Reflections on the Use of Empirical Research in Legal Policy Reform

Deborah R Hensler*

My topic today is the use of empirical research to shape legal and other public policy reform. Like many other socio-legal analysts, I first became attracted to conducting empirical research on the law because of what I saw as the promise of such research: the chance to shed light on citizens' legal needs and desires, and to better understand the consequences of legal rules and court programs. I believed that by bringing fact-based, non-ideologically-driven research to bear on legal issues I could contribute in a modest way to improvements in the legal system.[1] In my more cheerful moments, I still find myself believing in that promise, and I continue to find the research process itself enormously interesting and fulfilling. But after more years as a legal policy analyst than I choose to confess, my expectations about the impact of empirical research have diminished — perhaps, some would say they have become more reasonable. At the same time, I have become increasingly concerned about the potential for misuse of empirical research for legal policy reform and reform of other social policies. Today, whenever I reflect on the use of empirical research for public policy reform, I think not just about its promise but also about its perils and pitfalls. My reflections are based on my experience as a legal policy analyst in the United States, but all of us who conduct empirical research on legal policy face these same issues to a greater or lesser degree, regardless of the political context in which we work.

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* Judge John W Ford Professor of Dispute Resolution, Stanford Law School. My reflections on the use of empirical research in legal policy reform draw mainly on my experience as a public policy analyst at the Rand Institute for Civil Justice. However, the views expressed herein are my own and not those of Rand or its sponsors.

1 Whether socio-legal analysis is, can, or ought to be non-ideologically-driven are highly contested questions, which I touch on below.

I begin these reflections by specifying how I define 'empirical research,' since that term is understood differently by different people. Then I briefly describe five examples of socio-legal studies conducted in the US that I think display both the promises and perils of policy-oriented empirical research. I close by summarizing the lessons that I draw from these examples.

A Brief Definition of Empirical Research

Some academic and government analysts define 'empirical research' to mean quantitative research: statistical analyses based on 'hard data' comparing means and medians, determining whether differences are statistically significant or modeling causal relationships using multivariate techniques. I include these approaches to describing and explaining social phenomena under the rubric of 'empirical research'. But I also use the term to cover attitude surveys, case studies and qualitative data that are not susceptible to such statistical analyses. What distinguishes empirical research from other research, in my view, is that it attempts to draw conclusions from facts, rather than from formal deductive models or normative analysis. Empirical research often benefits from the use of quantitative data and analyses, but it is not — despite what some of my economist friends think — *defined* by the use of statistical data and econometric analyses.

Perhaps more controversially, I subscribe to the old-fashioned notion that empirical research can and should be objective, meaning that the researcher strives to find out what is actually going on, what people truly believe, how a program really does operate and what its consequences are. I understand and accept that none of us can be truly objective and that all of our research, from start to finish, is framed by our own values. I also accept that there are many truths about social behavior and that any one research project will, at best, be able to uncover one or few of these. But I distinguish empirical researchers who strive to minimize the effects of their own values, and who test their conclusions against multiple observations, methods and analytic approaches, from researchers who begin with answers and search for and report only those data that support those answers. Indeed, I think the feature that most distinguishes socio-legal studies from more traditional legal research is that the best socio-legal empirical researchers start with questions and search for evidence that sheds light on those questions, rather than starting with a legal position and searching for evidence to support it.

The Promise of Empirical Socio-Legal Studies

The promise of empirical socio-legal research, then, is that it can help us to more accurately measure legal knowledge, attitudes, and needs, and to better understand legal behavior and legal outcomes. In my view, empirical research tools can be applied in many different societies and cultures, although the specific theoretical frameworks and methods that are appropriate will likely vary across these societies and cultures. How to conduct empirical research well, how to report it in a fashion understandable to non-researchers, and how to translate its implications for decision-makers are key challenges for empirical socio-legal researchers who seek to inform legal policy. How well researchers meet these challenges depends critically on both their competence and their integrity. But researchers' ability to meet these challenges is often constrained by the nature of the legal policy problems the researchers are attempting to address, the political environment in which the research is performed and — sometimes — the integrity of *other* researchers.

Socio-Legal Research in Action

To illustrate these latter constraints, I will discuss five examples of socio-legal research conducted in the United States spanning more than two decades: the Civil Litigation Research Project ('CLRP') conducted by researchers at the University of Wisconsin; procedural justice research on alternative dispute resolution conducted by researchers at various institutions; the Civil Justice Reform Act ('CJRA') evaluation conducted by the Rand Institute for Civil Justice; a study of class actions in the United States that I conducted while I was still at Rand; and a set of analyses of the effects of gun-legalisation laws in the United States, conducted by my Stanford colleague John Donahue and others. These projects by no means represent a random sample of policy-oriented socio-legal empirical research; I have chosen them because I know something about them and because they illustrate different issues.[2]

The Civil Litigation Research Project ('CLRP') was a large-scale survey data collection and analysis project aimed at understanding the factors that lead people to make legal claims, how they pursue these claims and what the outcomes of litigation are.[3] It was conducted in ten state and federal trial courts in the US during the late 1970s. CLRP was not the first empirical research on civil litigation to be conducted in the US — one can find examples of empirical studies dating back at least to the early 1900s. But CLRP was the first consciously socio-legal study — the first empirical project, to my knowledge, that derived its approach from a theory of the claiming process ('naming, blaming and claiming'[4]) and the first to attempt to investigate the sociological underpinnings of liti-

2 In the space allotted, I obviously cannot do justice to the depth or breadth of these studies. I have included footnotes to the major publications associated with each study, for readers who are interested in learning more.

3 David Trubek et al, *Civil Litigation Research Project Final Report* (1983).

4 See William L F Felstiner et al, 'The Emergence and Transformation of Disputes: Naming, Blaming and Claiming' (1980–81) 15 *Law & Society Review* 631.

gation behaviour. It also may have been the first empirical research on civil litigation behaviour funded by the executive branch of the United States government.

CLRP was an ambitious project and its ambitions almost did it in. The researchers were not well versed then in the sociological methods they sought to apply and they seemingly were unprepared for the realities of court record data: how hard it is to access case records, how incomplete court records are, etc.[5] The survey data collection process was more arduous and expensive than the researchers expected and so time consuming that by the time the project was complete, a new administration in Washington had eliminated the office that funded the study, leaving no one in government who was interested in learning the study's findings. The burdens of the project also sorely tested the collegiality of the team of law professors and social scientists who had banded together to design and conduct the study and their commitment to the project. Had it not been for the perseverance and intellectual creativity of the study team, the results might have sunk from academic notice as well as policy notice. Instead, through the efforts of people like Herbert Kritzer and Marc Galanter, the key findings of the study shaped a generation of socio-legal scholarship not just in the United States but in other countries as well. For socio-legal scholars, CLRP established that theory-driven sociological research on legal behavior is both feasible and fruitful. Despite this success, the Civil Litigation Research Project — funded by a US Department of Justice that for a brief time only was persuaded that empirical research on litigation behavior would prove useful for policy-making — had little effect on civil legal policy.

Procedural justice research on court-connected alternative dispute resolution ('ADR') provides a second example of applying socio-legal theory to litigation behavior. Procedural justice theory was developed by social psychologists working in conventional laboratory settings, using college students as experimental subjects.[6] Based on their experimental research, procedural justice theorists argued that people evaluate dispute resolution procedures not just by looking to the outcomes they obtain but also by assessing the fairness of the process accorded them. When psychologists and others tested these theories with real litigants and real dispute resolution procedures (trials, non-binding court-administered arbitration, and negotiated settlements) and found that the laboratory findings held up in the real world, some lawyers and judges were astounded. Although they themselves were concerned about due process, many lawyers and judges assumed that ordinary litigants cared

5 For a discussion of the project's methodological challenges, see Deborah Hensler, 'Surveying the Litigation Landscape: The Civil Litigation Research Project' (1987) 51 *Public Opinion Quarterly* 571.

6 For a review of procedural justice research conducted in the laboratory and in field settings, see Tom Tyler and E Allan Lind, 'Procedural Justice' in Joseph Sanders and V Lee Hamilton (eds), *Handbook of Justice and Research in Law* (2001) 65.

only about winning or losing. Judges were also surprised to learn that lay people thought trials and non-binding arbitration procedures (which excluded juries and jettisoned some evidentiary protections) were equally fair and thought settlement negotiations — from which lay people were generally excluded — were relatively unfair.[7]

Early studies of court-connected non-binding arbitration programs finding that these programs satisfied lay people's notions of what constitutes fair process helped to power the court ADR movement in the US.[8] In some jurisdictions, the research findings stimulated discussions of about the role of ADR in civil case management. In others, judges and lawyers reached out to researchers for assistance in designing ADR programs. The courts' interest in learning and using the research results was critical, because the research relied on experimental designs, which required courts to randomly assign cases to ADR and non-ADR tracks. Although using such experimental designs seemed only natural to psychologists, the idea of conducting experiments in the context of a legal battle engendered serious debate about the potential for impairing due process.[9] Because the researchers and judges saw themselves as joint venturers, they were able to meet this challenge in a thoughtful and ultimately successful fashion.

But civil proceduralists in the legal academy who feared that substituting ADR for formal trial procedures would erode due process protections, and left-leaning socio-legal scholars who saw the ADR movement as an attempt by conservatives to foist 'second-class justice' on the poor, decried the research that yielded these procedural justice findings, terming them simply 'consumer satisfaction surveys.' It is perhaps telling that most of these critics were not themselves familiar with the psychological research that had preceded the ADR studies; nor did they go to great pains to learn more about that research, which lay outside their own disciplines. Ironically, the notion that courts that had adopted ADR procedures had done so on the arguably fragile basis of consumer satisfaction surveys was accepted by other courts, which then seemingly decided that as long as instituting new ADR programs did not result in widespread dissatisfaction they need inquire no further about the consequences of their new programs. In most of these courts, there was little discussion of whether the new programs were procedurally fair or what the court ought to do to ensure procedural fairness.

Over time, increasing numbers of state legislatures in the US and increasing numbers of courts mandated court ADR programs, ordering

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7 See E Allan Lind et al, 'In the Eye of the Beholder: Tort Litigants' Evaluations of Their Experiences in the Civil Justice System' (1990) 24 *Law & Society Review* 953.

8 See, eg Jane Adler et al, *Simple Justice: How Litigants Fare in the Pittsburgh Court Arbitration Program* (1983); Robert MacCoun, 'Unintended Consequences of Court Arbitration: A Cautionary Tale from New Jersey' (1991) 14 *Justice System Journal* 229.

9 For a discussion of the legal issues raised by randomised experimentation in court, see Federal Judicial Centre, *Experimentation in the Law: Report of the Federal Judicial Centre Advisory Committee on Experimentation in the Law* (1981).

litigants to attempt to resolve their cases through ADR before agreeing to schedule cases for trial. Money was appropriated to establish and run programs, judges selected to oversee them, and administrators hired to manage them. Many questions about the programs' consequences remained, including whether they in fact saved time and money – the main objective of the legislatures and courts that were mandating ADR. While the procedural fairness researchers had found fairness gains as a result of the new ADR programs they did not find any measurable differences in costs or time to disposition for cases that were assigned to ADR and cases that were not. But the leaders and administrators of the newly institutionalized programs had decreasing interest in conducting empirical research on the results of ADR. The twin notions that ADR saves time and money and that litigants like ADR had become well established 'truths' that few had an interest in challenging.[10]

Rand's evaluation of the Civil Justice Reform Act ('CJRA') provides my third example of the use of socio-legal research for policy purposes. The CJRA was adopted in 1990 by the US Congress to improve the efficiency of civil litigation in the federal courts, primarily by requiring active judicial case management. Although the CJRA arose out of a very particular political context in the US, it helped to stimulate civil procedural reforms in a number of other countries, most notably in the UK, where Lord Woolf incorporated some of the CJRA's central tenets in his reform program.[11]

A large segment of the federal judges in the US objected to Congress's intrusion into the realm of civil case management, and as a result of the jockeying for power between Congress and the judiciary, a sunset provision was included in the Act. To inform the decision as to whether or not the Act's provisions should be made permanent, Congress funded a full-scale evaluation of the outcomes of the CJRA, and a contract was awarded to Rand to conduct the evaluation.[12]

One provision of the Civil Justice Reform Act encouraged the adoption of court ADR programs. As a result, part of the evaluation addressed the consequences of federal court ADR programs. Building on the procedural justice researchers' success in mounting field experiments in court settings, the entire evaluation project, including the ADR component, relied on an experimental research design. Building in part on the CLRP researchers' experiences, which the CLRP researchers had documented in detail, the Rand researchers mounted an extensive data collection project, using both court and survey data.[13]

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10 For a discussion of changing views on the utility of ADR program evaluations, see Deborah Hensler, 'ADR Research at the Crossroads' (2000) 2000 *Journal of Dispute Resolution* 71.

11 See Right Honourable Lord Woolf, *Access to Justice: Final Report to the Lord Chancellor on the civil justice system in England and Wales* (1996).

12 See Jeffrey Connaughton, 'Judicial Accountability and the CJRA' (1997) 49 *Alabama Law Review* 251. Rand is the oldest think-tank in the United Sates. Unlike many newer think tanks, it is not aligned politically.

13 For a discussion of the study design, see James Kakalik et al, *An Evaluation of Judicial Case Management Under the Civil Justice Act* (1996).

By the time the CJRA evaluation was undertaken, many courts had abandoned their earlier court-connected non-binding arbitration programs in favor of mediation. These mediation programs were strongly supported by many in the bar, who were offering their services as paid mediators. (The arbitrators who served in earlier court programs were also lawyers but, by law, they served largely pro bono, for modest honoraria. The legislation that authorized court-mandated mediation programs did not impose any limits on mediators' fees.) These lawyer-mediators looked to court mandates requiring parties to mediate cases to build markets for their services.[14]

Rand's evaluation of the new mediation programs found that these programs had little effect on the time or expense required to resolve civil lawsuits. These findings were consistent with earlier findings on court-connected non-binding arbitration programs. Unfortunately, the research design did not allow for painstaking interviews with litigants, so the fairness questions that were so provocatively studied in the earlier research did not receive their full due in this study. The Rand researchers carefully caveated their findings on time to disposition and expense, noting that the findings applied only to the small number of courts that had mediated sufficient numbers of cases to allow for an experimental research design and analysis. In the time-honored tradition of researchers, Rand called for more research to determine whether their findings would hold in other courts and over time.[15]

Rand's findings infuriated the by-now considerable constituency promoting mediation. The mediation community feared that the findings would be used to justify cutting federal government support for mediation. Rather than joining in Rand's call for more research, mediation advocates called for Congress to authorize expansion of federal court ADR programs. Advocates' arguments were strengthened by the support they received from some in the academic community who were both mediation scholars and mediation practitioners.[16] While these scholar-practitioners agreed that more research was needed, they urged Congress to authorize expansion of federal court mandates for mediation. Interestingly, at no time did scholars urge expansion of federal mandates for less remunerative non-binding arbitration programs.

Rand also found that the judicial case management that was at the core of the CJRA program had little effect on civil litigation time and expense. Like the mediation advocates, judicial case management advocates

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14 For discussion of the evolution of court ADR in the US, see Deborah Hensler, 'Our Courts, Ourselves: How the Alternative Dispute Resolution Movement Is Reshaping Our Legal System' (2003) 108 *Pennsylvania State Law Review* 165.

15 See James Kakalik et al, *An Evaluation of Mediation and Early Neutral Evaluation Under the Civil Justice Reform Act* (1996).

16 For a discussion of the controversy over Rand's findings, with citations to critiques, see Deborah Hensler, 'In Search of 'Good Mediation': Rhetoric, Practice and Empiricism' in Joseph Sanders and V Lee Hamilton (eds), *Handbook of Justice and Research in Law* (2001) 231.

— among them some leading judges — were appalled by the findings. But with most federal judges still chafing at Congress' intrusion into the judicial management domain, there were no calls for Congress to disregard Rand's findings on judicial case management. The CJRA was quietly allowed to sunset; only its ADR provisions live on to this day.

My fourth example is drawn from my own research: a project on class action litigation in the US that I completed a few years ago.[17] I hesitated to include this in my talk today for fear of appearing immodest but I learned so much about conducting policy-oriented socio-legal research from the study that I think it is worth sharing. Unlike all of the studies I've have described so far, the class action study relied primarily on case studies and qualitative research methods. I had begun the study expecting to apply a far more quantitative 'cost-benefit' analysis. My adoption of the case study method derived in part from the lack of available statistical data on this litigation: although US courts now account for civil case filings separately from criminal cases and many courts distinguish different types of civil cases, no state court reports the frequency of class actions, and at the time of my study the federal data on class actions had been determined to be unreliable by the Federal Judicial Center (the research arm of the US federal courts). But I chose to conduct case studies of class actions primarily because my preliminary exploratory research convinced me that it is the ways in which these cases are litigated that is the primary subject of controversy in the US and quantitative research techniques, while good for counting and correlating characteristics of phenomena, to my mind are not very good for investigating processes, particularly highly contextualised processes.

Because of resource constraints, my study investigated only ten class action suits. Such a small number cannot purport to be a statistically representative sample of the universe of class actions. But even if my collaborators and I had been able to investigate a larger number of suits, we would not have been able to select them randomly because there is no list of all the class actions pending or resolved in the US In relying on so small a number of cases, the study team was susceptible to charges that it had selected cases to prove either that class actions were good or bad. But we were ultimately able to persuade readers of our study that we had not selected the cases so as to bias the outcomes by describing in great detail how we chose the cases for study and by reporting what occurred in the suits as even-handedly as possible and with great attention to their complexity. Indeed, not only had we not chosen the cases to showcase good or bad class actions, to this day I myself cannot point to any of the cases as wholly good or wholly bad — although many readers have been happy to do so.

The case studies persuaded me that assessing the costs and benefits

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[17] See Deborah Hensler et al, *Class Action Dilemmas: Pursuing Public Goals for Private Gain* (2000).

of US-style class actions is a well-nigh impossible task not just because it is extremely difficult to account for all the direct and indirect costs and benefits, but also because defining what is a 'cost' and what a 'benefit' is a normative task, coloured by one's judgments about the proper balance among market forces, regulatory processes and private law. Because the debate over proposed reforms to the US class action rule centres on different views of costs and benefits, and no consensus on these has emerged to date, I argued in the papers and book I wrote on the study that focusing on whether to eliminate or sharply cut back class actions was likely to paralyze reform efforts. Instead, I urged rule-makers to focus on the incentives in the class action process that lead to self-dealing and to adopt rules to restrain if not eliminate these incentives. The case studies provided vivid examples of such self-dealing — displaying the incentives for self-dealing in sometimes gory detail — and also showed judges trying to restrain self-interested behavior. By showing how some judges managed to rein in the potential for abuse in actual cases, the case studies illustrated how rule changes might improve the litigation process. On December 1, 2003, amended rules that in part reflect the research findings became effective in federal courts. A parallel reform effort in the Congress to cut back certain types of class actions entirely has stalled.

For my final example, I turn briefly to a highly contentious debate in the US arising from research on the results of gun-carrying laws. Although some states in the US have adopted various gun control laws, a significant number of states have made it easier for their citizens to carry concealed weapons. Several years ago, an economist named John Lott published a book titled 'More Guns, Less Crime',[18] claiming that such gun-carrying laws reduce crime. Lott used a variety of data, but his analysis mainly relied on multivariate econometric modeling of state and county level crime trends. Controlling for other factors shown to affect crime rates, he found that when states had pro-gun-carrying laws, crime rates dropped. Not surprisingly his results — which he has since expanded upon in a second book[19] — were enthusiastically taken up by pro-gun groups in the US[20] and have been widely cited by opponents of increased gun control in Congress and state legislatures.[21]

A number of other law professors and criminologists, including John Donahue, a Stanford colleague of mine, became interested in Lott's analysis, which seemed counter-intuitive to them. I think most of these

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[18] John Lott, *More Guns, Less Crime: Understanding Crime and Gun-Control Laws* (1998).

[19] John Lott, *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong* (2003). The publisher of this monograph, Regnery, describes itself as 'the leading conservative publisher in America.'

[20] See eg, Paul Gallant and Joann Eisen, Review of John Lott's book, *The Bias Against Guns: Why Almost Everything You've Heard About Gun Control Is Wrong* (2003) National Rifle Association — Institute for Legislative Action <www.nraila.org> at 1 July 2004.

[21] For citations to legislators' and other officials' endorsements of gun-carry legislation that rely on Dr Lott's analyses, see Ian Ayres and John Donohue, 'Shooting Down the "More Guns, Less Crime" Hypothesis' (2003) 55 *Stanford Law Review* 1193.

academicians would also admit to favoring gun control themselves, so I assume their interest was also piqued by their ideological dispositions. These researchers have taken various approaches to testing Lott's findings including extending his trend data over a longer period of time, specifying the econometric model differently and investigating Lott's coding of his data. The resulting econometric analyses are difficult for all but econometricians to follow. Suffice it to say, my colleague's analyses do not support the original findings.[22]

In a less contentious domain the story might end there: two or more camps of academicians researching the same topic, each purporting to have used the 'right' data and the 'right' analytic method, find different results and continue to snipe at each other in a long-running series of journal articles. And to some extent that is what has happened here. But Lott is now a fellow of the American Enterprise Institute (AEI), a prominent right-oriented think-tank that wields significant influence with the Republican administration in Washington. Although some AEI scholars reportedly have raised concerns about the quality of Lott's scholarship with the think-tank's leadership, Lott has become a high-profile commentator on a wide variety of social policy issues, frequently sought after by the media.[23] And notwithstanding the methodological questions that have been raised about his research, Lott's 'more guns, less crime' proposition continues to be cited in the political arena by opponents of gun control.

Lessons

There are many lessons we might draw from these five stories and I am sure that these lessons would differ at least somewhat depending on each of our disciplinary, methodological, and political perspectives. I'll close by identifying five lessons that are important to me.

One lesson is the importance of theory-driven research. One does not need to argue this point to most academic audiences, but theory — except perhaps for economic theory — receives short-shrift in much policy research. It was the theoretical perspectives that underlay the Civil Litigation Research Project and the procedural justice-based ADR research that gave that research its value and allowed it to live on, in applications of the theory to new domains and in work by new generations of scholars. To some extent, it is the lack of theory about what explains crime trends that leaves scholars in the gun control debate arguing about technical aspects of econometric modeling and makes it more difficult

22 A host of researchers have conducted analyses of the 'more guns, less crime' hypothesis. Some find evidence supporting it and some, like Ayres and Donahue do not. For references to the extensive empirical analytic literature, see Ayres & Donohue, above n 21.

23 See Benjamin Wallace-Wells, 'In the Tank: The Intellectual Decline of AEI' (2003) (December) *Washington Monthly* 24.

for disinterested observers who are not well versed in those techniques to assess the inconsistent findings of different studies.

A second lesson that is perhaps obvious to this audience is the importance of method. But method is important not just for reasons of reliability and validity that we all are familiar with. Method is also important because it determines the resources and time that are required to complete a good study and whether the policy-maker audience for the study will have the skills required to understand the results. Policy debates — and policymakers — move on from one issue to another, seemingly at an ever-quickening pace. If it takes five years to carry out, the ideal study may leave the researcher in the position of speaking her lines on an empty stage, after the audience has left. Few policymakers — and fewer still legal policymakers — are sophisticated quantitative analysts. Complex statistical models, when unaccompanied by other qualitative evidence, are difficult for most policymakers to take in and easy for them to dismiss, particularly when the results are subjected to attack by interested parties. The solution is not to conduct only quick studies or only case studies. Rather, researchers need to think through carefully the implications of adopting certain methodologies and devise ways for addressing the challenges these methodologies will pose. One can conduct long-term studies that make a policy difference, if one builds in regular interim reporting to policy-makers. One can use complex statistical analyses effectively, if one strives to present the results in a form intelligible to those who are not statisticians or — better yet — if one combines these analyses with other methods that are more intelligible to non-statisticians.

A third perhaps obvious lesson is the importance of sharing our work with each other, as you are doing at this conference. But we need to share not only our results, but also the boring details about how we reached those results. We need to share not only our successful methods and our statistically significant findings, but also our failures and our null results. There is little in the academic world to encourage such sharing of negative experiences and outcomes and quite a bit to discourage it. But we need to overcome these barriers if we are not to repeat each other's mistakes and are to move forward. The null results of experiments contrasting ADR with traditional litigation are very important for policy — although perhaps less so for basic research.

A fourth lesson that I draw from the research projects I have described is the importance of separating our roles as scholars and researchers from our roles as policy advocates and, in the legal academy, from our roles as practitioners. As I indicated earlier, I know that the idea of role separation is contested in this post-modern world. But what modest value empirical research has in the policy world lies in policymakers' belief that researchers speak a kind of truth, tinged though it may be with the researchers' personal values and political perspectives. When research is simply advocacy, its reason for being — and, on a practical note, the reason for funding it — is called into question. When every policy 'expert' is simply asserting her political opinion, dressed up with a few facts and figures, then empirical data themselves fall into disrepute. In the policy arena, the post-modern notion that there is no factual truth carries significant risk. To my mind, a world in which we rely solely on our instincts and beliefs — and ignore the evidence to the contrary — is a dangerous world indeed.

Finally, the empirical studies I have described and my experiences with many other such studies teach me the need to be modest about the promise of policy-oriented empirical research. I do continue to believe that empirical research can make a difference for policy. But no amount of empirical data can counteract the power of social legends. Marc Galanter and I can write forever about the fallacy that Americans are hyper-litigious, pointing to our various studies;[24] the notion is firmly engrained in American culture and will likely outlast both Marc and me. Once programs become entrenched in public bureaucracies or in the marketplace — as has occurred with ADR in the US — it will be difficult for program evaluators bearing negative findings to find a hearing. When policy is almost wholly ideologically driven — as on the issue of gun control in the US — it is unlikely that non-ideologically driven researchers will hold much sway. Those who labor in these vexed areas need hold on to the belief that someday, when bureaucratic or market imperatives fade, or when the tides of political opinion shift, a policymaker somewhere will take those old empirical studies off the shelves, dust them off, and have another look.

24 See, eg Marc Galanter, 'The Day After the Litigation Explosion' (1986) 46 *Maryland Law Review* 1; Deborah Hensler et al, *Compensation for Accidental Injuries in the United States* (1991).