

June 7, 1940

Dear Bliss,

I have been looking at your letter of July 28th last year, of which you have just sent me a copy, and at my previous letter of June 2nd on the same problem. To answer your letter, however, is like answering a fairly stiff examination paper: three hours allowed; ~~not~~ more than four questions to be attempted!

I am glad you tried how the process I illustrated in my letter would develop itself from scratch. Although the actual variation for each of the 10 treatments, without block correction, gives a fair start, in principle I suppose one should use the block correction at this stage. You say, p.2. "Biologically speaking, the differences between blocks would be interpreted as an increase or decrease in overall susceptibility ..", and propose giving different weights to the different blocks in accordance with their mean responses. Now, it seems to me that experimentally the causes of block differences, whatever they may be, ^{might} ~~must~~ act so as to give an increased susceptibility without the increased precision which would accompany such increased susceptibility if brought about by greater duration of exposure. One might try, I suppose, to see whether the different

experiments showed unequal variability, using the weighted mean square of the residuals from each. If one were sure of heterogeneity among them in this respect, one could either make them inverse to the weighted / mean squares obtained, or find a regression of the weighted mean square on the weighted mean, and use the latter as the basis for weighting. There is a good deal that I do not like about either process, and I feel sure that, unless the data actually scream for it, as in the case of your ^{ten} mean treatments, it is preferable to avoid weighting the observations, and, if one must weight them, it is important to do so on the basis of some formula in terms of treatment value, rather than in terms of mean response.

On p.3 you ask about degrees of freedom: I do not think that adjusting the weights so as to make the sum of wy^2 approximately constant for all 10 treatments can be said to absorb any d.f. in the variation of y , e.g., if one had only 2 d.f. left over, instead of 72, I do not think that these could be made to vanish by adjusting the weights.

Next, about the interactions blocks X level and blocks X slope, I imagine that the former gives the approximate standard error for one's evaluation of the effect of depth, and the latter for one's estimate of the slope, but I imagine also that each raises technical problems as to the conduct of the experiment which I am not in a position even to perceive. The fact that the residual error is so much smaller than these does, however, suggest that by special care the different experiments could be made much more comparable than in fact they are. In spite of awkwardness, I should imagine the two

interactions must be used separately unless, in fact, they can be traced to the same physical cause.

If the weights used were true amounts of information, the sum of squares would in fact be X^2 , but, in fact, we are very much in the dark as to what weights would be best. Your analysis suggests that perhaps better results would come from $b = 3$ or so.

^{For given}
~~In giving~~ weights I think you could compute missing observations by maximizing the treatment ^{1.4}effects, or the two d.f. in them, which seem to be real compared with residual error, but I suppose to some extent ~~one~~ ^{the} missing observation might also influence the weighting estimates. I am quite convinced that the use of weights, even at their simplest, is beyond the average worker. You set a high standard in this respect.

With respect to the sex hormone experiment, you seem to have got the departure from regression well within sight of the variation within groups, but I do not know how good is the evidence for your statement that there is a linear relation between log I and x. This should certainly be a most useful type of relationship where it exists, as in the Lactuca.

Thanks for suggesting a joint paper. No, I will not collaborate in this way; but do not on that account refrain from peppering me with your stimulating enquiries. Make any use you like of what I write to you.

Yours sincerely,