This is a comment on Fisher's attitude when an undergraduate at Cambridge; it is tempting to think that Mahalanobis was using Fisher's own words.

(6) In some ways Fisher's greatest methodological contributions, the analysis of variance including discriminant analysis, and ideas about experimental design, appear to owe relatively little directly to his ideas on general statistical theory. There are of course connections to be perceived subsequently, for example, to sufficiency and transformation models, and in the case of randomization somewhat underdeveloped connections to conditioning and ancillary statistics. Fisher's mastery of distribution theory was, however, obviously relevant, perhaps most strikingly in his approach to the connection between the distribution theory of multiple regression to that of linear discriminant analysis.

(7) In the normal process of scientific development important notions get simplified and absorbed into the general ethos of the subject and reference to original sources becomes unnecessary except for the historian of ideas. It is a measure of the range and subtlety of Fisher's ideas that it is still fruitful to read parts at least of the two books mentioned above as well as the papers that Professor Efron mentions in his list of references.

(8) I agree with Professor Efron that one key current issue is the synthesis of ideas on modified likelihood functions, empirical Bayes methods and some notion of reference priors.

(9) I like Professor Efron's triangle although on balance I would prefer a square, labeled by one axis that represents mathematical formulation and the other that represents conceptual objective. For example Fisher and Jeffreys were virtually identical in their objective although of course different in their mathematics.

(10) Finally, in contemplating Fisher's contributions, one must not forget that he was as eminent as a geneticist as he was as a statistician.

Comment

Rob Kass

Very nice, very provocative—as I read Efron's version of Fisher's profound influence on our discipline, I found myself wondering whether statistics could have succeeded to this point so spectacularly, across so many disciplines, if it had been based primarily on Bayesian logic rather than largely Fisherian frequentist logic. Without the historical counterfactual, the question might be stated this way: from a Bayesian point of view, when, if ever, is frequentist logic necessary?

I believe there are three places where frequentist reasoning is essential to the success of our enterprise. First, we need goodness-of-fit assessments, or what Dempster in his Fisherian ruminations has called "postdictive inference" (see Gelman, Meng and Stern, 1996, and the discussion of it). Second, although many principles have been formulated for defining noninformative, or reference, priors, it seems that good behavior under repeated sampling should play a role somehow. It is possible, as Cox implied in his 1994 *Statistical Science* interview, that we have not yet recognized how, and I have the sense that Efron shares this view. But the third and perhaps most important place even we Bayesians currently find frequentist methods useful is that they offer highly desirable shortcuts. This is related to Efron's point at the end of Section 4. The Fisherian frequentist methods for what are now relatively simple situations, such as analysis of variance, are certainly easy to use; it remains unclear whether standardized Bayesian methods could entirely replace them. Equally crucial, however, are the more sophisticated data analytic methods, such as modern nonparametric regression, which share a big relative advantage in ease of use over their Bayesian counterparts. In short, despite my strong preference for Bayesian thinking, based on our current understanding of inference, I cannot see how the next century or any other could be exclusively Bayesian. I have felt for a long time that the Bayesian versus frequentist contrast, while strictly a matter of logic, might more usefully be considered, metaphorically, a matter of language—that is, they are two alternative languages that are used to grapple with uncertainty, with fluent speakers of

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one being capable of arriving at an understanding of essentially any phenomenon that is understood by fluent speakers of the other, despite there being no good translation of certain phrases.

I suspect we all agree on Fisher's greatness. I like to say Fisher was to statisics what Newton was to physics. Continuing the analogy, Efron suggests that we need a statistical Einstein. But the real question is whether it is possible to obtain a new framework that achieves the goal of fiducial inference. The situation in statistical inference is beautifully peaceful and compelling for one-parameter problems: reference Bayesian and Fisherian roads converge to second-order, via the magic formula. When we go to the multiparameter world, however, the hope dims not only for a reconciliation of Bayesian and frequentist paradigms, but for any satisfactory, unified approach in either a frequentist or Bayesian framework, and we must wonder whether the world is simply depressingly messy. Indeed, some of the cautionary notes sounded in the 1996 *JASA* review paper I wrote with Larry Wasserman implicitly suggest that (as pure subjectivists are quick to argue) there may be no way around the fundamental difficulties.

It is clear that statistical problems are becoming much more complicated. I got the possibly erroneous sense (from his comment at the end of Section 8) that Efron connects this to his unease with the current situation and the need for a new paradigm—to be furnished perhaps by our Einstein Messiah. I would instead look toward a different big new theoretical development. Bayesian and frequentist analyses based on parametric models are, from a consumer's point of view, really quite similar. Bayesian nonparametrics, however, is in its infancy and its connection with frequentist nonparametrics almost nonexistent. I hope for a much more thorough and successful Bayesian nonparametric theory and a resulting deeper understanding of infinite-dimensional problems. Perhaps entirely new principles would have to be invoked to supplement those of Fisher, Jeffreys, Neyman and de Finetti–Savage. If it happens, an increasingly nonparametric future would not, in principle, move us toward the frequentist vertex of Efron's triangle. Rather, Bayesian inference would continue to play its illuminating foundational role, important new methodology would be developed, and it might even turn out that there is a genuine, deep and detailed sense in which frequentist methods could be considered shortcut substitutes for full-fledged Bayesian alternatives.

Comment

Ole E. Barndorff-Nielsen

It has been a pleasure reading Professor Efron's far-ranging, thoughtful and valiant paper. I agree with most of the views presented there and this contribution to the discussion of the paper consists of a number of disperse comments, mostly adding to what has been mentioned in the paper.

(1) One, potentially major, omission from the paper's vision of Fisherian influence in the next century is the lack of discussion of the role that ideas and methods of statistical inference may have in quantum mechanics. Such ideas and methods are likely to become of increasing importance, particularly in connection with the developments in experimental techniques that allow the study of very small quantum systems.

Moreover, there is already now, in the physical literature, a substantial body of results on quantum analogues of Fisher information and on associated results of statistical differential geometry, in the vein of Amari.

(2) It also seems pertinent to stress the importance of "pseudolikelihood," that is, functions of part or all of the data and part or all of the parameters that to a large extent can be treated as genuine likelihoods. Many of the most fruitful advances in the second half of the 20th century centers around such functions.

(3) My own view on optimality is, perhaps, somewhat different from that of Bradley Efron in that I find that the focussing on optimality has to a considerable extent been to the detriment of statistical development. The problems and danger stem from too narrow definitions of what is meant by optimal-

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