From the Editor

This is the final issue of Volume 17, the first of the three volumes to be produced during my term as editor. At the conclusion of this issue the reader will find a list of people who have served as outside referees for those manuscripts that have thus far been considered by me. The list is not complete, for it excludes Trustees of the Law and Society Association and members of the Editorial Board of the *Review*. Also, given the inevitability of human error, it may inadvertently exclude a few others. I apologize to those who fall in the latter group and promise that if they send me their names I will try to give them additional manuscripts to review so that they will be mentioned a year hence.

The profession should be grateful to all who have served as outside reviewers. The outside review process is essential both to the integrity of publication decisions and to the quality of what appears. By and large, I have been delighted with the efforts expended by readers of *Review* manuscripts. Their thoughtful, often lengthy, written comments usually serve well to explain the reasons behind negative decisions to those whose work is not accepted and greatly aid those whose work is accepted subject to revision. If memory serves me correctly, only three authors have complained about the quality of reviews they have received. Numbers of authors have made positive statements. Equally encouraging is the general willingness of scholars from a wide variety of fields to serve as referees. My experience so far is that about 95 percent of those we ask to read manuscripts agree to do so. The one group that is conspicuous by a higher than average refusal rate is law teachers, a phenomenon that I attribute in part to the fact that legal academics are accustomed to publishing in non-refereed student-edited journals.

Problems with the review process are few, but two merit attention. The first is delay. While most reviewers return their evaluations within a month or less, a minority take two months or more. The result is that I sometimes require more than six months to reach a decision, and on occasions I must reach a decision on the basis of only one review. Authors who submit to the *Review* deserve a quicker turn around time. The ideal

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solution is for busy referees to realize that the block of time they see three weeks hence in which they will be able to review the manuscript without any disruption in their other activities is almost always filled by other activities by the time it arrives; so better to set aside a specific time to review a manuscript shortly after receiving it than to wait until the sense that one is fulfilling an important professional responsibility has turned into nagging guilt. In the extreme case where burdensome obligations have unexpectedly arisen and a review within three or four weeks is impossible, reviewers should immediately return manuscripts so that they can be sent to another referee without further delay.

The second problem, which fortunately characterizes only a small handful of returned reviews, occurs when a reviewer loses sight of the fact that the goals of the review process are two: to aid the editor in deciding whether to publish a particular manuscript, and to help an author appreciate the strengths and weaknesses of his or her work so that the work may be improved if the piece is publishable with revision or so that the author may better appreciate what the standards of the profession demand if the work is not salvageable. It is appropriate in a review to point out theoretical weaknesses, methodological inadequacies, unfamiliarity with the literature, difficulties in communication, and even the banality of the enterprise. But nothing is gained by adding at the end of a critical review, "In short, this is a piece of drivel," or "The manuscript only serves to convince the reader that the authors don't know what they are talking about," or, and this theme recurs often enough that I think it is a favorite academic put-down, "I expect this is a college term paper." A review, in other words, should be evaluative and constructive, but as in all educational endeavors, it should not be degrading. It is painful to receive specific criticisms of one's work coupled with the message that it is not publishable. Authors of deservedly negative reviews must be frank, but at the same time they should take care not to exacerbate the pain of rejection unnecessarily.

Readers may wonder how the review process affects an editor's decisions. In my case the effects are substantial. If I receive two negative reviews of a manuscript that deals with matters I know little about, the piece has virtually no chance of being accepted since I do not have any basis for challenging the judgment of people far more expert than I. If the reviews in such a case are positive, I can assume the work meets the

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standards of its specialized sub-area but must ask whether it both meets the standards of law and social science and deals with an important topic in this area. Where I have some familiarity with the subject of a manuscript, I do not feel bound by the outside reviews even if they are unanimous one way or the other. However, I approach my decision with the idea that I must, in principle, be able to justify what I do to the outside reviewers. Thus, when my decision disagrees with the recommendation of a reviewer, I feel that I must be able to counter the objections the reviewer has raised if my decision is to publish or be able to point to flaws that the reviewer has not seen if I decide to reject. The discipline this process imposes is considerable, and when two reviewers are in agreement, I am seldom the odd man out.

Reviewers are perhaps most influential and most valuable when they suggest ways to revise clearly flawed pieces. Many manuscripts that are not publishable as submitted are potentially fine articles. A review that perceives this potential and suggests ways to realize it will typically forestall an immediate rejection. Instead the author will be encouraged to revise and resubmit, paying attention to the referee's remarks and any additional suggestions I can offer. Perhaps the most gratifying aspect of my editorship has been the opportunity it has given me to observe the revision process. First, I have been impressed with both the receptiveness of authors to peer criticism and their willingness to undertake the burdens of revision. Second, I have been delighted with the quality of the work that has often resulted, and I have become increasingly aware that the value of peer review is reflected not primarily in the choice of what articles to publish but, more importantly, in the quality of what appears. At the same time the revision process has led to some of my most difficult decisions. Some authors have done fine work in revision only to make it clear that the apparently correctable flaws in the original submission (usually having to do with the quality of available data) cannot, in fact, be overcome. Such articles have to be rejected, yet I cannot help feeling that their authors deserve better than seeing their efforts come to nought.

Turning to this issue, I am again delighted with the quality of the work that appears. The first article, "Prisons as Self-Regulating Systems: A Comparison of Historical Patterns in California for Male and Female Offenders" by Richard Berk, Sheldon Messinger, David Rauma, and John Berecochea, is in two ways a model for work of its genre. Like the work by

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Loftin, Heumann, and McDowall in Volume 17:2, it illustrates the virtue of securing a good sociological understanding of a process prior to any attempt at statistical modeling. In this case, historical records give the authors a good understanding of the crucial developments in the prison admission and release processes that occurred over time. This understanding is used to develop a dynamic model of these processes, to assess some of the potential limitations of that model, and to aid in the interpretation of the results of the analysis. This article also deserves to be emulated for the clarity with which the authors present the results of their technical analysis. The model building techniques used will be unfamiliar to many readers of this journal. Yet even readers who cannot fathom the mathematical details of the analysis and do not want to try should be able to understand the logic that underlies the model building, the crucial assumptions that are incorporated in the model, and what the analysis tells us about how prison incarceration and release rates have fluctuated over time.

If the work by Berk and his coauthors illustrates the virtues of mathematical modeling, Luis Salas' article, "The Emergence and Decline of the Cuban Popular Tribunals," emphasizes the value of theoretically sensitive, ethnographic approaches. Salas' article is particularly important because the example of the Cuban popular tribunal has often been cited by those who argue for the viability of "alternative forms of dispute settlement" in modern urban societies. This example, we learn from Salas, may carry just the opposite lesson, for the popular tribunal did not long survive in the land of its birth. The reasons for its demise are particularly instructive: the fact that it was not rooted in the popular culture to begin with; shifts in the revolutionary ideology that supported it; its estrangement from case-generating institutions like the police; and the rise of a post-revolutionary bar in Cuba, which like more bourgeois bars is concerned with professional hegemony. Analogous factors have hampered the growth of "alternative forms of dispute settlement" in this country.

Charles Pruitt and James Wilson in their article "A Longitudinal Study of the Effect of Race on Sentencing" examine the sentencing patterns of Milwaukee judges at three points in time. Their results suggest that blacks suffered because of their race in 1967-68 but not in 1971-72 or 1976-77. These findings are themselves important, but more important is the authors' effort to deal with *sample selection bias*, a problem that confounds much of the existing research on sentencing

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(Klepper *et al.*, 1983). Sample selection bias is a potential problem in any research that focuses on a population that is the residue of some earlier selection process. It is a special problem in sentencing research because it is plausible to assume that arrest and conviction, the processes which yield the population eligible for sentencing, are subject to biases that cause those ultimately sentenced for particular crimes to differ from those who are filtered out before the sentencing stage in ways that appropriately affect sentencing decisions but are, at best, only imperfectly measured. In these circumstances typical regression approaches, for reasons that Pruitt and Wilson identify, may fail to spot existing discrimination or may incorrectly label fair treatment as discriminatory.

Sample selection bias may be statistically controlled if the process by which people are selected for subsequent treatment can be modeled. However, the available data often do not allow such procedures. Pruitt and Wilson use another approach which depends on certain common sense assumptions plus the availability of data over time. A general lesson to draw from this article is that statistical threats to the validity of a design typically rest on hypotheses about the state of the world. Where preferred methods for controlling such threats cannot be employed, an alternative strategy is to assess the plausibility of the underlying hypotheses. If these hypotheses or their implications are implausible or inconsistent with available data, such threats are properly downgraded or dismissed.

The issue concludes with several brief pieces that follow up on earlier work published in the *Review*. Exchanges like that between Alan Wertheimer and Michael Philips—that is, critiques of an earlier article and the author's rejoinder—will be published during my editorship if they are justified by the quality of the exchange and to the extent that space permits. Critiques generally should be brief and should be sent first to the author of the original article. I will consider publishing such critiques only if I receive with them a copy of the original author's response upon seeing the critique or a statement that the original author has not responded.

Finally, Richard Miller discovered a programming error that resulted in the misclassification of some of the data that formed the basis for the fine article that he and Austin Sarat published in Volume 15:3-4, the special issue devoted to dispute settlement. In a brief comment, Miller corrects the earlier error and notes the ways in which the earlier analysis must be

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changed. Miller's comment emphasizes how vulnerable much of our work is to small, inadvertent errors; and the contrast between the corrected and the earlier analysis reminds us that social scientists can generally find a reasonable explanation for their results, however they come out. I am also left wondering how many researchers with less integrity than Miller discover flaws in their published work but, since no one noticed, decide against revealing the fact that they erred.

Incidentally, while on the subject of error, Herbert Jacob writes to tell me that in citing *The Roots of Justice* in his Presidential Address published in Volume 17:3, he inadvertently failed to include the name of Robert Percival, Lawrence Friedman's coauthor.

Richard Lempert
October 1983

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