varies so much within the social sciences that even someone as knowledgable as Clogg only feels comfortable discussing sociological methodology?

I understand Clogg's demurral fully. I would find it as difficult to write on the statistical contributions of psychometrics and sociological methodology. Clogg's article helped me understand not only the relationship between sociological methodology and statistics but also the relationship between sociological methodology and econometrics.

Econometricians and sociological methodologists have worked closely in some areas. In the early 1970s, they collaborated in the development of the latent variable models discussed by Clogg. See Goldberger and Duncan (1973). During the 1980s, both groups contributed to the development of rich models for the description of event-history data. Lancaster (1990) is a comprehensive and readable econometric treatment of the subject.

In other respects, econometricians and sociological methodologists have gone their separate ways. I was struck by Clogg's close association of categorical data analysis with the log-linear model, because the approach to discrete response analysis that took hold in econometrics during the 1970s was at most marginally influenced by the contemporaneous work on log-linear models. See the discussion in Manski and McFadden (1981).

I was also struck by Clogg's discussion of the survey-sampling literature on complex sampling, because this work has had essentially no impact on econometrics. Instead, we have developed the literature on estimation under "choice-based" sampling. The article by Hsieh, Manski and McFadden (1985), a survey written explicitly for a statistical audience, synthesizes this work and explains its relation to the biometric literature on "case-control" sampling. It is gratifying to be able to report that this effort at communication across disciplines has had some success. See, for example, Breslow and Cain (1988), who summarize and extend aspects of the econometric literature.

The point is that the various methodological disciplines form a complex social network, with strong relationships in some dimensions and weak ones in others. I find that econometricians and sociological methodologists speak much the same language on some subjects but can barely converse on others. I observe different mixes of the familiar and the strange when I read journals in psychometrics, biometrics and statistics. The various methodological disciplines have important shared foundations and objectives. But each one also has distinctive concerns which will, I suspect, keep them from coalescing any time soon.

ACKNOWLEDGMENT

I am grateful to Arthur Goldberger for his comments.

Comment: The Fence Between Statistics and Social Research

Ivo W. Molenaar

CLOGG IS RIGHT

Clogg (1992) defends the thesis that developments in sociological methodology and in quantitative sociology have always been closely related to development in statistical theory, methodology and computation. His impressive list of examples, from Quetelet's "average man" to event history analysis and finite mixtures, shows that he is right. It also shows that there was not just a relation between sociology and statistics: influences from quantitative researchers in psychology, education, economics, biology, demography, political science and management science can be found in many of Clogg's examples, and are indeed recognized by him. The development of covariance structure models, listed in his section "Models for Continuous Latent Variables," is an excellent example of how the concepts and skills of psychometricians, sociometricians, econometricians, statisticians and computer scientists were successfully brought together.

To the many examples cited by Clogg, I should like to add two: the adquate handling of missing observations, and the development of statistical computer packages.

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In both cases the influence of the social sciences has been remarkable.

DID ANYONE DOUBT THAT CLOGG WAS RIGHT?

The question remains why Professor Clogg presented his views, first, to the annual meeting of the American Association for the Advancement of Science and, now, to the journal *Statistical Science*. Is the negation of his main thesis, outlined in the first few pages of his paper, a straw man intentionally set up to be knocked down? Are the slightly arrogant views of Fisher that form the opening quote of Clogg's paper shared by enough others to make a counterattack desirable?

I think that it is a pity that this point is not elaborated in the paper. Hopefully some of my fellow discussants will step into the arena for a defense of the view that the social sciences have passively absorbed the bright statistical ideas developed by mathematicians and experimenters from the "hard" sciences. It has always surprised me that physics and chemistry, perhaps the "hardest" sciences, have a university curriculum in which formal training in statistics is virtually absent. When experimentation and/or observation become more difficult (as in astronomy, biology, agriculture and medicine) the use of, and appreciation for, statistics among empirical researchers increases. When experimenting and measuring becomes very difficult (as in the social and behavioral sciences) one has to rely still more heavily on statistical models and methods.

Statistics is concerned with variability, and tries to predict "what would happen if we did it again," in cases where the simple deterministic answer "exactly the same" is clearly incorrect. This may help to explain why most physicists and engineers can do with a simple calculus for their (very small) measurement errors (exceptions being, e.g., statistical mechanics and the Boltzmann and Fermi-Dirac statistics found in Feller, 1957). In empirical sociology, on the other hand, one may need a stratified two-stage sampling design, an analysis comparing nonresponders to responders, a model for measuring the latent variable of interest via indicators and/or a loglinear analysis of a five-way cross classification with some interesting three-way interactions.

Clogg (1992) illustrates how such advanced statistical tools were developed in a cooperation between statisticians and quantitative social scientists. There can be no doubt that only a small minority of the current generation of sociologists actively uses these tools: many others are never involved in empirical research based on statistical models, and still others stick to simple correlation and regression. I mention two Dutch sociologists who have made important contributions: Gadourek (1982) for his innovative use of a whole spectrum of advanced statistical tools and Hagenaars (1990) for his textbook on contingency table analysis involving latent classes.

SOFT SCIENCES REQUIRE HARD STATISTICS

My personal career is a drift from abstract to applied mathematics. At the age of 21 I was thrilled by topology and logic; at 25 my main topic was probability theory. I then spent 8 years at the Statistics Department of the Mathematisch Centrum in Amsterdam (now CWI), writing a few papers for the *Annals* and a thesis on approximations to discrete distributions. In the past 20 years I became more a psychometrician or sociometrician, and less a mathematician. So I have been at both sides of the fence, and from this background I want to sketch a few aspects of the importance of statistical models for the social and behavioral sciences.

Such sciences pose their own problems with statistical methods. First, they are characterized by an enormous natural variation. The feelings, attitudes and social actions of people show far more interindividual and intraindividual variation than, for example, the physiological measurements of animals or crops, let alone the physical properties of mass production units studied in industrial quality control. Second, most properties relevant for the social and behavioral sciences can only be indirectly measured, and the measurement error involved is many times larger than for "harder" properties. Third, a human being is very much aware of being measured, and may give socially desirable answers, or refuse to take part in the empirical investigation. Fourth, so many external variables may influence human behavior that either a complicated multivariate model or a tight experimental control is often required in order to find meaningful relations and conclusions. Fifth, the social sciences have a long verbal and philosophical tradition, and tend to attract many students for whom the strict rules of formal research methods and formal models are very difficult.

For me, statistics for the social sciences is fun precisely because it is so difficult. As shown above, it is not only the mathematical side of it which presents problems; perhaps the fundamental methodology about design of empirical research, measurement, modeling and analysis is even more tricky. The Clogg paper illustrates how an interplay of substantive and mathematical considerations is required in applying statistics to empirical social science. The problems sketched above make such applications both more difficult and more necessary in our quest for valid empirical knowledge. Statistical methods offer here a way toward better study design and more appropriate generalization of conclusions, as opposed to more informal and exploratory data analysis (see Molenaar, 1988).

THE PECKING ORDER

Clogg is not very explicit about his reasons for writing his paper. One could wonder whether it is fruitful and appealing to stage a contest between disciplines about who contributed most to the development of statistics. Throughout the paper I perceive a slight irritation that the contribution of the social sciences is denied or belittled by statisticians. This is surely incorrect for those who play the favorable roles in Clogg's examples. It is probably correct for others, but I do not know for how many.

Clogg's paper can certainly help to enlighten those who were simply unaware of the developments cited by him. It remains to be seen, however, whether those newly enlightened statisticians, as well as those who already knew about the developments, will change their views.

This brings me to what I consider to be the latent structure, or hidden agenda, of the paper. This is the academic pecking order of disciplines. Even within mathematics, abstract topology and functional analysis are generally perceived to have a higher status than statistics. Within the domain of mathematical statistics, *The Annals of Statistics* enjoys a higher reputation than, say, *Biometrics* or *Psychometrika.* Among disciplines, mathematics, physics and biochemics have a better image among the outsiders than sociology or psychology.

The soft sciences cannot boast of spectacular achievements like sending astronauts to the moon or giving a patient another heart. In their relatively short history as academic disciplines, they have made less visible progress in areas like personnel selection, teaching methods, structure of organizations, ethnic tension and deviant behavior. Perhaps the nonbelievers in the usefulness of systematic empirical research in the social and behavioral sciences form a majority. They are found both within these disciplines, and in the ranks of the "harder" sciences. It may well be that in both camps one shares a feeling that a positivistic research style is not suitable in the study of human beings.

Formal models, of course, never catch the full richness and variation of human behavior and human feelings. It is equally true, however, that formal models of mechanics never fully catch the movements of real objects. In both cases, the abstract model can only be an approximation. Everybody knows and accepts that engineers can work with such approximations. In the study of human behavior it is rather more common to detect feelings of "it cannot be done, and, even if it could, for ethical reasons it should not be done." The book by Bartholomew (1973, section 1.3) contains some interesting thoughts on this issue in the context of applying stochastic process theory to social phenomena. Unlike the correctness of proofs or computer programs, this is an area where each individual has a personal value system that is seldom changed by discussion. Nevertheless, it may be useful to sometimes reflect on such matters, and the stimulating paper by Professor Clogg gave me an opportunity to do so.

Rejoinder

Clifford C. Clogg

I thank the discussants for their stimulating comments, many of which I judge to be consistent with the themes in my paper. The discussants cover several areas of statistical methodology that I either neglected or did not emphasize enough, provide more evidence for the claim that the context of social research has had a major effect on statistical methodology and give alternative points of view concerning how particular methodologies have developed. I agree with almost all of the points they make and so will confine myself to just a few remarks.

BARTHOLOMEW

I strongly agree that the social sciences place new demands on statistical methodology, particularly in areas such as measurement and measurement error, modeling correlated observations and latent variables. Bartholomew is right to refer to multiple correspondence analysis and recent advances in sampling theory as cases in point. The contrast between the natural or hard sciences and the social or behavioral sciences, insofar as statistical methodology is concerned, is very important to both his and my arguments. I tried to contrast the natural science setting with the social science setting a bit in my paper; also see Clogg and Dajani (1991).

HOLLAND

I was not hopping mad when I wrote the paper, but it is true that my tolerance for foolishness is so low