OPENING THE TIME CAPSULE: A PROGRESS REPORT ON STUDIES OF COURTS OVER TIME

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This essay traces the rise of longitudinal studies of courts. In the United States, the rise of a sociolegal school of history (the "Wisconsin school") was the immediate stimulus. Longitudinal studies, in Europe and the United States, explored the relationship between law and the economy; they tended to find an inverse relationship. But more recent findings suggest some reversal of this trend. Changes in the nature of business relationships have stimulated increases in some forms of major business litigation. Moreover, in modern legal culture, freedom is conceived of in substantive terms; and the courts appear more and more to be the place where true justice can be found.

A small number of law and society scholars—a dozen or two at the most—have tried their hand at longitudinal court studies. This term means nothing more than the empirical analysis of data from some court or courts over time rather than at a single point in history. The majority of the scholars have studied trial courts. There has also been at least one fairly large-scale study of state appellate courts (Kagan *et al.*, 1977, 1978; Friedman *et al.*, 1981; Wheeler *et al.*, 1987). Most of the researchers have looked at American courts, but certain European jurists have studied their own systems; in fact, some of the seminal work has been done abroad.

The basic idea of these studies is disarmingly simple—so simple that one is tempted to ask why the work began so late and why it continues to be so rare. Among the dozens and dozens of law reviews and related journals, and in the jungle of literally thousands of books, articles, and treatises about courts, judges, and their work, only a handful have elected to use longitudinal technique. But, after all, many simple ideas are not socially simple; that is, they seem to require a certain context, a background, before they can flourish. For example, the idea of studying the lives of ordinary people, including housewives, in the eighteenth century is hardly the stuff of which Nobel prizes are made; yet apparently the idea did not occur to most historians until the recent upsurge of interest in women's history and in social history in gen-

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eral. Similarly, the social conditions for longitudinal court studies were not ripe before a certain small, dignified revolution took place in legal history and in legal studies generally: the rise of the "Wisconsin School," associated with Professor J. Willard Hurst at the University of Wisconsin.[1]

Before Hurst, American legal history was an arid, stunted field primarily concerned with the evolution and development of legal doctrine; it was in some ways a colonial lapdog of English legal history; and like legal scholarship in general, it centered almost exclusively on appellate courts.

Hurst decisively reversed the emphasis. He was primarily interested in the relationship between law and the economy. He rejected the "internalist" view of law—the view of law as "autonomous," to use the currently fashionable term—and treated it as a social product, in essence instrumental. In American society at least, law was a tool, an implement, which concrete interest groups and individuals manipulated for whatever ends they had in mind.

Law was therefore, by and large, a dependent rather than an independent variable, shaped by events and ideas from outside. Since law was an instrument, doctrine and legal theory were less important than law as actually used and experienced. This meant that legal historians should turn to or return to a study of "living law"; this in turn meant that they could not and should not neglect the trial courts. It would be wrong to concentrate on flashy events at the appellate level. There were valid, even vital reasons to emphasize the modal, the average, rather than the extraordinary, the piquant, the dramatic. Hurst himself did not study any particular court longitudinally, but the pioneer example of this art form was a book—pretty much forgotten or neglected today—on the trial courts of Chippewa County, Wisconsin, published in 1959 by Francis Laurent, under the direct influence of Hurst, who wrote the introduction.[2] For the first time, there were data on the number and kind of cases a particular court actually handled, and on how these had changed over time.

After Laurent, there came a period of comparative silence. A new start was made in the 1970s, and it began in what American legal scholars—on the whole a provincial lot—would consider an unlikely source: Spain. A young Spanish scholar, José Juan

[1] The opening shot in the Hurst revolution was the publication of his *Growth of American Law: The Lawmakers* in 1950. Perhaps the most influential of his many books was *Law and the Conditions of Freedom in the Nineteenth-Century United States* (1956). For a brief overview of the history of legal history see Friedman (1984); for a different slant, see Gordon (1984b). For another view of the influence of Hurst on longitudinal studies and on law and society theory in general, see Munger (1988).

[2] Hurst in Laurent (1959: xvii) reminds us that the data are relevant "not only to the history of the legal system itself, but also to the story of the law's living relation to general values and processes of the society." In fairness to the neglecters and forgetters of Laurent's book, it should be pointed out that the book consists mostly of tables and not much analysis or explanation.

Toharia (1974),[3] published a remarkable book in which he tried to examine the relationship between law and social change in Spain between 1900 and 1970, using statistics on the work of the Spanish courts over time. Toharia found that the caseload of the courts had not kept pace with economic growth. Indeed, stagnation and decline were most marked in those parts of Spain that had most advanced industrially; the courts had held their own in rural backwaters.

This somewhat surprising result did not mean that "law" in the broad sense was irrelevant to economic growth, or that "legal activity" did not reflect changes in the pace and nature of commercial and industrial life. Quite the contrary: Toharia also looked at other indicators—notarial acts, including the formation of corporations—that were equally "legal," although not judicial. Here he found the expected correlations: dramatic increases in quantity followed strong economic growth; and the correlations were most dramatic in the urban and industrial centers of Spanish life (*ibid.*, ch. 5, pp. 149–67).

Two of the themes of Toharia's book resonated with themes of the Hurst school: first, the (relative) irrelevance of formal courts and litigation in society and within the legal system; and, second, the intimate connection between the legal system *as a whole* and economic life. The declining function of the courts could thus be explained either externally (that is, in terms of characteristics of social behavior), internally (that is, in terms of some institutional defects), or through some combination of the two.

At the time, a number of commentators stressed the internal issues. The courts seemed too slow, too expensive, too formalistic, too hidebound and impervious to change.[4] The engines of economic life, as they grew monstrously great, would either crush the court system, trapped in its prison of Weberian formal rationality, or find ways around it. The courts themselves were instruments much too blunt to handle the *general* job of conflict resolution within society, for a variety of reasons.[5]

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[3] Toharia had been trained in law in Madrid and then earned a Ph.D. in sociology at Yale University.

[4] This seemed at the time not only plausible but also a general phenomenon in the modernizing Western world. Industrialization would at first bring about an increase in litigation, but this would be followed by a period of stagnation and decline. The reasons were simple: A mature industrial society should disfavor litigation, which interferes with a vigorous, ongoing economic life; hence in such a society, slow, hidebound, expensive courts are actually functional. See Friedman (1976: 25, 33).

Gutiérrez (1979: 240–41), studying the courts of Costa Rica longitudinally for the period 1945–70, found an increase in litigation rates, but he predicted, on the basis of the theories just mentioned, that litigation was likely to decline as the country modernized.

[5] See the discussion of the Macaulay thesis below. One might also mention the work of the Civil Litigation Research Project (CLRP), which also operated out of the University of Wisconsin, as an example of the general attitude in the law and society community toward litigation as such. CLRP tried

Other European scholars took up Toharia's theme (see Rottleuthner, 1985),[6] and a new wave of American longitudinal studies also began (Friedman and Percival, 1976a; McIntosh, 1980–81; Daniels, 1985). The American studies had various purposes, methods, and agendas. Although it would be too strong to say that they confirmed Toharia's thesis at all points, they were in the main consistent with his findings. These studies did not document an actual *decline* in litigation at the trial court level. The most that could be said is that they did not find positive evidence of any startling rise in litigation. Thus, in the 1970s, longitudinal researchers found themselves caught up in the controversy over whether there was in fact a "litigation explosion." Most of the discussion of this subject was empty talk; judicial statistics in the United States were woefully inadequate, and the "explosion" was a matter of impressions and anecdotes.[7]

In theory, longitudinal studies should have been able to answer the question definitively: Were litigation rates rising or not? In practice, however, they could not and did not solve the puzzle. This was in part because of problems of definition: What *is* a litigation explosion? What is litigation? What are we measuring when we count the number of cases filed in court (Friedman and Percival, 1976a)? Nonetheless, there had been such wild talk about litigation in the United States that it was quite proper to cite the longitudinal studies on one side of the issue. The historical evidence, ambiguous and difficult to read as it was, did not support the thesis of a terrible "explosion."[8]

Longitudinal and historical studies did not find, of course, that courts had been abandoned, that courtrooms were deserted and covered with cobwebs, that judges were snoring on the bench. They did find that disputes, and especially *economic* disputes, made up a declining portion of the caseload. They found that the docket, over the last century or so, had shifted in the direction of more "personal" matters, particularly those that did not involve an actual contest in court. For example, if the modal case in a trial court in 1870 was a dispute over title to land or an attempt to make sureties pay up on a promissory note, the modal case in 1970 was an uncontested divorce. Friedman and Percival (1976a: 296)

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to study the life cycle of disputes, that is, the way in which disputes arise and are dealt with in "real life"; only a few such disputes ever turn into lawsuits, of course, which was one of the issues that the project examined. On CLRP, see Trubek (1980–81a). One should mention as well the great research interest in what is known as "alternative dispute resolution."

[6] Some material on Scandinavian litigation rates had also appeared in Blegvad *et al.* (1973: 105–9).

[7] Newspapers and magazines regularly reported various horror stories about the litigation explosion, and scholars wrote articles denouncing "hyperlexis" and similar atrocities. See, e.g., Manning (1977).

[8] For a summary of the literature on this point see Galanter (1983a); Friedman (1985: ch. 2); see also Galanter (1986b).

spoke of a shift from trials of disputed matters to what they called "routine administration." Other studies too found the courts spending more of their effort on administrative work and spending less on actual litigation, although there was some dispute over details.[9] There were parallel trends in the appellate courts as well (see Kagan *et al.*, 1977); and in criminal actions, the percentage of felony cases that went to trial had also declined between the mid-nineteenth century and the present, as plea bargaining—a much more administrative mode of handling such cases—came to occupy a pervasive role in the system.[10]

The findings of these studies, like Toharia's, concerned the work of particular courts in a particular culture at one point in history; they were not evidence of laws about the behavior of courts, or about the relationship between courts and economic growth.[11] They did isolate and attempt to explain how courts operated under one special set of social circumstances. They were, in some ways, methodologically crude; in part this was unavoidable because the materials of study were in poor condition and hard to use. Moreover, the studies were primarily historical and only secondarily aimed at modern policy issues. Of course, they had to begin and end their data gathering at some point, and that point was never yesterday. Friedman and Percival (1976a), for example, carried their data only to 1970; and so did Toharia (1974).

The research continues, of course; and it not only revises and reinterprets the older studies, it also gathers more recent data. The new research has turned up some surprising facts; surprising, that is, in the light of the earlier longitudinal studies. Is it possible that the courts of the 1980s have come to occupy a position in soci-

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[9] The Family and Commercial Disputes Study, sponsored by the U.S. Justice Department and conducted by Arthur Young & Co., and Public Sector Research, Inc., attempted to "replicate on a national scale" the study by Friedman and Percival by examining state trial courts in five counties scattered about the country. Some of the data from this study are summarized in Lieberman (1984: 48 ff.). A memorandum by J.J. Perlstein dated January 21, 1981, also sums up the findings. The two main foci of the study were "changes in the frequency of filings in selected categories of cases" and "the courts' formal involvement in the disposition of particular actions," that is, its actual role in dispute resolution. The findings were "mixed." Some of the trial courts behaved very much like the two California courts studied by Friedman and Percival, but in certain regards others did not. See also McIntosh (1980–81).

Daniels (1985) found that only a small percentage of filed matters led to contested hearings on trials but that this pattern had held firm over time, which puts the findings for the Illinois counties he examined somewhat out of step with those in the other studies.

[10] There is of course a large literature on plea bargaining; see, e.g., Mather (1979a). On the history of plea bargaining, see Friedman (1979); Heumann (1975).

[11] In the late 1970s, the findings of Toharia's study and of the American studies seemed to justify more sweeping generalizations than those findings do today; see Toharia (1987: 70) and McIntosh (1980–81: 846–47), who states that the rate of civil litigation in the St. Louis Circuit Court "exhibits a nonlinear relationship with socioeconomic development."

ety somewhat different from the position of the courts of the 1960s? Toharia himself returned to the study of Spanish legal culture and examined what happened in his country's courts between 1970 and 1980 (Toharia, 1987). He found an astonishing reversal of some earlier trends. Civil litigation rose dramatically during the decade; indeed, it more than doubled. Toharia also cited figures indicating rising rates in other European countries as well.[12]

There are signs too of important changes in the United States. Federal caseloads continue to rise (Clark, 1981), and there is evidence of increasing litigation in some states, for example, Florida (Gifford and Nye, 1987; see also Marvell, 1987). More significant are certain qualitative changes. For example, the amount of money and effort spent on litigation has apparently risen dramatically over the last decades (Galanter and Rogers, 1988). It seems likely that a small number of very large, very significant lawsuits are responsible for the lion's share of this money and effort. These "megacases"—giant class actions, huge private antitrust suits, enormous trademark and patent marathons—enlist literally hundreds of lawyers, cost millions of dollars, and greedily absorb judge and courtroom time (Chayes, 1976).

There may be something of a pattern here. The "liability crisis" is often discussed in the same breath as the "litigation explosion." And, as with civil suits in general, there does seem to be objective evidence of increases in the amount and consequences of tort liability. Certainly, insurance premiums have risen, tort litigation has driven the asbestos industry to the wall, and individual lawsuits have bankrupted small businesses here and there. We hear about closed playgrounds and abandoned ski lifts, all as a consequence of litigation; and the threat of malpractice suits has apparently led some doctors to change their branch of practice or to engage in "defensive medicine" (Zuckerman *et al.*, 1986; Tancredi and Barondess, 1978). But here, too, important longitudinal evidence suggests that average tort recoveries have not been rising as rapidly as all the noise and hullabaloo would suggest. Rather, a few very large recoveries distort the overall picture.[13]

There are more surprises in recent research as well. For example, one hears now talk about the rebirth of contract litigation and an upsurge in lawsuits between businesses. This, if true, would flatly reverse the strongest trend of the first wave of longitudinal studies, trial and appellate alike—the decline of commer-

[12] The Spanish figures were, however, the most dramatic. There were very modest increases in England and Wales, and striking increases in Belgium and France. In the Federal Republic of German, however, there was a slight decrease.

[13] The evidence for post–World War II jury behavior and the size of awards comes most notably from studies sponsored by the Rand Corporation; see Shanley and Peterson (1983). On the late nineteenth-century situation, see Friedman (1987); another longitudinal study of tort actions is Munger (1987a).

cial litigation. But why should there be a revival of purely business or commercial lawsuits?

Perhaps one place to begin is with Macaulay's famous article of 1963, which examined the "living law" of contract among Wisconsin businessmen. On one level, Macaulay was interested in the relationship between the contractual behavior of his subjects and the demands and postulates of the formal law of contract. What he found was an unbridgeable disparity. Business people showed a marked disinclination to *use* their rights under contract law. When contracting parties stand in a valuable continuing relationship—as is true quite generally in the business world—they will avoid litigation over breaches of contract; they will instead find ways to accommodate each other. Business people are generally anxious to deal with each other again; highly formal negotiated contracts and a willingness to resort too quickly to litigation stand in the way of smooth relations. The Macaulay thesis harmonized neatly with the dominant ethos in the sociology of law of the 1960s, and it fit in beautifully with the postulates of the Hurst school (Macaulay himself was a professor of law at the University of Wisconsin and was closely associated with Willard Hurst). The impulse to pursue Macaulay's line of research was Hurstian to the core (classical legal scholars had better things to do than sit around and chat with minor officials of Milwaukee paper companies), as were his essential findings, which assumed an instrumental approach to law and debunked the centrality of formal legal doctrine, appellate courts, and litigation.

The Macaulay thesis had two sides to it; but later scholars rarely discussed or even noted the second side: why people *do* behave "contractually" and even litigate, and when. After all, there is and always has been a certain amount of business litigation, and Macaulay attempted to explain exactly what circumstances might (or might not) lead to carefully planned contracts and ultimately to litigation. Put most simply, contractualism and litigation could arise out of certain rare but highly complex business situations; or out of ruptured or one-time transactions in which long-term relationships were not of any particular value; or circumstances in which conventional business norms and customs simply did not govern.

Macaulay's business people avoided litigation because their business world *was* a world of long-term, continuing relations. In an important recent paper, Galanter and Rogers (1988: 3) argue that the "environment of American business practice" has altered, in a way that affects the Macaulay thesis, since the early 1970s: "[m]arkets have fissured, products have become more specialized, competition has increased, and business dealings are in general marked by greater instability." These factors of course describe a business environment that is less likely to foster the conditions that led to the "noncontractual" side of the Macaulay thesis. The

new competitiveness, for example, creates an atmosphere in which short-run "bottom-line" concerns predominate; it increases the "relative stakes in individual transactions," which each become "more 'all or nothing' " (*ibid.*, p. 6). Because of these factors, Galanter and Rogers predict a rise in business litigation; and they claim corroboration in a surge of contract filings in federal courts.[14]

The Galanter and Rogers thesis also gains a bit of support from research, some of which they discuss, that look at the phenomenon in another setting: the large law firm. Here too observers have described a tremendous increase in volatility since the 1970s. The "Wall Street" lawyer—the big-firm, corporate attorney—traditionally avoided litigation and enjoyed long-term stable relationships with the businesses the firm represented. Recent trends have shattered these cozy arrangements. The relationship between business and law firm has become more "transactional," that is, more ad hoc and confined to a single (often enormous) matter (Nelson, 1988a; Friedman, 1989a). The resulting environment is shaky, unstable. Law firms merge, unmerge, branch out, split, and in general show the same volatility as the business community that employs them.

These changes, as we have said, demand the rejection of neither the Macaulay thesis nor the first Toharia thesis, nor its reflections in Friedman and Percival and other American longitudinal studies. They do, however, call for a certain amount of reformulation and fine-tuning. No doubt business firms tended to avoid the courts in part because of failures in the court system itself: its stiffness, expense, and formality—in short, factors which led litigants to think of courts as costly and irrelevant. But the megacases are less troubled by factors of costs and delay. A rhinoceros laughs at a barrier that holds back gazelles.

However, there are undoubtedly other, deeper factors that led to avoidance of courts. The latest trends, if real, invite us to explore some of the social conditions that affected and still affect the courts. First of all, in this (and in most societies) there are by-pass systems, or alternatives to courts—ways of dealing with disputes—that function well among those who share some sort of norm system or sense of community. To deal with disputes, businesses have always used arbitration—or simply learned to work things out, as Macaulay (1963) so well described.

The business people in Macaulay's study were in many regards ordinary maximizers: They had their eyes on profitable business relations, and their behavior was rational enough to satisfy any economist of the classical school. These business people had sim-

[14] They admit that the evidence for such a surge in contract cases is much weaker in state courts; however, see the data on Florida in Gifford and Nye (1987).

ply learned that nothing was maximized but trouble if they behaved crudely and legalistically, offended their colleagues, and ruined all their comfortable—and profitable—relationships. It is richer and more realistic, however, to analyze Macaulay's findings in broader terms as well, that is, in terms of legal culture, and to speak of business norms and of a culture of "getting along" and avoiding courts. We should not view this culture of court avoidance as a mere quirk of the American business personality (similarly, one should be very careful about describing Americans as "litigious"). Rather, three factors interacted to produce the described behavior.

The first factor was the business environment, as explained by Macaulay: a web of continuing relationships, governed by informal norms, in which litigation was reserved only for extraordinary, rare, intrusive cases of "marital breakdown," as it were. The second factor was the court system itself, which betrayed certain repellent and "archaic" characteristics; these had survived because the system was insulated from the actual world of business, not to mention other worlds even more remote from the courtroom. The court system thus came to handle, in the main, two kinds of cases: routine administrative matters, and, more rarely, pathological and borderline cases—cases growing out of freak, unusual, or one-time situations.[15] Third, there was the legal culture itself, a general cluster of attitudes largely produced by the two other factors but which also influenced and interacted with them, insofar as the attitudes determined when and to what extent individuals, businesses, and groups would actually turn to law. A divorce rate—to take a nonbusiness example—reflects (1) the marital environment within which men and women interact; (2) the legal rules about divorce and the court system (how much does a divorce cost? how long does it take?); and, (3) last but not least, *attitudes* toward divorce within the culture (do neighbors, friends, and family support or stigmatize those who divorce?).

Legal culture is an elusive subject, which I have tried to deal with elsewhere (1990a, 1985). It seems plausible that general attitudes toward law in modern Western society differ strikingly from those that prevailed a century or more ago. One aspect of legal culture of the nineteenth century was a more sharply defined sense of the *limits* of law. There is, in general, an inverse relationship between the use of law (in the formal sense) and the strength of custom, traditional authority, and norms that are applied in face-to-face situations. Another, related aspect of the older legal culture was the view that there was a specifically "legal" domain, that is, certain types of dispute were appropriate for legal process, while others were outside this domain.

[15] See also Friedman (1965) on the appellate caseload of the Wisconsin Supreme Court in three historical periods.

In the twentieth century, for one reason or another, traditional authority has continued to weaken in Western countries. The nuclear family is not what it used to be, and the authority of teachers, the clergy, employers, and, generally speaking, the state also seems a bit shakier. In these societies, urban life, industrialization, and the increasing division of labor have vastly expanded the domain of interactions with strangers—the people who jostle us on the street, who produce the goods we eat, wear, and use, the experts who examine our bodies, souls, and portfolios. These relationships are the domain of "law," and the more ad hoc and impersonal, the more "legalized" they become. This is true for individuals and for Macaulay's business people as well.

In modern legal culture, populations consume and demand more law; this places increasing pressure on legal institutions, including the courts. The response in the United States and in many other countries has been such that, for better or worse, these institutions became or seemed to become more relevant than in the past. Social change has opened a door to expanded use of courts and to an expanded *conception* of courts. Viewed from this aspect, the findings in Toharia's 1974 book, and in the American longitudinal studies, although presumably accurate, were misleading insofar as they tempted law and society scholars to think in terms of very long-term trends. They were in fact only temporary phenomena.[16] At least this is a possibility—a starting point for theory and research.

To be sure, longitudinal and other historical studies have uncovered other periods with massive use of courts, although "litigation" may not be exactly the right word. The American colonial period is one example. A researcher who examined the court records of a tiny Virginia county during a three-year period in the seventeenth century (1633–36) found that the names of almost all the adult residents of the county appeared in the records as litigants, witnesses, or in some other capacity (Curtis, 1977: 274, 277). But the courts then served a rather different function than they do today. They reflected neither a minimalist nor an expansive conception of law; rather, the courts were general organs of government, at a time when the line between state and private was not clearly drawn; the courts operated paternalistically, as extensions of traditional authority, rather than as institutions that affirmed and protected rights, as places to which people went to demand redress for essentially private grievances (Hartog, 1976). Once again,

[16] Toharia (1987: 137) himself, on the contrary, thinks of the findings of his second study as the temporary phenomenon. He feels that the best explanation for the rise in Spanish litigation in the 1970s is the "important and sustained economic crisis" of that decade. Thus, the "litigation explosion" in Spain in that decade is best looked at as "a transitory phenomenon"; when the crisis abates, the level of litigation will return to the patterns that had been established for almost a century.

we must be careful not to speak of "courts" as if the term had some eternal, abiding meaning, isolated from social and historical context.

When all is said and done, longitudinal research on courts is obscure, scholarly stuff. It does not have much resonance in popular legal culture. It is good, honest research, and it looks for truth in a corner of life where propaganda and hysteria abound. But the hysteria is itself an important social fact. One cannot help wondering what impact the hysteria has on public attitudes and therefore, indirectly, on the use people make of the courts.

Public attitudes have been curiously ambivalent. The public is deeply suspicious of law, lawyers, and courts, and yet at the same time grossly exaggerates their power and efficacy and learns to lean on them inordinately. Modern American society is amazingly legalized;[17] it places a great deal of emphasis on law, legal process, and—yes—litigation. This does not mean, of course, that people are "litigious" or that everybody is suing everybody else. As noted, changes in legal culture and in social conditions generally may have consequences for litigation that on the whole are more qualitative than quantitative, at least so far; at any rate, the results are not all in.

One form of litigation that has grown tremendously in this century is constitutional litigation. Civil rights and civil liberties hardly figured in nineteenth-century dockets, even appellate dockets. By the 1970s, however, the constitutional rights of criminal defendants were a major element in such dockets (Kagan *et al.*, 1977).[18] Congress passed a series of important civil rights laws in the 1960s, and the number of suits brought under these acts has been high and continues to grow.

Why the great surge in constitutional litigation? There are surely many reasons, but in part it occurred because of the way in which the average person now conceives of freedom, rights, and entitlements. Lawyers on the whole have a procedural theory of

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[17] I use this term rather than "legalistic," which has pejorative overtones and in any event is quite misleading. "Legalistic" implies some kind of wooden, bureaucratic sticklerism, which in my view is absolutely false to American legal culture. By "legalized" I mean two things: First, relationships are subject to law or legal processes in various senses rather than defined as completely beyond the reach of such process; students' or prisoners' rights thus represent a higher stage of legalization than leaving students or prisoners absolutely to the tender mercies of authorities. Second, a process is "legalized" to the extent that it takes on some of the aspects of judicial process. For example, tenure decisions today are more "legalized" than they were two generations ago. There has been considerable national and international discussion of legalization. See, e.g., Werle (1982).

[18] There is surprisingly little empirical study of civil rights litigation, but see Eisenberg (1982).

justice; the layperson's theory is, to the contrary, substantive.[19] Concepts of freedom of choice, personal privacy; the right to work, quit work, and change work; the right to travel: substantive claims. The absence of discrimination, fair treatment in all settings, and decent opportunities—these are the essence of "law." And if we ask, what modern *institution* is concerned with enforcing substantive rights, particularly against government and large organizations? the answer is the courts, for all their limitations. The courts—not the legislatures, not the bureaucracy, not the executive branch—are where "justice" can be found. In this cultural climate, the whole social meaning of litigation gets redefined, and the use of litigation, one expects, changes accordingly.

Longitudinal research—and I include here not only the historical studies but also ongoing, longitudinal research—is of particular value, because it is sensitive to changes in legal culture, and in the functions of the courts. The existing research on this rather murky subject has accomplished a good deal. It has even greater possibilities for future contributions: to policy analysis and to legal theory. The studies, defective and scattered as they are, provide baseline data, against which to measure and monitor what is happening in our own turbulent times.

[19] This is a surmise, a hypothesis, if you will. I do not mean to suggest, of course, that procedural justice is not a matter of importance to people; it most surely is, and there is a body of research on what people think of as procedural justice. See, e.g., Tyler (1988).