

16 MUSEUM ROAD,
OXFORD.

February 24th: 1930.

Dear Fisher,

I am very greatly obliged to you for so kindly giving me your opinion on the paper I wrote. I am glad you think it worth publishing and your comments are most valuable and a real help.

As I expect you guessed, I had not seen Waldman's paper when I wrote. The Radcliffe Library gets its American publications late. The American Naturalist did not arrive until the end of January. As there are so many foreign things I go through them in rotation and, being so busy recently with a member of the staff away ill, I had not

come round to it! So I was quite ignorant of it when I sent you the paper, but saw it I think the next day. Had I not it before I would have written differently, but now I am referring to it in appropriate places.

I find all your comments most useful and am adopting them in practically every case. Still, granting the great probability of "saturation values" for genes which act quantitatively, I can not help feeling that the value of a single dose of a new mutation may sometimes fall above saturation level. Dominance

would have to be produced at the outset without selection. Though I do not suggest such a condition would be common, it might explain certain cases. But I fully realize that saturation level may be altered subsequently by changes in the gene complex, and this point I am stressing rather more. I feel also that your interesting alternative suggestion in regard to dimorphism should be given. Shall I include it (with an acknowledgment naturally)? Or (far better) are you writing on it?

I find the data on traits

columns much less satisfactory, but the
specimen's slope is correct beyond all
doubt. So I thought it better to stick to
that.

I would have answered before,
but had to go to London this week end.
Thank you again for the trouble you took,
I appreciate it very much.

Yours sincerely,
E. B. Ford