6 - J.A.B.

January 2nd, 1935

Dear Ford,

It was exceedingly good of you to send me your paper on Dardanus. I do think, as you know, that it is an extraordinarily valuable thing to set out together the records you have collected with provisional inferences, as the basis of future work.

It was certainly partly because I first tried to read your paper too hurriedly that I felt a little disappointed at the first reading: I mean that it did not leave so distinct an impression as I expected. I see now that this is partly because the necessary preliminary description of races and forms bulks so large at the beginning of your paper, and this requires that the genetic discussion which follows should be more strongly emphasized.

I have made some notes on this section which I enclose, but by far the most important is the last. I believe you must never expect readers to turn over pages in order to find frequency data. Further, I do not think as many as one reader in ten will look at the tables at all, but only such parts as you fish out for discussion. So I should quote quite extensively.

Do not for a moment let what I have said dishearten

you. It is really a question of emphasis only, and I think the most effective method of emphasising anything is to be absolutely sure of the clarity of what you intend to convey.

On quite another matter I have had the shocking experience lately of coming to the conclusion that the data given in Mendel's paper must be practically all faked. I cannot conceive that Mendel himself had any hand in it, and quite independently, and this is what I was really studying his paper for, I have come to the conclusion that his experiments were planned and set out exactly as he records. I mean, for example, that his primary crosses really were unifactorial, and that he had carefully selected them to be so. So, if the data were faked, I presume it was by some assistant who knew too well what was expected.

The first thing that struck me was that in testing homozygosity in plant characters Mendel used F_3 progenies of only ten and did not notice that the chance of a heterozygote being misclassified as a homozygote is not negligible, being between 5% and 6%. None the less Mendel's data agree with the 2 : 1 ratio requiring a compensating chance deviation which would only come about once in thirty trials. And then the same thing happens again later, and there is not a sign that Mendel saw the complication and allowed for it.

Now, when data have been faked, I know very well how generally people underestimate the frequency of wide-chance deviations, so that the tendency is always to make them agree too well with expectation. So I tested all the larger experiments and, finally, the whole of his recorded data, and in the aggregate the deviations are shockingly too small with χ^2 about 50 for 64 degrees of freedom. I have divided up the data in several different ways to try to get a further clue, e.g., by years and by the absolute sizes of the numbers, but as far as one can judge the subnormality seems to be uniform in these respects. The only subdivision which seems to make any difference is that those fifteen degrees of freedom for which bias has also been corrected have been less stringently adjusted to expectation than the remaining forty-nine where there was no original bias. It may be that when there was bias only the deviations on one side were adjusted, but beyond that possibility I can get no clue to the method of doctoring. As I said, I don't believe this touches Mendel's own bona-fides or the reality of the experiments he carried out; and I do not think it has any bearing on the way in which his contemporaries in Germany ignored his results. After all, Darwin's more prolonged experiments on cross and self-fertilisation, in spite of his great reputation, led to

nothing further at the time, and even a longer period elapsed between 1876, when he published his results, and the American work on inbreeding than elapsed between 1866 and 1900.

I was engaged on writing a paper under the title "Has Mendel's work been rediscovered" when I made my own abominable discovery. I suppose the title must stand with more irony than I had meant.

Yours sincerely,

Notes

p.18. From a ratio of 3 Hippocoon to 1 Dionysus which would be expected if Diouysus were recessive. The deviation from this theory is measured by χ^2 = 4.41 for one degree of freedom, or in other words the deviation is 2.1 times its standard error, a magnitude which would usually be taken as significant.

p.19 paragraph running on to p.20 leaves an impression of obscurity. Possibly the same thing could be said in a different order.

p.20. I believe everywhere in the genetic section you ought to quote the composition of the families to which you refer, and perhaps to make small tables of them so that the reader has before him each bit of evidence as it is discussed.