LONGITUDINAL STUDY OF TRIAL COURTS: A PLEA FOR DEVELOPMENT OF EXPLANATORY MODELS

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Longitudinal trial court studies lack a simple model of system litigation. This essay advances the argument that models which attempt to explain either trial court behavior or changes in civil litigation have little explanatory power. Rather, a system mobilization model is needed that has the following paradigms: (1) a perception paradigm that produces awareness of matters that might be the subject of a conflict or dispute; (2) a paradigm that explains how these detected matters are socially constructed as potentially legal matters; (3) a legal mobilization paradigm explaining how legal agents or some legal subsystem is mobilized to handle some legal matters; (4) a process paradigm explaining how matters that enter the trial courts become matters for trial; (5) a trial court mobilization paradigm followed by (6) an appellate process paradigm. The development of such a model of system litigation will resolve some of the methodological as well as conceptual problems that currently beset research on trial courts. A number of these problems relating to the longitudinal study of trial courts are discussed.

Nothing seems more problematic in the longitudinal study of trial courts than what is to be explained, and nothing seems more puzzling and labyrinthine than the explanations of the behavior of trial courts. To judge from current examples of longitudinal research, it is often not clear whether one is explaining variation in the behavior of different trial courts, or explaining the behavior of such inputs or outputs as disputes, complaints, filings, settlements, or jury decisions, or explaining such behavior of court participants and functionaries as decisionmaking by juries, by lawyers, or by judges. A cursory examination of the studies of trial courts discloses one source of this problem: we lack concise theoretical explanations of variation.

Lack of precision about what is to be explained is also reflected in the design and methods of research of these studies. Most have *retrospective* designs. A majority of these retrospective studies collect case information from one or more trial courts for some historical period and explain changes with variables that are not connected directly to the court case variables. This disjuncture makes inference problematic, yet rarely leads to rejection of hy-

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potheses because the absence of adequate theory about the connecting links leaves little ground upon which to reject even a weak explanation. Moreover, because in most instances the study is of but a single trial court, one cannot take into account how the properties of the court may account for the variation observed. Again, the absence of any theory about what produces variation in the units of a population of courts under study is a major drawback to serious inquiry.

A number of benefits would flow from more careful conceptualization of the problems to be analyzed in longitudinal trial court research. Studies could leave the realm of ad hoc explanations of past events and instead treat past events in terms of predictive theory. Further, solutions to many basic methodological problems, such as conceptualization and construction of rates, selęction of appropriate independent variables, and the design of the research would result from more careful and thorough consideration of what the research was about.

At present longitudinal trial court studies lack a simple system model of litigation. As I will show, theoretical development of a model of this type will bring into focus many of the other problems of the research. In this brief comment, then, I issue a plea for the use of explanatory models in trial court research as a focus for problems selected and as a guide to appropriate methods and design.

I. A SIMPLE MOBILIZATION MODEL

Studies of temporal change in trial court phenomena lack a system foundation. This is so in spite of long-standing recognition that one can understand litigation rates or courts as organizations only by taking into account the organization of the system of which they are a part (see especially Felstiner *et al.*, 1980–81; Mather and Yngvesson, 1980–81). Trial courts exist as part of an institutional legal system that is both formally and informally organized. The broader system includes both the organization of legal practice and alternative systems of dispute resolution, among other components (and see Mather, 1990, and Seron, 1990, for further discussion of these points). Moreover, the components are embedded in a social system that socially constructs conflicts and how they become legal matters.

Starting points for conceptualization of the system in which trial courts are embedded already exist in the theoretical literature on the emergence, transformation, and processing of disputes (Felstiner *et al.*, 1980–81; Abel, 1973; and see Mather, 1990), and in the parallel work on the mobilization of law paradigm in the processing of crimes (Reiss and Bordua, 1967; Black, 1973). As this literature shows, any such model for longitudinal court research must attend to at least four important problematics.

First, there must be a *perception* paradigm. A perception paradigm might be thought of as the combination of social-psychological and social-organizational factors that produce awareness of matters that might be the subject of a conflict of dispute. Felstiner *et al.* (1980–81) discuss some of the issues that attend conceptualization of the emergence of a "grievance" and its subsequent transformation into a "claim" against another (and see Coates and Penrod, 1980–81; Boyum, 1983). Development of general paradigms for perception will be at best a difficult matter because of the wide variation due to dependence of the process on the characteristics of individuals and the problems they experience (Felstiner *et al.*, 1980–81: 634–35, 641).

Second, the perception paradigm must be linked to a model of *how detected matters are socially constructed as potentially legal matters*. Note that the perception of a grievance can be regarded as independent of its definition as a potentially legal matter. What transforms the grievance over a construction contract into a legal claim or dispute is not obvious (Macaulay, 1963). And, indeed, just as significant others play a role in defining events as crimes and in determining whether the police are mobilized, so significant others and organizations play a major role in constructing individuals' issues as legal matters and in influencing the decision to seek advice or assistance from a lawyer (Mather and Yngvesson, 1980–81). Lawyers play a critical, though not an exclusive, mediating role between the official constructions and constructions of other actors.

Third, we need to understand how matters that are socially constructed as legal matters *lead to the mobilization of legal agents or some legal subsystem*. For example, what role do lawyers play once they have been drawn into a dispute? We need to understand much better what it is lawyers have some choice about and how that affects their social construction of matters for civil and criminal legal processing. Further, it is clear from what we know that there is considerable variation in the behavior of agents who transform disputes or who mobilize law depending on the substantive area of litigation. Interesting examples are provided by civil rights, products liability, and spouse-abuse cases where small subsystems of organizations or lawyer networks link citizens who have complaints to lawyers who litigate them as private or public law matters (compare Galanter, 1990, on the development and effects of such networks on litigation over time).

Finally, the fourth stage in mobilization is the *process by which matters enter the trial courts and become matters for trial*. This stage requires an understanding of the negotiation processes that go on between lawyers and their clients (see Sarat and Felstiner, 1986) and among plaintiffs and defendant lawyers in civil litigation. We need studies of how matters are defined for litigation akin to Sudnow's (1965) study of prosecutor and public defender decisions about "normal crimes" and how they are handled.

One suspects that the client may play a greater role in determining the fate of civil litigation than does the suspect in criminal matters (see Rosenthal, 1974).

These mobilization stages are followed by mobilization of the trial court itself and how it decides matters. And a final stage is the appellate process and its myriad levels.

In all these stages involving creation and transformation of grievances, mobilization of law, and dispute resolution, there is enormous variability that depends in part on variation in matters that are socially constructed as potential legal contests. Given the heterogeneity of the population of events that may become legal matters, it is unlikely that a single mobilization model will cover the detection of events, their social construction as legal matters, and the mobilization of legal agents. This in turn suggests that it may make very little sense to be concerned with explaining changes in overall civil litigation rates because of the heterogeneity in detection, social construction, and mobilization systems across case types and over time.

II. OBSERVATIONS ON METHODS OF RESEARCH: CONSTRUCTION OF RATES

All of this discussion of adopting a theoretical approach for the study of mobilization of law in longitudinal research on trial courts points up the fact that not only is there considerable complexity in understanding litigation rate changes but also the rates must be calculated for theoretical constructs which take that complexity into account. It is in this sense that *crude* rates (i.e., rates that include the entire caseload of a court) are rarely of theoretical interest and are soon abandoned for the calculation of rates for specific types of cases.

Further, many longitudinal studies of trial courts unfortunately use rates of litigation inappropriately as operational measures of theoretical constructs. Two problems should be noted about constructs and their measures. First, litigation rates are often used as measures of theoretical or empirical units that are not per se trial court events. For example, litigation rates from trial courts should not be regarded as proxies for either litigious behavior or litigiousness in persons or organizations. Litigation rates reflect the behavior of trial courts themselves in processing cases, but they do not directly reflect the sources of litigation. Litigious acts and litigiousness are socially constructed by discretionary processes that lie outside as well as within the trial courts.

Second, these concepts and their operational measurement using trial court case data invariably confuse measures of prevalence with measures of incidence. Criminologists have learned, for example, that aggregate crime and victimization rates are comprised of both a prevalence rate of offenders (or victims) and an incidence

rate of offending (or victimization). Changes in the aggregate crime (victimization) rate may result from a change in the prevalence of offenders (victims) or in the rate of offending (victimization) or both.

We may apply this logic to trial court litigation rates. Let us assume that a primary unit of analysis in longitudinal studies of trial courts is a *case* and that for any period of time we can calculate a litigation rate for a trial court. A court litigation rate may change because the prevalence of litigants changes, because litigation rate of litigants changes,[1] or both. That prevalence and incidence rates can change independently may be simply illustrated. More parties does not necessarily mean more cases. An increase in class suits, for example, might entail a substantial shift in the prevalence rate (because there are more parties) but no shift in the incidence of litigation (because there are no additional cases).

Inasmuch as there are plaintiff and defendant parties for every civil action, we have both plaintiff and defendant prevalence and incidence rates. What is more, there are separate prevalence and incidence rates for individual and organizational parties. The *prevalence* of organizational and individual litigants, for example, may change independent of each other either because of changes in their distinct underlying systems of mobilization or because of changes in the mix of kinds of litigation.

It is unfortunate that most studies of rates fail to take the individual or organizational status of parties to litigation into account. The importance of doing so becomes evident when individual or organizational rates of litigation are calculated using, respectively, individuals and organizations as the base of the rate (Reiss and Biderman, 1980). While individuals as parties to litigation are rarely "repeat players," organizations often are. Failure to consider the difference between organizational and individual parties in legal contests is then empirically as well as theoretically misleading.[2]

The foregoing should make clear the need to understand the relationship between any theoretically derived set of behavioral rates and the organizational intelligence systems that routinely generate information for those rates as a part of organizational operations.[3] Regrettably, our organizational intelligence systems such as police departments, prosecutors' offices, and criminal courts (and analogously, law offices and clerks of court offices)

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[1] Galanter's (1974a) typology of one-shot and repeat players in courts can be seen as qualitative expression of a litigant rate of litigation.

[2] For a discussion of how it can be theoretically misleading, see Galanter (1974a).

[3] All rates are based on some organized system of intelligence and therefore are subject to selection and attrition biases and validity and reliability problems that inhere in each particular organized system of intelligence. See Biderman and Reiss (1967).

rarely collect information either on the bases for rates or for variables that might explain variation in those rates.

What is more, since explanatory variables often are not available from (and generally not collected by) the organization that gathers information on rates, investigators engage in a practice of designating proxy variables. Thus, they may resort to the use of general economic indicators as proxy variables for measures of change in social and economic organization. It takes little examination to discover that the same variable—population or kind of litigation—is proxy to a host of different conceptual indicators. We should carefully scrutinize proxies used to explain changes in rates calculated from court data. And when the explanatory variables pertain to the behavior of the organizations or its members, for example, lawyers, judges, clerks of court, researchers may be surprised to discover how poorly that information is kept over time and how little information is available on any of the participants.[4]

These observations raise questions about attempting longitudinal studies of trial courts when there is virtually no decent data base for the calculation of rates or data on variables to explain variation in rates. Where the focus is on the behavior of the court or related organizations, the absence of such information is even more critical. One is left to construct rather simple descriptions of organizational change over time, given an absence of systematic intelligence on the behavior of the organization and its members. The absence of systematic intelligence raises some interesting questions about whether it might be more profitable to study how and why law offices and courts collect and retain the information they do (and don't) collect. What might change those organizational intelligence systems would also be a fascinating subject of inquiry.

It perhaps is well to remember that in the history of criminology, it has been necessary to forge organizational intelligence systems to aid both research and practice. Such special inventions as Uniform Crime Reporting, the National Crime survey of victimization by crime, and prosecution intelligence systems such as PROMIS were intended for both research and operational intelligence. By comparison, recent attempts at uniform court data reporting often appear to ignore both research and operational goals. A good example is provided by the attempts to report on offending by juveniles and their processing in state courts. Given considerable variation in state statutory definitions of juvenile offending and court jurisprudence over juveniles, much information is lost in the effort to effect comparability among states.

[4] For similar observations on the construction and explanation of rates in comparative research, see Ietswaart (1990).

III. OBSERVATIONS ON METHODS OF RESEARCH DESIGN

At the outset I noted that one effect of the failure to use theoretically grounded models in longitudinal trial court research was the practice in such research of employing *retrospective* research designs. As I explained, use of such designs invites ad hoc explanation and failure to attend to the micro structure of the mobilization of trial courts. As a final point I want to draw attention to the value of other research designs for longitudinal trial court research.

The absence of *prospective* designs is to a degree understandable, since one must invest considerable resources and wait the period of time necessary for the data to accumulate and to acquire the contemporaneous information to explain variation. Yet, prospective designs provide the best opportunity to test predictions and to understand the dynamic feature of trial courts. Their relative absence deprives us of the best means of understanding the micro structure and functioning of trial courts. Moreover, the absence of prospective designs that begin with civil matters before they reach the trial court also severely restricts our opportunities for understanding the mobilization of trial courts.

Retrospective research can be designed more effectively, however, than many trial court studies have been designed. One weakness of many retrospective studies is that they lack the data necessary for prediction. But by making prediction the objective of retrospective research, and thus by treating events in the past as if they might not have occurred, we would require that studies seek sufficient data to examine mobilization of law underlying litigation as specified by theory as well as sufficient data to control alternative explanations of the "future." Absent sufficient data to test the model, a retrospective research design might be deemed inappropriate (compare Lempert, 1990).

More appropriate designs can be used to focus retrospective research on the particular units of analysis made relevant by mobilization theory. We may distinguish two basic types of designs in terms of whether or not the behavior of the same units is measured repeatedly over time. We might, for example, follow court cases from filing to final disposition or we might follow private disputes from their origin in law offices to final disposition. Such designs have the advantage of giving a better description of the dynamics of change, including whether the units remain intact. We may learn something about how the dispute is transformed into litigation, how litigant composition and status change, and so on. Yet, panel designs have shortcomings. We may learn very little, for example, about how the events of a particular historical period affect the population from which the panel is drawn or whether the composition of the panel has an effect on the results.

A *cohort design* can overcome some of these limitations. Se-

lecting cohorts of courts or cases permits some examination of period and composition effects. Panel and cohort designs can be combined, moreover, to gain additional power in analysis. Following a panel of cases for successive cohorts, one gains the advantage of having each case as its own control for maturation effects while at the same time being able to estimate the effects of historical change and of change in the composition of a population of cases.

IV. CONCLUSION

The bottom line to these observations is that the need for the development of explanatory *models* that are theoretically grounded is palpable and pervasive. Yet, it seems we are unlikely to develop much explanatory power if we try to model something called "trial court behavior" or even if we try to explain general changes in trial court outputs. Just as our understanding of crime and criminal justice has been enhanced by modeling the social construction of crime and justice and by disaggregating crime into its myriad forms, so we need to abandon the idea that we can have a general model explaining change in civil litigation. Without more specific theoretical focus, the understanding we may think we have gained of temporal change in trial courts may prove to be chimerical.