

Presidential Address

**TRIAL COURTS IN THE UNITED STATES:
THE TRAVAILS OF EXPLORATION**

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This paper examines the last 12 years of research on trial courts. It focuses on three shortcomings of that research. The first is a failure to examine distributional questions about what kinds of benefits and sanctions are apportioned to which members of American society as well as the place of adjudication in the course of dispute processing. The second shortcoming is a failure to integrate the various theoretical perspectives which have won widespread acceptance. The third is an undue concentration on comparative designs to the neglect of longitudinal analyses within single jurisdictions.

The past decade has witnessed an unprecedented exploration of the worlds of trial courts. The tenets of legal realism have become conventional wisdom. Almost all the social science disciplines have joined in the search for understanding the role of trial courts in the American legal system and polity. The exploration has been pushed forward by the availability of funds, particularly from federal sources, for the study of the criminal justice process. It has produced an avalanche of books, articles, and reports. However, despite the immense effort I am left troubled and dubious. Is what we have learned worth knowing? Can we teach our students about trial courts with some confidence? Are we within sight of certain enough knowledge to guide policy makers?

I cannot answer these questions with a resounding "Yes." I am dubious because our explorations have been excessively unguided. The very nature of exploration, of course, is to travel without the benefit of accurate maps. However, one must have tentative notions about the lay of the land and the perils to be overcome. We have not lacked for such hypotheses, but I

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believe that we have followed them too inconsistently to produce the kinds of results for which we strive.

We have suffered from three distinct kinds of failings. First, we have neglected much of what is involved in trial court processes; consequently, we have not even attempted to explore important areas. Second, we have followed too many competing conceptual frameworks, causing, so to speak, our compasses to gyrate wildly. Thus, our knowledge often does not cumulate but instead appears to be drawn from entirely different universes. Third, we have paid too much attention to variation across jurisdictions and, therefore, have usually failed to develop local studies in sufficient depth or to understand how large or what kind of samples we need to justify confident generalization. Let me briefly examine each of these problems.

I. THE FAILURE OF VISION

It is now conventional wisdom that courts are part of the political system and should be studied in that framework. If so, we should adopt, without controversy, the formula once suggested by Harold Lasswell (1936) as a subtitle to one of his seminal books: *Politics: Who Gets What When and How*. There certainly is no reason to think that any of these elements is unimportant for understanding trial courts. However, when we examine our research efforts, we find that we have devoted almost all of our resources to describing and understanding the judicial *process*—the “how” of adjudication—with very little investment in the “who” or “what” and almost no attention to the question of “when.”

We have made great strides in understanding some elements of the work of trial courts. We feel confident in stating that bargaining and negotiation are more common than formal adjudication. We know that the work of the jury is very limited. It is generally recognized that the disposition of cases is a collaborative enterprise involving attorneys, judges, and perhaps litigants and others, rather than a solo performance by the judge (e.g., see Blumberg, 1967; Eisenstein and Jacob, 1977; Heumann, 1978; Feeley, 1979). Consequently, to understand how trial courts operate, one needs to learn about each of these participants and their interactions with one another.

Our knowledge, however, is skewed. Our knowledge of how criminal courts operate is much more firmly grounded than our knowledge of civil courts. On the criminal side, we have studied many more jurisdictions and levels of courts. We have examined a much larger number of participants and have

used a wider variety of perspectives which permit us to triangulate on the "real" characteristics of criminal courts with a feeling of some confidence.

By contrast, our knowledge of the way trial courts deal with civil matters is scant. We possess only scattered and sketchy descriptions of divorce proceedings (O'Gorman, 1963), tort cases (Ross, 1970; Rosenthal, 1974), personal debt adjudications (Jacob, 1969; Caplovitz, 1974; cf. Yngvesson and Hennessey, 1975) or contract litigation (Macaulay, 1963), to name just some of the most numerous instances of court usage. Imagine a book like Milton Heumann's *Plea Bargaining* (1978) on divorce, or one like Malcolm Feeley's *The Process is the Punishment* (1979) on housing disputes, or one like Lawrence Friedman's *Roots of Justice* (1981) on the work of the civil courts in one community over a long span of time. Any one of these would expand our knowledge immensely. Their absence is testimony to how little we know of the ways in which trial courts operate for most of what they do.

Of course, we do know something about the bargaining which occurs in personal injury cases from the work of Ross (1970) and Rosenthal (1974), among others. We also have some hints of the kinds of interactions which are in the background of some contract disputes from Macaulay's (1963) work which, however, still stands almost alone. Although divorce laws have changed substantially during the past twenty years and family dissolutions have become more common, little has been added to our knowledge of the dynamics of divorce proceedings since O'Gorman published his book in 1963. From these works and others involving consumer debts and housing disputes, we have learned that, as Galanter (1974) aptly characterized it, one-timers are often at a disadvantage when confronting repeat players in the courts. We have been reminded by Mnookin and Kornhauser (1979; also see Ross, 1970; Lempert, 1978) that what happens in court affects not only the dispositions that reach trial but also the negotiations that avoid formal adjudication. Civil litigation, like plea bargaining in criminal courts, takes place "in the shadow of the law." But despite these studies, our knowledge of civil litigation remains so fragmented that it is the gaps that stand out. We don't know how negotiations are conducted and how they vary across types of civil actions or within types, across different kinds of parties. We know little about the role of judges in these cases. Even the formal trial has been neglected, so we have little systematic knowledge

about the ways in which information is transmitted and manipulated in the courtroom setting.

Indeed, some might question whether civil actions are simply the inverse of criminal cases or whether they are so different that their study requires a distinctive approach. Certainly, a larger share of the significant activity takes place in lawyers' offices rather than in courtrooms or courthouse corridors. In addition, while the majority of cases involve individuals with small stakes in absolute terms, a small number of civil actions involve business transactions that have large implications for workers, investors, and consumers. Such cases have been rarely studied.¹ Other actions involve challenges to public policy. Important questions have been raised by Horowitz (1977) and also addressed by others (Gambitta *et al.*, 1981) about the adequacy of court proceedings for those civil cases which, like school desegregation cases, involve policy disputes. But these cases also have been little studied and we must color most of the map white—i.e., uncharted.

Students of civil litigation have largely been drawn off to the study of disputing in a more generic sense (*Law & Society Review*, 1980-81). The result is that we are on the threshold of considerably greater understanding of the sources of litigation—especially that which individuals initiate—but we have not yet studied intensively the litigation process itself. The historical dimension has begun to be tapped by Lawrence Friedman and Robert Percival's (1976) study of two counties in California (see also Lempert, 1978), by Stephen Daniels' (1981; 1982) studies of litigation in four downstate Illinois counties, and by Wayne McIntosh's (1980-81) study of St. Louis courts.

By contrast, we have a fairly complete picture of the characteristics of those involved as defendants in criminal cases (e.g., see Eisenstein and Jacob, 1977: 204; Vera Institute, 1981; Silberman, 1978: 48-165; Solomon, 1982). They are predominantly male, young, poor, and disproportionately black. We know less about how these characteristics relate to the charges which bring people to court. Thus, we do not know whether burglars as a group are very different from robbers or assailants. Indeed, the more recent studies (Petersilia *et al.*, 1977) of criminal careers suggest that offenders tend not to be specialists although the risks of apprehension may be higher for one crime than for another. Also we know little about how

¹ I am grateful to Malcolm Feeley for alerting me to these dimensions of civil actions.

widespread experience in the criminal courts is, although a recent estimate for Illinois puts the proportion of people in the labor force with arrest records somewhere between 16 and 25 percent (Lucas, 1982). Surely criminal courts are much more prominent in the experience of some ethnic and social class groups than others, but we lack precise knowledge about such distributions.

Our knowledge of participants in civil cases is also limited. Experience with the civil courts is more widespread than criminal court involvement (Yankelovitch *et al.*, 1978: 15). We can guess that a broad cross-section of the population is involved in divorce and personal injury proceedings, but we possess only limited knowledge about their involvement in consumer debt actions and housing disputes.

Even less is known about what happens in court. There has been little systematic study of bench or jury trials; we know little about the strategies lawyers employ in presenting evidence, the variety of roles judges play, or many of the other elements of the trial as a forum for exchanging information. We also receive no meaningful accounting from our criminal courts about the sanctions they dispense. Although the trend toward determinate sentences has reduced the variability between the sentence pronounced in court and the sentence served in prison, the gap between the two still exists. Moreover, it is difficult to sort out the incidence of sentences, fines, suspended sentences, and probation (Hagan, 1974). The best evidence appears to be that there is a small relationship between the severity of the sentence and whether the defendant insisted on a trial (Brereton and Casper, 1981), and there is some evidence that blacks receive jail sentences more frequently than whites, although they generally are given shorter sentences (Spohn *et al.*, 1981). Women receive lesser sanctions than men (Simon, 1975: 49-67; Uhlman and Kritzer, 1977; but see Solomon, 1982). Sanction severity is related to the severity of the offense originally charged as well as that on which the defendant is convicted (Eisenstein and Jacob, 1977). There are substantial differences between states and regions in the severity of sanctions (Flanagan *et al.*, 1982: 472), and in general, sanctions in the United States are harsher than in other Western industrial countries.

Our knowledge about the outcomes of criminal court actions cannot be matched on the civil side. Aside from the pioneering work by Wanner (1974; 1975), we have almost no systematic knowledge about the outcomes of civil cases. We

know little about the results of personal injury cases, whether they eventuate from settlements or in court judgments. Moreover, even though a large portion of the population experiences divorce and must either pay or receive support, we know almost nothing about the range of support payments, their duration (but see Chambers, 1979), or how they vary across the population. A little more is known about the outcomes of consumer debt actions since they have been studied in conjunction with bankruptcy laws and laws regulating the extension of credit. Thus, we have learned about the prevalence (at least in some large cities) of sewer service of complaints (Caplovitz, 1974), the predominance of default judgments (Caplovitz, 1974), and the general success of commercial lenders in confronting debtors in court actions (Jacob, 1969; Yngvesson and Hennessey, 1975). Housing disputes have also been occasionally studied with results that are similar to those for consumer debt cases (Mansfield, 1978; Ruhnka, 1979). We do know that enormous sums of money are transferred through court actions and out-of-court settlements, but the pattern of those transfers has not been systematically studied.

Finally, the timing of trial court actions has been almost entirely ignored. It is often presumed that going to court is the last resort, but we know little about what precedes it and how extensive the pre-adjudication settlement process is in different kinds of cases or with different kinds of disputants. Do poor people go to court more rapidly than the rich? How important is the availability of legal counsel? Does the size of the claim or the likelihood of success play any role in hastening or delaying the trip to court? In the case of criminal behavior, we know a good deal from the work of Donald Black (1980) and others (Reiss, 1971; Rubenstein, 1973; Bittner, 1970: 107-13) who have studied the interaction among police, offenders, and complainants and explored the ways in which the workflow of the police affects the likelihood of their making an arrest. The variety of behavior in civil proceedings appears greater, and we know less about it.

Thus, in terms of who gets what when and how, large portions of our map remain uncharted. On every dimension, we know more about criminal than about civil proceedings. Our concentration on criminal proceedings has yielded important insights on the process and on the characteristics of defendants and the sanctions imposed on them. Still, many elements of the criminal process remain unstudied, and our

concentration on criminal cases has diverted us from examining more generally the population of clients that courts serve and the benefits and sanctions which they distribute. The importance of timing has been almost entirely neglected.

II. THE FAILURE OF THEORY

The study of trial courts does not lack theoretical models to guide researchers. The trouble is that there are, perhaps, too many competing perspectives, and conflicts between them remain unresolved. The consequence is that research projects often go past each other instead of providing cumulative knowledge.

The jurisprudential approach to the study of trial courts has been almost completely abandoned by socio-legal researchers. Such research concentrates on the content of formal rules of evidence and procedure and focuses on the judge's decisions concerning them. We have perhaps gone too far in neglecting such formal rules, as Doreen McBarnet's (1981) book on Scottish trial courts suggests. Law schools, of course, devote much time to such matters, and law journals still publish many articles on them. But such efforts typically involve doctrinal analysis rather than efforts to learn more about the empirical reality of how rules come to be formulated and applied.

One legacy of the traditional jurisprudential approach has, perhaps, not been entirely lost: the single-minded concentration on judges and *judicial* decision-making. Most of the research that developed from the judicial behavior tradition examined Supreme Court justices and their work. The same perspective extends to trial courts, where there is a tendency to equate decision-making in the courts with decision-making by judges. This is particularly pronounced in investigations of sentencing patterns of criminal trial courts (e.g., Cook, 1973; Uhlman and Kritzer, 1977; Uhlman, 1977; Gruhl *et al.*, 1981; Gibson, 1982) because the sentence is formally pronounced by the judge. Yet we know that sentences are often the result of a negotiating process in which the judge may not even have been involved. In addition, judges' decisions about such matters as jury instructions often reflect the suggestions of the trial attorneys, and sentences after a trial reflect the input of pre-sentence reports by probation officers. Thus, models which treat trial court decisions as if they turned on the judge are likely to be misleading since we know that judges have only a limited role in many trial court decisions.

Three models most frequently compete for the attention of trial court researchers. One is an organizational model; the second is a role model; the third is a decisional model. Each targets a particular element of the world of trial courts; none is holistic.

The organizational model became prominent through the pathbreaking book, *Criminal Justice* (1967), in which Abraham Blumberg argued that the criminal process in trial courts more nearly resembled a bureaucratic than an adversarial model (see also Packer, 1968). James Eisenstein and I (1977), Peter Nardulli (1978; 1979), and others (e.g., Clynych and Neubauer, 1981) have since tried to elaborate on this model. But as Lawrence Mohr (1976)—an organizational theorist more than a legal researcher—has pointed out, not all elements of the conventional organizational model apply comfortably to courts. What the organizational model does best perhaps is to call attention to the interactional elements of trial court proceedings. Both trials and out-of-court settlements involve interactions among various members of the courtroom work group. That work group is characterized by continuing relationships of varying intensities. The fact that work group members interact with varying frequencies over a long period of time affects the ways in which they deal with one another. It especially affects their communication patterns by allowing them to develop shorthand ways of transmitting information and by building trust or distrust among the work group. Since the heart of trial court decisions is the communication of information, organizational links are presumed to have important consequences for the work of courts.

Ideally, the organizational model would lead us to look for structured interactions and for the effects of those interactions on communications. This, however, requires intense observation of a relatively fragmented structure. A trial court is composed not just of the judge and clerks who are located in the courtroom and its adjacent chambers. Other important members of the work group, especially the attorneys who practice there, move in and out of the courtroom. No small team of researchers can shadow all of those who are important to a case. Criminal courtrooms are relatively well structured and almost all organizational studies look at them rather than at civil courtrooms. Much more of the business of the civil courts is done in lawyers' offices, and the number of attorneys practicing in a particular civil court is often far greater than what one finds in its criminal counterpart. Thus, studying the

civil side is more difficult. Perhaps for this reason we have almost no studies that attempt to utilize the organizational perspective for civil courts.

Role theory has been invoked by others—notably James Gibson (1978; 1981; 1982)—as an alternative perspective (see also Flango *et al.*, 1975; Ish, 1975; Unga and Baas, 1972; Galanter *et al.*, 1979). This stream of research is heavily influenced by research on appellate judges, where the role concept was first applied (Glick and Vines, 1969; Glick, 1971). In trial court research it has been applied principally to characterizations of judges, although Heumann (1978) examines not only the socialization of judges to their roles but also that of prosecutors and public defenders. Role theory focuses more on individuals than on groups even though roles are by definition the consequence of perceptions by others that come to have behavioral significance for the role player. Although role-focused analyses are frequently presented as alternatives to the organizational model, the two are quite compatible. The link, however, has not been effectively made, and the role model by itself has not guided enough studies to produce comprehensive results.

The third type of model is the decisional model. Such models reflect the judge orientation of appellate court studies. The empirical research here attempts to examine the degree to which criminal court judges show biases of various sorts—toward women, toward blacks, toward various classes of defendants. Most of the research has concentrated on the sentencing decision, as if the judge made it alone. There are also a handful of studies which attempt to relate judges' backgrounds or the political context in which they work to their decisional propensities. Peltason's (1961) study of how judges handled southern school desegregation cases in the 1950s is an early example of this genre; another is Martin Levin's (1977) comparison of Pittsburgh and Minneapolis judges. A more recent example is the study by Kuklinski and Stanga (1979) of the relationship between judges' decisions on marijuana use and local voting on this question. Once again, this approach has been applied to civil cases less frequently than to criminal ones, but it has figured in studies of civil cases that involve important policy questions.

The organizational, role, and decisional models may be seen as complementary to one another rather than as mutually exclusive. Organizational models are the most comprehensive of the three. They readily incorporate the concept of role and

in some versions focus quite explicitly on the conditions governing decision-making. However, they have not been so used. Rather than being employed to guide research by identifying critical problems that might substantially enhance our understanding of trial courts, these models are more frequently used after the fact to understand data that have been collected with different questions in mind. My own research with Jim Eisenstein illustrates this weakness. We began our investigation with questions about the prevalence of plea bargaining and were not guided by a theoretical model. We elaborated our organizational model after we had begun collecting our data when we realized that we could best understand what we were observing if we adopted the organizational framework. As a consequence, we failed to collect some data that were critical to an organizational understanding of trial courts.

The theoretical perspectives that I have been discussing are limited in that they relate largely to only one of the four aspects of trial courts that we have identified—the “how” of the judicial process. They have little to say about the characteristics of outputs, the distribution of outcomes, or the timing of adjudication. For such matters, we need to turn to still other theoretical perspectives.

The entire adjudicative process has been put by some scholars into the perspective of conflict resolution or dispute processing (Abel, 1974; Felstiner *et al.*, 1980-81). Largely derived from the work of anthropologists (e.g., Gulliver, 1973; Nader, 1969), these models view adjudication in a larger social perspective. Adjudication in trial courts is seen as only one of a large repertoire of procedures available to disputants for solving their conflicts. Many of those procedures do not involve public intervention and depend on private processes which may or may not be enmeshed in the legal system, while others are associated with non-judicial governmental institutions. Most disputes, it is estimated, are handled by these alternative procedures. If one recognizes this fact, one tends to view the judicial process in a different light. The disputes and conflicts that come to courts are neither all the conflicts in a society nor a random sample; rather they are a biased selection which take the judicial trajectory for special reasons. One subject for research, therefore, is to compare the characteristics of disputes in different arenas to understand why they gravitate to one resolution process rather than another. Considerable research is now directed toward such questions, as one can see

from the publications emanating from the Civil Litigation Research Project at the University of Wisconsin (e.g., *Law & Society Review*, 1980-81). This view of the judicial process tends to see disputes as continuing through a series of processing arenas, and not necessarily “resolved” by a court decision. Instead, such a decision may only set the stage for the next step in a sequence of actions that lasts until the dispute loses its salience to the participants.

The conflict resolution or dispute processing perspective is in some ways the inverse of the other models I have described. Conflict resolution and dispute processing focus less on the way in which the process works than on the character of decisions at various steps and their distribution among the various participants. This is not to say that this perspective ignores the “how” entirely; indeed, it has made very substantial contributions by alerting us to the role of mediation and arbitration as well as two-party negotiation. But the details of these procedures in particular institutional settings are of less interest than the variety of payoffs which these procedures allow and the manner in which those payoffs are distributed to players.

Finally, we may point to yet another theoretical perspective that has guided the interpretation of some findings: the view that law and adjudication are among the “social control” devices (e.g., Balbus, 1973; Thompson, 1975: 258-69; Trubek, 1977) used by the modern state to maintain its hold over the population, either in the name of capitalism or socialism. Like the other models, social control is more a heuristic than a set of specific testable hypotheses. While dispute processing and conflict resolution models extend the focus of socio-legal research to the antecedents of litigation, the social control perspective calls attention to macro-level elements of trial court operations, especially the ways in which they affect particular groups in society through the invocation of specific sanctions and benefits. Such an approach focuses more on ultimate consequences than on immediate effects.

How can I say that we have been led astray with such a wealth of theoretical models to guide us? The trouble is that on the whole we have been unwilling to follow rigorously the implications of any one theory and have, in fact, not developed very fully any of these models. For instance, it has proved difficult to mesh what we have learned from those who take a dispute processing perspective with the results of research guided by an organizational model. The former usually seeks

to describe parties and outcomes while the latter focuses more on how the process works. Thus, it is difficult to reach conclusions about the relationship between how decisions are made and what they are. We are blessed with an abundance of discrete findings, but they are difficult to relate to one another. Although many of us share the same enterprise, to judge by what we write, we do not really speak to each other much of the time.

III. PROBLEMS OF SAMPLING

The final difficulty that I want to touch on involves the question of sampling. We are both blessed and cursed in the United States with a multitude of jurisdictions. Each of the fifty states and the national government have their own courts. No two sets of courts operate identically. Moreover, our time frame is quite limited. Most studies concern themselves with a short slice of contemporary experience—often no more than several years. Consequently, the studies we possess cover a court here and another court there, one for one set of years and the other for a somewhat different set. What sense can we make of this hodgepodge?

One of the great handicaps that researchers in the United States work under is an uncertainty about the appropriate unit of analysis when studying trial courts. Courts themselves are inappropriate because they consist of a single courtroom in some places and dozens of courtrooms in others (especially in large cities). Courtrooms may be sampled but only with the understanding that a variety of activities may occur within them during the course of a day or week; some are misdemeanor courts in the morning and felony courts in the afternoon; others are motion courts in the morning and trial courts in the afternoon or on alternate Tuesdays. These variations make it difficult to draw a random sample of courtrooms. When attempting to sample cases, researchers often encounter shoddy, inconsistent record keeping which makes the drawing of a random sample problematic. Consequently, it is nearly impossible to draw a random sample of court activity. As a result, most studies are conducted with a collection of convenient courts, courtrooms, or cases, but it is difficult to know how such a collection relates to the universe of trial courts in the United States. All that we know with certainty is that few of the existing studies have used truly random samples and none can make confident inferences to the population of courts, cases, or litigants.

In the face of these facts, I suggest that some of us follow a different strategy. Rather than trying to study a larger number of jurisdictions less thoroughly, let us study a single jurisdiction in great depth. We will not lose much in breadth because almost all studies encompass no more than a handful of jurisdictions and that handful can, in any case, not constitute a random sample from which to draw statistical inferences.

When we study a single jurisdiction, we can invest far greater resources in data collection and can analyze a far broader range of relationships than if we spread our resources across many jurisdictions. Moreover, concentration on single jurisdictions may permit us to expand our analysis over time so that we can more easily observe causal relationships between what is occurring in trial courts and what is happening elsewhere—in the social, economic, and political arenas. Such a focus might also allow us to insinuate ourselves into judicial institutions and to induce them to collect data in ways that will be helpful both to them and to our research. If a few of us concentrated our efforts in this way, we would within a few years have very intensive analyses of trial court processes in a few jurisdictions. Together with the continuing tradition of comparative, cross-sectional studies, such intensive longitudinal research would, I believe, provide a more certain basis for understanding trial courts than our present collection of studies, which are confined to a narrow time span and which depend heavily on unreliable data.

IV. CONCLUSION

There is little danger that the exploration of the worlds of trial courts will cease. It is much more likely that it will flourish. The challenge becomes ever more exciting. The very fact that it is difficult incites us to try harder. The benefits are obvious: To understand courts is rewarding in its own right, and if we understand them more fully, we might be able to help design them to do their work more effectively. The prognosis for future research is also good because many of us are eager to continue the exploration, and we are equipped for our task with methodological tools that earlier generations of scholars did not even dream of.

Our task is to employ our methodological skills in fruitful ways. We must fill in the numerous gaps which beg for exploration. We must examine the content and impact of trial court decisions. We must make estimates of their effects on various groups in the population. We need to see how

adjudication fits into the larger pattern of dispute processing and conflict resolution. We need to understand more fully how what is done in the courtroom emerges from the interactions of officials, litigants, and their representatives. We must emphasize longitudinal research and must explore ways of looking at courts prospectively as well as retrospectively.

Measured against some absolute standard of excellence, the questions with which I opened this paper must be answered in the negative, and we might leave this exercise despondent over our lack of knowledge. An absolute standard, however, demands too much. If instead we compare our knowledge today with our state of ignorance twenty years ago or with what we currently know about legislatures, the presidency, the bureaucracy, or interest groups (to name just a few of the elements of the political arena), we may be proud of our accomplishments. We can now teach students more confidently than before, pointing out both what we know and what we can only guess at. We can begin to inform policy makers about some of the probable unanticipated consequences of their actions even if we cannot state with certainty the outcome of reforms. This is about all that most social scientists can do in even the most thoroughly researched areas. In these respects, the study of trial courts is very much in the mainstream of social research. While it suffers from the faults common to social research, it also benefits from the strengths of the social scientific approach to knowledge.

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