

January 11th. 1936.

DEPARTMENT OF
ZOOLOGY AND COMPARATIVE ANATOMY,
UNIVERSITY MUSEUM, OXFORD.

Dear Fisher,

I am immensely obliged to you for allowing me to read your splendid paper. It is remarkable that so much that is new can be extracted from Mendel's account. You have treated the difficult matter of the faked data in a most tactful way.

In the left hand margin I have corrected a few unimportant typing errors, and suggested one or two slight additions of punctuation and the like. These are on pp. 1,2,3,12,17,20,21,22,29,34,35,36. All are in pencil, and so can easily be removed if not required. The only sentence which did not read quite clearly to me the first time is p.10, line 8 from bottom. I think the two commas suggested would help it.

The only suggestion which occurs to me, and that a tentative one, is that the paper might be provided with a summary. I always think this rather helpful, and the end seemed perhaps a little abrupt. You see you have naturally had to be so careful in your statements on the faking, that the point seems rather immersed in the paper

as a whole. A summary would obviate this.

The most exciting idea is your suggestion that Mendel may possibly have deduced his laws originally as a mental construct - It would be difficult to pay him a higher compliment. Assuming that this is so (and it does seem possible now), it raises a difficulty in regard to the strategy (apart from the tactics) of Mendel's work. In such circumstances he must have reached his conclusions as a wide generalization. So it seems extraordinary that he should verify them, and (in particular) demonstrate them to ~~some~~ ^{other} people, with reference to a single species. Had he used ^{only} enough characters in Pisum to demonstrate independent assortment, and employed the time so gained in demonstrating his ratios between a pair of characters in even two other widely different organisms (say an animal and a very different plant), his position would have been immensely stronger. It would then have been clear, perhaps with no more work than the peas required, that he had discovered a principle of wide, instead of local, application.

DEPARTMENT OF
ZOOLOGY AND COMPARATIVE ANATOMY,
UNIVERSITY MUSEUM, OXFORD.

Again, many thanks for letting me see this.
It has been a great pleasure - it is certain to cause
wide interest. I dont know where you are going to publish.
But where ever it is, there are certain to be poeple
interested in the history of biological theory who will
not see it unless you also send a short account to Nature.
I hope you will do this.

Yours sincerely,

E. B. Ford