CONCLUSION

AFTERWORD: STUDYING LITIGATION AND SOCIAL CHANGE

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In the course of planning the conference that led to this issue of the *Review* and while these essays were being prepared, I had an opportunity to consider what my colleagues said and wrote about longitudinal studies of trial courts in the broader context of research on law and society. As Lawrence Friedman comments at the beginning of this Special Issue, only a small number of scholars have actually pursued studies of courts over time; yet, this work speaks to issues that hold the interest of many. In this Afterword I would like to add my own observations about developments that hold promise for studies of law and society as well as for longitudinal research on trial courts. These observations reflect the perspective of many of the essays here that trial courts are a site for research about law and change rather than a specific object of research. Indeed, I will argue that trial courts may not be separated from their social context and that to study them is to simultaneously study issues relevant to many law and society subfields.

I. SOCIAL CHANGE

The most important contribution of longitudinal research on trial courts is its systematic attention to social change. Generally speaking, because all social action is dynamic, it is important that we develop temporal views of action, organization, and culture through our research. In particular, actions influenced or constituted by legal institutions are part of processes taking place over time, and examining them over time seems essential to a full understanding how law might be implicated in their origins, in the ways that they unfold and in their effects (see Moore, 1990). These discoveries, in turn, will illuminate processes that are basic to law's role in society.

Critics have argued that earlier research on law and social change focused exclusively on the gap between the intended and actual effects of law, ignoring both the law's displacement of alternative conceptions of order and its constitutive role in maintaining

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everyday and routine social life.[1] Such criticism raises questions concerning what studies of law and social change should be about. Few have attempted to describe appropriate alternative theoretical frames of reference, but by drawing on essays here, I would like to suggest that not only are alternative frameworks for the study of law and social change beginning to emerge from longitudinal research on trial courts, but also that the temporal dimension they incorporate is of vital significance for our understanding of law and society more generally.

A. Small-Scale, Multicausal Models

First, longitudinal research on trial courts affirms a conclusion reached by Richard Abel (1980), in his review of law and society research a decade ago, that it is the interaction of legal institutions and society on many levels that is interesting and important.[2] The picture emerging from recent research on trial courts reveals a multicausal interaction in the form of many interlocking small-scale processes among actors. For example, contrast a classic impact study of the effects of court decisions on the behavior of toxic tortfeasors with the complex understanding of litigation that underlies the concept of "case congregation" described herein by Marc Galanter, a concept that incorporates into the relationship between a court and its environment the network of interactions between courts and lawyers, lawyers and clients, and lawyers and other lawyers that evolves over time to produce a distinct longitudinal pattern of asbestos or DES litigation. As Sanders suggests in this issue, at this stage of our understanding of the relationship between law and society it seems both necessary and theoretically appropriate to explore how such interlocking small-scale processes operate in order to understand how larger aggregate patterns are created and changed.

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[1] Reviewing law and society research a decade ago, Richard Abel (1980) argued that concern for legal effectiveness, a concern embedded in the ideological premises of the legal system itself, so dominated theories about law and social change that research was limited to impact studies—studies that examined the law's effectiveness in producing behavior conforming with legal norms. In a related criticism, Austin Sarat and Susan Silbey (1988) have suggested that the field's fascination with policy questions focused law and society research on the gap between legal ideals and institutional practices. Studies of law and social change, they argued, ignored both the law's displacement of alternative conceptions of order and its constitutive role in maintaining the everyday and routine patterns of social life. Both criticisms have suggested that research on law and social change has often served to reinforce the ideological premises for the legal system itself.

[2] Although Abel uses the term "function" to describe the role of legal institutions in society, feedback mechanisms seldom exist to link the effects of law with the behavior of participants in legal institutions. Thus, "functional" is inappropriate as a formal theoretical concept describing law's causal role. Nor do I believe that Abel uses the term in its formal sense. As I discuss further in note 7, the purposive and instrumental quality of legal behavior is captured better by another theoretical concept, "intentional" causation.

B. *Closing the Gap: Merging "Court-centered" and "Dispute-centered" Research*

Longitudinal research also clearly reveals that trial courts and communities are linked on many levels. Indeed, one emerging realization is that legal institutions and community interpenetrate so thoroughly that legal institutions simply cannot be understood without explicit attention to their community context. This insight is inconsistent with any meaningful distinction between "dispute-centered" and "court-centered" trial court research, a distinction that has been used to characterize longitudinal trial court research depending on whether the interest of the researcher is the origin and processing of disputes or the work of the courts (Friedman, 1989b).[3] The characterization of the field as made up of dispute-centered and court-centered research also risks perpetuating the theory that courts are independent, neutral processors of disputes, rather than part of a legal system that is intertwined with disputing and conflict in the society.

Studies of courts over time show that we cannot maintain the distinction between "dispute-centered" and "court-centered" approaches to understanding the interconnections between legal institutions and community. Legal institutions and their ideology may be deeply embedded in community life (Merry, 1985), constitute an omnipresent and potent alternative to traditional means of dispute resolution (Yngvesson, 1985b; Merry, 1982; Cooter and Rubinfeld, 1990) and underlie the tensions created by efforts to remain outside of the influence of legal culture (Greenhouse, 1986; Moore, 1978; Engel, 1984).

Research that purports to be court-centered is equally about the relationship between law and community. Definitions of conflict, the legitimacy of nonlegal authority, and the influence of the social organization of the community (and the state) on the perceptions and actions of courts and court personnel are issues for court-centered research. Lempert's reconstruction (1990) of his longitudinal study of the Hawaii Housing Authority demonstrates that these influences may alter the significance of formal outcomes for both disputants and tribunal or far outweigh them in importance. Such findings are also a reminder that "court-centered" research should not focus exclusively on formal or intended effects of legal decisions or legal change.[4] Therefore, investigators must

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[3] I disagree with Friedman's characterization of most longitudinal litigation research as court-centered, because the goal of most researchers is in fact to explain the origin and processing of disputes, and the court as a separate entity is treated as mere recordkeeper, playing little or no role in the explanation of disputing or litigation.

[4] This perhaps explains why there has been surprisingly little interest in formal litigation outcomes in studies of the relationship between community change and trial courts (Friedman, 1989b), since community and courts are intertwined in so many other significant ways. In a different theoretical frame of reference outcomes have been of great interest to those attempting to un-

recognize that courts and disputes are part of the same social and political processes of conflict and conflict resolution in society.

C. Incorporating a Temporal Perspective into Theory

The temporal perspective of longitudinal research should improve our theories of what happens when courts and community interact. Some illustrations will demonstrate how a temporal perspective contributes new insights about the role played by trial courts and, more generally, by law.

Continuing Relations. One of the central insights of law and society research has been the continuing relations hypothesis (Macaulay, 1963; Lempert and Sanders, 1986; Blegvad, 1990), which characterizes relationships as either episodic or continuing and predicts greater use of law to resolve conflict by parties in episodic relationships than by parties in continuing relationships. The insight has become an axiom in cross-sectional research. Yet, its fullest implications are longitudinal, for in longitudinal perspective it is possible to see that the maintenance of continuing relations is always problematic and must be explained just as the maintenance of all forms of social order must be explained. Viewed in a longitudinal perspective, legal institutions, including trial courts, may contribute to either maintaining or changing the reciprocity underlying a continuing relationship (Engel, 1984; Blau, 1964). Further, over time, many continuing relations are maintained through hierarchy rather than through reciprocity, and the law's role in contributing to the maintenance of such a continuing relationship is an important issue (Yngvesson, 1985a). Simply classifying types of relationships as continuing, without examining the processes by which they continue or change, conflates actor choices and the social structure within which actors make choices about dispute resolution. By viewing continuing relations as a process taking place over time, we discover the contingent alternatives—the social structures—that shape actors' choices at a given moment. The presence of choice gives us something to explain, and as David Engel observed (1990) in his essay in this issue, ongoing change produces conflicting claims and conflicting systems of dispute resolution to choose among in every society.

Actor-oriented Perspective. Research on trial courts that has incorporated the dimension of time has strongly suggested that an actor-oriented perspective is needed to detect the ambiguity created by change-induced conflict and to understand the choices made by actors from among competing interpretations and com-

derstand the internal working of courts as organizations because formal outcomes bear a much more direct relationship to the formal and informal processes within trial courts than they do to the external environment of courts (Padgett, 1990; Seron, 1990).

peting claims of authority. Pierre Bourdieu (1977) has explained why. Because of the presence of ambiguity in the situation that an actor does but the observer does not see, individuals in the middle of a stream of events perceive the significance of their own decisions differently from the way an observer does who examines the completed sequence of events. Thus, without benefit of an actor-oriented perspective the observer may be unable to understand changes in the individual's behavior when they occur. Our theories about changes in the activities of trial courts and their effects will be able to explain more if they take into account the meanings that situations have for the actors that are presented with them. Authors in this issue (Yngvesson, 1990; Blegvad, 1990; Lempert, 1990; Sanders, 1990; Cooter and Rubinfeld, 1990; Mather, 1990; Galanter, 1990), and many others (e.g., Greenhouse, 1986; Friedman, 1985; Boyum, 1983; Zemans, 1983; Schwartz, 1964) make this point eloquently in their research, by showing, for example, that an individual's participation in formal dispute resolution depends on beliefs about its meaning and likely effects that may not be deducible from the "objective" perspective of a nonparticipant. Their suggestions for the development of theory may provide assistance in fashioning a systematic approach to research. For example, microeconomic theory may provide a logical framework for understanding actor choices (Cooter and Rubinfeld, 1990; Peterson and Priest, 1982). The logical structure of microeconomics may accommodate a wider range of factors than is customary in microeconomic explanations (see Sanders, 1990, for discussion and compare Edwards and Tversky, 1967). Blegvad (1990) suggests another theoretical starting point in the study of cultural orientations, or "thematizations," that result in framing similar actor choices in quite different ways (see also Luhmann, 1981).

Organizational and Institutional Actors. Longitudinal research can explore the special roles of "repeat players." Indeed, it may be necessary just to identify repeat players, because their experience or influence is accumulated over time and possibly through many different types of contacts with legal institutions. It takes longitudinal research not only to identify the "repeat players," but also to examine the effects over time of the accumulation of experience and playing for the longer run on the capacity, goals and success of repeat players in dispute resolution.

Research over time on courts convincingly demonstrates that the courts and the state are important factors, a compelling conclusion that has often been ignored. Almost unremarked in analyses of longitudinal trial court data are the changes in court processes that affect litigation—changes in court jurisdiction, or personnel, or laws affecting the legal bases for cases or the conditions under which they may be brought. Likewise, the impact of far more subtle organizational and ideological changes in court processes may

often be present but not noted. Indeed, it has been rare for studies to look at such changes as interesting in themselves or as other than random events that require adjustments in the analysis of docket data to make litigation rates comparable over time. If such changes, initiated by the courts themselves, were systematically studied, it would be readily apparent that courts themselves are important actors in shaping perceptions of what constitutes a litigable "case" (Galanter, 1990), in reflecting the competition among groups contending for power through control of the jurisdiction of the courts (Stookey, 1990), as a measure of the impact of professionalization of roles within the legal system (Padgett, 1990; Seron, 1990), as an indicator of local or national policymaking (Heydebrand, 1990; Monkkonen, 1990), and as a manifestation of the power of the state in community life generally (Moore *et al.*, 1990).

Thus, *longitudinal* research on courts is important for the development of theory because the presence of temporal processes that give rise to, maintain, or change the social organization of law may easily be overlooked in studies of law at a single point in time. Longitudinal research seems particularly suited to studying the effects of processes that make legally significant relationships and meanings contingent and to studying actors whose power uniquely fits them to exert influence over the longer run.

II. THEORY

Critics of longitudinal studies have noted the need for greater precision of thought about the processes of litigation and change (Krislov, 1983; Daniels, 1984; Munger, 1988).[5] They have also argued that, unlike the relatively narrow theoretical focus of previous studies, a broad range of ideas might be explored by means of research on trial courts over time. Both suggestions direct researchers to pay more attention to theory, for theory focuses both data collection and analysis on particular questions. A proposition so basic to social science research might once have required no further discussion. But, because important questions have been raised about the usefulness of theory, both with respect to law and society research (Sarat, 1985; Peller, 1985; Trubek and Esser, 1989) and

[5] Critics often point to the gap between the concepts employed and the measures constructed from statistical data or litigation rates to represent them, the often inappropriate use of statistical techniques, and the absence of detailed data about the decisions of litigants and courts. But most critics have argued that the problems of the field lie deeper, in weaknesses of conceptualization and theory. For example, critics have often begun by examining the units of analysis that docket studies have often taken for granted, "case," "court," and "dispute" (see Engel, 1980, 1990; Kidder, 1980-81; Yngvesson, 1988), arguing that the meaning of each unit of analysis depends on the perspective chosen from which to view it. Thus, a "case" means something different to a potential litigant and a court clerk; the "court" has different boundaries for the litigant, the court clerk, and the observer employing a theory of complex organizations.

social science research more generally (e.g., Gouldner, 1970; Foucault, 1973; Unger, 1976; Harding, 1986), two points about its value in pursuit of understanding and its costs to the researcher may be helpful.

First, it must be remembered that choosing a theory is choosing a starting point.[6] Recognizing that theory is necessary means recognizing that all research requires provisional commitment to assumptions and questions that guide inquiry (compare Nelson, 1988a). We have moved well beyond thinking that a dispute, case (cf. Engel, 1990), or court (cf. Seron, 1990) is an obvious or natural unit of analysis and an obvious starting point for longitudinal trial court research. A particular unit of analysis is derived from a theory that offers a provisional explanation of whatever interests the researcher. Yet, because there are no natural units of analysis, adoption of a theoretical perspective highlights another problem. There are no research questions that do not raise problems of perspective which may deeply divide the research community (Abel, 1980). While making a commitment to a theory results in more coherent and precise development of insight, a researcher may see this commitment as imposing a cost if the researcher recognizes its provisional nature and is simultaneously drawn to other theories. Of course, some theoretical premises may reflect a researcher's deeply held values (e.g., the belief that discrimination or hierarchy are central problems for law), while other premises required to develop the assumptions and questions for the particular research project do not (e.g., the choice to use interpretivist methodology to examine the context of potential litigants' decisions to use law to resolve conflict). Making values, cognitive framework, and methods explicit helps both researcher and audience interpret the research more accurately.

Second, the utility of theory lies in part in guiding the researcher to relevant data. This lesson is useful in view of the increasing emphasis on units of analysis that take their meaning from contextualization and actor orientations, for example, by looking to the parties to disputes or litigation for the definition of a "dispute" or a "case." An approach that relies on denser contextualization and greater emphasis on actor orientations fits well with the advice of the field's most constructive critics (compare,

[6] A theory formulates questions for research. The mere fact that research is theory-driven has no bearing on the choice of methods for research, for example, whether they are quantitative or qualitative, observational or archivally based, or whether still other techniques or some combination of those just mentioned are selected. But the adoption of a *particular* theory may have important implications for the particular methods that one might use to answer questions, since some ways of gathering or studying information seem better suited to answering certain kinds of questions, and because some theories imply a particular relationship between this researcher and the subject of research, as, for example, in the case of Bourdieu's claim (1977: 8–9, 171) that a mere observer cannot understand the whole process of contract formation.

e.g., Engel (1990) and Reiss (1990), who while approaching the subject from quite different research perspectives, agree on the need for more contextualization). At the same time, such an approach reflects movement in the field itself toward research on legal ideologies, the meanings actors ascribe to their activities, the contexts of dispute resolution, and interpretive methodologies. While this enrichment of actor-oriented perspectives has been an important development for studies of courts over time, it further underscores the need for theory to guide data collection. Researchers who pursue disaggregation and contextualization of litigation must remember that these research strategies may lead to greater detail but not necessarily to better understanding. The accumulation of detail provides closer views of actors and behavior, but it makes it more difficult to find threads of meaning or causal links of significance for such important units of analysis as the state, a court or a community, family or corporation. Theory can help distinguish useful from superfluous detail in the search for coherent explanations.

Drawing on essays in this issue and elsewhere I will describe theory that appears to be promising for longitudinal studies of trial courts.

A. Revisiting Disputes

Focusing on the dispute (and when appropriate—the case) will continue to be important. Further, as argued persuasively by Mather (1990), longitudinal studies of litigation have much to offer studies of dispute processing. I will not repeat her excellent survey of theoretical issues that can be addressed by this research, but I will mention the following as particularly important areas for development.

The most important, of course, is the opportunity longitudinal research on trial courts provides to study dispute processing as a true sequence of events and as a process that changes over time. Two conclusions follow. The field would learn much from studies in which particular conflicts are followed over time. These studies are needed in order to understand how particular disputes emerge, or do not emerge, from a context of social conflict and how participants choose among alternatives for action. The longitudinal perspective might be of relatively short duration, even focusing on the life history of particular disputes (see, e.g., Yngvesson, 1988), or of relatively long duration (compare Wollschläger, 1990), but in either case the investigator should be sensitive to the relationship between social conflict, a context for action, and disputes, which are particular manifestations of conflict (Kidder, 1980–81).

Second, Galanter's "case congregations" is a unit of analysis that is far superior to the case-type or dispute-type unit of analysis used in most longitudinal studies. The case congregation is defined

by the social construction of disputes or cases rather than by their doctrinal or formal characteristics. Further, the concept incorporates the idea that the social construction of disputes or cases changes over time. To Galanter's list of endogenous factors that affect the social construction of a case congregation we should now add the following, as suggested by research reported in this issue that underscores the importance of the court's role in litigation: the court's role in maintaining the legitimacy of the state, the tension between professional and managerial roles of judges, and the political pressures transmitted to courts through dependence on the executive branch of government for resources, prosecutorial policy and statutory rights.

Notwithstanding the fact that most would consider Galanter's (1974) description of the litigation advantages of "haves" over "have nots" among the most fundamental in the law and society field, longitudinal research has often failed to make adequate distinctions among litigants, ignoring in particular the influence of organizations or the government—typical "haves" who dominate many areas of law or litigation. As I have explained, longitudinal research provides an opportunity to identify the frequency of participation by particular classes of litigants, the cumulative effects of repeated contacts with the legal system on "repeat players" or "haves," and the long-run effects that repeat players have on other actors.

B. The Context of Litigation

Although many insights have been gained through case studies of courts that trace subtle lines of influence and connection between courts and communities, there is room for more systematic development of theory about the influence of particular characteristics of communities. As Bourdieu (1977) recognized in his argument for the importance of adopting an actor-oriented perspective, there are relatively fixed social structures that constrain the behavior of the individual actor. Yet, relatively little longitudinal research has explored the effects of family, neighborhoods, or community, or the influence of class, religion or ethnic groups on disputing or litigating (with some wonderful exceptions, e.g., Greenhouse, 1986; Upham, 1987).

Culture (and its localized manifestations—legal culture, dispute culture, professional culture, etc.) is often mentioned, but poorly theorized, in longitudinal research as a potential explanation for change. Culture is manifested in preferences for particular social practices where alternatives exist. To understand such preferences longitudinal studies must incorporate enough knowledge about the historical context of trial courts to identify the alternatives available to contemporary actors and to explain the preference for particular choices in dispute resolution or litigation.

In an essay in which he argues that culture, namely, popular expectations of noncompensation, underlay the limitation of tort liability prior to the early twentieth century, Friedman (1987) illustrates how research on the influence of culture can be undertaken. He shows that systematic examination of parallel institutional developments (in litigation, the insurance industry, medical treatment of industrial accidents, corporate management, and the legal profession) permits inferences about the assumptions of everyday citizens that underlie the differences between late nineteenth-century and contemporary twentieth-century preferences for types of risk management. Friedman's recreation of the context in which risk was experienced was clearly necessary in order to infer that a culture with particular preferences or expectations existed, and that that culture may be linked to compensation seeking through the tort system, receptivity to expansion of tort liability by courts, and awards of larger amounts of money by juries, all of which typify post-nineteenth-century changes in the U.S. system of tort liability. Thus, incorporation of culture into longitudinal studies of trial courts in a way that will permit accurate conclusions about its content and role in change—in a way that permits development of theory—will require research that penetrates even more deeply into the context of litigation in order to understand the choices made by those whose actions are being studied.

C. Lawyers

One of the most surprising oversights in longitudinal trial court research (and more generally dispute resolution research) has been systematic study of the role of lawyers and the legal profession. Lawyers constitute a resource for litigants and exert a significant influence over the social construction and transformation of disputes. Access to justice issues have historically focused on the availability of lawyers for underserved classes of potential clients, such as the poor. Determining what lawyers are available to which potential disputants and how the contacts are made is an important issue in its own right as well as an important factor in explaining patterns of litigation in trial courts over time (compare Galanter, 1974).

Reiss (1990) noted the need to understand better how clients and lawyers negotiate meanings and strategies of dispute resolution, and these interactions have implications for trial courts. The business of trial courts first passes through the hands of lawyers who must negotiate with clients and with other legal professionals about what to do next. In turn, lawyers interpret trial court behavior for clients and, more broadly, the public, affecting their perceptions of disputes and dispute resolution.

Lawyers, of course, not only have a vital influence on dispute resolution and litigation, they are repeat players in their own

right, who may have a large stake in the outcome and whose interests differ from those of disputants or litigants. Thus, their influence over the social construction and transformation of disputes reflects not only litigants' ability to purchase lawyers' services but also the general ideology, training, and needs of the profession itself. Longitudinal studies of trial courts offer a starting point to examine the influence of changes in the organization of law practice and even the influence of lawyer training. In this respect, patterns in the handling of routine cases will be more revealing. Patterns of case filing, settlement, litigation or use of particular legal strategies, when fully explored, may be usefully understood as having been influenced by the goals, needs, or rhythms of law practice (Gordon, 1984a; Munger, 1987a).

D. Trial Courts as Complex Organizations

In paying greater attention to the context of cases or disputes and to the orientation of actors, longitudinal trial court research must not overlook the importance of studying disputing, litigation, and trial courts from the perspective of other levels of social organization. The dispute centeredness of much of the existing longitudinal research has drawn both quantitative and qualitative research in the field toward theories that explain disputing or litigating from the perspective of the individual litigant (see especially Felstiner, Abel, and Sarat, 1980–81; compare Sanders, 1990). As a result other perspectives or units of analysis have been ignored.

The prevalence of the dispute-centered approach has led to an important theoretical omission, the organization of the courts themselves and the differences among courts (Seron, 1990; Daniels, 1990; Clark, 1990; Ietswaart, 1990; and compare Harrington and Merry, 1988). Courts are not a constant in patterns of disputing and dispute resolution but are continuously interacting with lawyers and litigants, constructing interpretations of disputes, and changing internally in ways that affect both of these processes (Seron, 1990). As Jacob has maintained for some time, more emphasis on the study of courts as organizations is long overdue (1983a, 1983b). Longitudinal research has only recently begun to consider trial courts in light of the theory of organizations, for example, by examining the effects of their formal and informal organizational structure on what they do or by examining the activity of litigants, judges, prosecutors, attorneys, juries, court clerks, or other court officials from the perspective of their roles in a complex organization (Seron, 1990; Padgett, 1990).[7]

[7] In some obvious ways the "gap" or "legalist" paradigm of law and social change still poses important empirical questions that must not be overlooked as we move toward a broader, more inclusive understanding of law and change. First, the primary question posed by the legalist perspective, namely, what are the symbolic and instrumental effects (Lempert, 1966) of decisions

Not only must the changing internal structure and processes of trial courts be better understood in longitudinal research, but interactions between courts and their environments should be understood as self-interested. No published longitudinal study that I know of considers the relationship of trial courts to alternative forms of dispute resolution or how the emergence of rivals to courts might change the behavior of the courts (and further what effects those changes may have on the rivals) (but see Tanase, 1990).

Although a number of essays here warn of pitfalls of comparative research, comparisons among courts will be useful and revealing, particularly where the courts share a common legal culture and political system. Padgett's exploration (1990) of the influence of court caseload, political environment, and judicial background on plea bargaining in various federal district courts is a brilliant example of how comparative research may illuminate historical processes we cannot observe but which we can reconstruct from the variations in conditions for litigation produced by differences between courts. Many state trial court studies have involved more than one state trial court, and yet, with the exception of Daniels's essay here, different trial courts are treated as a constant having no effect on the patterns of litigation. Such an assumption seems unwarranted, and invites even closer examination of these data.

E. Doctrine and Politics: Trial Courts as the State

Longitudinal research can help to develop another perspective, that of trial courts as part of the state. Trial courts are necessarily tied to the political system of the society. For example, they have always depended on resources provided by other units of government, been influenced by the politics of legislation and judicial selection, and been affected by the political and administrative priorities of state prosecutors as well as by those of other state agencies responsible for the creation and administration of state policies that may be challenged or enforced through the courts. In

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and other trial court actions, is not uninteresting or unimportant. Second, even if the answer is that these effects are small or irrelevant to the role played by law and by trial courts, or conversely, marginal to the frame of reference of some participants in conflict or dispute resolution outside of courts, the theory that they do have an effect strongly influences the behavior of many of the actors in settings studied by longitudinal trial court research. Indeed, the paradigm of "intentional" causation, in which an actor is influenced by the anticipated effects (Elster, 1985) is often a more accurate description of the relationship between law and social action than a "functional" causation paradigm in which an actor is said to influenced by the effects of action through a further feedback effect. Thus, what has been called a "gap research" paradigm may be pursued with other objectives in mind, for example, with the objective of examining beliefs about the legal system that continue to make the goal of effectiveness central to the maintenance of the behavior of trial courts and other legal institutions.

addition, courts are constrained by the major function they perform for the state—legitimation (compare Scheingold, 1974). Prior research has overlooked the important connections that the courts provide between community and the state by legitimating and distributing power in response to political processes taking place within (Heydebrand, 1990) as well as outside the state (see Handler, 1978; Burstein and Monaghan, 1986). In examining each of these aspects of the embedding of trial courts in the state, investigators may find useful Erik Monkkonen's insight (1990) that the American State has its federal, state, and local moments, each with its own political process, and that it may be the political economy of the local state that most influences trial court behavior.

Finally, trial courts provide a record of changes within the legal system itself, including the evolution of causes of action, of court procedures, and of professional roles. The role of formal legal process is of great importance to the state and often evolves as accommodation by courts to changes as experienced and perceived at the trial court level. Thus, the evolution of the formal legal system reflects the interests of the state, judges' interpretations of the law and of their professional role, and the changes in courts created by the conditions under which they operated. In turn, these changes in the operating definitions of appropriate uses of state power have an impact on the work of courts and the conflicts that are brought before them. Longitudinal research on the interaction of doctrinal change and the behavior of trial courts and litigants is an important emerging area of research (Peterson and Priest 1982; Green, 1985; Burstein and Monaghan, 1986; Eisenberg and Schwab, 1987; Schwab and Eisenberg, 1988). Other opportunities exist for exploring the internal organization of state power and its effects. For example, many of the North American longitudinal trial court studies have encompassed the period in the late nineteenth and early twentieth centuries in which administrative law emerged, first through the use of common law writs to review the actions of government officials, and later as an exercise of statutory jurisdiction. The parallel evolution of administrative law and the administrative capacity of the state is an important chapter in the evolution of U.S. law, but it is also important in the evolution of the economy and the American state. In both contexts, by exploring trial courts in greater depth longitudinal research will add to our understanding of motivations for, resistance to, and the structure of responses of courts and the state to the shifting nature of political and economic conflict.

III. LIMITS IMPOSED BY RESEARCH METHODOLOGY

If the important characteristics of longitudinal research on trial courts are its focus on change and its emphasis on closer examination of process, what, then, are the implications of these characteristics for the methods employed by this research?

A. Rates

Many criticisms of longitudinal trial court research have attacked the methods used to construct litigation rates (see Munger, 1988; Lempert, 1978; Sanders, 1990; Reiss, 1990). These criticisms seem sound. The present state of research still leaves much work for those who wish to employ quantitative measures. But the improvement of measures creates greater demands for data. For example, distinguishing between rates of incidence and those of prevalence, as forcefully argued by Reiss, requires that data on litigation, or any other aspect of disputing or litigating, reveal the identity of the party with sufficient clarity that we can determine how often particular parties appear. Further, disaggregating types of cases requires that the researcher bridge differences in official reports, or better, create appropriate categories for cases based on a content identified as relevant by theory or by sociologically (and cross-jurisdictionally) meaningful categories of behavior. But these problems may be relatively simple when compared with the task of constructing rates that properly reflect the effects of the intermediate processes of litigation or dispute resolution, for doing so means finding a base that can measure or stand for underlying causes. Thus, while sloppy practices in construction of rates may no longer be acceptable, given the extensive criticism offered to date, they may be extremely difficult to remedy due to the unavailability of information about underlying sources or causes of litigation.

B. Models

Fewer critics have focused on the unique problems of modeling processes of conflict, conflict resolution, or litigation over time. Modeling of causal relationships presents fewer problems if it is assumed that causal processes in the past are the same as those in the present (or at other times studied). But we know that moments in the past may be culturally different from those at or nearer the present and that the social organization of conflict and litigation may have quite different causes at different times. It is for this reason that Reiss (1990) argues that research on the past should be conducted *as if* it were being conducted in the present, as if we do not know the real outcomes of social processes and are thus forced to learn enough about then-contemporary culture and social organization to make a reasonable prediction about the direction of change on the basis of prechange information alone. For

example, reasonable arguments constructed in the 1890 about the evolution of the process of compensating for industrial injuries might have called attention to very different aspects of society and social change than arguments about that time constructed by someone living today, because arguments constructed today would take for granted the causal forces that may appear from our present vantage point to have determined the changes and ignore others that appeared significant in 1890s and influenced the behavior of those who believed they were significant (see Friedman, 1985, 1987; Reiss, 1990).

Cultural differences between pasts or between past and present are not, however, the entire extent of the modeling problems. In addition, longitudinal research on trial courts must solve a problem in modeling causation familiar to historians, namely, that the flow of events in history may be the product of unique, not continuing, or repeating, processes and that a series of events occurring in the past may change the conditions under which similar events subsequently occur (Stinchcombe, 1968).[8] To date there has been limited effort to take account of such "period effects" in longitudinal research on courts, though there are examples of excellent efforts to do so in this Special Issue (e.g., Padgett, 1990). Galanter (1990) provides an elegant example of an attempt to account for period effects in explaining litigation patterns. He describes how litigation of a sequence of similar cases over time changes the conditions under which later cases of a similar type are brought, thus causing changes not only in the pattern of litigation but also in the way external factors (e.g., accidents, debts, crimes) influence the pattern. In longitudinal research more generally, each change in court structure, law, or legal culture may be viewed in a similar way—as a unique event that may change how litigation or court processes are conducted for all future cases. Models that can account for the effects of such unique events are obviously more complex than models that assume that the patterns of causation explaining litigation remain the same over time (see Cooter and Rubinfeld, 1990). Yet, such complexities will have to be carefully considered in order to create models that explain patterns of change over any significant period of time.

C. Data and Databases

Richer contextualization, new theories, more precise measurement and analysis, and better modeling will require greater efforts to collect data. In conducting new research more effort will have to be expended to gather data that capture more detail. Greater

[8] I am indebted to Mayer Zald for this observation on the importance of historical models of change and the frequent misapplication of ahistorical causal models in longitudinal research. The comments were made at the Conference on Longitudinal Research on Trial Courts at SUNY Buffalo, August 1987.

economy will have to be exercised, particularly if the collection of dense contextual data is combined with the collection of docket data. More attention to theory in advance of data collection will, of course, produce economy of design and data collection, but given the rising costs of data collection, it is also important to think about preserving and sharing data.

Data sharing is always a good idea, if less widely practiced than advocated. Yet it has problems that ought to emphasized with an example. Before these essays were solicited, I had thoughts of gathering existing longitudinal trial court data sets and arranging them in the same format so that comparisons could be made and critiqued more easily. The attempt was thoroughly discouraging. Data collection appeared to be incomplete in some cases; that is, the original design was not completed and only partial sets of data were collected. Next, many longitudinal data sets were incompletely or poorly documented. Finally, data sets that had drawn their inspiration from the same source, and reputedly shared a common or similar design, were in fact so dissimilar that it was impossible to identify case classifications as common as tort or property in a similar way in purportedly similar studies.

My conclusions from this failed attempt to create a database from existing studies are both substantive and critical. First, the research methods for carrying out and documenting research designs used by all of us have often been less than ideal, rendering data unusable by others. Second, and more importantly, the difficulties of creating common categories for caseload analysis even across North American jurisdictions confirm arguments made in these essays about the importance of local legal culture. Differences in the local meaning of legal action should be anticipated and, if possible, documented. Ideally, the differences might be systematically documented and examined by collecting data other than official records, although in retrospective research this may not be possible. Third, and equally important, investigators who have quite similar perspectives on courts and change and who have addressed a similar issue (e.g., the impact of rapid economic expansion on litigation) have framed what purports to be "the issue" in significantly different ways, and have thus been led to collect different data or categorize data in different ways. The reason for some differences is in part that insights of different investigators have led in different directions and in part, no doubt, a result of pressures to make an original contribution. At the same time, the lack of careful attention to how others have examined the same question has made it difficult for results to be compared. Fourth, and more generally, it seems unlikely that investigators with substantially different interests or perceptions of processes of dispute resolution litigation could agree on data that would satisfactorily meet the needs of each.

Notwithstanding this discouraging experience, database con-

struction and data sharing may make an important contribution to longitudinal trial court research. The great costs of longitudinal research make it worth contemplating how each particular project might be made as useful as possible to other investigators (compare Fienberg, Martin, and Straff, 1985).[9] The goal for the individual investigator might be to gather data that will allow comparison with, and interpretation in light of, relevant work by others who share an interest in the same questions. The investigator also bears the responsibility to attempt to contextualize the records so that other understandings of actions or events occurring inside and outside of the court (e.g., a dispute) can be utilized and distinguished from the official record of the events (e.g., Clark, 1990; see Ietswaart, 1990). Remaining sensitive to the problems of uniqueness and self-reference inherent in official documents and attempting to offset them in the initial research design will go a long way toward making data more useful to colleagues.

Beyond such self-imposed collegiality, the pressure to economize will urge the creation of data bases that may be shared as well as compared.[10] Collaborative, long-term data-collection projects exist in many other fields of study. While much that has been said in this Special Issue argues against the possibility of creating a set of standardized conceptual categories for longitudinal research, the usefulness of data on courts may be increased by including from the outset contextual data from sources other than official records (cf. Ietswaart, 1990) that permit users to avoid assumptions about the constancy of the meaning attached to formal processes and official categories for behavior. Nevertheless, creating a database for use by a large number of investigators will involve choices of perspective, concept definition, and theory. These choices should be sensitive to the need to link future research to existing work and to the importance of comparing and contrasting alternative conceptualizations of processes and events relevant to courts and change. Such decisions about the investment of resources are of concern to all interested in law and society research, and should be informed by a broad range of interests and views on their substance and the methods of the research.

What part should collection of docket data play in the construction of databases? Many of the conceptual and methodological problems discussed in this issue have arisen from the misuse of docket data. Notwithstanding these problems, longitudinal research on courts may continue to depend in part on docket data.

[9] This report by the Committee on National Statistics of the National Academy of Sciences addresses the need for and technical aspects of data sharing as well as the benefits for a discipline and its policy applications. I am also concerned, as this report is not, about the problems and limits of attempting to specify theoretically substantive goals for a data collection as a matter of public policy.

[10] These may usefully be thought of as alternative models of collaboration. See below.

These data will be useful to investigators interested in conflict resolution, disputing, the courts, lawyers, the state, and many other significant areas of study. As Lempert suggests (1990), much useful research might simply extend the focus on the formal context of litigation in particular directions, as sources permit, with each study drawing on a common core of data derived from official sources and incorporating its categories—dockets, budgets, and administrative and legislative records. We have recognized that such information is not self-explanatory or "objective," but that does not make it less central to many questions about courts and social change. Thus, docket data might continue to comprise one important kind of information included in a database that would be of interest to many investigators, particularly investigators engaged in retrospective research.[11]

For the foregoing reasons, longitudinal research on trial courts can be a collaborative effort. Either of two models may be adopted. First, individual investigators can make a conscientious effort to construct parallel and interlocking research projects providing related, yet alternative, perspectives on common issues. Second, databases may be created for general use. In either case, it will be wise to remember that there is no agreement about core issues for research, though there is widespread interest in many of the particular questions discussed here. Yet there can be agreement about the value of some types of data, in particular data about the courts themselves. I have argued for the importance of making data useful to as many users as possible. To further this effort, I have stressed the particular importance of gathering infor-

[11] What data should be included in a particular database for longitudinal study of trial courts seems a less difficult problem than the more important issue of what data are required to answer particular questions (see below). For retrospective research the options for data collection will be limited in most cases by the nature of the surviving court records and the likelihood that other types of information will be unavailable. These limitations make specification of "core" data easier by default but will make theoretically driven research more difficult because of the lack of information about theoretically significant units of analysis or processes. A retrospective study of trial courts employing docket data should attempt to include the basic information that exists in most official records together with usually available supplementary information that will render the docket data useful to a broad range of perspectives and theories. For example, the investigator should gather information about the number and types of cases, types of litigants, presence of lawyers, intermediate processing (such as jury trial), and formal disposition of cases. The investigator should also include basic data about the court structure and personnel, e.g., numbers and backgrounds of judges if available, number and types of other court personnel, and details of the court's jurisdiction, organization, and control by other governmental bodies. Many existing docket studies have not systematically gathered these data, even though the data were available. Gathering such data does not guarantee comparability or usefulness for all research purposes, of course, since there are no standardized categories or measures of court activity. These data, however, recorded in the terms and categories created by the courts, are directly relevant for research on the behavior of the courts themselves and so provide one important starting point for other types of data collection about disputes, cases or litigants.

mation that permits contextualizing official records as a first step toward linking research addressing different questions and examining trial courts in different times and places.

D. Prospective and Retrospective Research Designs

To this point I have discussed rates, models, and data as if we might freely choose the best combination, subject only to funding limitations. A major reason for the gaps in the design of previous research, as well as for my discovery that projects that are similar in design often employ uniquely operationalized concepts, is that retrospective research must make do with available, often severely limited, data. There are often no supplementary sources to help explain or contextualize the official record of cases. The evidence drawn from official records of cases is thus "thin" (Krislov, 1983) by comparison with data that might be gathered about current disputes or court cases. Notwithstanding this limit, historical or retrospective research is important because it extends the range of available examples of human behavior over many years, where the alternative would be to begin collecting similar information now and wait years, or centuries, for it to accumulate.

A first lesson to draw from the difficulties encountered with historical or archival data is that research designs should be appropriate to the available data. This means that some kinds of questions cannot be answered adequately using historical data. For example, as the comments of several authors here make clear, conclusions drawn about disputes or disputing based on docket data alone will have many weaknesses, although such conclusions have been routinely drawn in existing docket studies. Where additional historical data on context are available, the design may be broadened to include disputing processes occurring wholly within the community outside the courts, legal culture, professionalization of court personnel, and many other aspects of the role of law inaccessible from docket data alone. Contextualization available at one point in time may permit richer interpretation of "thin" data over a broader time range by revealing a process whose existence may be tracked, if not examined directly, through docket data. It may be the case, for example, that a contextual study of tort litigation that occurred at some particular point in the past may explain low rates of tort litigation. While the contextual study may be possible for only a few years, an explanation for the continuation of low tort litigation rates over a longer period of time may be extrapolated in this way (see Lempert, 1990).

We may simply not be able to examine some hypotheses using the available historical data, and we should therefore consider the alternative, prospective longitudinal research. While prospective longitudinal research requires waiting for data to accumulate, research on some issues will demand this approach. Data sets cre-

ated prospectively should differ in at least one important respect from those created from historical and archival data: the information collected can be, and should be, conceptually grounded and not limited to the contents of official records. Thus, for example, the key unit in a prospective longitudinal trial court study need no longer typically be the case, as defined by the court system and described in a single data source—the case file. Rather, the researcher might seek information directly describing the set of actions—potentially including formal acts by officials or courts—that constitute the process the researcher is interested in (Ietswaart, 1990). Guiding and justifying choices among the many alternative units of analysis available in prospective research will be the role of theory. Prospective creation of data sets for longitudinal study of trial courts may be a valuable undertaking for the field.

IV. THEORY AND POLITICS

I view theory as a necessary part of longitudinal research on trial courts. Research will be advanced by using theory to stimulate alternative insights and to develop a more precise focus for inquiry, research design, and data analysis. At the same time, it should be clearly understood what I am *not* saying about theory. Theory need be neither value free nor universal. Its importance lies elsewhere. Theory is an attempt to formulate our understandings of the world as precisely as possible, understandings that are guided by values and are always incomplete and provisional explanations whose utility may be altered through additional experience with a changing world or which may be superseded at any moment by changing our minds about which questions ought to be answered.

The selection of a theory or starting point for research is not only of critical significance to the individual investigator, it is also of significance for the field of research (Abel, 1980).[12] The field, viewed as a whole, has spoken to some issues but ignored others. As I stated in my introduction, longitudinal studies of trial courts are fundamentally about change, about resisting change, about the evolution of conflict and difference, and about competition for power to control conflict and difference. Initially, the field chose to address these issues from a very limited perspective. Now, the issues are driving researchers to develop the field beyond its original boundaries.

The energy in the field was derived first from an interest in demonstrating that trial courts mirrored social change, litigation reflecting both the interests of those affected most by social

[12] As one participant remarked at the SUNY–Buffalo conference, "The question of what question to ask is the most fundamental, almost political question." This comment was made still more meaningful by the one which preceded it, in which the same participant observed that funds for research are limited.

change and the instrumental role of law in social change. More recently, interest has grown in the constitutive role of law, law viewed as embedded in a complex set of institutions and behaviors that constitute stability or change. In the constitutive view, legal institutions give meaning to social action, not only by defining rights but by contributing to the meaning of other, often empowering, elements of society, for example, difference—class, gender, and race—and by helping to define important goals or desirable procedures for the exercise of power both within and outside of formal legal institutions. Starting points for research based on this constitutive view of legal institutions may, of course, lie wholly outside formal legal organization and may consider trial courts as of minimal importance.

The attraction of examining the constitutive role of law in social change is that it represents an alternative to "legal centrism," capturing valuable insights about the independence of social organization from particular legal institutions and the competition between law and other forms of dispute resolution (see Griffiths, 1986; Engel, 1990). But in pursuing this view, one must not make the mistake of underestimating the constitutive role of the most powerful actor in many societies. Even though its role in certain instances may be a contested one, the state circumscribes the roles played by other actors in resolving conflict. Conflict over the distribution of power is ultimately conflict about control of the state. Thus, while we may recognize that a view of dispute resolution that considers only the formal activity of courts is too narrowly centered in the legal institutions of the state, it is highly important to attempt to understand why and how the state makes claims to power and to authoritative dispute resolution. Behind these claims lies the political as well as the self-interested role of the state.[13] Thus, to fully understand the constitutive roles of law, continuing thought and research are required to examine the state's role in conflict, including its role in selecting among competing forms of conflict resolution, its role in the distribution of power generally and among potential litigants in particular, and how the state both exercises its power and makes effective claims to the power it exercises. These are among the most significant issues for another generation of longitudinal trial court studies.

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[13] With respect to the political functions of courts, at the conference Lawrence Friedman observed that North Americans think of courts as part of their "government" while many other cultures, notably European, think in terms of a "state." "Government" and "state" have quite distinct connotations. Thinking of courts as part of government suggests that they are like administrative offices of government and exercise what is fundamentally perceived as nonpolitical authority. The concept of the state is more suggestive of power relations and the political uses of power, and societies with different political cultures are quite comfortable with the view that courts are a part of such a state. This difference in political cultures has, quite clearly, also influenced the approaches to courts taken by North American social science.