

OBSERVATIONS ON AN UNCOMFORTABLE RELATIONSHIP: CIVIL PROCEDURE AND EMPIRICAL RESEARCH

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I. INTRODUCTION

The history of the relationship between empirical research and the reform of civil procedure has been one of alternating enthusiasm and disappointment. The natural tendency of the legal community is to demand solutions from empirical research and then to blame the research for failing to resolve the debates that it was—at least in the eyes of the legal community—supposed to resolve. We can hear some of this criticism in reactions to the RAND Report¹ on the implementation of the Civil Justice Reform Act.² The evaluation was probably the single biggest investment in empirical research about civil justice in United States history,³ but in the eyes of some observers, nothing was resolved. The researchers from RAND could respond, of course, that the subjects misbehaved, failing to maintain control groups, refusing to behave in the experimentally-dictated fashion, or simply failing to understand the results. But the real problem is a misunderstanding of the role of empirical research, underscored also by the way that the carefully worded results tended to be turned into polemics for the next round of

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1. JAMES S. KAKALIK ET AL., RAND INSTITUTE FOR CIVIL JUSTICE, JUST, SPEEDY, AND INEXPENSIVE? AN EVALUATION OF JUDICIAL CASE MANAGEMENT UNDER THE CIVIL JUSTICE REFORM ACT (1996) [hereinafter RAND REPORT].

2. Civil Justice Reform Act of 1990, 28 U.S.C. §§ 471-482 (1994).

3. The \$4.5 million for the RAND study compares to the Civil Litigation Research Project (CLRP) at the University of Wisconsin, which was funded at the \$1.2 million level in the late 1970s. For the CLRP data, see HERBERT M. KRITZER, *THE JUSTICE BROKER: LAWYERS AND ORDINARY LITIGATION* (1990). Of course, the Federal Judicial Center and the National Center for State Courts have ongoing programs that require extensive funding, but I am referring here to specific funded research.

debates.⁴ This misunderstanding, I will suggest in this article, is exacerbated by the failure of both researchers and reformers to make clear that the social significance of empirical research is much more complex than is usually presumed.

The RAND study provides an opportunity to cut through some of these knee-jerk reactions. The role of empirical research is growing, admittedly in a somewhat halting fashion,⁵ and there are good reasons for that growth. The key reason, especially in light of the ambiguous history of empirical research, cannot be that lawyers expect the research to end debates and resolve problems. Lawyers are not getting those answers now any more than in the past. One reason has to be that lawyers *need* empirical research more than in the past. The reasons for that need are poorly understood. I will not claim to present systematic research here about that need, but I think some light can be shed on the issue. More specifically, it is important to understand what empirical research is accomplishing and what it can do, and those will be the tasks of this article. Such an examination, moreover, can suggest some possibilities for a more effective use of empirical research not only in the reform of civil justice, but also more generally in explaining and producing the law.

4. Note the controversy described, for example, in *Civil Justice Reform Act Produces Minimal Savings Reports RAND Institute*, 8 WORLD ARB. & MEDIATION REP. 51 (1997).

5. Early empirical research on civil justice, associated with legal realism at Yale and Columbia, is reported in JOHN HENRY SCHLEGEL, *AMERICAN LEGAL REALISM AND EMPIRICAL SOCIAL SCIENCE* (1995). Of particular interest, perhaps, is the work of Charles Clark, who later translated his expertise into the Federal Rules of Civil Procedure. In the late 1950s and 1960s, the realist strain was picked up, with prominent examples HARRY KALVEN, JR. & HANS ZEISEL, *THE AMERICAN JURY* (1966) and MAURICE ROSENBERG, *THE PRETRIAL CONFERENCE AND EFFECTIVE JUSTICE: A CONTROLLED TEST IN PERSONAL INJURY LITIGATION* (1964). The Federal Judicial Center was established in 1967, and it has conducted many studies. See Russell Wheeler, *Empirical Research and the Politics of Judicial Administration: Creating the Federal Judicial Center*, 51 LAW & CONTEMP. PROBS. 31 (1988). The CLRP Project at the University of Wisconsin was funded by the Office for Improvements in the Administration of Justice of the U.S. Department of Justice in 1979. See HERBERT M. KRITZER, *supra* note 3, at 20. An excellent discussion and bibliography is in Vol. 51, Nos. 3 & 4, of LAW & CONTEMP. PROBS. on the subject of "empirical studies of civil procedure." It is notable also that the ALI at the same time drew on empirical research. See Thomas D. Rowe, *American Law Institute Study on Paths to a 'Better Way': Litigation, Alternatives, and Accommodation—Background Paper*, 1989, DUKE L.J. 824.

This Article will be in three parts followed by a brief conclusion. Each part will focus on particular rationales for the encouragement of empirical research about civil justice. The first part will examine the one universally—in the law—“accepted” justification for empirical research. The idea is to use social science methods to see “what works” and what “does not work” in civil justice reform. This view of empirical research, in my opinion, lies beneath the CJRA, and it has much to offer. The problem is that what is gained in the specificity of focus may sometimes be lost by the constraints of agendas defined more by professional ideology than sound research strategy. This research can be quite important, but I want to suggest that the legal profession should try to understand “what works” and “does not work” in this particular approach to empirical research.

The second part will examine a neglected but increasingly crucial reason for empirical research done according to social science standards, namely the “civilizing” of potentially unsettling debates. Part of the task is “getting the question right.” Indeed, the need for empirical research responds to a difficulty of finding a common language or authority in an age where it is no longer enough to speak with the authority of the courts or the bar. Beginning especially with the Realist revolution,⁶ there has been a growing demand for social science information about the impact of laws and legal procedures. Unfortunately, as we shall see, the demand for social science legitimacy does not necessarily lead to a supply of useful and competent research.

The third part will make a different case for social science, insisting on the importance of another kind of empirical research driven not by the specific questions and concerns of law—or legal ideology—but rather by the theories and concerns of social scientific disciplines. Lawyers are only beginning to understand the potential power of research that focuses the best disciplinary tools to explain laws, legal institutions, and legal processes. This kind of research can be frustrating to the legal community, since it asks questions that may not appear to be central to the law. I shall try to show, however, that law has much to gain in legitimacy and in usefulness when it draws on what is best in the disciplines—and the best people in the disciplines. The best

6. See SCHLEGEL, *supra* note 5.

people do not want their research to be framed solely to respond to a lawyer's definition of the problem. Put in terms relevant to the study of civil justice reform, my suggestion is that the best insights about litigation, alternative dispute resolution, judicial behavior, and lawyer behavior—all quite relevant to civil justice reform—may not come from studying costs and delay in the courts. This is not surprising. Doctors know that medicine should be accessible and effective. But we are likely to learn more about the incidence of heart disease requiring medical care by studying behavior out of the doctors' offices than by focusing on medical procedures. Lawyers know that the courts are supposed to be speedy, inexpensive, and fair, but obviously courts exist in a social world that they cannot always—or perhaps even often—control.

II. WHAT WORKS AND WHAT DOES NOT WORK

The easiest justification for empirical research is to measure which of a variety of possible programs or reforms operates best.⁷ The basic idea of the evaluation of the CJRA is to measure the results of the reforms mandated or promoted by that act, allowing us then to select and implement those that were most effective. The criteria are supposed to be those of the Federal Rules of Civil Procedure as set out in Rule 1: the speedy, just, and inexpensive resolution of disputes.⁸ The RAND report takes this mission very seriously, suggesting in the conclusions that the report does indeed have something to say about this very practical agenda.⁹

To repeat what the report says a number of times, early case management can save time but tends to raise costs, an early trial date tends to save money and time, and a relatively early discovery cut off tends to save time.¹⁰ It looks almost as if there is a recipe that can be followed to improve civil justice in

7. See, e.g., Maurice Rosenberg, *The Impact of Procedure-Impact Studies in the Administration of Justice*, 51 LAW & CONTEMP. PROBS. 13 (1988); Laurens Walker, *Perfecting Federal Civil Rules: A Proposal for Restricted Field Experiments*, 51 LAW & CONTEMP. PROBS. 67 (1988).

8. FED. R. CIV. P. 1.

9. See RAND REPORT, *supra* note 1.

10. See *id.*

the federal trial courts. These conclusions, however, are offered rather tentatively, and critics understandably are troubled that their favorite reforms, especially mediation and early neutral evaluation, seem not to have had any significant impact in the favored terms of cost and delay.

There are a number of reasons why it is difficult to draw these kinds of conclusions from the RAND Report. Most of them apply to these kinds of evaluations more generally, and they are therefore worth examining. My hope in explaining these problems is not to criticize the RAND study, but rather to suggest its limitations for these purposes.

A. *Measuring Fairness or Quality*

RAND simply could not make comparative evaluations of fairness or quality.¹¹ Since it is important for the courts to try to please litigants, RAND was instructed to try to sample litigants and assess their views of the processes. Unfortunately, large scale surveys of litigants are doomed to failure for a number of reasons. One is simply that their addresses are sometimes impossible to find in court files, and the costs of tracking down large numbers would be prohibitive.¹² Low response rates are therefore inevitable, meaning that nothing can be done statistically with any reliability with the responses that are available. Surveys of lawyers and judges are also possible, and RAND has sought these opinions of the processes and reforms. A problem here, however, is that lawyers and judges tend to be comfortable with whatever the procedure happens to be. They typically fear innovation, such as mandatory exchange of information without formal discovery, but they tend to find ways to get along.¹³ The

11. See, e.g., John P. Esser, *Evaluations of Dispute Processing: We Do Not Know What We Think and We Do Not Think What We Know*, 66 DENV. U. L. REV. 499 (1989); Marc C. Galanter, *The Quality of Settlements*, J. DISP. RESOL. 55 (1988); David J. Luban, *The Quality of Justice*, 66 DENV. U. L. REV. 381 (1989).

12. It may be that smaller samples with more intensive efforts to produce responses would help provide a better picture of litigants' perspectives, but the survey approach tends to avoid this strategy.

13. The RAND REPORT notes, for example, that lawyers fear the prospect of exchange of documents without discovery requests, but they can live with it without much difficulty. RAND REPORT, *supra* note 1, at 17.

basic conclusion, therefore, is simply one of caution. It is very difficult to find ways to evaluate the quality of dispute resolution systematically.

*B. Control Groups, Random Assignments,
and Selection Biases*

The best way to compare two procedures and to hold everything else constant is to conduct a randomized experiment, assigning one set of cases to one procedure and another set with the same characteristics to another procedure. There have been some splendid studies, including at least one by RAND, that have sought to evaluate alternative dispute resolution by this method. The RAND study of court-annexed arbitration in the central district of North Carolina found that the arbitration did not in fact save total time from filing to disposition, nor could the impact on costs be clearly measured, but that the litigants were in fact more satisfied with a process that enabled them to tell their story to a third party than they were when they merely settled out of court.¹⁴ The experiment was thus evaluated positively, which justified its continuation. Later, "mediation" became more fashionable than "arbitration," however, and North Carolina adopted a somewhat different approach.

The CJRA did not lead to experiments through random assignment,¹⁵ and, therefore, the comparisons were conducted with matched cases that were handled in one fashion or another. One possible problem of the approach taken is that there was almost certainly a "selection bias" problem. Selection bias has become an increasingly recognized problem in evaluation research. The obvious example of selection bias is a job training program that succeeds in placing 90 percent of its graduates in gainful employment. The success rate, however, cannot be extended to the general population. The reason is the selection bias. Success depends in part on the bias of those who enroll or who are selected for the program. Indeed, 90 percent of the

14. E. ALLAN LIND, *ARBITRATING HIGH STAKES CASES: AN EVALUATION OF COURT-ANNEXED ARBITRATION IN A UNITED STATES DISTRICT COURT* (1990).

15. There evidently were some efforts, but the experiments were not truly randomized as it turned out.

group in the program might have been individuals who would have succeeded without the program. Similarly, with respect to the CJRA, the cases that were handled by some procedures may have differed systematically in ways that affected the results. Another problem of selection bias would have occurred if the judges who followed some procedures were judges who, for example, were good generally at moving their cases along. The RAND report therefore points out that the impacts that it found as part of the "recipe for success" are probably the maximum that could be expected.¹⁶ In general, randomized experiments in civil justice are quite rare, and one reason is that there is always the temptation to "cheat" by assigning cases "appropriate" to the experimental track. If the sponsors of the experimental program believe that one approach is better, it is difficult to refrain from using it just because a group of social scientists says so. The result is a very serious selection bias distortion.

C. *The Problem of Snapshots*

The RAND study could not compare "before" and "after" the various reforms listed in the CJRA. Few of them, after all, were real innovations. Active case management, the setting of early trial dates, and discovery cut-offs, have generally been considered "best practices" by federal judges for some time. They have been taught to new judges and encouraged generally since at least the late 1970s. To see what difference these activities made today, the RAND study sought to compare particular cases where one or another of these procedures were followed with those where they were not. As already noted, there is inevitably a selection bias in this method. There is also the problem of how to make genuine comparisons over time. The RAND study was able to examine cases in the period 1990-94, and the study did not find very much difference in the cases that followed one procedure or another. This finding tempts the critics of case management to say that we do not need active case management to expedite litigation. It makes relatively little difference, according to the RAND study. The problem is that we cannot say from

16. RAND REPORT, *supra* note 1, at 9.

the RAND study what difference the *innovation* of active case management made in the federal courts; nor can we say what would happen if judges stopped active management. Case management, defined as much greater judicial attention to the progress of a case file than in an earlier era, now dominates the teaching of judges and their ideal practices, but the changes took place in general over a decade ago.

We can surmise that most lawyers in federal court know today that the general rules of the game have been changed. There is some evidence, indeed, that the change in incentives has helped to redefine litigation practice. It is plausible to imagine that litigators in an earlier period held large portfolios of federal cases and acted sporadically to move one or another case along—increasing their investment from time to time in particular cases in response to perceptions about the best way for the lawyer to maximize his or her total return.¹⁷ Passive judges could simply wait for the cases to arrive ready for trial or to settle, counting mainly on the clients to exert whatever pressure there was on the lawyers. For reasons that need not be explored here, the cases probably came to trial generally in a reasonable time. Judges today can remember distinguished federal judges who would have considered it almost a violation of due process to use judicial pressure to push lawyers to invest time before the lawyers were ready. We cannot say on the basis of statistical data when and whether case management changed the behavior of such lawyers, but I am confident that no lawyer today in federal court feels free of judicial pressure—whether from clerks, court deputies, or a general sense of what is expected today. If this assumption is true, then we would have to say active case management had a very large impact, some of which would be evident statistically, and that the general impact was far greater than the impact that RAND could measure—after the rules of the game had been changed.

On the other hand, if lawyer behavior, client expectations, and general judicial expectations have changed over the past

17. Earl Johnson, Jr., *Lawyers' Choice: A Theoretical Appraisal of Litigation Investment Decisions*, 15 L. & SOC'Y REV. 567 (1981); HERBERT M. KRITZER, *LET'S MAKE A DEAL: UNDERSTANDING THE NEGOTIATION PROCESS IN ORDINARY LITIGATION* (1991).

twenty years, we still do not know what would happen if judges were now to decide to leave lawyers alone again. We could predict change toward the older system of lawyers tending to spend their time in their own interests, which could be conducive to delay and expense, but the patterns of behavior now in place might be slow to change. Or perhaps a somewhat lighter touch today would be enough to keep the system working at a more or less managed pace. The point, to repeat, is that it is difficult to say what works and what does not work without a better sense of change over time than was possible with the CJRA. We need to develop and trace relevant indicators of change in civil justice over time.

D. The Problem of What is Being Measured

Another problem, which can be seen here in two dimensions, is the issue of "what to measure" in determining success or failure of particular reforms. This problem can be conceptual or one of definition. In both cases, one reason for the problem is that the categories and standards of measurement that typically are dictated by the lawyers involved in the processes are not always the ones that would be most useful in understanding what is happening. The measures may not relate to deeper problems and underlying causes.

The first example is simply the question of time. From an outsider's perspective, the most obvious question about referrals to magistrates, special tracks, and alternative dispute resolution would be what they do for expenditure of time by federal judges who make the referrals. If judges refer their cases or aspects of their cases to *someone else*, it ought to have implications for their own time. First is the question of whether there is a savings, and next are questions such as whether judges who make such referrals dispose of fewer cases or if more are terminated by judicial motions, what it means for trials generally, and whether more time is spent by judges in other case-related matters—or even attending more conferences and judicial education. Unfortunately, since lawyers are typically conditioned to ask only how these innovations affect the duration and cost of cases to *litigants*, we tend not to ask this much more intuitively logical—but also more difficult to measure—set of questions about

judicial behavior. The RAND report, perhaps understandably, does not speak to this issue, but we should recognize, nevertheless, that the question is quite relevant to how judges would react to, evaluate, and use these procedures.

The second example is one I have written about elsewhere.¹⁸ There is a natural tendency to think that we can compare mediation and arbitration, for example, by comparing the procedures that go by those names. The problem that empirical research shows is that these processes are changing over time and at any given moment may represent a multitude of very different approaches and procedures.¹⁹ Efforts to try to measure the success of "mediation," therefore, are flawed by the lack of a shared understanding of mediation.²⁰ The conduct of the process, for example, depends on who the mediators are, when they intervene in the process, and what they do to try to produce the settlements. Not only are there variations, but these practical issues about what mediation should be continue to be hotly contested in the literature.²¹ Nearly everyone competing for the business of mediation has a definition of what mediation is supposed to be.²² Until we have standard understandings, and practices that reflect those understandings, it will be difficult to do effective comparisons. This is not only true of mediation and arbitration, but also of such phenomena as "active case management"—which presented difficulties for the RAND study as well.²³

For a variety of reasons, in short, it is difficult to measure what works and what does not in the reform of civil procedure. The effort is definitely worth making, but it is necessary to understand the problems. One lesson is to emphasize the importance of experiments that can control for all the factors except the experimental ones. There are some very successful experimental studies of jurors and jury behavior that use simulations

18. See Yves Dezalay & Bryant G. Garth, *Fussing About the Forum: Categories and Definitions as Stakes in a Professional Competition*, 21 L. & SOC. INQUIRY 285 (1996).

19. See *id.*

20. See *id.*

21. See *id.*

22. See *id.*

23. See RAND REPORT, *supra* note 1, at 7-9.

with real jurors to measure the impact of changes in jury instructions, kinds of expert testimony, and the like;²⁴ but it is harder to succeed once the research is moved outside the laboratory. It is also important to see that the utility of empirical research does not depend on its ability to resolve debates—the implicit hope of the “what works” approach. Empirical data should be collected for other purposes as well, and those purposes may over the long term prove to be more important. We next turn to the purpose of “civilizing debates.”

III. CIVILIZING DEBATES; EMPIRICAL RESEARCH, COMMON LANGUAGES, AND LEGITIMACY

The second justification is that empirical research can help civilize debates by providing a certain common ground, or at least a common language. This justification can be applied to the RAND report and can be used to build understandings that will also make evaluation research easier. I am not arguing here, however, that the common language or civilizing discourse ends controversy. The suggestion is that the controversy is channeled in a slightly different path. Furthermore, in my opinion, this function of empirical research is not merely a suggestion that I or others are making. It is already becoming a necessity. To see this necessity requires a brief discussion of legitimacy in the law.

While difficult to measure precisely, studies of lawyers and the legal profession make it clear that lawyers and judges often act to build the legitimacy of the system in which they operate.²⁵ Leaders of the profession and judiciary know that they must find ways to respond to criticisms that the courts are not operating effectively, that access to justice is rationed unfairly, that criminal justice is discriminatory, or more generally, that particular laws are ineffective or unjust. It may have been possi-

24. See, e.g., Shari Seidman Diamond & Jonathan D. Casper, *Blindfolding the Jury to Verdict Consequences: Damages, Experts, and the Civil Jury*, 26 L. & SOC'Y REV. 513 (1992).

25. See generally YVES DEZALAY & BRYANT G. GARTH, *DEALING IN VIRTUE: INTERNATIONAL COMMERCIAL ARBITRATION AND THE CONSTRUCTION OF A TRANSNATIONAL LEGAL ORDER* (1996) (showing how competition in arbitration is expressed in terms of legitimacy).

ble at one time in the history of American law to respond to such criticisms by stating simply that precedents or natural law dictated what the law was, and it did not matter what the consequences were. After the Realist revolution in the 1930s, it is no longer sufficient—if it ever was—for lawyers simply to assert their authority in defense of particular laws or practices. They must show by legitimate argument what they are doing and how they are succeeding. What can be said in a “legitimate argument” is changing over time. Anyone who has paid attention to the evolution of law over the past two decades, for example, knows that legal reforms will have to stand up against the arguments of the “law and economics” proponents.²⁶ Increasingly, I think, legitimate argument today also requires that “data” be examined, if available. Put in a different way, one way to gain an advantage in an argument is to produce data that support the particular position. And when one side produces data, the other side needs data as well.

Skeptics will assert that data are too often ignored or distorted for political ends and that political power is what really matters. I do not mean to downplay the importance of political power. Political actors, however, must fight their battles in legitimate terms, which means now that they must take into account or distinguish credible and legitimate evidence. How the evidence is used is not always a pretty sight, but the cumulative effect of battles fought in terms of evidence is to make the evidence gain importance. The empirical evidence is more important today than in the past in the reform of civil procedure. One of the reasons is that this terrain has become more “political,” meaning that it is not easy to find a clear consensus without the help of social science.

A few examples may illustrate the importance of finding a civilized discourse and common language for potentially contentious debates. Contests about civil procedural reform, for example, can be fought in the abstractions of Congressional power versus the independence of the judiciary, but those big abstractions provide only a recipe for conflict. Both sides need to justify their positions in terms of what is best in order for the courts to

26. See, e.g., Richard A. Posner, *An Economic Approach to Legal Procedure and Judicial Administration*, 2 J. LEGAL STUD. 399 (1973).

fulfill their functions in dispute resolution and in the production of law. The RAND report, seen from this perspective, helps to build a certain consensus about what can and cannot be said. For example, no one after the RAND report can say with any credibility that case management today is radically changing litigation for better or worse; nor that litigants are particularly upset about any recent changes in judicial behavior; nor that ADR is detested by litigants or lawyers; nor that ADR as practiced dramatically saves litigant time and money. The large-scale surveys can pick up dramatic changes, and clearly, there were none. Those who take particular positions with respect to reforms proposed as fundamental changes—for example, certain enthusiasts of ADR—must think of arguments in support of their positions that are consistent with the basic findings of the RAND report.

The debates about products liability, punitive damages, medical malpractice, and jury verdicts, similarly, are extremely political in nature. At one level, they are about insurance companies looking to save money and personal injury lawyers looking to make money; but even these debates cannot ignore the empirical data that has been growing steadily on these issues.²⁷ Once Marc Galanter, for example, demonstrated that the \$300 billion price tag put on the tort system was an invention based on pure speculation, proponents of tort reform had to argue in other terms.²⁸ Despite the usefulness of the empirical data and its admitted relevance, it proves to be difficult to domesticate these debates, for the simple reason that there is so little reliable data on which to ground the debates.

In the criminal justice arena, in contrast to that of civil justice, we now have victimization surveys and reliable crime statistics. We know if crime is increasing or decreasing nationally and in particular localities, and that knowledge prevents a politics of pure anecdote and fear. These data do not, of course, prevent politicians from saying that there is too much crime or

27. See, e.g., STEPHEN DANIELS & JOANNE MARTIN, *CIVIL JURIES AND THE POLITICS OF REFORM* (1995); NEIL VIDMAR, *MEDICAL MALPRACTICE AND THE AMERICAN JURY* (1995).

28. See Marc Galanter, *Real World Torts; An Antidote to Anecdote*, 55 MD. L. REV. 1093 (1996); Marc Galanter, *News from Nowhere: The Debased Debate on Civil Justice*, 71 DENV. U. L. REV. 77 (1993).

that the judges are too lenient, or, less often, that too many young people are in prison; but they help to shape the debate in very important respects. As we have argued elsewhere, however, we have relatively little reliable data over time on the civil justice system on such matters as the costs of civil justice, alternative dispute resolution, or even relative increases or decreases in particular kinds of cases in the state and federal courts.²⁹

One potentially useful result of the RAND study is that it will provide some baseline data that can be used in the future to measure change. Unfortunately, however, there is no way to guarantee that research undertaken in the future will facilitate accurate measures of change. The last major governmentally-sponsored study of civil litigation, the \$1.2 million Civil Litigation Research Project (CLRP), which began in 1978, has not been replicated. The RAND study, however, did at least pay attention to the CLRP data and make possible some comparisons.³⁰ The CLRP data, which covered state and federal courts, found especially that most litigation was rather small, with a median amount in controversy in state courts of only \$4500, excluding small claims courts; and the federal median was only \$15,000.³¹ The median amount of work in the federal cases by attorneys was reported as forty-five hours, and in 49% of the cases there was no discovery at all.³² If there was discovery, then about 24% of the lawyer time was devoted to the conduct of discovery.³³ This information, serving as a baseline, should not be forgotten.

The RAND study will allow some comparisons over time. In particular, we will know after further analysis how much time is devoted to discovery and how many cases have no discovery. The number of hours worked in the typical federal case of the RAND study appears to be more than sixty, and it will be important to see what the figure is and what its components are.³⁴ Obviously, the median amount in controversy has increased with the in-

29. See Marc Galanter et al., *How to Improve Civil Justice Policy*, 77 JUDICATURE 185 (1994).

30. RAND REPORT, *supra* note 1, at 309-310.

31. See KRITZER, *supra* note 3, at 29-31; RAND REPORT, *supra* note 1, at 310.

32. KRITZER, *supra* note 3, at 80-88.

33. *Id.* at 90-93.

34. See RAND REPORT, *supra* note 1.

crease in jurisdictional amounts in federal courts, but it may also be the case that there is more business litigation with higher amounts in controversy. It should be obvious that some of these indicators of change, over time, will be quite helpful in determining how our civil justice system is changing. It may be that case management or adversarialism or an increase in the stakes has led to more lawyer investment in each case that goes to federal court. Comparing differently designed studies is not the best way to measure change over time, but it can be helpful and make up for some of the glaring inadequacies in our general knowledge of the changing civil justice system.

As empirical research increases in importance in debates about the civil justice system, it is doubly important to find ways of generating reliable information. There are, of course, major institutions charged with monitoring the federal and state courts; and they have conducted numerous important studies.³⁵ Their missions, however, have been defined more in relation to the immediate needs and interests of their constituencies than the interest in finding ways to track change over time systematically. These and other institutions should be given the resources to build reliable indicators of change in the civil justice system. More generally, this role of empirical research should be recognized explicitly and encouraged. The legal profession can obviously be much more effective in promoting reform (and in succeeding) when backed by legitimate empirical research (even if that research is not definitive).

IV. UNDERSTANDING AND UPDATING THE LAW

This third dimension of empirical research about the law is more difficult to explain and requires a rather different understanding of the role of social science. Not everyone will agree that the kind of social science that is referred to in this section is even relevant to the law. The argument here is that research *around* the law is essential to the law.³⁶ The basic argument, which will be elaborated below, is again that the legitimacy of law depends on more than simply the authority of judges, law-

35. See *id.*; *supra* note 5.

36. See RICHARD A. POSNER, *OVERCOMING LAW* 91-102 (1995).

yers, and legislators. The law must be seen to conform to public opinion and above all, to expert opinion, which helps to mold public opinion. Put another way, the law must constantly import from disciplines around the law in order to stay up-to-date. Economic analyses of law framed in the language of neo-classical economics, for example, developed long before there was any real demand for them by courts and legislatures.³⁷ Indeed, the authors of economic analyses often disclaimed that their research was even supposed to be relevant.³⁸ The authors meant only to understand the law in economic terms. It turned out, however, that this economic research became quite relevant when the economy changed and required new laws and new understandings of the role of law in the state and the economy.³⁹

More generally, the proposition here is that efforts to understand the law *in social science terms* are not only useful for what they tell us about the law, but also useful in keeping the law up to date. When it appears necessary for the law to change in response to internal (within legal institutions) or external (economic crises, etc.) pressures, the knowledge brought to bear by social science research can insure that the change is consistent with the best and most legitimate social scientific arguments and approaches available. Those social science arguments and approaches also change over time through changes in the academy and changes outside that affect what goes on in the academy.

I will give three examples of research areas in the social science disciplines—exemplified here by psychology, economics, and sociology—that can and indeed already are bringing new insights to law. The methods, as will be seen, are very different, involving experimental studies, large data bases, and systematic qualitative research. What is common to each method is that the questions to be examined about the law come mainly from the social science, not the law. From one perspective, that grounding in social science arguably moves the research away from principal legal concerns. For example, it may shift the attention from

37. MARC ALLEN EISNER, *ANTITRUST AND THE TRIUMPH OF ECONOMICS: INSTITUTIONS, EXPERTISE, AND POLICY CHANGE* 48 (1991).

38. Cf. POSNER, *supra* note 36, at 97 (making such a claim).

39. William M. Landes & Richard A. Posner, *The Influence of Economics on Law: A Quantitative Study*, 36 J.L. & ECON. 385, 385-87 (1993).

the concerns of cost and delay found so prominently in the RAND report. From another perspective, however, this approach from the social sciences avoids potential blinders that come from research that fits only a legal perspective—or even the particular ideological needs of the legal profession and the judiciary. That is not to say that the social sciences have no blind spots of their own, nor that they will necessarily turn out to be relevant and useful. For many reasons, it is difficult to match the best in social science with law, and it does not often happen simply as a matter of course. But, as the examples suggest, there are some powerful tools in social science that can be quite helpful to the law. It is important to understand and encourage their deployment.

A. *Psychology and Procedural Justice*

Experimental psychologists in the 1970s, working initially with law trained academics, began a line of research that became identified as “procedural justice.”⁴⁰ The question that began to dominate the research from a relatively early period was whether individuals cared more about procedural fairness or substantive outcomes in evaluating how their disputes were resolved.⁴¹ Most of the research was done using students in laboratory experiments, which is typical of how psychologists test their hypotheses.⁴² Allan Lind, who moved from the Federal Judicial Center to Illinois to RAND to the American Bar Foundation (ABF), and Tom Tyler, who was at the ABF and Northwestern before he moved to Berkeley, are the names most closely identified with this line of research.

Their book, *The Social Psychology of Procedural Justice*, published in 1988,⁴³ represents an especially important contribution to the literature. What they showed in a variety of settings was that disputants generally tend to care more about the procedures than the outcomes.⁴⁴ In particular, disputants grant

40. E. ALLAN LIND & TOM R. TYLER, *THE SOCIAL PSYCHOLOGY OF PROCEDURAL JUSTICE* 7-8 (1988).

41. *Id.* at 7.

42. *Id.*

43. *Id.*

44. *Id.*

legitimacy to processes that allow them to tell their stories to a respected, neutral third party.⁴⁵ The opportunity to tell the story was important, they suggested, not only because it made the parties more likely to be satisfied with the process, but also because it meant they were more likely to obey the law.⁴⁶ This research, as I hope I have shown, can be summarized in quite understandable terms, but it was written mainly for psychology journals and employed many of the technical terms found in psychology. Findings that became accepted within psychology, as it turned out, became useful to the law—in part because of their credibility as good psychology.

Neither Lind, nor Tyler, nor others who worked in this tradition, were oblivious to the potential importance of these findings for the law and legal procedures,⁴⁷ and a number of RAND evaluations⁴⁸ in particular began to use the findings of procedural justice in evaluations of real world procedural innovations. There are many examples of these evaluations.⁴⁹ The main points, as it turned out, were the findings that procedural justice research supported court-sponsored programs in mediation and arbitration, and that the research argued against the fairness of settlements that were negotiated without any opportunity for the litigants to tell their story to an impartial third party.⁵⁰ More dramatically, the findings of procedural justice research legitimated programs that could not be defended on the basis that they saved time and money.⁵¹ Proponents who had promised savings of time and money changed their tune.⁵² They began to argue instead that the new procedural mechanisms were important to litigants and the legitimacy of the legal system not

45. LIND & TYLER, *supra* note 40, at 13-16.

46. TOM R. TYLER, *WHY PEOPLE OBEY THE LAW: PROCEDURAL JUSTICE, LEGITIMACY AND COMPLIANCE* (1990).

47. *See, e.g.*, E. Allan Lind et al., *In the Eye of the Beholder: Tort Litigants' Evaluations of Their Experiences in the Civil Justice System*, 24 L. & SOC'Y REV. 953 (1990).

48. *See, e.g.*, MACCOUN ET AL., *ALTERNATIVE ADJUDICATION: AN EVALUATION OF THE NEW JERSEY AUTOMOBILE ARBITRATION PROGRAM* (1988).

49. *See also* LIND, *supra* note 14.

50. LIND ET AL., *supra* note 47, at 961.

51. *Id.* at 984.

52. Carrie Menkel-Meadow, *Dispute Resolution Begets Disputes of Its Own: Conflicts Among Dispute Professionals*, 44 UCLA L. REV. 1871, 1928-29 (1997).

so much because they saved time and money.⁵³ They based their observations instead on the findings of the psychology of procedural justice.⁵⁴

The change in the nature of the debate described in the preceding paragraph could be characterized as opportunistic or even illegitimate. When the promised savings in time and money did not appear, the entrepreneurs of ADR shifted to another strategy.⁵⁵ From my perspective, however, the process was perfectly legitimate. The new arguments served to strengthen the law and legal procedures for the resolution of conflict by bringing those procedures in tune with sophisticated social science research. The pressures for reform, indeed, while expressed typically as concerns about time and expense, may have come from elsewhere; and it makes sense that the potential "solutions" were in part imported from empirical social science. The imported definition of the problem was in this instance better suited to its resolution than the definition from within the law—expressed in the usual terms of time and expense.

B. The New Empirical Economics

The predominant approach in economics over the past several decades has been intensely mathematical and theoretical, but computer technology has advanced to the point where sophisticated empirical research and analysis have regained a key role in economic analysis. The tools of this analysis have not been directed to litigation and the courts to any great extent, but I think it is useful to describe the developments in economics as they relate to my more general point. There is much to be gained, I contend, from empirical research that is defined and generated from within economics rather than strictly from within law.

Scholars within the tradition of theoretical economics have suggested that anti-discrimination laws cannot be helpful to minorities for a simple theoretical reason: it would be economically "irrational" to discriminate on the basis of race, gender or

53. *Id.*

54. *Id.*

55. *Id.*

ethnicity.⁵⁶ Businesses would naturally hire the best talent for the lowest price, and that means that any discrimination would be self-correcting through the labor market.⁵⁷ Taking these theoretical assumptions seriously, but subjecting them to rigorous testing, a new generation of empirical economists has creatively developed approaches to measure the impact of law. More precisely, they have found ways to measure all other possible impacts, then leaving law as the "unexplained variance."⁵⁸ While it is impossible to measure precisely the contribution of law by focusing on law, the focus on what is more easily measured produces a very powerful demonstration of the impact that the law must have had. In this case, economics scholars Jim Heckman and John Donohue, who is also law trained, have demonstrated that the Civil Rights Act of 1964 clearly had an impact on the wages of African Americans—at least in the South.⁵⁹

The number of economists with these skills and interests is growing, and we can anticipate, hopefully, that they will seek to understand some of the large questions of the costs and benefits of legal regulation. Since litigation has been one of the key components of regulation in products liability, health care, securities, antitrust, trade, and a host of other areas, in addition, of course, to civil rights, we can hope that economists will be able to find measures of the determinants of business investment in law, the rewards of that investment, and the costs associated with it. One of the underlying themes of many of the debates about tort reform, for example, is the idea that business spends too much money on law and litigation—which then translates to pressure on the courts expressed, quite often, in terms of time and expense of litigation. The larger questions of cost and benefit of legal investments, which are most relevant to economists, may be much more important than the smaller question of the

56. James J. Heckman & Brook S. Payner, *Determining the Impact of Federal Antidiscrimination Policy on the Economic Status of Blacks: A Study of South Carolina*, 79 AM. ECON. REV. 138, 139 (1979).

57. *E.g.*, RICHARD ALLEN EPSTEIN, *FORBIDDEN GROUNDS: THE CASE AGAINST EMPLOYMENT DISCRIMINATION LAWS* (1992).

58. *E.g.*, Heckman & Payner, *supra* note 56; John J. Donohue III, *Advocacy Versus Analysis in Assessing Employment Discrimination Law*, 44 STAN. L. REV. 1583 (1992).

59. *See, e.g.*, John J. Donohue III, *Employment Discrimination Law in Perspective: Three Concepts of Equality*, 92 MICH. L. REV. 2583, 2583 (1994).

efficiency of the courts.

The same kind of analysis can be made with respect to contract enforcement. Enforcement through the use of litigation or other legal approaches can be assessed, measured, and compared to other approaches over time. Again, the position of the courts and civil litigation can in this manner be situated in a much larger empirical literature about the behavior of businesses and how they invest in different activities. Unfortunately, to mention a theme I shall return to below, it is not always easy to turn the best social scientists to the subject of law, even if they define the research questions according to their own disciplinary tools and approaches. This problem is as true of psychology as of economics, and, in fact, the example from psychology was one of the relatively few examples of successes.⁶⁰

C. The New Institutionalism in Sociology and Political Science

The third example comes from sociology and to some extent political science, with echoes also in economics.⁶¹ The basic idea of this "new institutionalism" is that the activities of individuals must be related to the institutions in which they operate.⁶² The institutions, furthermore, must be understood as frameworks that operate according to a certain logic, and that logic relates to what provides the basic legitimacy of the institution. That is to say, the behavior of individuals can best be explained not simply as the result of the rational behavior of "economic man," but rather as the result of people acting reasonably according to the incentives and opportunities that are presented to them at specific times and places.⁶³ Economic markets are themselves institutions that must be created, legitimated, and maintained.⁶⁴

60. Jury studies are another example. See Diamond & Casper, *supra* note 24.

61. DOUGLAS C. NORTH, INSTITUTIONS, INSTITUTIONAL CHANGE AND ECONOMIC PERFORMANCE (1990) (North won the Nobel Prize in economics for reemphasizing institutions). In political science see for example, DEBORAH J. BARROW ET AL., THE FEDERAL JUDICIARY AND INSTITUTIONAL CHANGE (1996).

62. See, e.g., THE NEW INSTITUTIONALISM IN ORGANIZATIONAL ANALYSIS (Walter W. Powell & Paul J. DiMaggio eds., 1991) [hereinafter NEW INSTITUTIONALISM].

63. *Id.*

64. See, e.g., THE SOCIOLOGY OF ECONOMIC LIFE (Mark Granovetter & Richard

The logic of institutions, moreover, cannot always be understood by studying what is produced by the institutions for their own legitimacy.

Academic institutions, for example, produce brochures that celebrate the ideals of the pursuit of knowledge for its own sake; yet we know that even pure natural scientists must find a way to obtain research funds and tenure, and that they compete for prizes and for status in the academic community. The reception of their work, in addition, will depend on what is fashionable at a particular time in a particular discipline—nature as opposed to nurture, for example—and also on what is going on outside the academy. In order to understand the choices that an individual scientist makes at a particular stage of a career, it is much more useful, according to the new institutionalists, to understand the “field” in which the scientist operates than it is to focus only on the legitimating ideal of the quest for truth.⁶⁵ It is also necessary to understand that the legitimating ideal is not irrelevant or just a smokescreen for self-interest. Advancement in science depends on the ability of the individual to show that his or her “strategic”⁶⁶ decisions are accountable to the norms of pure science—including the quest for pure knowledge.

Sociologists of institutions have begun to see the relevance of these approaches to the law and legal institutions,⁶⁷ but we so far have very little of such work on the courts or on litigation. My own recent work is related to this approach, and I will refer briefly to some of what I have been studying; but the main concern here is to suggest that much can be gained by building on what is happening in social science as social science.

We can also return to the judiciary and recall an earlier theme. I suggested in the first part of this Article that it might have been useful if the RAND study had been able to see the impact of ADR devices on the time spent by individual judges

Swedberg eds., 1992).

65. NEW INSTITUTIONALISM, *supra* note 62.

66. “Strategic” here means choices made in relation to opportunities provided in a particular field at a particular time. It does not mean that the motive, for example, was necessarily self-interested.

67. See Mark C. Suchman & Lauren B. Edelman, *Legal Rational Myths: The New Institutionalism and the Law and Society Tradition*, 21 L. & SOC. INQUIRY 903 (1996).

who refer cases to ADR.⁶⁸ My suggestion was based on a hypothesis that this information might have some explanatory power. I would now like to relate it to more systematic research that might be undertaken.

Judge Posner has written a chapter asking, "What do judges maximize?"⁶⁹ which raises a relevant question, but does not answer it completely. He speculates as to the answer, but he does not step out of his role and its legitimating ideology.⁷⁰ While he explores what may affect and shape the individual career strategies of judges, he does not ask how that might be changing.⁷¹ He focuses on opinion-writing, which is a declining part of the judicial role.⁷² A good institutional account of the judiciary and litigation, needless to say, would go well beyond an investigation of the problems of cost and delay and how they have changed over time.⁷³ The account must be both of changes that are internal to the courts and rules of procedure and to events that are external in the worlds of clients, the government, and the economy.

Within the courts and the judiciary, of course, the general rhetoric of legitimation has not changed substantially—nor should it. The ideal of the Federal Rules of Civil Procedure is an inexpensive, speedy, and just resolution of disputes brought to the federal courts.⁷⁴ But if we take this ideal and ask what it meant in 1965 and then compare that to its meaning in 1995, it appears in practice to be very different. Without systematic research of a kind that has not yet been undertaken, I will suggest a few hypotheses based on conversations with a relatively small number of judges and others. In the institutional setting of the early 1960s, federal judges were passive, letting the lawyers and litigants control the processes not as a matter of judicial laziness, but rather as a fundamental canon of fairness.⁷⁵

68. See *supra* p. 111.

69. POSNER, *supra* note 36, at 109-44.

70. *Id.*

71. *Id.*

72. *Id.*

73. While helpful, the focus of BARROW ET AL., *supra* note 61, is also too narrow, focusing on the politics generally of the judiciary.

74. FED. R. CIV. P. 1.

75. See Judith Resnik, *Managerial Judges*, 96 HARV. L. REV. 374, 376, 397-98 (1982) (noting that the American legal system has embraced a classical view of the

Judges were not supposed to descend to the level of the parties and lawyers. We may speculate that, since there were fewer federal cases and fewer federal judges, the institutional structure for ambitious judges fueled judicial investment in writing and publishing legal opinions. Even though most cases of course settled, judges could gain the respect of their peers by writing well-reasoned opinions either to decide cases by motion or to resolve them when cases were tried to the court.

The passage of the Speedy Trial Act in 1974,⁷⁶ which put pressure on civil cases and civil trials, coupled with the increase in civil cases in the 1960s and 1970s,⁷⁷ appears to have changed the institutional structure of the federal courts and accordingly the incentives and opportunities available to individual judges. Without trying to provide details on a story that has yet to be written, I suspect that it became more difficult to make a career as an author of leading opinions; it became more important to manage the caseload; and it became more important to find ways to resolve disputes that threatened to go to trial. A judge who wanted to be an effective and respected member of the federal judiciary now faced different incentives. The "best"—and most respected—trial judges now were those who could handle their dockets and bring cases to a close, and many of the most famous judges became not the authors of great opinions but rather the leaders of new devices for resolving dispute—early neutral evaluations, court-annexed arbitrations and mediations, summary jury trials, mini-trials, and the like.⁷⁸

As we shall see, the factors that produced these pressures and transformations did not relate solely to changes within the courts. Some scholars suggest that judges simply decided for their own reasons that case management should be undertaken, but my suspicion, based in part on my own clerkship at the district court level with Chief Judge Robert Peckham in 1978-79,

judicial role where judges are disengaged as a matter of fairness, not as a matter of laziness as some commentators have suggested).

76. 18 U.S.C. § 3161 (1994).

77. Lawyer behavior was also changing to become more "litigious." See DEZALAY & GARTH, *supra* note 25, at 100-113.

78. One example of change is that federal magistrates are now sometimes promoted to positions in the federal district court as judges, suggesting the importance of managerial skills. For the change to managerialism see Resnik, *supra* note 75.

was that the judges who were committed to passivity as a matter of principle found that they increasingly had problems getting cases to come to an end. The judicial entrepreneurs of case management as an idea were then taken much more seriously, and they ultimately carried the day. Conscientious and ambitious federal judges today, I suggest, now behave rather differently than they did twenty years ago.

The story is, of course, much more complicated. A focus only on the internal story is especially misleading, since it implies that the efficiency of the courts depends only on factors that are within the control of the courts. This idea has the virtue of conforming to the traditional lawyers' way of thinking about the courts, again consistent with the relentless focus on delay and expense, but it misses the external factors that can have a huge impact on the role of the courts, their workload, and how they are used. The role of the federal government from the New Deal and after obviously had a huge impact on the courts, and I would suggest that the changes in the global economy after the oil crisis in 1973 had an equally large impact, promoting mergers and acquisitions and a general reorganization of business. Not incidentally, there was also a dramatic change in the quantity and quality of business litigation. Once litigation became a weapon in business competition,⁷⁹ it is not surprising that the courts felt an enormous pressure in terms of discovery battles, contested motions, and adversarialism in general.

I would hypothesize further that the much greater use of the courts by business helped change the institutional incentives of judges and the courts more generally. In contrast to the earlier period of legal opinions and grand legal principles, especially involving federal law, the story today is of a legitimacy that is based—at least in the eyes of corporate litigants, who represent a very powerful group—on the efficient resolution of business litigation. Again, from a sociological perspective different from the usual views of the courts promoted by lawyers and judges, we can suggest that “litigation” today means something rather different than it did twenty years ago. If this image has indeed changed, it would not be surprising to see that the incentives and approaches of judges in relation to their cases will also

79. See DEZALAY & GARTH, *supra* note 25, at 100-113.

change.

To go further, it is relatively easy to see today that the federal courts are part of a market—both public and private—that is in many respects competing for business litigation. With judges now under close scrutiny by businesses with powerful friends in the Congress, it is even less surprising that empirical research has become more relevant to judicial policy debates. In an era of scarce resources and corporate downsizing, judges cannot assert that they are the best judges of what is necessary for the judiciary to function effectively. Continuing a trend that has been set for several decades, the judiciary now needs empirical research to justify its claims for resources and deference.

An institutional study that identified and explained the activities of federal judges within their own worlds of litigants, clerks, and other judges would provide both a better basis for court reform and a better legitimation of what, in fact, is done in the courts. Instead of battling over whether judges have done enough in terms of expense and delay, we would understand the constraints that operate today and what hardworking judges are doing to make the system operate effectively in the eyes of its constituents. I am not sure the judiciary is ready for this kind of sociological scrutiny, but I predict that it will come before long. Transparency can be both a source of reform and a powerful legitimator of that reform.

More generally, sociological studies of institutions provide ways of asking very different questions than those that typically are framed by lawyers and law professors. These studies also provide means of getting beyond the rhetoric that may be used to justify positions that are taken by the various players. To repeat a theme that I have already stated several times, we can only gain limited insights from research that starts with the questions of cost and delay and ends with the same questions. In part, it may be useful to explain why the debate is framed in such narrow terms, but I believe it makes perfect sense in terms of the legitimacy of the legal system. Beyond that observation, however the broader social science concern is with elaborating the reasons for *effects* that might finally be measured in terms of cost and delay. Those reasons depend on the institutional structures and sets of incentives that operate within the world of the courts, and they depend also on the external factors that help to

explain the demand for particular services of courts. The reasons for the external pressures also must be studied, and indeed they may be more easily controlled as a matter of policy than factors that are internal to the courts. The best way to save judicial cost and time, in fact, may be to look at incentives for behavior outside the courts.

*D. Research that Responds to Social Science Concerns:
A Preliminary Conclusion*

The general argument of this section is that it is essential to make a place for the application of the best in social science to legal issues and concerns. I have found a few examples of successes and some places where I think that social science has much to offer. The social science approaches often raise very different questions than those that would be raised by lawyers. They force attention to factors that might get lost in research shaped mainly by lawyers. And finally, they can help us understand why the lawyers' questions are framed so narrowly. I am arguing, in part, for pure social science applied to law, but the situation is more complex than simply favoring social science purity. The biases of social science today in the disciplines that I have mentioned lead them to address the core questions of *the disciplines* rather than questions that might be of some relevance to law. It is important, therefore, to find ways to direct the best in social science to legal concerns. This channeling of talent and method can provide lasting insights about lawyers, judges, courts, and litigation. Unfortunately, this kind of research remains all too rare.

At another level, moreover, I am arguing that the legitimacy of the law depends on its ability to use and incorporate the best of social science. Law must be legitimate not only to lawyers, but also to the other disciplines that study social phenomena. If legal procedures are out of touch with accepted social science trends, they can be delegitimated and questioned. The position of law in American society depends on its ability to stay abreast of developments that are taking place around the law. The best way to do that, which is not always easy, is to encourage social science to take on law and legal institutions, make them as transparent as possible, and then allow reformers to build on

the best possible explanations of how the system is working. That means, to put it bluntly, that the researchers must get beyond the narrow concerns that tend to motivate lawyers and social science that is ruled by lawyers.

This proposition should not be as controversial as it might seem at first impression. The adaptation of law to the new economic reality in the 1980s was the result in part of a relentless attack by economic theorists on those who, they said, were using the law for a purpose other than economic efficiency. Seeking to defend the law consistent with what became accepted economic orthodoxy, lawyers in effect assimilated the attack and made sure that the law responded to the criticisms. Law remained up to date and legitimate in an era rather different from the activist state as envisioned in the 1960s.

My argument now is that, even in economics, the empirical component has become essential. The strength of law will depend on its ability to withstand close empirical scrutiny and, similarly, to assimilate the findings of legitimate and accepted social science. In order to accomplish this assimilation, it is vital to find ways for law and lawyers to stay abreast of what is best in the social sciences. That also means that lawyers must, from time to time, let the theories and approaches of leading social scientists take precedence over the way that lawyers would like to define the problem and the scope of the research. Needless to say, even if this proposition can be stated and accepted as a matter of theory, it is bound to be very difficult in practice. There is professional competition involving very strong egos and points of view. At best, the relationship between law and social science is going to be uneasy. Tension and conflict, however, may be exactly what the law needs. New knowledge does not depend on the motives or personalities of the researchers.

V. A CONCLUSION IN FAVOR OF PLURALISM

I have seen many grant proposals where the investigators begin by saying that, surprisingly, there has been very little empirical research about this or that topic in the law. Such a statement betrays ignorance about both law and social science. It would be much more accurate to be surprised whenever there has been significant empirical research on a particular legal

topic. With a few notable exceptions, it has been in almost no one's interests to do and to fund such research. The RAND study and the circumstances that produced it suggest that the situation is finally beginning to change beyond the relatively sporadic commitments in the past.

The next step in using and encouraging productive empirical research is to redefine the justification for that research. There is, of course, overlap in the three justifications that I have given in this article. There is a tendency, however, to place too much emphasis on empirical research as mainly a device for the evaluation of solutions to particular problems. One result of that tendency is to raise false expectations and then produce disappointment that is more often than not unjustified. Another pernicious result is to allow the promoters of the research to ignore the costs of parochial research strategies that often result when lawyers dominate the shaping of research questions. Once we realize the importance of empirical research in understanding, updating, and legitimating the law, however, we have still only gone part way. We should also appreciate the importance of finding ways to encourage high quality research. Despite the demand for empirical research in debates over civil justice, the incentives in place do not tend to encourage the best in social science to meet that demand.

The federal funding of the RAND study is a major step forward for many reasons. One is that it provides some interesting material that can be used to evaluate civil justice reforms. More important, in my opinion, are other aspects revealed by the RAND study—not least of which is the difficulty of evaluation without experiments that can be monitored to control selection bias problems. Beyond revealing problems, the RAND study is an opportunity to emphasize the importance of collecting data over time that will enhance the function that I described as “civilizing debates.” Finally, and perhaps more difficult for most lawyers to see, I suggest that we need more empirical research that draws less on lawyer definitions of the problem—e.g., cost and delay in the courts—and more on broader social science theories and methods. These theories and methods can offer much toward a better understanding of how law operates and what operates on law—what produces legal change, and what legal change produces.

