JOHN INNES HORTICULTURAL INSTITUTION
MOSTYN ROAD, MERTON PARK, LONDON, S.W.19
Telephone LIBerty 3645

16th June 1942.

Dear Professor,

It was very kind of you to go through my paper in such a thorough manner. I can see that I have not made myself clear in some cases, especially in the last section, on breeding systems. I certainly had no intention of giving the impression that I thought the majority of plants had systems which would see them through the next era or so, nor did I intend to ~~be~~ give the impression that I fancied that plants predicted the conditions to which their descendants would be subjected. I think that much of this turns on my use of the word future. I apparently had not made it clear that I was referring to the future of plants in the long past, whose descendents we can now examine, and only by inference was referring to the fate of existing organisms and their descendents. Perhaps we shall have an opportunity to go into this a little more on Thursday when I see you at the Piccadilly Hotel. I should also like to discuss the definition of polygenic variation with you ~~here~~ if possible.

The lamentable oversight by which I omitted to acknowledge your suggesting the cave animals to me has now been rectified. I am quite ashamed of it. If I remember correctly you suggested it in the train coming back from Cambridge last December, the day after Wigam had given an exposition of the general idea of polygene intermingling and correlated responses to selection, with special reference to fertility in some of our experiments.

I am writing to Ford for a reference to Spooner on Gammarus. I am very glad to know of this ~~case~~ case. I noticed that you altered one of my references (to your "Genetical Theory of Natural Selection", on the conservation of variability) to a different paper in, I believe Science Progress (1932). I did not know of this paper, and it is not in your list of papers in "Statistical Methods". Could you please give me the full reference, and also a reprint if available and not too precious.

I was glad to have Ford's comments, though in some cases, I think that he would have found the answer to his queries if he had read on a little. This was especially true of his remarks on the last section, which, I remember, he said he was forced to read rather hurriedly.

I am going over the whole text again now, and am hoping to remove most of the things to which you objected though there are a few which I am prepared to defend. As you said in your letter, many of the questions will require more critical analysis in the future. I could not undertake this at present, partly because of space difficulties and partly because of the paucity of data. Heterosis is the question I am particularly anxious to deal with next, and I am hoping to have some experimental data in two years time.

Yours sincerely,

K. Mather