From the Editor

Issues of the *Review* that are organized around a theme or reflect a particular approach to law and social science are ordinarily planned as special issues. They originate either in a series of papers prepared for a conference and offered to the *Review*, together with special funding, for publication, or in an editor's decision to seek out work of a particular genre and devote an issue to it. This issue is more a creature of coincidence. A series of excellent papers examining legal change in historical perspective emerged from the review process so close together in time that it was natural to group them in a single issue.

The only difficulty this posed for me was the question of how to characterize the issue. "Historical and Longitudinal Studies," which for me characterizes the papers in this volume, is in a sense redundant, for longitudinal studies are necessarily historical in that they reach back to tell us about behavior at an earlier point in time. But with a few notable exceptions (e.g., Friedman and Percival, 1981), few longitudinal studies of the kind that appear in social science journals convey the rich sense of past behavior that one associates with good legal history.

Most longitudinal studies in the law and social science tradition have focused on records of court activity. Perhaps because it is a genuine accomplishment to collect, code, and organize court data over time, much of the early research of this sort did little more than present the data collected and call the reader's attention to interesting patterns in the data and changes in these patterns over time. Attempts to explain what was discovered were speculative—usually based on some loose hypothesis of a relationship between law and economic development—rather than rigorous. Specific models of change were seldom tested, nor were historical data, apart from docket or opinion data, presented for their bearing on the interpretation of what occurred. In some cases basic legal changes, for instance in jurisdictional amounts or the enactment of important laws, were not considered even though they might have helped to explain patterns attributed to socioeconomic development or other extra-legal phenomena.

If studies of court dockets, outcomes, and opinions are to further our understanding of how the legal system articulates with

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other systems of society and how legal action changes over time, there are two—not mutually exclusive—directions in which such research should go. First, such studies should become more historical. The data captured in court records form a limited and biased picture of past legal life and the social world it reflects. Researchers should, to the extent possible, immerse themselves in other aspects of a jurisdiction's legal history (attention to the way a jurisdiction's formal laws and procedures have changed over time should be routine), and if ties are to be made to social and economic development, relevant social histories of the periods studied should also be considered. If the time studied extends into living memory, interviews are a further source of potentially valuable information. Second, longitudinal studies should focus more directly and more rigorously on law and social science hypotheses that may profitably be tested in a longitudinal framework. This means that both the hypotheses in question and the data that potentially bear on them should be clearly specified and that appropriate methods for linking the data and hypotheses should be used. Plausible rival hypotheses must also be investigated. To some extent this enterprise can proceed by the tabular approaches that have thus far characterized the work of many of those who have investigated court docket and opinion data over time, but the masses of data and the many time periods encompassed in such studies mean that researchers must develop clearer and more reliable ways of reducing and presenting their data. Editors concerned with space often cannot afford to present pages of tables to substantiate simple points, and readers may not have the patience to wade through them. There is, in addition, an important place for formal modeling and similar quantitative approaches in examining the implications of court data over time. As hypotheses of legal action and change come to be more specifically formulated and tested, I expect we will see more of this.

The four longitudinal studies in this issue make advances in these directions. The paper "Courts and Public Schools: Educational Litigation in Historical Perspective" by David Tyack and Aaron Benavot that opens the issue illustrates the virtues of a richly historical approach. Using West digests as an index of appellate cases, it examines the volume and character of litigation from the earliest recorded school cases until 1981. But it does not rely on numbers to carry the burden of the argument. Instead it traces the way in which school cases reflected specific concerns of specific periods and tells us something of the reasons for these concerns and the cases to which they gave rise. We see that the

history of school cases is inseparable from the history of education, and that each illuminates the other. Ultimately we understand why courts were called on to resolve certain types of problems at some points in time but not at others. A less historical investigation might have suggested many of the same associations, for little of what Tyack and Benavot report is surprising. But it would not have led to the same degree of understanding and appreciation of how cases that surface in appellate courts articulate with the ongoing concerns of society.

In "Continuity and Change in Patterns of Case Handling: A Case Study of Two Rural Counties," Stephen Daniels examines the more general question of whether there is substance to the widespread assumption that the "master pattern" of handling cases has changed over time such that civil cases are more likely to be settled out of court and criminal cases are more likely to be plea bargained. Looking at both the criminal and civil dockets in two rural counties from 1870 to 1960, he finds little support for the thesis that there was a time when most cases filed in courts reached trial. In no period he examines, on either the criminal or civil side, were as many as one out of four cases brought to court resolved by a trial. Daniels' primary contribution, however, is to alert us to an important methodological point. Aggregate patterns in the way cases—involving more than just the incidence of trials—are handled do change over time, but the changes found are considerably dampened and sometimes disappear when case type is controlled. Thus, it appears that some changes in case handling revealed by other studies reflect changes in the kinds of cases that go to the court more than they do changes in the stance that lawyers and judges take to cases. This finding carries with it the unwelcome implication that those who study court dockets will have to collect still larger samples to avoid misleading aggregations.

Wayne McIntosh examines 150 years of litigation in a state trial court of general jurisdiction in his paper "A State Court's Clientele: Exploring the Strategy of Trial Litigation." Taking a cue from Galanter's (1974) observations on the potential importance of party characteristics to the outcome of litigation, McIntosh codes litigants as organizations or individuals and examines the resulting patterns. Perhaps his most surprising findings are that in the court he studied organizations are defendants more frequently than they are plaintiffs, and that claims made by organizations are, on the average, less than those made by individual defendants. Like Daniels, McIntosh finds that it is important to break down cases by type to understand litigation

rates. His data demonstrate over time a finding that others (e.g., Mayhew, 1975) have noted in cross-sectional surveys. Patterns of involvement in litigation and tendencies to pursue cases at trial reflect the position of litigants and what is at stake. This finding holds for both organizations and individuals.

Peter Harris' paper "Ecology and Culture in the Communication of Precedent among State Supreme Courts, 1870-1970" draws our attention back to appellate courts. Harris, however, is not concerned with changes in the pattern of litigation over time. His interest is in the reciprocal influences that state supreme courts exert on each other, and in how these influences and patterns of interstate citation have changed over time. Using the state supreme court citation data collected by Bliss Cartwright, Lawrence Friedman, Robert Kagan, and Stanton Wheeler (see, e.g., Kagan *et al.*, 1977), Harris constructs and tests formal models of interstate citation for three time periods. He finds that precedent tends to flow from less populated and rural states to more populated and urban ones, and he finds that cultural regionalism as reflected in interstate migration is a good predictor of which courts will cite which others.

The issue concludes with an article by Richard Moran, "The Origin of Insanity as a Special Verdict: The Trial for Treason of James Hadfield (1800)," which is firmly in the historical tradition. Moran brings to life the trial of James Hadfield for the attempted assassination of George III, and the legal change consequent on it. The case is important because it gave rise to the special verdict of not guilty by reason of insanity which was linked with automatic confinement for an extended period of time. Moran argues that the special verdict did not represent some special compassion for insane defendants or judgments of lack of responsibility, but was from the outset linked to the idea that society must be protected from the insane and that even the criminally insane might be deterred through fear of punishment.

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