

latent cause is often treated as a treatment measured with error. Right now I don't have an answer as to whether or not latent variables can be causes. I suspect that it might depend on other considerations. But I am sure that it is at least as important to know the answer to this question as it is to know how to fit a very complicated structural model by maximum likelihood.

Why go on and on about causal models and latent variables? Here is my answer. If, as Clogg rightly asserts, sociological research is influencing statistical research in the study of causation, what, then, should this influence be? Should statisticians jump on the band wagon and develop more and more procedures for fitting these models, following the path so well blazed by, say, Anderson and Rubin (1956)? Should the reaction of statistical research be simply to continue to add to the list of structural models that can be fit to data in finite computer time? Or should it spend

some effort to give these models a better foundation based on the known past successes of statistical science? In my view, the latter is one of the many contributions of the approach to causal inference that Rubin started. His approach grows out of work in many fields all bearing on the problem of causal inference when there is heterogeneity, variation and noise—*Unruly's feast*—and there is plenty more to do there. Again, it's the interplay between statistics, science and mathematics that's important.

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## Comment

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I can easily understand Professor's Clogg's frustration with the belief that advances in statistics "trickle down" to the social sciences. Statisticians must feel the same way when it is said that advances in probability theory trickle down to statistics. Clogg's account of the historically productive two-way flow of ideas between statisticians and sociological methodologists is well written and instructive. It is easy enough to document a similarly productive flow of ideas between statisticians and econometricians. Some examples follow.

#### COLLABORATION BETWEEN STATISTICIANS AND ECONOMETRICIANS

In the 1940s and 1950s, statisticians and econometricians concerned with the estimation of linear model systems worked closely together with the support of the Cowles Commission for Research in Economics. The statisticians in the group included, among others, Ted Anderson, Herman Chernoff, M. A. Girshick and Herman Rubin. The economists included, among others, four later winners of the Nobel Prize: Trygve Haavelmo, Lawrence Klein, Tjalling Koopmans and

Herbert Simon. The Cowles Commission work revolutionized econometrics. See the seminal volume edited by Hood and Koopmans (1953). The atmosphere and substance of the collaboration between statisticians and econometricians is conveyed well by Anderson (1991), written on the occasion of Haavelmo's receipt of the Nobel Prize.

From the 1970s through the present, Bayesian statisticians and econometricians have met on a regular basis. An important medium for these contacts has been the conference series organized by Arnold Zellner, a prominent econometrician who is currently the president of the American Statistical Association. Several published volumes have emerged from these conferences. See, for example, Fienberg and Zellner (1975).

Over the past 10 years, statisticians and econometricians working in the area of nonparametric and semi-parametric analysis have developed increasingly close working relationships. The fruits of collaborative research have appeared in co-authored articles, such as Heckman and Singer (1984) and Pakes and Pollard (1989). Several conferences have brought together statisticians and econometricians, with tangible product in the form of conference volumes such as that edited by Barnett, Powell and Tauchen (1991). Knowledge of mutually interesting developments has also diffused through the routine process of exchanging working papers.

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