

ROTHAMSTED EXPERIMENTAL STATION

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HARPENDEN
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My dear Ron,

I am now returning your paper. By and large I am in agreement with it, but the fact that people such as Barnard find difficulties suggests that some further discussion may be worth while.

The crux of the matter is, I suppose, what is meant by "Once in ten (or 1/P) trials" (your p.8). Welch can claim for his test that this property holds in repeated sampling from the populations with the same σ_1/σ_2 , but only by ignoring the actually observed ratio s_1/s_2 (or should I rather say, using it to enter the table with, but ignoring the other information it contains).

Logically I think every right minded person would agree that all relevant information should as far as possible be taken into account when making a test of significance. The snag is in the phrase "as far as possible". We are, I believe, fully agreed that vague and indefinite information (such as that of the a priori distribution type) is best excluded from formal tests and taken into account in the subsequent overall assessment (or should I say, considered judgement) which rational beings normally make after doing their figuring. But once it is admitted that certain information can be excluded the question arises where to draw the line? In the analysis of the results of an experiment we agree to ignore the information provided by the actual pattern of treatments in that particular experiment and merely to utilize the information that the design was selected at random from a set of designs having certain properties. This is justified by just the argument which you condemn in your reply to George Barnard, and the purist, using the same argument might well say the fact that he gets too small a chance of significance when he was lucky enough to select a Knut Vick 5 x 5 Latin square is not compensated for by the fact that he gets too high a chance in the diagonal square. And that, if that is the best the statistician can do he proposes to use Knut Vick squares in future, which have been shown statistically (as common sense would indicate) to be the more accurate. (I know there are other arguments

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Is there a test
as to how to limit
Vick's?

against this course, but I don't think they are relevant to the present issues.)

I bring up this point because it does show that the statistician considers himself free to ignore inconvenient information, and also emphasises, and rightly I think, that some value is attached to getting 1/P significance in repeated sampling from the same population. The fact that in some common cases, such as the ordinary t test, this occurs while at the same time taking into account all relevant information, has rather blinded people to the fact that it does not necessarily occur. But it might be worth while giving examples in this paper - I have particularly in mind the linear regression case, and the 2 x 2 contingency table. *The second paragraph of your letter of 19th is also very relevant.*

On the paper as it is at present I have little detailed comment, except that I am not very happy about your comments on the quotations. James, I think, might well be dropped. As I see it the reason for his footnote was that he had himself cast doubts in the text on the mathematical accuracy of Welch's solution. I don't think it is a propos de rien - indeed, I might well have added just such a footnote myself in similar circumstances. The other people have taken the stand they do because they regard 1/P significance in reputed repeated sampling the desirable feature. I doubt if Bartlett will now regard Welch's solution as in the least intolerable. Perhaps he ought to, but that is a different matter.

The paragraph which you added contains the phrase "the error of these calculations". The error is surely that of refusal to take into account relevant information, and not of calculation.

I hope these random notes will be of some use to you. I received your cross product table for linkage today.

Looking forward to seeing you on Thursday
Yours,
Fisher

Professor Sir Ronald Fisher