness in looking through that long paper so thoroughly. It will benefit immensely from your suggestions, which I am carefully following.

Of course my object in writing is the hope that it may lead people who have the chance of breeding *dardanus* to do work of genetic value. Also to point out with how little extra trouble their results might become of real use. For I do think these polymorphic forms important.

The preliminary description

of races and forms etc is necessary (it has never been at all fully done), but I think it has become too long. I may say that Poulton has been urging me to put in more and more, and it is with difficulty that I have kept it down as much. He is so keen, and so anxious to be helpful, that it is not easy to resist him. The point is that he has long wanted to monograph dardanus himself, and I am afraid he feels that he has left it too late, and that such a task is rather beyond him now (he is nearly 80, you know). He sees in this paper a chance to get a number of things published which might otherwise never appear, and is therefore anxious to retain the non-genetic parts. Naturally, in so far as it does not outweigh the genetic side too much, I am most glad to help by putting in what he wants.

I am rather appalled by your discovery. Your analysis is a remarkable piece of work, but what it reveals is really very shocking. Clearly, as you say, Mendel himself is not to blame. He must have set to work with no idea what ratios to expect. ~~[illegible]~~ (Dominance could never have been allowed for at this stage, in a — theoretically possible —

with the complication of 3:1 instead of 1:2:1, brilliant mental construct, and without any knowledge of cytology at all, it is hardly theoretically possible, after all). So all the foundation must be based on real results. And it is simply incredible that a man of his intelligence could want to fake, <u>after</u> he had found out what to look for by honest work.

When you write, do stress that Mendel's greatness lies not so much in his discoveries as in his deductions – and in planning the work. I mean you may feel it too obvious to say so, but many biologists don't seem to realize it. It is important to point out that other people had noticed segregation

(Romanes did – in rabbits): and though possessed of the very clue which Mendel used so effectively, entirely failed to grasp it.

I think you are right: there must have been an assistant. It is some time since I read Mendel's originals, and one might forget such a point, but I don't recall any suggestion that he was helped. If he was – and I think he must have been – he is at fault in not saying so.

Too much has hung on Mendel's results to suppress the matter: though one does not want to wash dirty linen in public unnecessarily. I think you are bound to publish. To take a relevant parallel: it seems an entirely different matter from that affair in 1900 – carrying a rather unpleasant suggestion – when de Vries first published Mendel's results and omitted Mendel's name. Then (on hearing that they had been found independently by the other two) hurriedly published them again, adding the name. Now that seems the sort of thing better forgotten. It *may* have been a mistake! At any rate de Vries himself did his best to put it right. I never mention the affair to students, or in lectures. But what you have found is quite different.

Please excuse such a long letter not type-written. I look forward immensely to seeing your paper on the Mendelian work.

With my very best thanks for all your kind help.

Yours sincerely,

E. B. Ford.