Preface

I am a part of the generation of social scientists for whom multivariate statistics became a routine research tool. Dramatic advances in statistical theory, computer power, and user-friendly software, coupled with the growing availability of large data sets, opened up a new world of research possibilities. Like so many social scientists, I was thrilled by the prospects; virtually all scientific and policy questions would be meaningfully addressed. The only real constraints were resources.

I was encouraged by the post-World War II generation of social scientists whose energy and scientific optimism were an inspiration: Don Campbell, Dudley Duncan, Jim Coleman, Jim Short, Al Reiss, Pete Rossi, and many others. But as the years passed, I began to hear grumblings that the reach of quantitative social science was far exceeding its grasp. For too many researchers, scientific optimism had become scientific arrogance, and innovative tools were being applied thoughtlessly. In personal conversations with a number of senior researchers, growing disillusionment was candidly expressed. There were even some serious second thoughts about much earlier work.

Nowhere were the grumblings louder than when social science was applied to matters of social policy. I recall a conversation with Don Campbell in which he openly wished that he had never written "Campbell and Stanley" (1963). The intent of the justly famous book, *Experimental and Quasi-Experimental Designs for Research,* was to contrast randomized experiments to quasi-experimental approximations and to strongly discourage the latter. Yet the apparent impact of the book was to legitimize a host of quasi-experimental designs for a wide variety of applied social science research. After I got to know Dudley Duncan late in his career, he said that he often thought that his influential book on path analysis, *Introduction to Structural Equation Models,* was a big mistake. Researchers had come away from the book believing that fundamental policy questions about social inequality could be quickly and easily answered with path analysis. But by far the most influential was Pete Rossi. Over the years, we worked together on many applied research studies closely linked to policy. He was (and remains) extremely demanding of his applied research and the applied research of others. And as a New Yorker, he was a contrarian by nature. A lot of Pete's style was infectious.

Some of the grumblings eventually found their way into print. Dudley Duncan's *Notes on Social Measurement* (1984) was especially compelling.

Lieberson's *Making It Count* (1985) was on target for many of the larger issues. Illustrations of more focused but equally skeptical writing include Oakes's book on statistical tests (1986), Ed Leamer's powerful condemnation of causal modeling in economics (1978), and critical papers on causal inference by several very visible statisticians (e.g., Holland, 1986; Rubin, 1986; Freedman, 1991).

The language in which concerns were expressed was often quite pointed. Widely noted, for instance, was George Box's statement that "all models are wrong" (1976:792). Moreover,

> since all models are wrong, the scientist cannot obtain a "correct" one by excessive elaboration. On the contrary, following William of Occam he should seek an economical description of natural phenomena. Just as the ability to devise simple but evocative models is the signature of the great scientist, so overelaboration and overparameterization [are] often the mark of mediocrity.

Only a bit less sweeping were Leslie Kish's views on significance tests (1987:19):

> Tests of statistical significance are particularly ineffective as they are commonly used in social research: to test the null hypothesis of zero differences, or null relationships. Such hypotheses are trivial reflections of the actual aims of social research.

Finding myself increasingly drawn into the skeptics' camp, I began expressing my doubts in print (e.g., Berk, 1977, 1988; Berk et al., 1995). I also began to reexamine a lot of my earlier work. But as an applied researcher, I also tried very hard to be constructive: How could empirical work useful for policy be done better? In the 1990s, I moved full-time into the Department of Statistics at UCLA while at the same time including among my research activities an increasing fraction of projects in the environmental sciences. The result was exposure to a much wider range of statistical and scientific thinking than I had seen in the social sciences. This book is part of the process by which these broadening experiences are being integrated. I am trying to bring together in one place not just the contrarian views to which I am drawn but constructive suggestions about how improvements could be made.

Science and policy making are collective enterprises. Insofar as this book contributes to either, I am indebted to several wonderful colleagues with whom I have had many animated discussions over the past few years: Jan de Leeuw, Don Ylvisaker, Rob Weiss, Rob Gould, and especially David Freedman. David Freedman, Jan de Leeuw, Coen Bernaards, and Herb Smith provided very helpful written comments on this manuscript. I also offer thanks to the policymakers with whom I have worked for more than 30 years. They are a mixed

bag to be sure, but the many good ones convinced me that it was possible to do sound applied research that really mattered. Finally, there is Peter Rossi. He was with me when this long trip began and has remained a steadfast friend and honest critic. To him I am especially indebted.