

20th May, 1935

Dr. J.C.Drummond,
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W.C.1

Dear Drummond,

I am afraid I shall have to bother you to let me know more fully why you were inclined to question the validity of the experiment reported by Simpson and Wood, as, on looking it over, it seemed to me to be, at face value, an exceptionally careful and well designed experiment; and, assuming basic honesty, I do not see how the statistical significance of the results can be doubted. In saying this, you will understand that I am not saying it of the medical interpretations which are put on these results.

The two ways in which precision can ordinarily be increased, by increasing the number on which the totals or averages are based, and by the elimination of disturbing factors by a more or less elaborate system of control, are both so commendable in theory, that I hate to see an advocate of one decrying the other, as so often happens. I hope we are getting past the stage at which any experimenter will ignore homogeneity and comparability, and the other items of adequate control, and put his money on a mere increase in numbers, in the belief that these will average out all causes of discrepancy; or when any

opponent of this view will ignore the necessity of adequate replication. The question of adequate numbers really turns on the question whether another batch of 96 children taken from the same milieu, would have responded appreciably differently, or, in other words, on whether the sampling errors have been validly estimated. Judged strictly, the experiment has one weak point. There are, strictly, no parallels, and, for an absolutely satisfactory estimate of error, it would have been more convincing had pairs of children, instead of single children, been put through the diet sequences chosen; instead of the tests of significance being based, as they are, on pairs of children receiving the same diet for a period, but as part of a different sequence of diets. But, in using this as a criticism, it is only fair to say that, in so far as the sequence makes any difference, it will have tended to increase the error as estimated, and so to give a lower idea of significance than would have been gained from different children treated strictly in parallel.

I should like to have seen a full analysis of variance, so as to assure myself that the higher order interactions gave no reason for suspicion, and, as I see that J.O.Irwin was advising the experimenters, I should think I could get this from him as I have no doubt he has done it in full.

If you think it worth while, I will do this.

Yours sincerely,