

1938 June 2.

Dear Fisher,

Thanks very much for your informative letter - dated May 30, but it only turned up this morning. I had not thought of the harmonic variation in the level of the field - I don't know the southern word; in Northumberland they are called rigs. If the arrangement is along and across the rigs it will help a lot to make the xy term negligible. I rather think that some people have a habit of randomizing one square and then just repeating it. This would be harmless in some circumstances, but with three replications the chance of at least one treatment being associated every time with positive Σxy is not negligible, ~~if Σxy is not negligible~~ and if the arrangement is repeated this will become a $\frac{1}{2}$ chance that every treatment gets Σxy with the same sign every time.

I should analyse your telepathy question by setting up q as a hypothesis of complete randomness; but there would be no need to confine my attention to one type of systematic departure. Thus I could consider $\approx q$ and $\approx q$, where a and b are two different adjustable parameters. Testing them in turn against q I should get

$$K_a = P(\approx q | \mathcal{O}) / P(\approx q | \mathcal{O}); \quad K_b = P(\approx q | \mathcal{O}) / P(\approx q | \mathcal{O})$$

It might well happen that both of these came out small, and q must be rejected in any case. But then by division

$$P(\approx q | \mathcal{O}) / P(\approx q | \mathcal{O}) = K_a / K_b$$

and the alternative to be asserted is the one that gives the smaller K . Then if this is b , we can introduce a as an alternative

and work out $P(\tilde{z}_1 | a) / P(\tilde{z}_1, \tilde{z}_2 | a)$, asserting the significance of a if this comes out less than 1. This would deal with the possibility that the assertion of b may explain all the evidence that would otherwise appear to support a. That bother about the node of Venus is a case in point. As the data stand they support a systematic anomaly against complete randomness; but the possibility of an internal correlation in the errors without a systematic anomaly might explain the evidence if the data were presented in a form capable of testing it. There is another case in the lunar nutation; there is a discrepancy between the observed value and that found from the independent evidence of the mass of the moon, which obstinately stays between 2 and 3 times the standard error as the latter diminishes. Nasty little internal correlations running through a long series of observations may play havoc with one's estimate of uncertainty; and one can't randomize which observations are to be taken by Bradley, Hinks, or Jackson.

By the way did you go to the Physical Society last Friday? I had to be at a geophysical discussion at the R.A.S.; I told the secretary about it twice, but he put me on the card nevertheless. Campbell's paper was remarkably fatuous; to test whether his throws were in accordance with the hypothesis of a chance he should have given the individual results or at least those of consecutive blocks of 10, but that was just what he didn't give.

Yours sincerely,

Harold Jefferys
