As one step in this direction, Waterman and Lindsay (1995) have considered an intermediate asymptotics for the Neyman–Scott problem in which the number of parameters goes to infinity, but as the square root of the number of observations. In this setting, the maximum likelihood estimator is asymptotically biased, but the bias can be removed by using projection onto the second-order Bhattacharyya scores, and the resulting estimators attain the asymptotic Cramér–Rao lower bound.

Comment

Peter McCullagh

The modern theory of conditional inference is an attempt to develop a sensible theory of confidence intervals, that is to say, inferential statements about parameters in the absence of prior information or with the explicit declaration of prior ignorance. In that sense, the impetus for recent developments in this area is the same force that motivated Fisher over a period of three decades to develop a solid foundation for his theory of fiducial inference. Although the terminology and formal mathematical theory are due to Neyman (1937), the essential idea and repeated sampling properties of confidence intervals were first spelled out clearly by Fisher (1930). Any ordinary mortal would have been delighted by the enthusiasm with which his ideas on likelihood and interval estimation were espoused, mathematized and extended by Neyman, Pearson and others. For various reasons, Fisher subsequently disowned, and even condemned with characteristic polemic, the idea of confidence interval as an inferential statement. The principal objections raised by Bartlett and Fisher to confidence statements concern their sometimes poor conditional properties and the necessity to specify in advance a particular error rate. While the second of these objections can be overcome to some extent by constructing a set of confidence intervals and presenting the result in the form of a confidence distribution, the first objection is more difficult to surmount. Fisher's effort, though admirable in its goal and skillfully argued, was ultimately unsuccessful.

Neo-Fisherians set themselves a more modest goal. The conditionality principle in some form is accepted, but its consequence, the likelihood principle, is not. If reasonably firm prior information is available, it must be used in Bayes' theorem. This is uncontroversial. If no prior information is available, neither personal opinion nor "objective ignorance prior" is regarded as a satisfactory substitute. Inferential statements must then be constructed without recourse to Bayes' theorem, and such statements must have acceptable conditional properties, at least in large samples. One cannot expect good agreement among statisticians on the basis of small samples because prior information and/or choice of sample space necessarily plays a nonnegligible role. The best that one can hope for is good agreement in large samples. Reid's paper provides a timely opportunity to review the extent to which a satisfactory large-sample frequency theory of inference has been developed in the past two decades.

Before delving into details, it seems pertinent to ask how it is proposed to construct a satisfactory theory based on a mathematical contradiction. Conditionality and sufficiency are accepted, but the likelihood principle is not, in apparent contradiction of Birnbaum's theorem. This prima facie indefensible position cries out for an explanation. The thinking on this issue seems to run as follows:

(i) Many applied statisticians find significance tests very useful in practice.
(ii) Any tool that has proved to be so useful over such a long period cannot be all bad.
(iii) Any statistical principle that denies a role for significance tests cannot be a good principle.

One need only examine the literature on the convergence of the Gibbs sampler or Markov-chain simulation methods to see that even avowed Bayesians find significance tests useful. The indirectness of the interpretation of p-values, a point of sharp criticism in all discussion of principles, does not seem to present a serious obstacle to use. A strong reluc-

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