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Carrie Menkel-Meadow & Bryant Garth, *Process, People, Power and Policy: Empirical Studies of Civil Procedure and Courts*, in *OXFORD HANDBOOK OF EMPIRICAL LEGAL RESEARCH* (Peter Cane and Herbert Kritzer, eds., forthcoming).

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Georgetown Public Law and Legal Theory Research Paper No. 10-04
January 2010

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Process, People, Power and Policy: Empirical Studies of Civil Procedure and Courts

Carrie Menkel-Meadow and Bryant Garth

For *Oxford Handbook of Empirical Legal Research*
(Peter Cane and Herbert Kritzer, editors, Oxford University Press)

I. Introduction: *Empirical Studies of Procedure in Service to Which Procedural Values?*

Courts play a central role in both the legal and political processes in many countries, especially in the common law world. Legal actors have a stake in making sure that legal processes and procedures are perceived as legitimate, both by the general population who might use the legal system, and by the professionals who operate it. A relatively constant series of issues about whether courts are fair, efficient, and provide justice serve to structure a longstanding debate about how courts operate and the best rules of process to determine how disputes and substantive legal claims are resolved.

In the field of civil procedure, where there is a continuing demand for some procedural rule reform, empirical studies of how rules actually operate have, for the most part, been used in partisan ways to advocate for particular reforms in the interests of one or another legal or client constituency. Empirical studies have been commissioned by policy makers and rule drafters to learn how much litigation costs, how long it takes, whether other forms of dispute resolution should be employed instead of trials, how much discovery or information should be shared in each case, whether particular rules have their desired effects (such as summary judgment rules, sanctions for inadequate verification, and taxed costs for failure to accept settlements), what role judges should play in managing cases, and whether there are particular patterns of outcomes for particular litigants.

Only relatively rarely has empirical study of civil procedure been conducted by more disinterested or “neutral” social scientists and legal scholars, such as Martin Shapiro’s attempt to explain the “universal” in triadic structures of disputants and third party neutrals (1981). Indeed, as this essay reveals, many of those conducting or commissioning empirical studies of civil procedural processes have been directly involved as advocates for particular procedural reforms, such as Charles Clark (law professor, Dean, then federal judge) in the United States development of the 1938 Rules of Civil Procedure and Lord Harry Woolf, architect (as a law lord and judge) of the 1998 Rules of Civil Procedure in England and Wales. Thus, this essay reviews the political, personal and policy issues that have marked empirical studies of civil procedure and justice.

Most studies of civil procedure recognize the inevitable tensions between values of *accuracy* (truth ascertainment), *efficiency* (time to disposition), *access* (costs of litigation and availability of representation, and relative transparency and simplicity of rules), achievement of *substantive justice*, and more recently, *procedural justice* (or perceptions of or satisfaction with the process of dispute resolution itself (Zuckerman, 1999, Tyler, 1997). To achieve true decisional accuracy (a factually correct result) or true substantive justice (combinations of accurate fact finding, law application, and considerations of equity) often involves long, costly, complex, labor intensive, and intrusive actions on the part of litigants (parties), their representatives, judges and court personnel, and other adjuncts of the legal system. And evaluation and consideration of who is served by these competing values requires assessment of whether costs should be borne by parties (or their lawyers) or the system, and whether the purposes of any procedural system are for the parties alone (dispute resolution) or for the larger society (law and precedent generation). Is the purpose of civil procedure and process to allow

both private and public parties to peacefully resolve their disputes or are their disputes “public goods” generating law and normative orders for the rest of us (Menkel-Meadow, 1995)?

Can answers to these perhaps irresolvable questions be supplied by data and empirical study? Since so many countries have recently explored these issues in various efforts to reform civil procedure (Hanson & Rothman, 1999), we can here review the uses to which empirical studies have been put in debates about such concrete issues as pleading rules, discovery and information exchanges, verification (Rule 11, Fed. R. Civ. Proc. for Americans), summary judgments and proceedings, class actions, case management and alternative dispute resolution.

Looking at more macro level issues, social scientists have demonstrated that process, often without overt relations to outcomes, is integral to how disputants experience the fairness of the legal system (Lind & Tyler, 1988), and also that process itself can defeat important substantive claims for individual justice, as well as larger systemic goals, where the “process may be the punishment” (Feeley, 1979, Bumiller, 1988; Utz, 1978,).

This chapter will therefore show a rich body of empirical research about courts, rules, and their role in different societies. Part of our task is to analyze that research and how it has been used in policy debates and reforms. We also discuss the question of the demand and supply for empirical research about rules of procedure and courts. The story of social science and civil procedure is inseparable from the story of individual and institutional investments in social science at particular times and places -- and how such investments are rewarded. Individual stories reveal both the increasing returns to investment in empirical research and the difficulty of doing empirical research that goes beyond a focus on the institutional needs of the courts themselves and the reformers interested in their own court-reform agendas. Our challenge in this review is to tell a story that is simultaneously about the “progress” of empirical research and the

accumulation of knowledge about process and also about a systematic structural tilt toward political uses of that research that works to deprive us of systematic social science research that can actually explain the changing position of courts, litigation, and law in the larger economy and state.

The questions that social science researchers and procedural reformers have asked about process, procedures and rules are typically defined by the institutional needs of the courts. For example,

1. Is the process fair (as perceived by those within it, and those who are affected or governed by it) (Pound, 1906; Pound Conference, 1976 (Levin & Wheeler, 1979) and accessible (Cappelletti & Garth, 1978; Genn, 1999)?

2. Is the process efficient? (Is there a reasonable relation between costs of use, time of use, amount of use, and production of outcomes (Priest, 1989)?

3. Is the amount of process and use of the legal system appropriate for societal needs? (See arguments about the “litigation explosion” (Galanter, 1983), alleged cultural (e.g. Ramseyer & Nagazato, (1999); and class differences in use of processes and outcomes delivered (Galanter, 1974.)

4. Do differences in process (fora, decision-makers, method of process) make a difference? (In outcomes, in user satisfaction, in legitimacy of process or the larger legal/political system (E.g., Lind et. al, 1990; Clermont & Eisenberg, 1992)? How can we usefully compare different processes to each other, where experimental conditions for real case analysis and comparison are virtually impossible to achieve?

5. How do claimants/disputants/transaction makers choose particular processes? (Consider the controversial ‘selection hypothesis’ in litigation (Priest and Klein, 1984),

avoidance (Felstiner, 1995) of claiming, and the social construction of disputing (“naming, blaming and claiming” (Felstiner, Abel & Sarat, 1980) and “reframing.”

6. How do the requirements and resources of different processes affect their use and outcomes? (E.g. The relative role of attorneys and non-attorney representatives in processes (Kritzer, 1998); amount and type of discovery and information exchange, economic and linguistic resources (Conley & O’Barr, 1998; White, 1990) of parties and decision makers (juries/judges).

7. What are the effects of substantive resource allocation rules as incentives for the use and outcomes of processes? (E.g., punitive damages, attorney fee shifts, loser pays rules, class actions).

8. How are process effects measured? (Quantitative vs. qualitative assessments of process differences; issues of methodology, and the problematics of operationalization of such variables as fairness, satisfaction and justice, see Menkel-Meadow, 2010 (this volume).

Our brief review of the history of empirical research in civil procedure will seek to reveal both the progress of this research and some of the limitations that are necessarily built into it. This chapter concludes with a critical review of some of the current controversies in assessing the empirics of rule and process reforms.

II. *A Brief History of Empirical Research in Process and Procedure: Law Reform, Career Capital or Academic Interest?*

Civil procedure, in both the Anglo-common law family and in the civilian legal systems, has been historically known for its complexity, technicalities, and esoteric requirements, generally requiring professional assistance for the pursuit of a civil legal claim. Without

repeating a full history of the movement from technical rules in pleading to “notice” pleading and more “equitable” conceptions of how to get to court and what courts can do when the litigants arrive (see e.g. Subrin, 1987; Menkel-Meadow, 2004), the twentieth century produced a vast amount of procedural reform in many countries, all seeking to simplify, streamline, and in some cases “unify” the rules of process so that the “merits” of the matter, and not some procedural technicality, would control the outcome and grant the parties substantive justice. Beginning with reforms in the 19th century (in the United States with the Field Code in 1848, and in England with Benthamite reforms leading to the 1875 Judicature Acts), pleading rules were simplified and courts’ powers somewhat unified and rationalized. Indeed, as a continuation of this project across national boundaries, most recently, the American Law Institute and UNIDROIT have joined in a project to draft streamlined procedural rules for “harmonization” and unification of rules for transnational civil litigation (Walker, 2009).

In the United States federalism continued to produce different sets of rules for federal courts and state courts at the beginning of this reform period, and so, beginning with the famous speech by Roscoe Pound in 1906 on “the Causes of Popular Dissatisfaction with Justice,” movements to reform the rules of procedure grew in force (with strange exchanges and mutations of “political” views as conservatives and liberals eventually joined) to support the reforms that led, in the United States, to the Rules Enabling Act and the 1938 Rules of Federal Procedure (unifying procedure in the federal court system, which merged actions at common law with those in equity, produced new rules on discovery and information exchange, and authorized summary proceedings, and more liberalized rules for joinder of claims and parties), all done on a platform of both efficiency and justice on the merits for the parties.

As these reforms were being proposed in the 1930's, the Legal Realists, drawing on the new visibility of the social sciences, attacked procedural formalism by calling for empirical studies of how rules and laws actually worked (Schlegel, 1995; Garth, 1997). With the mechanistic study of formal legal rules increasingly discredited, understanding the behavior of lawyers and judges within legal institutions (called "the administration of justice") was considered necessary in order to perfect systems through law reform. As some of the Legal Realists sought to explore the social science side of the "science of law," not through deductive derivation of legal principles from reading cases, but through more inductive methods of data collection about what happened in courts (beyond the production and elaboration of legal rules), they developed at least two radical projects. One was to use different methods to study legal phenomena (a project which involved both scholarly and curricular challenges to legal knowledge). The other was to use social science instrumentally as a hopeful arrow in the quiver of social change. To the extent that debates about rules of procedure and the role of courts and judges have always been politically contentious, early use of social science was deployed (with all the militaristic connotations such a word invokes) both to "win" particular arguments about particular legal reforms and to claim a new form of argumentative high ground--the "objectivity" of statistics. These themes in the early use of social science in legal process continue today, as we will trace with some representative examples.

Charles Clark, in his empirical work on procedure at Yale, began his study of the operation of the Connecticut civil courts by stating that he was to study "the actual effect of procedural devices on the progress of litigation" (Hutchins, 1927; Clark, 1937). Modeled somewhat on the early Pound-Frankfurter Cleveland Crime Survey, Clark's study was one of simple (not sampled) counting and classification of such items as the frequency of jury trials, use

of motions, defaults, etc. What he found, as John Schlegel aptly puts it, is that with the large number of uncontested and settled matters, the largely “administrative” nature of state court civil litigation had already emerged in urban Connecticut by 1925. Clark was also among the first to study the new “small claims” court, designed to simplify procedures for ordinary people and allow lawsuits without representation, but in the beginning, as now, these courts were actually most often used by companies to collect debts from individuals.

When Clark expanded his Connecticut work through the Wickersham Commission (the National Commission on Law Observance and Enforcement, organized during the Hoover administration, in 1930, to fortify attempts to enforce Prohibition and deal with “the crime problem”), the stated goals of the research (conducted with William O Douglas, then a member of the Yale faculty, later Supreme Court Justice) were to “collect concrete factual, statistical information in order to illustrate and test the efficiency of our rules of procedure and our general methods of administering justice in the federal courts”(Clark, 1934). As a political matter, Herbert Hoover (then Secretary of Commerce, later US President), and others had already begun blaming “court congestion” for general lawlessness, and court and rule reform, directed at reducing delay, were thought to be effective ways to promote law compliance and general domestic order.

The early Clark studies of court statistics illustrate several important aspects of empirical research on courts and procedure that persist to this day. First, they were highly dependent on particular people -- their energy and ambitions, their funding, their research minions, and their political objectives. These early studies, like their later counterparts, could not be conducted by a single scholar as was the norm in traditional legal scholarship (and even in sociological scholarship). Second, even from the beginning, with the difficulty of mounting large funding

sources, governmental, political, and policy interests were behind the research, if not controlling or desiring particular outcomes. Third, the results were often disappointing to the sponsors of the research for failing to completely prove that cumbersome rules or delay in the administration of justice were responsible for some legal or social problem. Indeed, the Wickersham Commission data demonstrated that, as with civil cases, most criminal cases settled easily and early with plea bargains and fines (Ernst, 1997). Fourth, Clark had used a set of completed cases and thus helped establish the methodological norm for future studies of courts (as opposed to the assembly line or mortality model of following a case from filing, until it fell off the docket in some form of termination). Fifth, the work was time consuming, expensive, and demoralizing when the years of work and numbers of cases collected failed to produce desired outcomes. Even before modern academic productivity measurements were used, this was surely a disincentive to continue such large scale-low yield projects.

More modern readers of this early work have been able to mine it for other observations; for example, that so much plea bargaining and settlement might indicate a very efficient system, with accurate charging and selective prosecution--a kind of rough efficient justice. Astute readers have suggested that formal court data might need to be supplemented with data pre-dating any case filings in order to determine how formal case filings do or do not reflect all that could have entered the formal legal system (Clermont and Eisenberg, 1992). Critics at the American Law Institute at the time were reported to have asked for more “interpretation” and fewer facts, (Schlegel, 1995 at 96) suggesting that the tension between norm-oriented lawyers and number crunching social scientists has been with us since the beginning.

Clark as a law professor, Dean, and eventual Reporter and drafter of the Federal Rules of Civil Procedure and then federal judge, was able to use his claim to social science expertise to

build his career and to legitimate reform. When the Wickersham data were finally published by the American Law Institute he noted, among other things, that the most common usage of diversity jurisdiction (another continually contentious issue in American procedural history) was in suits against foreign corporations doing business in the forum state and he urged that such corporations be treated as citizens of the forum state in such circumstances.

Other Legal Realists engaged in early empirical projects about the law (like William O. Douglas' work on failed businesses and bankruptcy), and they too gained stature and influence through these studies. As some of these early scholars took their place on the bench or in the New Deal alphabet agencies, however, their stance shifted from critics armed with social science to legal insiders operating mainly with the usual legal tools. In the Realist era, it is difficult to detect any major changes inspired by the empirical research. The research established and built credentials in law for particular individuals, but law itself was not affected in any substantial way. The legal and policy battles that formed the resultant Federal Rules of Civil Procedure were more the product of bar and legal politics (big firm lawyers vs. smaller state based practitioners) than rigorous discussions of what the data demonstrated. This theme will be repeated often in modern procedural reform (Leubsdorf, 1999).

With the busy legal activity of academics in the New Deal and World War II efforts, there was little further investment in empirical projects on courts until the 1950s. The rising prominence of social science after the war led some important foundations, including the Ford Foundation, the Walter E. Meyer Research Institute, and the Russell Sage Foundation, to invest at least temporarily in blending law and social science. Some of their allies were individuals inspired by or linked to the Realists -- exemplified especially by James Willard Hurst of Wisconsin. Grants to particular law schools allowed small critical masses of researchers at

Berkeley, Chicago, Columbia, Wisconsin, Yale, Northwestern and Denver to study juries (Kalven and Zeisel's *The American Jury* (1966), consumer behavior, criminal law, business practices, environmental law, courts and trial (Skolnik's, *Justice Without Trial* (1966), labor elections, and auto accidents, tort regimes and insurance (Calabresi's, *Costs of Accidents* (1970), Laurence Ross, *Settled Out of Court*, 1980), as well as the first study of lawyer-client relations and their influence on legal outcomes and satisfaction with the legal system (Douglas Rosenthal, *Lawyer-Client: Who's In Charge*, 1974).

One of the most successful of the studies of courts and rules produced by this new burst of empirical energy was Maurice Rosenberg's study of the pre-trial conference rules and practices in New Jersey (1964). Drawing on funds from the Meyer Institute and forming a partnership consistent with the spirit of collaboration of the day, Rosenberg, a civil procedure professor at Columbia, joined with the Columbia University Bureau of Applied Social Research. In what remains one of the classic studies of courts today, Rosenberg found that the pre-trial conference, which was often promoted as a delay diminishing device, actually prevented judges from taking the bench and resolving cases through trial. With the cooperation and assistance of the Chief Justice of New Jersey, Rosenberg had cases assigned experimentally to mandatory pre-trial conference and non-mandatory (lawyer choice) treatment conditions. With the authority of this science, Rosenberg shifted the defense of pretrial conferences away from the idea of saving time and expense. He suggested another defense for the process, namely "improving the litigation process." Although judges who did pre-trial conferences had less time to preside over trials and terminate cases, the pre-trial conference did often turn out to be beneficial in clarifying issues both for trial simplification and for settlement purposes. Following this research, the New Jersey mandatory pre-conference rule was changed according to the study's recommendations.

Rosenberg's findings have been confirmed in a variety of replications (Kakalik et.al. 1996; Menkel-Meadow, 1985).

Kalven and Zeisel's pathbreaking study of the American jury was another product of this era. In another collaboration of lawyer and social scientist, this research demonstrated that judges and juries had agreement rates of about 80% (at least in criminal cases). At a time when the jury system was under attack, the research buttressed the system against charges that juries were poor decision-makers. Modern socio-legal scholars are currently attempting replication of this study (with access to actual juries for study purposes almost impossible to gain), as well as other research designs to study (from real cases, rather than simulated laboratory settings) jury behavior (Vidmer and Hans 2007) in decision making (as compared to judges), and as examples of group decision making on such issues as liability and damage assessment.

The era of interdisciplinary ferment also attracted psychologists who spawned a series of laboratory experiments in process that helped develop the field we now call the "social psychology of procedural justice." This new field can be traced especially to Laurens Walker and John Thibaut's studies of the differences that adversarial and inquisitorial processes make to perceptions of fairness and a series of 'tests' of a variety of different rules and procedures of evidence. More recent work, following from this early work, but looking at actual court settings and other real legal procedures, has explored greater subtleties and differences in processes, ranging from full adjudication and arbitration to mediation and negotiated agreements (Lind et.al., 1990). The procedural justice literature, in very much the same manner as the Rosenberg study, provided a scientific defense of procedural innovations different than the presumption that the reforms saved time and expense. Court-annexed arbitration plans, in particular, gained legitimacy from the finding that litigants who were provided with an opportunity to tell their

stories and receive an authoritative decision were more satisfied with the processes than those without such opportunities, such as when their lawyers settled without them in private negotiations or at pre-trial settlement conferences.

The growing social science interest in courts and procedures in the 1970s culminated in the Civil Litigation Research Project (CLERP), funded by the United States federal government and housed at the University of Wisconsin. CLERP collected extensive federal and state court data in five federal and state jurisdictions from interviews, surveys and other material made available from court officials, lawyers and clients. The CLERP data set provided insights about a number of contested issues about the functioning of the legal system. Some of the remarkable findings were that most cases were handled with low lawyer intensity (very few negotiation interactions, Kritzer, 1991), that there was little discovery in the average state and federal case, (Trubek et. al, 1983; Willging et. al 1998 more recently confirmed this finding), and that lawyers' fees presented very different incentives for lawyer expenditure of activity than might be what clients expected. As with respect to the other general studies of courts, the main finding was that the processes operate in a very mundane and unsurprising manner, and without major problems. Perhaps because the findings fit no group's political agenda, or perhaps because the Reagan administration was much more interested in economic than legal reform, there was once again a relatively long period without major new attention to empirical research on courts. At the time of the CLERP research project, there was hope of permanent funding for a research oriented Institute of Justice in the US Department of Justice (modeled on the National Institutes of Health for medical research) to systematically study justice issues, but such was not to be. (A small research division of the Federal Judicial Center does conduct some empirical research, as

directed by Congress and the federal courts, when there are appropriate allocations of funds and statutory authorization for study of particular issues — see below).

At the same time, however, there were important academic initiatives that were important in the development of social scientific understandings of civil procedure. In particular, then Professor Wayne Brazil conducted empirical studies on the behavior of lawyers and judges in discovery and settlement activity (1978, 1984). While he too found that discovery was mostly an ordinary and unproblematic process, though there were complaints about its “abuse” in some cases, e.g. “big” antitrust and class actions, his studies provided important ammunition in a call for more judicial attention to the discovery process (and his own expertise moved him from academia to a position as a federal magistrate which he held for several decades, becoming a judicial innovator and leader in the alternative dispute resolution movement).

The social reform agenda of the 1960s and 1970s also brought both theoretical and empirical studies relevant to procedural issues. Marc Galanter’s classic “Why the ‘Haves’ Come Out Ahead” article, published in the *Law and Society Review* (1974), suggested a taxonomy of cases and classes of litigants -- principally wealthier and more resource-rich and experienced corporate litigators -- advantaged by their ability as “repeat players” to win cases and control procedural and court reform. At the same time, Galanter’s diagnosis suggested that by changing the endowments of the players, using class actions (procedural reform) and legal services lawyers (access), the less well endowed could be made repeat players too (Kritzer & Silbey, 2003). Research on procedure and its impact on broader social reform issues continued in the 1970s and 1980s. Looking more closely at why litigation did not seem to lead to the social reform that proponents sought (Rosenberg 2009, 2nd ed.), other researchers examined the context of litigation in more detail, focusing on “the transformation of disputes” -- how a dispute is

recognized, framed, and dealt with by different forms of social and legal organizations (Felstiner et.al. 1980).

More recently, linguists (Conley & O’Barr, 1998) and some lawyers (White, 1990) have studied the micro-processes of the use of language and class, race, and gender differences in the system, remarking a version of Galanter’s point that rules are never neutral and objective. Demographic, economic, and other endowments make equal access to, and use of, rules and procedures virtually impossible. Within this tradition, the research of the 1980s tended to demonstrate that rule reforms still perpetuated the absolute power of those with economic, race, gender or other legal super-endowments. Even rules that seemed to be intended to alter the balance of legal power to create new forms of repeat players, like the class action, were derailed by others (including securities lawyers and mass tort defendants who learned how to use the rules to benefit themselves (Coffee, 1995)).

Consistent with the Reagan era emphasis on economic reform, promoting business growth, and shrinking the state, the holders of political power sought to discourage litigation as a tool to bring benefits to disadvantaged groups. Partly in response to the social reform literature about how the courts could be used for social change, conservatives promoted changes in procedural rules to curtail “frivolous” litigation (changes in requirements for verification of facts and sanctions for failing to do so in Rule 11 (Burbank, 1989; Spiegel, 1999 (documenting differential effects of this rule on classes of litigants, such as civil rights plaintiffs) and cost sanctions for failure to accept settlements in Rule 68 (Macklin,1986), and to divert cases out of the courts and into “alternative dispute resolution” fora. There were also procedural reforms by substantive legislation limiting class actions in both securities litigation and prisoner civil rights

litigation, e.g. the Private Securities Litigation Reform Act of 1995 and the Prisoner Litigation Reform Act (Slanger, 2003).

Empirical research has played an important role in the attack on these sets of politically-inspired reforms. Indeed, these rule changes provoked a whole new group of empirical researchers from the traditional legal academy challenged by the new conservatives and forced to resort to empirical research since their political arguments no longer succeeded. Claims that rules were meant to be “neutral” but were believed to be having “disparate impacts” on classes of litigants spurred research on a variety of rule changes. Studies of Rule 11, Rule 68, and the new discovery disclosure rules (Marcus 1993; Mullinex 1994, Garth, 1998) prompted both academic researchers (from within the legal academy) and policy researchers (RAND, and the Federal Judicial Center) to try to demonstrate the actual impact of the rules and whether the reforms “cured” the abuses they were ostensibly designed to remedy or produced other “unintended” effects and distortions. With the political stakes now recognized in debates about civil procedure, investment in social science research increased. Different sides recognized that their positions could be advanced to the extent that they could mobilize the authority of social science on their behalf.

Accordingly, a few distinguished researchers have begun to utilize large data sets to analyze the quantitative patterns revealed by court docket data, now more systematically collected by the Administrative Office of the Courts at the federal level and the National Center for State Courts at the state level in the United States. Among those researchers, Theodore Eisenberg, working with a number of collaborators in different substantive fields of law, has spent the last twenty years studying important empirical trends and patterns in civil rights, bankruptcy and civil actions. (E.g. Eisenberg, 1998, 1990, 1989). This work has demonstrated,

for example, that certain classes of cases fare poorly in the federal system, vary in their treatment before judges, rather than juries, and that those who transfer or remove cases within the federal system do better after transfer from state courts. Further, this work has demonstrated that cases that go to trial and have events recorded on docket entries may not be representative of the full universe of cases. Recently, Eisenberg and his colleagues have documented that foreign litigants are faring quite well in the American courts, even after September 11 (Clermont and Eisenberg, 2006).

As another team of researchers working on litigation and settlement patterns, Kent Syverud and Samuel Gross (1996) have examined the differences in cases settled and litigated and discovered differences in the behavior of different classes of cases. For example, medical malpractice cases are less likely to settle and more likely to go to trial, as long as physicians have reputational, as well as insurance, premiums to worry about as counter-incentives to settle.

Taken together, this body of research gets behind the ideal of “trans-substantive” rules of procedure toward an understanding of how the rules of procedure are used by particular groups at particular times and how even uniform procedural rules may have differential effects in different substantive areas of law (Marcus, 2009).

Specifically commissioned research to look at the empirical data supporting rule change has been employed by other nations as well. Lord Harry Woolf’s multi-year study and report on Access to Justice (1996) in England and Wales drew on the research of socio-legal researcher Hazel Genn (1999) on usage patterns of English courts, data on costs and timing of litigation, and comparative study of litigation practices in the US (for supporting alternative dispute resolution and case management initiatives) and Germany (for supporting court appointed experts and fixed fee schedules). Now that many procedural reforms have been enacted,

researchers and a new generation of judges are busily studying the data on the effects of the “Woolf reforms” of case tracking, judicial case management, recommendations to mediate, and other reforms. The preliminary results are mixed. Judge Rupert Jackson (2009) has reported on the increased costs of “front-loading” cases with increased management devices, even if time to disposition is reduced and more cases are settling, while Professor Dame Hazel Genn (2009) has recently criticized the turn to mediation and settlement (though there is little empirical evidence in England of increased use of court recommended mediation) as a failure of the civil justice system to take its public role of dispute settlement and law making more seriously. Other researchers in the UK are documenting and decrying the decreased allocation of public funds for legal aid, thus decreasing access to the courts at the same time that there are formal legal incentives to attempt out of court settlements. In a loser pays regime, recent case law (see e.g. *Cowl and Others v Plymouth City Council* [2001], EWCA Civ 1935 (parties seeking public money must consider ADR), *Dunnett v. Railtrack plc* [2002] EWCA Civ 2003 (costs not granted to party who refused to mediate) and *Hurst v Leeming* [2001] EWHC 1051 Ch (up to the judge to decide if mediation is appropriate), and *Leicester Circuits Ltd v Coates Brothers plc* [2003] EWCA Civ 290 (withdrawal from mediation contrary to rules of procedure), but *cf. Halsey v Milton Keynes General NHS Trust* [2004] EWCA Civ 576 (holding courts have no power to order mediation, which might arguably violate Article 6 of the Human Rights Act, guaranteeing rights of hearing) has suggested that a party may not claim attorneys fees if it has refused to attempt mediation and legal aid incentives also push toward out of court resolution. To the north, Scotland (Scottish Consumer Council, 2005) commissioned its own separate assessment of whether civil justice reform and rule changes were, in fact, necessary. (Scotland’s legal system

operates separately from England and Wales and is a hybrid of common law and some civil law traditions).

We have reached a point in time where courts, judges, and governments more readily invest in social science research as a response to calls for reform or challenges to existing practices. Recently, for example, some courts have turned to social science as a policy tool in the measurement of discrimination (race and gender bias task forces at both federal and state levels in the United States) and litigation trends for long-range planning, and now-popular state and federal court “Futures Commissions.” Part of that investment in social science comes because the courts themselves can no longer rely on their experience and credibility as judges in order to promote their own agendas, especially in seeking public funds for the judicial branch. With growing sophistication in both measurement technologies and various advocates’ use of social science, data collection and presentation are now themselves often the contested sites of policy conflicts in rule and court reform. (See Part III below).

III. *Procedural Policy Controversies and Data: A Few Examples*

A selected review of more current empirical studies of courts and procedure illustrates how empirical work has recently been deployed in some of these highly contested policy issues - the “litigation explosion” controversy, its opposite and more recent cousin, “the vanishing trial,” (including research on the effects of rule and case law changes encouraging summary proceedings, verification requirements, and incentives or pressures to settle), the use and effectiveness of ADR, and the desirability and effects of discovery reform.

If there is one constant in the contests that have occurred around civil process and litigation (with slightly different versions of the same controversies in criminal law), it is the question of whether there is too much cost and delay and just too much litigation altogether.

Note that these two concerns have a way of appearing to be inconsistent with each other. To the extent that there is too much cost and delay in the system, parties will undoubtedly give up and exit the system (lumping it or avoid disputing (Felstiner, 1975), using self-help or arranging private settlements) or will begin to use the myriad new ways of processing disputes in some form of public or private alternative dispute resolution. If there is too much litigation (meaning the queues to trial are too long), presumably fewer people will likely initiate litigation. In an important insight (as yet empirically untested, as far as we know), George Priest (1989), a socio-legal scholar of an economic bent, has suggested that litigation queues will find an equilibrium point (perhaps at one to two year waits for trial in most matters). If waits for trial are too long, parties will go elsewhere, using ADR or private settlement or dispute avoidance. If ADR, case management, and other efforts to reduce the wait for litigation are effective, then trials will be available at shorter time intervals and the “supply” of trials (courtrooms and judges) will increase and more cases will be attracted back into the system. Delay will ensue (unless more judges are appointed and courtrooms are built), and litigation filings will decline again or be redirected to other ADR or private dispute resolution programs. Thus, according to Priest, litigation will find its equilibrium point. Others have suggested, however, that if the courts and their adjunct institutions appear to be providing efficient and high quality justice, perhaps through a variety of fora, then more satisfied users will be attracted to the system and total “access” to the civil justice system will be increased and one of our leading democratic institutions--the courts--will be deemed more responsive to the populace (Hornby, 1994; Peckham, 1985).

Whether attraction of new and more cases to civil court processing is a good thing or not evokes the “litigation explosion” controversy which has been highly contested for at least two decades. Responding to claims by Warren Burger, Chief Justice of the U.S. Supreme Court, and

other academic commentators that the United States suffered from ‘*hyperlexia*’ (Manning, 1982), several social scientists have attempted to measure and evaluate how much is too much and how different nations’ “litigiousness” should be compared, either to some baseline measure of its own (a “temporal” measure), or to some other comparative baselines with other nations. Important judicial reformers, with ties to research and legal academe, like Judges Robert Peckham, William Schwarzer, and Magistrate Wayne Brazil in the United States, and Lord Harry Woolf in England, have been instrumental in the case management movements in their respective countries, while writing about and commenting on the quantity and quality of litigation in their courts. Meanwhile, academic researchers like Marc Galanter (1983) and a few others (e.g., Genn, 2009; Marvell & Daniels, 1986), have rigorously demonstrated that such questions about how much litigation is appropriate or “too much” or “too little” (Galanter, 2004, “the vanishing trial”) cannot be answered without considering particular social and cultural contexts. As scholars are now drawn to “the vanishing trial” debate it might surprise them to note that Clark and Shulman (1937) noted decreasing trial rates as early as the 1930’s (less than 4% of civil cases filed were tried), though we know those rates later increased, suggesting that trial rates may be more variable over time than suggested by those current scholars who lament the current low trial rates (about 1% of civil cases filed in federal courts) (Galanter 2004).

Scholars have also suggested that there are complex issues in the methods chosen for data collection and comparison in both national and comparative research (where is the baseline to be drawn, what is to be defined as a dispute or formal litigation, when is a lawyer involved, how do we really compare litigation rates among nations, whose dispute resolution systems may be structured differently or are responsive to different cultural imperatives, (Zuckerman, 1999; Galanter, 1983) that can be manipulated for desired policy conclusions. It is not clear that

competing data sets will ultimately resolve this issue, as the question of whether social conflict, directed to courts or court-like institutions, is a productive use of society's resources remains a heavily loaded value question (Reno, 1999; Bok, 1983).

As another take on the litigation explosion, complaints about the conduct of litigation have suggested that lawyers, particularly in discovery, but also in trial and in other settings, have become nastier, more adversarial, less "gentlemanly" and have abused the system of discovery established in 1938 to counteract the 'trial by surprise' regime of older, more conventional, and, certainly cheaper, litigation. While some lay the blame for bad behavior in psychological and cultural terms (a short-fuse, fast paced, less mentored, more aggressive legal culture), others suggest that "scorched earth" and aggressive practices can be explained more by economics (successful class action plaintiffs' lawyers who finance big litigation by seeking "smoking gun" documents in one litigation to be used to finance another, e.g., asbestos to tobacco to guns) or defense lawyers who use scorched earth tactics to beat back litigation altogether with *in terrorem* attacks on plaintiffs of all kinds (Nelson, 1998). While socio-legal scholars can explore the dimensions of whether cultures (either professional or national) have changed or whether the business or economic interests of the profession have become more dominant, the response of legal rule makers has been to enact (partly in response to Wayne Brazil's (1978) excellent studies and articles documenting this behavior) new rules (the automatic disclosure requirements of Rule 26(a) in the United States and pre-trial protocols in England and Wales (by case-type), and increased sanctions for dilatory or unethical behavior (in Rules 26 and 37 in the United States) or denied fees and costs in the UK, in an effort to change this undesirable "overzealous" advocacy.

What a perfect project for socio-legal scholars concerned about process! Scholars (mostly legal and policy, not sociologists or psychologists) have jumped into the arena to debate the effectiveness of rulemaking as a behavioral change agent and have, for the most part, found it wanting (Marcus, 1993). Researchers have documented the remarkable strength and resilience of the adversary system, the larger legal culture in which this activity is embedded, and the professional self-interest of the lawyers who feed it (Sorenson, 1995; Mullinex, 1994). There is no evidence, as of yet, that the allegorical beast of adversariness has been tamed.

Scholars continue to point out, however, that the empirical world of discovery is varied and complex. Single rule fixes may not be appropriate when there are different realities for the big cases which have large discovery volumes and more abuse than in the more modal and smaller cases. In England, Lord Woolf's proposal to track cases in treatment by size and amount in controversy was an effort to provide procedural variations in treatment to different kinds of cases (with perhaps an explicit acknowledgement that transsubstantive or uniform procedural rules will not suffice in this modern age (Marcus, 2010).

Other scholars have explored, whether, as in the Rule 11 (verification and sanction) studies, there are case type differentials in the granting of motions to dismiss or summary judgment, or whether changes in case law, suggesting more permissive rules for granting summary judgments without trials, also are differentially affecting different types of litigants (Kritzer, 1986; Schneider, 2007). As a result of two recent United States Supreme Court cases (*Bell Atlantic v. Twombly* [550 U.S. 544 (2007)] and *Ashcroft v. Iqbal* [556 U.S. (2009)] which seem to require more specificity in pleading (and reversing 50 years of 'notice' pleading, Bone, 2009), there will undoubtedly be studies to see whether these "enhanced pleading" rules

will have disparate impacts on particular categories of ‘disfavored’ cases (e.g. anti-trust and civil rights from which cases these rulings were derived).

Perhaps the most contentious use of social science in law in recent years has been the multi-million dollar effort to evaluate the effects of the Civil Justice Reform Act of 1990. Designed as a Congressional program to tame the ferocious “cost and delay” problem of the United States ‘ federal courts, each federal district court was asked to consider a number of case management devices, such as mandatory settlement conferences, firm trial dates, tracked litigation (different handling for more complex cases) and some forms of ADR. The RAND Corporation was authorized by Congress to evaluate the effectiveness of these different devices and the Federal Judicial Center was given a small budget to examine a group of five demonstration courts that were working on particular projects or programs to be tested.

Several million dollars later, the RAND Report confirmed some of Maurice Rosenberg’s early findings: of the various recipes that were tried, the most effective way to reduce time to trial was to set a firm trial date and to cut off discovery early. Intervention by judges in extensive case management was itself time consuming and expensive (though some evidence suggested that costs of case management to the larger system were offset by savings to the litigants in earlier dispositions and better tried cases). Similar findings on the increased costs of “front-loading” case management and pre-trial procedures, even in a tracked system, are now being confirmed in England and Wales (Jackson, 2009; Genn, 2009). Whether “case management” is effective, both from an efficiency and philosophical perspective, has also become one of the most discussed issues in studies of comparative (particularly civil vs. common law) procedure (Shoenberger 2009).

On the other hand, much to the chagrin of the proponents of ADR, the RAND Reports (Kakalik, et.al, 1996(a) (b) failed to demonstrate any cost or time savings from the use of mediation, arbitration or early neutral evaluation in a number of courts which had pioneered such alternative processes. The proponents were able to find some methodological problems with this evaluation study -- the courts studied were moving targets, some of whom had begun their experiments before the study began, others instituted programs during the study period, and some courts had forms of ADR that the ADR community did not support (Menkel-Meadow, 1997; McEwen & Plapinger, 1997). Some critics suggested that the study itself asked the wrong questions and narrowly focused on the more measurable issues of cost and delay, rather than the more interesting and complex jurisprudential issues of what constitutes a fairer, more “just,” and higher quality result from the legal system. Others suggested arguments untested by the RAND study, for example, that ADR provides more party tailored and Pareto optimal solutions to problems, permits the kind of party participation that procedural justice scholars have told us is so important, and provides a variety of processes tailored to the different structures of different disputes studied by anthropologists and legal scholars (Gulliver, 1979; Fuller, 2001). What was striking, however, was the strength of the attack that came from the ADR community, including the federal judges identified with ADR experiments.

As discussions about whether civil and common law systems are converging or diverging are explored at an ever increasing number of comparative procedural venues (Marcus, 2009; Zuckerman, 1999), issues of common trends or issues include case management and the use of ADR as explored above, whether different fees and costs structures (punitive damages, contingent or conditional fees) affect patterns of court usage (Kritzer, 2009), and whether litigation rates are decreasing (the “vanishing trial” in the US (Galanter, 2004) is alternatively

attributed to ADR, formal and informal pressures to settle, and the growing criminal docket in a time of “three strikes and you’re out” (forced incarceration after three convictions reduces plea bargains and increases court usage for criminal, not civil, trials). Studies of more divergent processes include mapping the use of class actions in the United States and elsewhere (Watson, 2001) and creative uses of technology, both for discovery and for trials themselves. Whether rigorous empirical studies of comparative civil (and criminal) process are possible (Jacob et. al. 1996) in widely varying local, national, and legal cultures remains an open and often contested question, even as new justice systems seek guidance about dispute system design (in informal, formal and sometimes, transitional, Menkel-Meadow, 2009) justice institutions.

IV. Toward A Socio-legal Jurisprudence of Process, Procedure and Courts

Our review of the literature of social science and civil procedure suggests an increase over time in the importance of social science research to debates about civil procedure. The emerging empirical research has developed into a small industry oriented toward the Federal Rules of Civil Procedure in the United States and measurement of rule and process efficiency and comparisons outside of the United States. We have seen a move from a time when Charles Clark used his authority as an empirical student of the courts to produce the Federal Rules of Civil Procedure, and Lord Woolf used similar authority, backed up by empirical study and comparative arguments, to produce major rule change in England and Wales, to one where the rules are constantly being tested through empirical research.

Almost all of the research we have discussed can be characterized as legally driven research. Researchers use empirical research to question or support the need for a particular procedural innovation, whether discovery reform, case management, ADR, fees and costs, or

jury reform. The leading empirical researchers, and even the judges who support and interpret the procedural research, gain stature as leading voices about the courts and dispute resolution.

Social science and social scientists matter increasingly in these policy debates as rule reforms are proposed. At the same time, however, it is useful to reflect more deeply on the process of studying court processes. First, while the value of the empirical research is clearly increasing, there are some timeless patterns in the conduct, engagement with, and use of this work.

The patterns include criticism of the courts for their failure to work effectively, suggesting that they are too expensive, too adversarial, or too slow. Typically the criticism is made on behalf of a group that thinks the courts should serve its needs better, but the criticism is expressed mainly in terms of cost and delay. Responding to the criticisms, reformers suggest changes in the rules that will make the system work better. Since the relevant actors know that any change will serve some groups (including some judges, Macey, 1994 or litigants) more than others, the potential reforms must be cloaked in neutral language, such as efficiency. Then the policy reformers recognize that they can buy some time and potentially mobilize support for their position if they can call for systematic empirical research and hard data. The research, however, rarely provides definitive information in support of or against particular reforms. The courts and the lawyers who appear in them do not change very quickly, and one of the recurring findings is how ordinary most litigation is, even in the federal courts.

From Clark's Connecticut study to the Civil Litigation Research Project to the recent RAND study, we find little evidence of any major problems for most litigation. We find relatively few "runaway" jury verdicts, relatively few examples of discovery abuse, and expenses that are not typically that high in relation to the amount in controversy. Nevertheless, the

empirical studies can usually be used to support some change that is responsive to external criticisms. The external critics are to some extent mollified, and the system proceeds until new rounds of criticism emerge. Improvements in empirical research and the increasing investment in empirical research help to tame this process. Radical criticisms are seen to be overstated and not all reforms gain support.

There is a related process that complements the empirical research on cost and delay. Increasingly, it appears, the actors connected to both federal and state judiciaries compete for innovation and distinction in issues related to court reform, and they often use highly quantitative empirical research as one of their tools. This phenomenon is not new. The study by Maurice Rosenberg was used as a way to justify pretrial hearings, and the pattern of the study is instructive. The evaluation, conducted in terms of cost and delay, revealed findings of no major impacts, and the “data” was used to construct another justification for the reform -- improving the litigation process. Similarly, procedural justice research was used extensively to evaluate court-annexed arbitration in the 1980s. The evaluations were conducted in terms of cost and delay, often under the auspices of the RAND Institute for Civil Justice, but the theories of procedural justice could be used to provide another justification for the innovations -- namely that the parties would be more satisfied with the procedure because they would be permitted to tell their stories to a decision-maker.

The same kind of trajectory can be found for mediation-based alternatives. Again there is considerable innovation in the rules and practices in many systems directing the parties to mediation or other “alternative to trial” modes, followed by empirical study that is inconclusive as to cost and delay reduction, if slightly more fulsome with respect to satisfaction with use. When the results are not compelling in terms of cost and delay reduction, the proponents of the

experiments attack the study and offer alternative justifications based on the data of “user satisfaction”.

The cycle may be continuing. There are jury experiments in Arizona now subject to comprehensive experimental evaluations. While not bound in this instance by concerns of cost and delay, it will nevertheless be interesting to see how the innovators, suggesting new jury processes (such as jury note-taking and allowing jurors to talk during the trial), will deal with inconclusive data conclusions (Diamond and Vidmer, 2001). If trials have not totally vanished, juries certainly have in most of the world. The United States and just a few provinces and states in Canada and Australia continue to use juries in civil cases (the UK and a few others still use them in criminal cases), though some nations (especially those with young legal systems or new constitutional orders see experimentation with juries as a way of enhancing democratic participation in the polity. As Japan, for example, begins to use juries, the social scientists are ready, waiting to study group process, deference, and decision patterns in a society thought to be more homogeneous than many others. Questions of how participation in litigation systems affects democratic participation are important ones, but are fraught with complex methodological, inferential and evidentiary difficulties.

And, as we move from “litigation explosions” to “vanishing trials,” different sets of reformers argue about how much litigation is optimal, both for the parties themselves and for the important public function of generating court decisions and legal precedents for the rest of us. Counting the number of trials does not directly respond to the question of how much “public or private justice is enough?”

What may be surprising is that there is very little attempt in law or in social science to get beneath the formal legal categories to try to understand the changes in procedures and procedural

reform in relation to the changing social role of the courts. If we ask only if mediation works better than litigation, for example, we neglect the way that both processes change over time -- different kinds of cases, different kinds of lawyers, and, above all, different types of mediators. Scorched earth litigation is called the “adversary system,” when in fact it has almost no resemblance to the adversary system of a generation ago. The problems of the courts are considered timeless, requiring solutions which are gradually being improved with new technologies such as ADR, case management and now use of computers in courts. Can data help us answer the question of what kind of processes are optimal for what kinds of disputes?

If we go beneath these categories and the rituals of criticism and reform, we find a number of issues that are difficult to assimilate into those categories. For example, prior to the 1970s and 1980s, large firm litigators were unlikely to appear in the federal courts except on the side of the defense. Big businesses did not sue other big businesses. Now they do. This phenomenon suggests more attention to who uses the federal courts – not just litigants but lawyers. It may make a difference *who* handles particular cases. A leading trial lawyer is selected because he or she will most likely get a larger settlement offer. There are some law firms who are hired for “bet the company” litigation, and we can hypothesize that the tactics and costs of the cases that they get will be different from the tactics and costs of other cases that may, on the surface, seem the same. There are studies of how civil procedure reforms affect different litigants, but not how they may affect different kinds of lawyers and law firms. Again this research is hard to do, since differences among lawyers that are known to litigants and courts are not necessarily readily apparent to researchers.

To return to historical issues that studies of cost and delay tend to miss, class actions enjoyed a period of ascendancy in the 1970s before declining in the 1980s, and moving back up

in the 1990s, but the nature of the class action changed dramatically from civil rights -- with some antitrust -- to tort and securities. Similarly, legal services and public interest lawyers dominated most of the showcase litigation of the 1970s and 1980s, while business lawyers and those who represent plaintiffs in securities and personal injury litigation have dominated in the last fifteen years. We now routinely recognize that litigation is part of “business” used for strategic economic reasons, not simply a matter of a “dispute”. Litigation may be an economic tool to put competitors at a disadvantage. The Justice Department lawsuit against Microsoft, for example, was recognized by many as a fight begun by Netscape, Sun and the companies of the Silicon Valley. These changes over time are often hard to see in quantitative studies, since the categories that are available to sort the cases may not change even if what is meant by the category does.

Judges, who used to make their careers through the quality and quantity of their published opinions, now tend to make their reputations for efficient case processing -- and for innovations in manners of case processing. Leading judges now retire and go private (a growing issue, not only in the United States but in England as well (Genn, 2009), making much more money as private judges for business disputes than they made as public judges. The leading academics in the 1970s could assert that the federal courts only existed to resolve and extend constitutional values and the civil rights litigation that implemented them. Their approaches now seem almost anachronistic -- as do the many studies of inequality in the resources of those who sought to enforce civil rights. The paradigmatic case in the minds of the rule reformers today is a business lawsuit, which may be wasting shareholder assets, rather than rectifying or elaborating public values.

What this suggests is that courts are playing a very different role today than they were a generation ago. It appears obvious also that the phenomena of scorched earth litigation, discovery abuse, alternative dispute resolution, and even cost and delay are related to the “social” and “economic” changes that are external to the courts, but are experienced and problematized as inside the formal justice system. These changes also affect quite dramatically the agenda for reform -- the rise of in-house counsel, for example, relates to the increase in business litigation and then to new criticisms of the courts. The new agenda that responds to the criticisms gets translated into issues of cost and delay, but the agendas for reform are also ones that are designed to respond to the constituencies who are now most prominent in the minds of judges and scholars. When all is said and done, the new cycle of criticism and reform, aided by innovative judges, and social science that stays within the categories of cost and delay, allows the courts to remain relatively the same while repositioning themselves to be more responsive to business concerns. The repositioning moves at the same time away from responding mainly to the concerns that used to be identified with the activist state. Now only a few call for “more active” courts promoting a social agenda (Genn, 2009; Galanter, 2004).

There is very little that social science research and data can do to help resolve these fundamental questions about what purpose courts serve and for whom (dispute resolution for the parties or public law generation for the larger society).

We suggest some useful questions to look at in future research about courts and process by looking at who is doing and who is using that research. One is to look at how the researchers who do empirical research on the courts are rewarded within law, and whether the “price” for that reward (especially when courts themselves are sponsoring the research) is to abandon the theories that come from the social science disciplines themselves in favor of the problems that

respond to and sustain the legitimacy of the courts. Are researchers who bring external perspectives able to do such research and gain recognition in their disciplines or in law? Second, what kinds of activities reward judges, both within the judicial sphere, and in reconverting their capital as judges into other positions -- private judges, politicians, corporate lawyers? Is it correct, as we have asserted here, that judges a generation ago made their careers much more through opinion writing and law generation, than case management?

What civil procedure and the courts are today -- and the role of the social science that attempts to study these issues -- depends on the institutional incentives that surround those whose activities as judges, lawyers, rule reformers, and scholars focus on the courts and their procedures. It would certainly be useful to have more information on the players in the reform of civil procedure. What kinds of cases and clients, for example, do the lawyers and judges active in reform have? How did they get to be the spokespersons, committee members, and rule-drafters on the issues they take up? How do they compare with the spokespersons a generation ago? Whose issues and whose needs are represented in debates about court reform and who chooses the researchers to study them? These kinds of research questions may be more “external” to particular rule reform research in the sense that it derives from theories about institutions, institutional actors, and the forces which produce institutional change. This kind of research, which ranges from rational choice to more historical work, offers some potential to change our understanding of courts and procedures by placing them within an evolving social context. This kind of research could also challenge our models of “pure” research, our beliefs in the fundamental importance of tinkering with procedures for reasons of cost and delay reduction, and the faith in research that simply counts cases or steps in litigation. If empirical research conducted on courts and civil procedure can be self-reflective and ask of itself, for what purposes

is it being conducted and, in whose interests, then perhaps it can do more than be a “pawn” in the ongoing and cyclical policy debates of rule reform. We could perhaps find out what people want of their justice system and how different processes might serve both different people and different causes differently.

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