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Reply to comments

By J. D. KALBFLEISCH

I would like to thank the discussants for raising some very interesting points. Space is not sufficient to reply to all of the points raised though I would like to make some brief comments.

I find myself in substantial agreement with what Barnard has said. His stable and unstable ancillaries are certainly closely related to experimental and mathematical ancillaries. There is, as well, some theoretical advantage to considering the behaviour of ancillaries when a given parameter space is expanded rather than their behaviour when a very general parameter space is restricted as I have tried to do.

Birnbaum has pointed out that an equivalence relationship cannot by itself lead to a directive to make substitution. Of course the directive to replace the original experiment with a conditional version when experimental ancillaries are present follows not from C_E but rather by making subsequent principles apply to minimal experiments. The choice of S' and C_M was made, not so much from a conviction that they are always the best principles to use, but rather that they seem consistent with standard statistical procedures.

There will not always be a clear distinction between experimental and mathematical ancillaries and, as Barndorff-Nielsen points out, there will not in general be a unique maximal experimental ancillary. The example he gives is interesting and worth noting. My own feeling would be that one would normally be investigating the probability structure in the table as well as the recombination parameter π : data from this experiment would most likely be checked for consistency with the stated proportions. I agree with the notion that a requirement of a unique inference is probably unduly restrictive, but I differ from Barndorff-Nielsen in that I think it worth exploring the ways in which sufficiency and conditionality come into conflict and what can be done to resolve this conflict.

McLaren suggests that the only reason I have given for the priority principle C_E and the consequent weakening of the sufficiency principle is the negative one that this sequence avoids the likelihood principle. In any investigation, it is necessary to make judgements and simplifications as to what sources of variability are to be included: in the cloud seeding example, what would have been done had there been a mechanical failure or, indeed, what process led to the choice of this particular time? The purpose of C_E is to give guidelines as to what the essential part of the experiment is. Once its necessity is recognized, the likelihood principle no longer follows. There is, to be sure, some subjective input to C_E just as there is subjective input to the choice of a model. McLaren claims that, this being the case, the subjective Bayesian approach is more honest. Because some subjectivity is inevitable, should we make a virtue of it?

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