THE SOCIOLOGY OF LAW: WHERE WE HAVE BEEN AND WHERE WE MIGHT BE GOING

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This review of work and trends in the sociology of law makes no claims to being exhaustive or comprehensive. If it succeeds in pinpointing and highlighting major work, important problems, and gaps in the field, in showing trends that have developed, and in suggesting ideas for future direction, we shall have accomplished our objectives.

More or less, we are looking back over a field that has about a sixty-year history in the United States. Two scholars have recently reviewed that history. Both make negative evaluations of its accomplishments and pessimistic forecasts for its future. Reviewing where the law and society movement has been, Lawrence Friedman observed:

> It is true that law and society people are more common outside law school than inside. But their position is nowhere very strong. People who study the legal system seem to be marginal, wherever they are. Sociology of law, students of judicial systems in political science, anthropology of law, psychology of law—all of these, alas, are not in the "mainstream" of their disciplines; they are not "where the action is." Mainstreams are of course mere matters of convention of definition. They change course very quickly. Still there are no signs that this is about to happen. Indeed, the trend may be heading the other way: studies of public law and judicial behavior are on the verge of extinction in some departments of political science. And there is no *financial* base. Billions of dollars are spent in this country on research of all sorts. Precious little flows into law and society work. There is a well-run program inside the National Science Foundation; the money it spends per year on research would not sustain high-energy physics for one day. Law and society scholars are beggars fighting for a handful of coins. When an *investment* is so terribly small, it is hard to get new recruits; and the output too will be small. This rather weak position reinforces, if it does not add to, what appears to be a substantive weakness in the field itself. To many observers, the work done so far amounts to very little: an incoherent or inconclusive jumble of case studies. There is (it seems) no foundation; some work merely proves the obvious, some is poorly designed; there are no axioms, no "laws" of legal behavior, nothing

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> cumulates. The studies are at times interesting and are sporadically useful. But there is no "science"; nothing adds up. Law and economics offers hard science; CLS offers high culture and the joy of trashing. The law and society movement seems to have nothing to sell but a kind of autumnal skepticism. The central message seems to be: It all depends. Grand theories do appear from time to time, but they have no survival power; they are nibbled to death by case studies. There is no central core. And, to be sure, there is some truth to these complaints—though only if the standard of legal "science" is a universal, timeless, and impossible one. (Friedman, 1986: 779)

Even more negative is the dismal future that Robert Tremper projected in his 1987 piece, which starts off: "Law and social science is on the couch. Wracked by anguish, self-doubt, role confusion, and occasional impotence, the field has been undergoing a decade-long analysis session in the literature" (Tremper, 1987: 267–68). According to Tremper, the sources of the disillusionment are the insufficient respect the work has received from the courts and the lack of satisfaction that the researchers themselves have with the methods, breadth, and orientation of the studies they have conducted.

The first problem reminds us of the frustrations social scientists expressed a half century ago when they complained of being forced by the legal profession into positions in which they had to make definite, specific responses to issues and problems about which they had only vague and ambiguous data. Tremper noted that

> the multiple demands of conducting research for legal and scientific audiences assure that most studies will be found deficient in some way. Some inadequacies become especially troublesome when social scientists offer court observations extending beyond the bounds of their demonstrable knowledge. (ibid.: 270)

How valid and how justified are these indications? Our attempts at answering that question became the focus around which much of this article is organized.

I. ORIGINS AND INTELLECTUAL ROOTS

In the late 1920s and early 1930s, the legal realists under the guidance and leadership of a small group of law professors (Llewellyn at Columbia; Pound at Harvard) and members of the appellate bench (Cardoza, Holmes, Brandeis) sought to restructure legal education and reorganize legal thinking by advocating the participation of social scientists in solving legal problems in the classrooms, as expert witnesses in the courtrooms, and as research partners in the laboratories.

The movement did not have wide support among legal educators for proposed curriculum changes. For example, in the teach-

ing of family law at Columbia University Law School, social scientists introduced demographic data on the American family, and sociological analyses of its roles and functions were rejected by most of the faculty, who preferred to retain the traditional case law approach. Social scientists who had been involved in other "grand experiments" to work with legal scholars as intellectual partners had been discouraged at the treatment they received. Too often they found themselves examined as expert witnesses called upon by one or the other side in a dispute and expected to answer specific questions with a definite yes or no. Social scientists usually could not produce the kinds of answers that legal scholars found useful; and they in turn often resented the manner in which the lawyers pursued issues.

The first phase in the development of the sociology of law movement did not have a strong academic foundation, and by the end of the decade it seemed to be sputtering off into oblivion. Its rebirth came about a decade and a half later in the mid-1950s.

II. THE LAW AND SOCIAL SCIENCE RESEARCH PROJECT

In the 1950s the purposes and organization of the efforts were directed at specific research programs. Under the leadership of its Dean, Edward Levi, the Law School at the University of Chicago obtained Ford Foundation support for interdisciplinary study of three important institutions: the jury system, the tax system, and commercial arbitration. The institutional arrangements had the legal scholars select the problems to be studied, decide the questions to be asked, and determine the purposes to which the results might be used. The social scientists invited to participate were expected to design the projects, devise data collection techniques, and determine the reliability and validity of the answers they obtained. The social scientists were thus the craftsmen and technicians needed to operationalize and analyze the research problems that legal experts believed important. The research projects initiated at the Law School of the University of Chicago more than three decades ago are often credited with arriving at a successful formula for carrying out interdisciplinary research between law and the social sciences. A careful examination of how much in fact was produced by the projects provides more bases for disappointment and a sense of failure than for exhilaration and a feeling of success. Of the three topics selected for interdisciplinary study, only the jury project produced scholarly papers and monographs. Early on, the tax project (which had been defined as a study of public knowledge and attitudes toward the personal income tax) was aborted because the social scientists could not devise an instrument that the legal experts—and there were several on the law faculty at the University of Chicago who had done a good deal of work on the personal income tax—believed asked the relevant

questions, and that the social scientists believed they could field and analyze.

The commercial arbitration project fared somewhat better. A design was prepared, data were collected, and some of the materials analyzed and published. But expectations far exceeded output. No major articles or monographs were produced, and there are hardly any citations to the work on commercial arbitration. It is an almost forgotten episode. That leaves the jury project; and it is the jury studies for which the entire law and social science research effort is remembered in a positive and important light. Indeed, the jury project is credited with having inspired a new tradition of interdisciplinary research in law and social science. The division of labor employed on that project became a paradigm for future projects involving lawyers and social scientists that continued for several decades.

Shortly after he spent a year at the University of Chicago Law School and observed the various research endeavors, Philip Selznick distinguished three stages that the sociology of law would pass through and indicated that he believed the field was in the second stage, a stage that Selznick characterized as belonging to the "sociological craftsman" (Selznick, 1959). According to Selznick, the second stage is one in which researchers bring specific sociological techniques and findings to bear on the study of particular legal problems or institutions. He warned of the dangers during such a stage:

> A serious risk is entailed and should not be overlooked. If we emphasize technique, we inevitably design projects that are congenial to the skills at hand. To be sure, such projects often have a market value in that they promise information that seems to be of immediate practical use to a client. Yet we know from experience that technique-stimulated research is seldom effectively guided by significant theoretical concerns or even by matters of the greatest long-run importance to the client himself. Attempts to apply small-group theory to the study of juries may seem an exception, but in fact they are not. The study of small groups, beyond certain first principles, is one of the more weakly developed areas in sociology; if this work is pushed to the forefront in legal sociology, it will be less for the sound knowledge it can offer than for the opportunity it presents to apply sophisticated research technique. (Selznick, 1959: 119–120)

Close examination even of the products of the jury studies reveals that far less was achieved than had been anticipated and planned. The Kalven-Zeisel study *The American Jury* is, of course, the centerpiece of the effort, and it exemplifies the division of labor between social scientists and legal scholars that Selznick described for the second stage (Kalven and Zeisel, 1966). Hans Zeisel, a widely respected methodologist (author of *Say It with*

Figures) and public opinion analyst, designed the study, which called for judges reporting of juries' verdicts and their own opinions of the cases, along with a description of the charges, characteristics of the defendants, number of witnesses, and assessments of the closeness of the evidence and the complexity of the issues. Working with the Zeisel design, Kalven underwent a crash course in reading statistical output; then worked closely with Zeisel and his group of social science associates in analyzing and interpreting the data and describing the results. The original plans had called for at least two volumes, one evaluating judge-jury comparisons for civil actions and the other for criminal cases. *The American Jury* refers briefly to the civil actions but is devoted almost entirely to criminal trials. A thorough analyses of the civil actions has yet to be reported.

The output from the "experimental" jury studies, which also focused on civil actions, designed by Fred Strodtbeck, has never been reported in a single volume. Articles published by Strodtbeck and his social science colleagues focused primarily on social process in jury deliberations, jurors' status characteristics and levels of participation, and coalition formation in the jury. For better or worse, as Selznick noted, the theoretical context into which the experimental jury studies were cast was "small groups." The insights and theoretical perspectives of that sociological subfield became the major paradigm for analyzing the American jury. Strodtbeck did not find a legal writing partner, the consequences of which were that most of the substantive data about how jurors evaluate testimony, respond to expert witnesses, understand and follow legal instructions, and place more or less emphasis on various aspects of the trial have never been reported for civil juries. In *The Jury and the Defense of Insanity*, Simon adapted the Strodtbeck design, which involved having real jurors serve as subjects, listen to audiotaped trials, and deliberate until they reached a verdict, and reported juries' responses to criminal trials in which defendants introduced a plea of insanity (Simon, 1967).

A third major piece of the jury project that appeared in print in bits and pieces was the work of Dale Broeder, at that time a recent graduate of the law school, who, with the help of Zeisel and Strodtbeck, designed a study whereby he sat in the same courtroom for two years and observed every jury trial that occurred before the same judge. Trained in law, he made his own assessments about the weight of evidence, the quality of the attorneys, and the clarity of the instructions. Then, after the jury returned its verdict, he discussed the case with the trial judge and with each member of the jury. In essence, Broeder asked each juror to recapitulate what had transpired in the deliberation, to evaluate the extent and quality of each juror's contribution, and to determine how satisfied each respondent was with the verdict. Like the Strodtbeck materials, only selective aspects of Broeder's work ap-

peared in journals. The full account of his project has never been published. In Broeder's case, he lacked the collaboration of a good ethnographer and apparently could not organize the mountain of data he had collected into a theoretical perspective that lent itself to a coherent analysis and narrative.

Thus the results of the law and Behavioral Science Project at the Law School of the University of Chicago, which extended over more than a decade beginning in 1953 and have been cited as the stimuli for the rebirth of interest and work in the sociology of law, produced important results on one of the original three projects. But even for the one project, much that had been planned, and even data that had been collected, remains unanalyzed and unreported, in part because of the failure of legal scholars and social scientists to join together and take advantage of each other's expertise. Nevertheless, the findings of the jury project spawned an intellectual industry that is still functioning at full capacity. We will examine more recent work on the jury later in this article.

III. LAW AND SOCIETY ASSOCIATION

Little more than a decade after the Chicago experience, the field moved into another stage with the establishment of the Law and Society Association in 1964 and the founding of the *Law and Society Review*. In his capacity as chair of the ad hoc committee that preceded the formal establishment of the Law and Society Association, Harry Ball observed that the association is a response to the rapid growth of interest in social science contributions to the study of law (Ball, 1965: 111). Richard Schwartz, the first editor of *Law and Society Review*, observed:

> The publication of the *Law and Society Review* results from a growing need on the part of social scientists and lawyers for a forum in which to carry on an interdisciplinary dialogue. During the past decade, each of the social sciences has found it necessary to face legal policy issues of highest relevance to the disciplines themselves and to the society as a whole. In political science, the decision process in the courts and administrative agencies have been explored to an extent which parallels earlier and continuing work on the legislatures. Political scientists have also turned their attention to the implementation of legal decisions, especially where the institutions of government have been seen as an important determinant of the impact of law. Sociologists, too, are showing increasing interest in the legal process. Their studies have been concerned with the manner in which the population is affected by law in such areas as civil rights, poverty, and crime. Both professions have joined with the anthropologists in studying the relationship between society and culture on the one hand and the nature and operation of legal institutions on the other. In addition, other professional groups—notably economists, social workers, clinical and social psycholo-

> gists, and psychiatrists—are increasingly called upon for information thought to be of value in the formulation of legal policy. Above all, the legal profession has moved from a position of reluctant consumer of such information to an active participant in the research process. (Schwartz, 1966: 6)

One of the first acts of the newly formed association was to commission "reviews of the literature" by eminent scholars. Jerome Skolnick's "The Sociology of Law in America: Overview and Trends" reviewed work that had been done and suggested directions toward which the field might aspire (Skolnick, 1965: 4–39). Skolnick's review cited most of the major empirical work that had been done during the past decade. That the review covered thirty-nine printed pages is one form of evidence of the perceived richness of the field at that time. The Jury Project was reviewed in considerable detail as the "first major category of contributions to the sociology of law in America" (ibid.: 8). In his assessment of the work completed thus far, Skolnick wrote:

> There is little that is exciting about the findings, either theoretically or philosophically. There is neither analysis of the administration of justice as a social system, nor the development of higher order concepts for interpreting the findings. Furthermore, materials so far published reveal little in the way of relating findings to such major jurisprudential issues as the adversary system or the presumption of innocence. The virtues are those of positivism; so are the deficiencies. (ibid.: 12)

Skolnick reviewed other major themes and issues in "The Sociology of Law." Work on the legal profession was characterized by him as being carried out in the conventional tradition of "American survey sociology" and as fitting into the format of the sociology of the professions rather than of the sociology of law. The work of Smigel (1964), Carlin (1962), O'Gorman (1963), and Ladinsky (1963: 47–54) provided illustrations for Skolnick's criticism. He argued, "Just as to a certain extent students of the jury initially perceived it as a natural small group interaction and interesting to the sociologist on that account, so too were lawyers seen as a category of professional workers" (Skolnick, 1965: 10). The legal profession, like the jury studies, is another topic we will examine later on in this article.

Additional themes cited by Skolnick were projects that examined legal services to the disadvantaged in American society and how both civil and criminal law might be used to reduce social injustices. Earlier and contemporary studies describing access to the law by various classes and ethnic or racial communities is still another theme that we will review in this article.

The early work in the sociology of the law was undertaken and assessed with a great deal more hope than that expressed in the more recent evaluations by Friedman and Tremper. Nonethe-

less, some of the same problems identified in these more recent evaluations were evident earlier. Law and social science and lawyers and social scientists were uneasy partners in the sociology of law. Each came to the enterprise with their own objectives that never quite melded. Lawyers regarded social scientists as technicians, and social scientists viewed the law as an opportunity to explore issues of general theoretical interest. The seeds of this uneasy relationship were sown early on, and they have grown to produce some of the conditions and perhaps the pessimism cited by Friedman and Tremper.

IV. RECENT TRENDS IN THE SOCIOLOGY OF THE LAW

We shift the focus of this essay from an overall evaluation of where we are and where we have been to an examination of selective themes and trends that we see as having developed during at least the past three decades. These topics include the relative attention given the civil and criminal law, the legal profession, work on the jury, access to the law, macro theory, and cross-national studies.

A. A Comparison of Productivity in the Criminal versus the Civil Law

In reviewing the literature in the sociology of the law, it is apparent that a great deal more work has been done on the criminal law than on the civil law. This is particularly the case for empirical social science research. Moreover, this research is much more readily incorporated into the policymaking process than research on the civil law.

There has been substantial research done on a wide variety of topics pertinent to the criminal law. There is a constant stream of theoretical and empirical work on the etiology of delinquent and criminal behavior. Virtually every aspect of the criminal justice system's response to crime has been investigated, from mobilization of the police to sentencing and the social organization of correctional institutions. There are no fewer than ten major journals that serve as outlets for research on the criminal law, including *Law and Society Review*, *Journal of Criminal Law and Criminology*, *Journal of Research on Crime and Delinquency*, *Criminology*, *Crime and Delinquency*, *Journal of Quantitative Criminology*, *Journal of Criminal Justice*, and *Justice Quarterly*. In addition, major journals in social science disciplines have recently been more likely to include articles that address issues in the sociology of the criminal law. These journals are widely distributed.

In contrast, the research done on the civil law is much less voluminous. Many aspects of the civil justice process have never been the subject of empirical study. Others have been examined once or twice, but they have not received the intensive and re-

peated investigation from a variety of perspectives that characterizes the sociology of criminal law. There are fewer outlets for research on the civil law. The bulk of the empirical work on the civil law appears in a few journals such as *Law and Society*, *Judicature*, *Law and Social Inquiry*, and the *American Bar Foundation Journal* and in a handful of law reviews. These journals do not have the distribution of those in the criminal law area. Moreover, some of the most interesting work in the civil justice area is done by contract research firms who are either restricted from publishing or do not have incentives for publishing in journals.

The research done on the criminal law has found its way into policy debates and the actual implementation of policy to a much greater extent than that on the civil law. Parole prediction studies have been used in decisionmaking regarding parole. Evaluation of release on one's own recognizance in the bail area was instrumental in its acceptance, and it has been used to assess the probability of failure to appear. The guidelines of the Federal Sentencing Commission were developed with a great deal of input from empirical studies on the effectiveness of various sentencing alternatives and from estimates of system impact. There is no evidence that empirical research on the civil side has had the same effect on policy.

While none of the foregoing is news to those interested in the sociology of law, these obvious differences in the development of the sociology of the civil and criminal law may tell us something about the relationship between the social organization of the fields and productivity. Why has the sociology of the criminal law developed so much more than that of the civil law? Why has the research in the criminal law found its way into the policy debate to a greater extent than research on the civil side? What can the answers to these questions tell us about achieving a more balanced development in the field? More will be said about these issues in the following sections.

B. The Sociology of the Legal Profession

In the late 1950s and early 1960s, a substantial body of work appeared on the social organization of the bar. This research, which included Carlin's early work on the solo practitioner, Smigel's study of the Wall Street lawyer, and O'Gorman's profile of the divorce lawyer, were cited in the Skolnick review. In addition, Blaustein and Porter, Ladinsky, Thielens, and Lortie conducted studies of the social organization of the American bar, and the process of legal education and professionalization. All of the studies concluded that the bar was not a unified professional group in the sense that it shared a common expertise or a common set of values (Carlin, 1962; O'Gorman, 1963; Smigel, 1964; Lortie, 1959; Blaustein and Porter, 1954; Ladinsky, 1963; Thielens, 1957: 131–52). It was

highly differentiated and stratified. Second, they argued that such a differentiated group could not be expected to regulate itself to ensure either competent service or ethical behavior. There was not sufficient consensus in the profession to ensure that positions of the bar would be adhered to by the majority of the profession. Third, to the extent that the formal organization of the bar represented anyone, it represented the interests of an elite group. This elite group was defined by the economic power of their clients, the type of law practiced, the law school attended, and the social background of its members. White, Anglo-Saxon Protestants in Wall Street firms ran the bar. It represented their interests to the outside world. Its rules for self-regulation were slanted in favor of the elite and against the lower reaches of the bar. Fourth, the stratification of the bar ensured that poor persons would be served by the least qualified and least ethical members of the bar. Finally, this work exposed the unenviable working conditions prevalent among the lower-status members of the profession.

These studies were enlightening and important. It would not be an overstatement to suggest that they contributed to the intellectual climate in the late 1960s that encouraged publicly funded alternatives to the private bar for services customarily provided by the lower end of the profession.

Some twenty years after this work, research on the social organization of the bar took another quantum leap with the studies of a group of sociologists and attorneys at the American Bar Foundation and the University of Chicago (Heinz and Laumann, 1982; Halliday, 1987). This group returned to many of the same issues addressed by the earlier researchers, but reached some different conclusions. To some extent these differences are due to the fact that the legal profession has changed in the interim (and pointing out these changes is one of the major contributions of this work). Some of the differences may be the result of more sophisticated methodological techniques that revealed relationships hidden in the previous work.

Laumann, Heinz, and their associates provide a picture of the bar that is at the same time similar to and different from that offered earlier. The legal profession is still highly stratified. Stratification in the profession is still affected by the type of client and the background characteristics of the attorney. The nature of this stratification, however, is more complex. The stratification is not unidimensional but multidimensional. Client type—corporate or individual—is the major defining dimension, but nature of practice—litigation or office—is also a powerful influence. The relationship between prestige in the profession and the conditions of work is also more complex in the more recent work than it was in the studies of the 1950s and 1960s. In the latter, prestige in the profession was directly related to control over working conditions. The more prestigious the attorney, the greater his or her control

over conditions of work. Heinz and Laumann contend that the power and sophistication of corporate clients restrict the professional autonomy of the elites who serve corporations. In contrast, the less prestigious sectors of the bar have less economic security but have greater control over working conditions because their clients are relatively unsophisticated.

The overwhelming importance of client type in differentiating the bar leads Heinz and Laumann to hold the profession in less awe than earlier researchers. The "Wall Street lawyers" who were so powerful within the bar are seen as being relatively powerless compared to their clients who control the conditions of work for the attorney. The Chicago group concludes that the profession has no core and is largely irrelevant as a self-regulating or self-protecting body. The profession is a set of groups defined by client type and nature of practice who reflect the values and images of their clients rather than any common core of values.

The work of the Chicago group places the bar in a larger social context where it is the object of more complex social forces and not the relatively free agent that the earlier researchers found. Specifically, the dramatic rise in corporate power and the enhanced importance of the law for corporate decisions has increased the need for corporations to control their lawyers. The in-house counsel is the agent for that control. More corporate legal matters are handled by staff attorneys, and in-house counsels closely supervise the work of outside firms. The growing importance of the law has changed the nature of the bar. Legal service to corporations became too important to be left to lawyers. This kind of perspective on the bar is useful because it helps us avoid attempting micro solutions to macro problems.

The only disappointing feature of the more recent work in the social organization of the legal profession is that it does not draw the implications for social policy as clearly as the earlier work had done. If the problem in the 1960s was an elitist bar, then steps could be taken to protect the markets and conditions of work for the nonelite sectors of the bar. This would encourage quality legal services for those served by the less prestigious members of the profession. The implications of the Heinz and Laumann work are not clear. If the legal profession has lost its core, what does this loss mean for the provision of legal services to society? How can we assure the training and regulation of attorneys? What do we, who are not corporations, do to ensure quality legal service? How do we prevent other legal institutions such as the courts from becoming two-tier systems in which the corporate tier consumes the ablest talent? Although the most recent work on the social organization of the bar does not pretend to be policy research, it would have been more satisfying if the authors had drawn some of the policy implications from their findings.

C. Jury Studies

In a 1982 review of research in the psychology of law, Shari Diamond referred to a "flood of articles" on the jury that appeared in the 1970s: eighty-seven between 1970 and 1976 (Diamond, 1982: 12). Had she continued counting during the second half of the 1970s and into the 1980s, she would have found that the flood had not receded in the past decade. The high proportion of research funded by the National Science Foundation on the jury compared to other topics is another reflection of the prominence that jury studies have achieved.

In her review, Diamond agreed with much of the criticism of the simulated jury experiments conducted in the 1960s and 1970s and welcomed the recent greater emphasis on realism and concern for setting. The irony about the criticism of simulated jury studies and the desirability of introducing complex stimuli (e.g., trials that last for several hours) involving real jurors instead of college sophomores, as well as comparing jury verdicts against real standards (e.g., judges), is that these techniques constitute the basic elements employed in the 1950s jury research.

The 1950s experimental jury studies involved the use of real jurors (persons who had been called for their regular period of jury duty) who listened to lengthy audiotaped trials based on real cases. Attorneys' opening and closing arguments and the judge's instruction pertinent to the trial were also taped. The setting in which the studies were conducted were real courtrooms in major cities. The subjects were introduced to the study by one of the judges in the court and remained under the jurisdiction of the bailiff for the length of their participation. Each trial usually lasted one day with the jurors taking a lunch break before they heard the attorneys' closing arguments and judge's instruction. They deliberated until they reached a verdict, which they then reported to the judge who had explained the special circumstances of their service to them earlier in the day. One indication of how realistic the setting and overall experience were to the jurors is that occasionally after hours of deliberating, if they had not reached a consensus but were unwilling to declare themselves a hung jury, they were kept overnight in nearby hotels and continued their deliberations the next day. Replay of the tapes of the deliberations also revealed how seriously the jurors took their responsibilities, even though they understood that their verdicts would have no immediate or practical consequences for the plaintiffs or defendants.

Many of the subsequent studies of the jury in the 1960s shifted the setting from the courthouse to the university, altered the subjects from jurors to college students, simplified the stimuli from full-length trials to summaries presented orally or in written form, and shortened or eliminated the deliberations.

By the mid to late 1970s, the direction shifted once again, and

more realistic studies were designed. Saks (1977) and Hastie *et al.* (1983) for example, used real jurors, exposed them to videotaped trials that lasted several hours, and had the jurors deliberate until they reached a verdict or declared themselves hung.

A criticism leveled against both the Chicago jury studies and the more recent crop of simulated jury research is the matter of consequences. Does the lack of practical impact on persons or institutions seriously impair the value of simulated or experimental jury studies such that they are not useful proxies or predictors of real jury performance? Various strategies have been devised to try to determine how valid such criticism is. The Chicago project found itself in all kinds of trouble when it bugged "real jury" deliberations in order to be able to compare them against the experimental jury deliberations. Other researchers interviewed jurors at the end of their period of service. Mostly, the researchers relied on structuring as realistic an environment as possible in order to produce realistic results.

Although only rarely used because of the lack of support from the bench, a relatively new element in the design of jury studies has been the use of "shadow juries," that is, jurors sitting in the courtroom during a real trial serving as a "second jury" whose decisions will not matter but whose verdicts may be compared against those reported by the jury chosen to decide the case. Comparisons of shadow and real jury verdict patterns would be one way of determining how meaningful and realistic the simulated jury verdicts are. Unfortunately, "shadow juries" have made only rare appearances in U.S. courts, and not enough comparisons have been carried out to substantiate support of experimental jury research or to allay the doubts and criticisms of opponents of such work.

Other dimensions of work on the jury consisted of finding out how best to communicate instructions to the jury and to determine how fully juries understand and follow legal rules. The impact of variations in jury size and verdict forms has become a favorite topic within the area of jury studies. Numerous studies have been conducted to determine whether verdicts differ if deliberations are carried on by twelve-, six-, or nine-person juries, and whether hung juries are more or less likely to occur with smaller juries. The absence of a unanimity rule has also been the focus of study primarily to determine whether it reduces the likelihood of hung juries and increases the chances of guilty verdicts against defendants. Like other topics in law and the social sciences, the jury studies are becoming the exclusive province of sociologists and psychologists, without the partnership of legal scholars.

D. Access to the Law

The distribution of access to the law has long been of interest to social scientists connected with legal institutions. The question of whether there is equal access to the law across the social structure is central to the issue of whether the law promotes equity or inequity in society. If access to the law is concentrated in the upper reaches of the social structure, then an argument can be made that law is more oppressive than protective. This question has been addressed by social scientists for several centuries. Earlier work in this area emphasized a macro-sociological perspective and institutional analysis. In the more recent past, sociologists, political scientists, and others have used survey research to investigate the mobilization of the law. There are advantages to each. Macro-sociological studies are somewhat imprecise, empirically. More empirically rigorous studies at the micro level run the risk of missing the forest for the trees. Nonetheless, some of the major work done in this area during the last two decades takes this micro-sociological, empirical approach (Curran, 1977; Marks, 1971; Levine and Preston, 1970; Mayhew and Reiss, 1969).

The study of the use of legal services has systematically increased our understanding of the distribution of access to the law. Mayhew and Reiss demonstrated that use of attorneys was not equally distributed across the social structure. Wealthier citizens used attorneys more than the poor, and minority group members used legal services less than members of the majority. Reiss and Mayhew concluded that this distribution of access to the law was more indicative of the distribution of property in society than it was of systematic exclusion from access to legal services. A large portion of the instances in which legal services were provided involved the transfer of property or the resolution of disputes involving property. Since the wealthy had more property to transfer or to dispute over, they employed attorneys more often than the poor. The data available to Reiss and Mayhew did not include information on the prevalence of legal problems and could not be used to test their assertion. Levine and Preston addressed this issue directly in their survey of the legal needs of the poor. They asked citizens to report whether they had had specific types of problems that were often resolved or addressed with the aid of an attorney. They found that the poor used attorneys in a small proportion of the instances in which legal assistance could be sought. Moreover, the use of attorneys varied across legal problems. It was highest for torts and lowest for discrimination cases.

While the Levine and Preston (1970) work was able to establish the prevalence with which legal assistance was used, they were not able to address the question of the distribution of access across the social structure because they focused only on the poor. Almost a decade later, Curran (1977) addressed the issue again us-

ing the same methods as Levine and Preston, but for a representative sample of the U.S. population and not just the poor. The results suggested that the use of legal assistance, and specifically attorneys, varies substantially by type of problem and by one's position in the social structure. Estate problems and wills were always handled by attorneys, while attorneys were almost never involved in job discrimination and property damage problems. The use of attorneys varied by demographic groups even when the type of problem was held constant. Whites use attorneys more than nonwhites in all matters except torts and juvenile matters. The wealthier respondents used attorneys more than the poorer in estate matters and grievances against the government, but less in torts and consumer problems. These and other findings from *The Legal Needs of the Public* were highly suggestive. They begged for explanation and evaluation, but none was forthcoming.

Several years after the work of Curran and her colleagues, the Federal Justice Research Program funded the Civil Litigation Research Project (CLRP) (Trubek, 1980–81). This was an ambiguous research program designed to increase our understanding of the disputing process. This process began with the recognition of a problem or grievance (naming) and proceeded to identifying a party from which redress could be expected (blaming), seeking redress (claiming), and having that claim rejected, which started the dispute (Felstiner *et al.*, 1980–81). From this point the project sought to capture the phases through which disputes passed as they proceeded toward litigation. A small but important part of this research examined the use of attorneys and other dispute resolution mechanisms across the social structure. Although the empirical work did not add much to what Curran and her colleagues had done, those involved in the CLRP did offer some useful hypotheses about why legal assistance was sought by some persons for some problems and not others. These explanations range from simple cost-benefit calculations for seeking assistance, to arguments that for some problems the use of attorneys or other assistance has been routinized to the point where their use is virtually automatic (Miller and Sarat, 1980–81). Still other explanations have focused on the social psychological impediments to seeking legal assistance (Bumiller, 1988).

At this point, we are left with more questions than answers regarding why some sectors of American society use legal institutions more than others. Whether this is a problem of access is not so simply established. It is clear, however, that interest in exploring the reasons for differential use of legal institutions has waned. The potential of the ABF data and the CLRP data have not been exhausted. In the seven years since the first CLRP publications, only Bumiller's work has explored these issues. It should be noted that her work did not use these data, but employed less structured

interviewing techniques on a small group of respondents. Attention has shifted from assessing the equity of access to the law to more efficient ways of dealing with the caseload confronting the courts. This is unfortunate. More attention should be given to continuing this research tradition through the secondary analysis of existing data sets.

The research on access to the criminal law has explored some of the same themes as that focusing on the civil law. Investigators are interested in whether the mobilization of the law is uniform across the society. They are also interested in explaining why differences should exist. The mobilization of the criminal law seems to be a much simpler matter. The overwhelming evidence is that mobilization of the criminal law or calling the police is more a function of the type of crime than of the position of the victim in the social structure. Events involving serious injury or extensive property loss are uniformly reported to the police, while crimes with less serious outcomes are not (Gottfredson and Gottfredson, 1980). These relationships hold regardless of the characteristics of victims or offenders.

For the most part, these two traditions in the study of access to the law or the mobilization of the law have proceeded independently. This division is unfortunate because each tradition has much to contribute to the other. These contributions can occur because they share the same methods and questions. Sample surveys, for example, are a useful means of establishing the existence of a set of behaviors that can result in the mobilization of the law (e.g., legal needs). Obtaining exhaustive or unbiased reporting of these behaviors is crucial if we are to identity differences in access to the law. It is difficult, however, to develop survey techniques that encourage complete and unbiased reporting of legal needs. Criminal victimization surveys have been the focus of a great deal of methodological work designed to improve the reporting of crimes (Biderman and Lynch, 1981; Reiss, 1982; Biderman and Cantor, 1984; LaVange and Folsom, 1985). Surveys of legal needs on the civil side could benefit from the lessons learned from victimization surveys.

Contributions can also result from the fact that the two traditions are so different. Why should the mobilization of the criminal law be so invariant across the social structure, while the use of the civil law is so different? Is it because the criminal law is better understood than the civil law? Is it because the police serve as a "catchall" agency that will take any kind of complaint and select out the criminal matters? No similar agency exists on the civil side. The answers to these questions may help explain differences in the mobilization of the civil law across types of problems.

V. MACRO THEORY IN THE SOCIOLOGY OF THE LAW

There are at least two more observations that need to be made about the state of the field: where it has been over the past decade and where it might be going.

The first observation concerns the matter of a "grand theory"—have we developed any recently; and if we have, how has it been received? *The Behavior of Law*, published in 1976 by Donald Black, has been offered by its proponents as meeting the requirements of a "grand theory" (Black, 1976). To our knowledge, it is the only such work to appear in the past ten years or so. Black treats law (which he defines as "governmental social control") as a quantitative variable and examines how it increases and decreases in different social settings. The quantity of law, for example, varies in time and space and by status and amount of integration. Law, according to Black, also varies inversely with other forms of social control. Black provides specific propositions which, in the best theoretical tradition, are empirically testable, for example: "The more stratification a society has, the more law it has"; "The relationship between law and social differentiation is curvilinear"; "There is less law where people are undifferentiated by function; but as differentiation and dependence increases so does law"; "As social life develops beyond interdependency to symbioses, law decreases." Law also varies directly with culture; according to Black, the more culture, the more law.

Black's work has been subjected to empirical testing and, according to one critique, found wanting (Greenberg, 1983). Greenberg also found lacking in Black's work a logical linking of the theoretical propositions. Each appears to stand by itself. Much of the testing of Black's theory has used criminal data and has lacked support. Studies have failed, for example, to support Black's propositions that law varies directly with stratification, or that downward law is greater than upward law, or that law varies with other forms of social control. In the end, Greenberg applauded Black for his efforts and his aspirations, but concluded that Black's work did not provide a grand theory of law.

In a critique of Greenberg's critique, Horowitz defended Black's work, claiming that it has brought the sociology of law "beyond the conventional style of thought that has long marked the field" and that "it formulates a rigorous system of propositions at a purely sociological level" (Horowitz, 1983). As far as empirical verification is concerned, Horowitz argued:

> As evidence accumulates, the various propositions of Black's theory will probably have to be modified. But for the proper specifications to be made, researchers must consider the whole body of evidence, use relevant studies, con-

> trol for other variables in the theory and examine the theory in its own terms. (1983: 31)

The jury is still out and will probably stay out for quite some time on the quality and validity of Black's theory.

VI. CROSS-NATIONAL STUDIES IN THE SOCIOLOGY OF THE LAW

The second observation is directed at considering how well the sociology of law has met the challenge of understanding the relation between law and social organization in different cultures and under different economic and political systems. In terms of numbers, there is little doubt there has been a lot of work on law and society issues carried on outside the United States and published in the *Law and Society Review* and other U.S. publications concerned with law as a social phenomenon. For a long time, going back to Weber, Petrajitsky, Malinowski, and the work of European scholars before World War I, there was an interest in and a commitment to comparative legal systems and institutions. The work that was done was either carried out in a traditional legal mode, or it was grand theorizing about entire societies or cultures. On some topics, such as the legal profession and dispute management, and on criminal issues pertaining especially to drunk driving and drug usage, there have been significant strides toward producing a comparative view of these matters. Thus work on the Dutch and West German drug laws, the organization of the bar and variations in the size of legal firms in Western Europe, how plea bargaining works outside the United States, and application of deterrence theory to explain drunk driving in different societies made explicit analogies to other legal systems. Most of the work, however, has been case studies that describe how law or some legal institutions function in less developed or non-Western societies. They are descriptive and self-contained (e.g., sexual politics of law in Morocco, judicial authority in Indonesia).

There have been some pieces that fit a Marxian mode and analyze the function of a legal institution or set of controls within a state-run economic system—for example, labor courts in Yugoslavia, law in the Soviet workplace, or the legal order and industrial workers in Poland. But clearly the majority of the recent work in the sociology of law is not even implicitly comparative. What we have observed is a greater self-consciousness about the importance and relevance of comparative studies and paradigms that fit cross-culturally, but not much in the way of empirical work or grand theory that encompasses different legal systems and social institutions.

VII. IMPLICATIONS OF RECENT TRENDS FOR THE SOCIOLOGY OF LAW

In light of the foregoing, the assessments of Tremper (1987) and Friedman (1986) seem to be essentially correct but overstated. The sociology of law as a field of study has not produced an integrated and inclusive body of knowledge. There is little grand theory. Isolated case studies seem to predominate, and the courts place little weight on empirically based research findings. Nonetheless, in the recent past, attempts have been made to develop a theory of the law, and while it is not complete, it is a useful step in the right direction. There are some research traditions that have endured, involved a number of social scientists and attorneys, and produced a body of cumulative knowledge. Moreover, there is some evidence, at least on the criminal side, that social science research on legal institutions is being used in policy debates and in the implementation of policy. While it is useful to know where the sociology of law stands as a field, it may be more fruitful to compare those areas where the discipline has fared well and less well in order to draw the lessons for the future.

It seems clear that research on the criminal law has been more extensive than that on the civil law. The former is characterized by more extensive and harmonious collaboration between lawyers and social scientists than the latter. Research on the criminal side has been more influential in policymaking than that on the civil law. Why is this the case?

Certainly, as Friedman points out, funding for research in the criminal law is much greater than that available for studying the civil law. This observation, in turn, is related to the fact that the criminal justice system is almost entirely public. Consequently, most of the issues central to the criminal law are matters of broad public debate, and research is a valuable weapon in that debate. In contrast, the civil justice system is overwhelmingly private. Only a small portion of civil law matters involves a public agency—the court—and this particular agency makes the bulk of its policy decisions—court rules—without wide public debate. Specific groups such as the insurance industry or the trial lawyers' associations have intense interest in the structure of the civil justice system, but the fact that they have an immediate stake in the outcome of research is not conducive to the building of a cogent body of knowledge. Without broader public interest in the debate about civil justice matters, it is unlikely that sufficient funding and attention will be focused on the sociology of the civil law. The newly created State Justice Institute is an encouraging sign, but it remains to be seen whether this effort will engender broad interest on the part of the research community.

While the funds available for research on civil justice are meager relative to those available for criminal matters, the complexity

of the civil justice system is substantially greater than that of the criminal system. The range of behavior that becomes the object of the criminal law is much more restricted than that of the civil law. There is much greater consensus among citizens about what is appropriately a police matter or potentially a violation of the criminal law. The civil side has no screening agency similar to the police to receive citizen complaints and choose from among them the relevant criminal matters. Consequently, there is no clear starting point for the disputing process on the civil side. This complexity requires extensive methodological and theoretical work of the magnitude not likely to be supported in the near future.

These differences in the social organization of research on the civil and criminal law make it unlikely that work on the civil side will approach the volume of that on the criminal side. The response to these conditions on the part of the field has been to narrow the focus of research to those matters that have reached the court and to the relative effectiveness of alternative modes of processing these disputes. This is appropriate, but it leaves many other matters unattended. For more labor-intensive or less applied issues, it may be possible to use the institutions organized to support research on the criminal side to foster work on civil justice matters. Routine statistical series used to measure crimes and system responses can be supplemented at low cost to produce data on the civil justice system. Some of the issues studied on the criminal side, such as the mobilization of the law, are similar in many respects to those on the civil side, but researchers practicing in one area are seldom aware of those working in the other. Attempts should be made to identify issues common to both traditions and to compare the result of work done in each. These efforts, in turn, could foster cooperative arrangements in which general issues could be explored with attention given to specific instances in both the criminal and the civil law. In this way, the resources available on the criminal side could be used to expand our knowledge of the civil law.

Some research traditions in the sociology of the law, such as the legal needs area, have amassed some very useful data that have been described but not thoroughly analyzed. The American Bar Foundation data on the legal needs of the public and the cost of civil litigation data fall into this category. Steps should be taken to encourage the secondary analysis of these data. These data sets are archived at the Inter-University Consortium for Political and Social Research. Other relevant data sets should be placed in this or some other accessible archive. A small grant program ($10,000 or less) could be started at NSF or the SJI to encourage the use of these data. This small grant program could be targeted to special topic areas or a simple open competition (with minimal application requirements). Relevant journals could stimulate the use of these data through special issues that focus on a data set or a topical

area. Money for data collection in the sociology of law, and particularly the civil law, will never measure up to the need for new information. It is essential that we utilize existing information to the fullest. Secondary analysis is difficult, and it will not be pursued unless some incentives are available. One way to change the noncumulative nature of investigations in the sociology of law is to encourage secondary analysis in relevant issue areas.

A final comment takes us again to issues of cross-national comparisons. We noted that comparative empirical studies of legal systems are rare, in part because of the inherent difficulty of doing cross-national empirical work. The access and information problems that abound in the investigation of legal systems in the United States is compounded in cross-national comparisons. Making international comparisons will become more manageable as the number of and the demand for such studies increase. Cross-national comparisons could be encouraged by leading journals in the field through special issues focusing on international comparisons of empirical work. These special issues would be particularly useful if they emphasized comparisons at the system rather than the program level. Comparisons of common law and code systems, for example, would be more desirable than comparisons of specific dispute resolution within the two types of systems.

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