February 11th, 1936

My dear Rasmussen,

Many thanks for returning my paper on Mendel, and for the trouble you have taken in considering my problems. I should like to quote, probably as footnotes, some of your comments, which I think will be most valuable. As I have taken the liberty of rewording them slightly for publication in English I am sending you exactly what I propose to say on some of the points you raise, though I am writing at present from home and without having seen Iltis's book, which is probably now awaiting me in London.

(a) footnote

I am obliged to Dr J. Rasmussen who has extensive experience of genetical work with <u>Pisum</u> for the following explanation of Mendel's probable method of selection:

"It is my impression that the classification was made throughout on dry plants in Winter. That is to say that Mendel harvested his plants in Autumn, probably ~~tried~~ tied them up plot by plot, and for scoring loosened up the bundh of plants and picked out from it one plant after another. That is the method which first presents itself in work of this kind; it is also the method I am accustomed to use. The fact is that, working in this way, one will unconsciously choose the best plants first. This happens to me, whether I do the work myself or have other people picking out the plants from the bunch."

(b) In respect to the average yield Dr Rasmussen also says:-

"About 30 good seeds per plant is, under Mendel's conditions (dry climate, early ripening, and attacks of Bruchus pisi) by no means a low number. It seems to me indeed rather a good one, and I feel convinced that Mendel classified all the seeds from these plants."

The question whether the bias in the F_3 data can be explained by vigour associated with heterozygosis seems to require a good deal of consideration, and there are some points which you may or may not have weighed fully before writing to me.

It was the discrepancy of the F_3 ratios which first led me to suspect sophistication, but this suspicion was immediately and strongly confirmed by the χ^2 test. For example, setting other evidence aside, is there any other explanation of the far too close agreement of the 4 frequencies in each of the five experiments on gametic ratios?

As I see the evidence now I think something should be said of the increased size often found on comparing heterozygotes with their parents, and which may sometimes be found on comparing heterozygotes with their sibs, though this is a very different comparison. I should, at present, be inclined to add something like the following, on which I should much value your opinion, though I should also be glad to insert as yours any additional comment which you would care to make.

(c) An explanation of a different type is that the selection of plants for testing favoured the heterozygotes. In the first series of experiments the selection might have been made in the garden, or, if the whole crop was harvested, on the dry plants. In either case the larger plants might have been unconsciously preferred. It is also not impossible that, in some crosses at least, the heterozygotes may have been on the average larger than sister homozygotes. The difficulties to accepting such an explanation as complete are three. (i) In the tri-factorial experiment there was no selection, for all plants grown must have been tested. The results here do not, however, differ in the postulated direction from those of the first series. On the contrary, they show an even larger discrepancy. (ii) It is improbable that the supposed compensating selection of heterozygotes should have been equally effective in the case of five different factors. (iii) The total compensation for all five factors (21.6 plants) must be supposed to be greater than would be needed (16.8 plants) if families of 11 had been grown, and less than would be needed (30.0) if nine only had been grown, though nearly exactly right for the actual number 10 of F_3 plants in each progeny (22.5)

You will, perhaps, agree that if the selection of heterozygotes in preference to sister homozygotes has in fact been acting it will only explain Mendel's record if supplemented by a number of further postulates, each not, perhaps, impossible, but somewhat improbable, e.g. that Mendel used a different and more exacting technique in the trifactorial experiment, and that sampling errors in his experiments as a whole were very funnily compensated, whether he was classifying plants or seeds.

As a popular teacher, Mendel may, from time to time, have relied on students at the technical high-school to assist him, and to interest them in his work.

More probably, perhaps, he relied on novices or young members of his fraternity. Nevertheless, I would not put the drunk old gardener out of the question. I must see what Iltis says about him. That he was often drunk is no evidence that he was not crafty enough to deceive his chief, if he thought it would pay him, but it is, I think, good evidence that Mendel would not have relied on him for an accurate record. Still, my real reason for postulating an assistant is that I cannot conceive the man who had made Mendel's discovery cooking the data. After all he was not a Slav!

d) It is particularly gratifying that this conclusion is supported by Dr Rasmussen, basing his opinion upon existing types of garden peas, and on the development of these types since Mendel's time. He writes:-

> "From the most probable assortment of varieties available to Mendel there would be no difficulty whatever in making unifactorial crosses in all characters. Indeed the assortment at hand seems to have been much better fitted for such crosses than for other combinations"

I do hope you will find time to lunch with me when you are here towards the end of the month.

Yours sincerely,

Mrs Fisher sends her greetings.
She was as interested as
myself in your notes.