TRIAL BY JURY OR JUDGE: TRANSCENDING EMPIRICISM

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INTRODUCTION

Pity the civil jury, seen by some as the sickest organ of a sick system. Yet the jury has always been controversial.[1] In America even before 1800, New York canal builders, perceiving excessive jury awards, sought and secured legislation to avoid jury determination of land values.[2] In the nineteenth century, several states, consciously promoting railroad growth, restricted the jury right.[3] Today, jury trials are the hot focus of much law reform debate, as reformers suspicious of juries repeatedly urge policymakers to modify the right to jury trial. Tort critics catalogue problems with juries.[4] Businesses resist antidiscrimination legislation that would grant jury trials, claiming it would subject more businesses to the lawsuit "lottery."[5]

One might suppose that, with so much at stake for so long, we would all know a lot about the ways juries differ from judges in their behavior. In fact, we know remarkably little.[6] This Article provides the first large-scale comparison of plaintiff win rates and recoveries in civil cases tried before juries and judges. In two of the most con-

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[1] *See* Hans Zeisel, *The Debate over the Civil Jury in Historical Perspective*, 1990 U. CHI. LEGAL F. 25. *Compare* Stephen C. Yeazell, *The New Jury and the Ancient Jury Conflict*, 1990 U. CHI. LEGAL F. 87, 103-17 (continuity of controversy's themes) *with* Delavan J. Dickson, *Autonomy, Discretion and Representation: A Comparative Study of Jury Development in Athens, England, and the United States of America*, 49/09-A DISSERTATION ABSTRACTS INT'L 2794 (1988) (unpublished dissertation on file with the *Cornell Law Review*) (more evolutionary view, seeing shift at late stage to irrationality and inefficiency arguments that mask political aims).

[2] MORTON J. HORWITZ, THE TRANSFORMATION OF AMERICAN LAW, 1780-1860, at 67 (1977).

[3] *Id.* at 84.

[4] *E.g.*, PETER W. HUBER, LIABILITY: THE LEGAL REVOLUTION AND ITS CONSEQUENCES (1988); Dan Drazan, *The Case for Special Juries in Toxic Tort Litigation*, 72 JUDICATURE 292 (1989); *see also* Stephen Daniels, *The Question of Jury Competence and the Politics of Civil Justice Reform: Symbols, Rhetoric, and Agenda-Building*, LAW & CONTEMP. PROBS., Autumn 1989, at 269, 270 (critics see civil jury as "causal link" in current crisis); Stephen Daniels & Joanne Martin, *Jury Verdicts and the "Crisis" in Civil Justice*, 11 JUST. SYS. J. 321 (1986) (refuting those who blame jury system for civil liability crisis).

[5] *Civil Rights Act of 1990: Joint Hearings on H.R. 4000 Before the House Comm. on Education and Labor and the Subcomm. on Civil and Constitutional Rights of the House Comm. on the Judiciary*, 101st Cong., 2d Sess. vol. 2, at 96 (1990) (statement of Mr. Victor Schachter).

[6] *See* ROBERT J. MACCOUN, GETTING INSIDE THE BLACK BOX: TOWARD A BETTER UNDERSTANDING OF CIVIL JURY BEHAVIOR 41-42 (1987). On the state of empirical research in civil procedure, see generally Symposium, *Empirical Studies of Civil Procedure* (pt. 1), LAW & CONTEMP. PROBS., Summer 1988, at 1.

troversial areas of modern tort law—product liability and medical malpractice—the win rates substantially differ from other cases' win rates and in a surprising way. Plaintiffs in these two areas prevail at trial at a much higher rate before judges than they do before juries. Furthermore, in several categories of personal-injury liability, the mean recovery in judge trials is higher than the mean recovery in jury trials. This Article's goal is to explore how and why these win rates and recoveries differ.

The empirical results prove resistant to simple explanations. Differences in the size of award, for instance, do not fully explain differences in win rates. This Article considers the results in light of the parties' ability to choose between a judge trial and a jury trial. It concludes that (1) the most plausible explanation of the data lies in small differences between judges' and juries' treatment of cases and, more importantly, in the parties' varying the selection of cases that reach judge and jury; (2) litigants' stereotypical views about juries may lead them to act unwisely in choosing between judge trials and jury trials; and (3) the surprising win rates in product liability and medical malpractice cases may stem from the especially strong misperceptions litigants hold about judge and jury behavior in these cases. More simply put, certain groups of plaintiffs do far better before judges, but the reason likely lies in prevailing misperceptions about juries.

Part I sketches stereotypical and scholarly impressions of judge and jury trial outcomes. Part II tests these impressions by using data on trial outcomes in judge and jury trials for major areas of tort and contract law. Part III seeks to explain the unpredicted patterns of the data by taking into account the size of awards and local factors. This unsuccessful effort leads us to consider the role of misperception and the settlement process as sources of the observed results. Part IV contrasts perceptions of judge/jury behavior with the limited empirical evidence available, finding little support for the widespread perception that juries are biased or incompetent. Part V combines the realities of judge/jury behavior with the litigants' settlement behavior to formulate our explanation of the data.

I

IMPRESSIONS OF OUTCOMES IN JUDGE AND JURY TRIALS

It is helpful to separate two sources of opinions about differences between judge and jury trials. Lay and professional perceptions about jury behavior are one source. Recent insights of scholars supply the other.

A. The Popular View

The first source, which we shall call the "popular" view, is the mass of stereotypical views about juries held by the public, including the legal profession.[7] Tort reformers and others portray juries as having a pro-plaintiff bias.[8] Juries are believed to find liability when judges would not, to grant higher awards than judges, and to grant inappropriate punitive damages awards.[9] For example, if asked whether product liability plaintiffs prevail at a greater rate before judges or juries, most people would answer "juries."[10] Informal surveys of professors and students strongly confirm this for both

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[7] *See* Joe S. Cecil et al., *Citizen Comprehension of Difficult Issues: Lessons from Civil Jury Trials*, 40 AM. U. L. REV. 727, 742-44 (1991); *cf.* Daniels, *supra* note 4, at 305-08 (public opinion polls); Edith Greene et al., *Jurors' Attitudes About Civil Litigation and the Size of Damage Awards*, 40 AM. U. L. REV. 805, 811-18 (1991) (questionnaires to jurors). By labeling this source the popular view we do not mean to attribute it only to the uninformed laity. As this view is developed below, it is perhaps more strongly the popular view within the legal profession and among policymakers. *See infra* text accompanying notes 61-65.
One popular misconception probably limited to the laity is that all trials are jury trials. In fact, our database for 1979 to 1989 shows that 55.5% of the 130,686 federal civil trials are judge trials. *See also* CHARLES A. WRIGHT, THE LAW OF FEDERAL COURTS 644 (4th ed. 1983) (majority are judge trials). For state courts of general jurisdiction, because of their heavy domestic relations and estate dockets, about 96% of the vastly more numerous state civil trials are judge trials (98% in state courts of limited jurisdiction). VICTOR E. FLANGO ET AL., THE BUSINESS OF STATE TRIAL COURTS 51-53, 130-39 (1983). Incidentally, some 98% of the world's civil jury trials take place in the United States. Hans Zeisel, *The American Jury, in* THE AMERICAN JURY SYSTEM: FINAL REPORT OF THE 1977 CHIEF JUSTICE EARL WARREN CONFERENCE ON ADVOCACY IN THE UNITED STATES 65, 65 (1977). *But cf.* Basil S. Markesinis, *Litigation-Mania in England, Germany and the USA: Are We So Very Different?*, 49 CAMBRIDGE L.J. 233, 246-47 (1990) (recognizing in comparative study that continental judges might behave like American juries).
The number of federal civil jury trials, as well as the fact that a fair proportion occur in nontort cases, makes the federal courts an appropriate locus for study, despite their relatively small docket. *See* Marc Galanter, *Jury Shadows: Reflections on the Civil Jury and the "Litigation Explosion," in* THE AMERICAN CIVIL JURY: FINAL REPORT OF THE 1986 CHIEF JUSTICE EARL WARREN CONFERENCE ON ADVOCACY IN THE UNITED STATES 15, 18-19 (1987); *see also infra* note 46 (discussing data from state courts).

[8] *E.g.*, HUBER, *supra* note 4, at 119-20, 185-88; *see* Marc Galanter, *The Civil Jury as Regulator of the Litigation Process*, 1990 U. CHI. LEGAL F. 201, 206-09.

[9] *E.g.*, HUBER, *supra* note 4, at 127-32; W. KIP VISCUSI, REFORMING PRODUCTS LIABILITY 88 (1991); Pete V. Domenici & C. Everett Koop, *Sue the Doctor? There's a Better Way*, N.Y. TIMES, June 6, 1991, at A25 ("outrageously high jury awards").

[10] In civil rights and other specialized areas, intuitions about juries may differ. For example, juries may find those suing the government and its officers less appealing than they find the defendants. Theodore Eisenberg, *Litigation Models and Trial Outcomes in Civil Rights and Prisoner Cases*, 77 GEO. L.J. 1567, 1594-95 (1989); Jon O. Newman, *Suing the Lawbreakers: Proposals to Strengthen the Section 1983 Damage Remedy for Law Enforcers' Misconduct*, 87 YALE L.J. 447, 454 (1978). Any pro-plaintiff jury bias may be outmatched by the jury's possible prejudice against the likely black, Puerto Rican, poor, felon, or drug addict status of those victimized by government. *Id.* For purposes of this study, this countervailing factor is not relevant. Our data emphasize tort and contract cases against nongovernmental defendants, where discrimination and other problems plaguing many civil rights plaintiffs do not have a systematic effect.

product liability and medical malpractice cases. In those cases, the law professors, dispute resolution experts, and upperclass law students surveyed predicted significantly higher success rates in jury trials than in judge trials.[11]

Outside the area of personal-injury tort litigation, perceptions of jury bias are less clear. In contract actions, for example, one lacks an injured plaintiff seeking recovery from a larger, more powerful defendant. Popular wisdom and informal surveys suggest that the jury effect would be less drastic outside the area of personal-injury tort litigation.[12]

B.   The Academic View

A growing body of literature on what has come to be called the selection effect is the second source of forecasts about the relationship between judge and jury trials.[13] We shall call this the "aca-

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[11]   The following table shows the mean predicted plaintiff trial win rate (in percent) as well as the significance level of the judge/jury difference within each group of respondents. The number of respondents is in parentheses.

TABLE 1

	Product Liability			Medical Malpractice		
	Judge	Jury	p	Judge	Jury	p
Law Professors (46)	43.6	62.9	.000	41.5	62.1	.000
Dispute Experts (10)	32.5	42.5	.012	28.5	37.5	.027
Law Students (14)	38.2	52.7	.002	38.9	53.8	.001

One can view the above table as exploring the hypothesis that each group predicts the same mean trial win rates for judge and jury trials. By convention, the hypothesis being tested usually is called the null hypothesis. GEORGE W. SNEDECOR & WILLIAM G. COCHRAN, STATISTICAL METHODS 64 (8th ed. 1989). The p values (also called significance levels) reported in the table are the probability of rejecting the null hypothesis when it is true. The p values provide an inverse measure of the likelihood that the difference in means show real differences in the predicted judge/jury trial win rates rather than mere random variation. *Id.* Thus, for law professors there is less than 1 chance in 1000 of erroneously rejecting the hypothesis of equal predicted win rates for judge and jury trials. Another way of describing rejection of the tested hypothesis is to say that the difference in means is significant at the level indicated by the p values. The law professor results thus are significant beyond the .001 level. Throughout this Article we use the term "significant" in the formal statistical sense of significance level. The smaller the p value or level of significance, the more surprised one would be to observe the difference in means if the tested hypothesis (equality of means) were true. By arbitrary convention, results that are significant at or beyond the .05 level are often described simply as statistically significant. *E.g.,* THE EVOLVING ROLE OF STATISTICAL ASSESSMENTS AS EVIDENCE IN THE COURTS 197 (Stephen E. Fienberg ed., 1989).

[12]   Compare the following table with Table 1, *supra* note 11.

TABLE 2   Contract

	Judge	Jury	p
Law Professors (46)	50.0	48.8	.304
Dispute Experts (10)	44.0	41.5	.244
Law Students (14)	45.0	43.8	.476

[13]   *E.g.,* George L. Priest & Benjamin Klein, *The Selection of Disputes for Litigation,* 13 J. LEGAL STUD. 1 (1984). *Compare* Donald Wittman, *Is the Selection of Cases for Trial Biased?,*

demic" view. In general, "[t]he selection effect refers to the proposition that the selection of tried cases is not a random sample of the mass of underlying cases."[14] More specifically, theorists have tried to describe the biased trial sample, most often proceeding as follows. Cases only go to trial when the parties substantially disagree on the predicted outcome of trial. In other cases, both parties can save the extra cost of trial by settling in light of their knowledge of the applicable legal rules and all other aspects of the case. When the legal rules clearly favor either the plaintiff or the defendant, the case tends to settle readily. Difficult cases falling close to the applicable legal standard tend not to settle, because the parties are more likely to disagree substantially in their predicted outcomes. These unsettled close cases fall more or less equally on either side of the legal standard, regardless of both that standard's position and the underlying distribution of disputes. Thus, even if the legal standard highly favors plaintiffs, one might not observe a plaintiff trial win rate well above 50%. Instead, the selection effect will leave for trial a residue of unsettled cases exhibiting some nonextreme equilibrium win rate.

This selection effect theory does not necessarily question the popular perception of pro-plaintiff jury bias in personal-injury cases. It does question whether the popular perception of juries, even if true, translates into higher observable trial win rates before juries than before judges.[15] Parties adjust their settlement behavior not only in light of the applicable legal standard, but also in light of the decisionmaker, including the mode of trial. Even if juries are more favorable to plaintiffs than are judges, one might not observe this through higher plaintiff win rates in jury trials. If both parties perceive that one party has a highly favorable adjudicator, the case is unlikely to be tried. In cases that could be tried before juries, the parties who know of the pro-plaintiff bias will adjust their settlement behavior to account for the increased value of the plaintiff's claim. If a judge is known to be pro-plaintiff, the defendant will demand a jury trial or try to settle the case in a manner that reflects knowledge of the judge's behavior. Those cases that survive the settlement process and reach jury or judge trials should not be easy wins for

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14 J. LEGAL STUD. 185 (1985) (criticizing Priest and Klein's model) *with* George L. Priest, *Reexamining the Selection Hypothesis: Learning from Wittman's Mistakes*, 14 J. LEGAL STUD. 215 (1985) (defending the model).

[14] Theodore Eisenberg, *Testing the Selection Effect: A New Theoretical Framework with Empirical Tests*, 19 J. LEGAL STUD. 337, 337 (1990).

[15] Selection effect theory has not previously been extensively developed as a source of theory about judge and jury trial differences. Some implications of selection effect theory for this area, however, seem clear. *See* Eisenberg, *supra* note 10, at 1595-96; Eisenberg, *supra* note 14, at 337-38; Priest & Klein, *supra* note 13, at 5, 7, 35.

plaintiffs or defendants. Just as legal standards favorable to plaintiffs (such as strict liability) need not and do not translate into high trial win rates,[16] use of adjudicators who favor plaintiffs (such as, perhaps, juries) need not lead to high trial win rates.

Thus, according to this selection effect theory, any judge/jury distinction that the parties evaluate without systematic inaccuracy should not lead to a difference in win rates before judges and juries. Indeed, under simplifying assumptions, selection effect theory suggests a trial win rate of 50% before both judges and juries.[17] Although comprehensive trial data do not support the 50% hypothesis,[18] the underlying insight—that close cases tend to dominate the limited universe of tried cases—is important in analyzing win rates in judge and jury trials. We are concerned here not with any specific win rate, such as 50%. The question is whether, even assuming a pro-plaintiff jury bias and allowing for departures from 50% success, there is reason to believe that observed win rates before judges and juries should differ substantially. Much of the thrust of selection effect theory suggests that trials before judges and juries ought to result in similar win rates.[19]

Although basic selection effect theory developed to date would not predict sharp judge/jury differences in win rates, it is worth thinking through the circumstances under which a more fully developed theory would forecast observable judge/jury differences. Two possible sources of differences are worth separating: (1) factors that may be developed within the context of existing selection effect theory and (2) factors that reveal the incompleteness of existing selection effect theory for modeling the choice between judge and jury trial.

First, within the context of existing selection effect theory, adjudicators highly favorable to one side, like favorable legal standards, do not necessarily lead to different observed trial win rates, as already noted. What factors might lead to different rates?

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[16] For example, see the 42.8% win rate in product liability cases reported in Priest & Klein, *supra* note 13, at 37-43.

[17] *See* Priest & Klein, *supra* note 13, at 17-20.

[18] Eisenberg, *supra* note 14.

[19] In our informal surveys covering product liability and medical malpractice cases, dispute resolution experts forecast significantly (p $\leq$ .05) lower differences in success rates between judge trials and jury trials than did law professors. *See supra* note 11.

We are not prepared to argue that the academic view forecasts no differences between judge and jury trial win rates. *See, e.g.,* Priest, *supra* note 13, at 228-29 (cases in which main dispute concerns damages would increase win rate); J. Mark Ramseyer & Minoru Nakazato, *The Rational Litigant: Settlement Amounts and Verdict Rates in Japan,* 18 J. LEGAL STUD. 263, 284-85 (1989) (similar). It is sufficient for our purposes to suggest the mechanism by which the win rates should tend toward equality.

Different stakes to the parties is the most common explanation of win rates that depart from selection effect theory's presumptions.[20] The doctor whose reputation may be harmed may have more at stake than the money damages sought by the plaintiff. The company defending a product liability action may have more at stake than the money sought from it in the particular case. Such differential stakes may make defendants more willing to settle strong suits and may lead to plaintiff win rates at trial lower than either 50% or whatever other level one expects absent the differential stakes. Greater stakes to plaintiffs may raise win rates. In studying differences between judge and jury trials, however, this differential stakes explanation carries little weight. The doctor and the company may have stakes different from the plaintiffs', but those stakes do not vary with the mode of trial. The reputational harm to the doctor and the potential flood of product liability cases against the company result whether these defendants lose in judge trials or jury trials.[21]

Instead, misperceptions about the adjudicator's standard of decision would provide a powerful explanation for different judge/jury win rates. If the parties perceive the adjudicator to be favorable to the plaintiff, but the adjudicator turns out not to be, then the apparently close cases would turn out to be losers and the win rate would drop. Similarly, if the adjudicator appears to be neutral, but turns out to be unfavorable to the plaintiff, then the win rate would drop. Imagined biases or unperceived biases therefore affect win rate. And if these misperceptions prevailed in different kinds or to differ-

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[20] *See* Eisenberg, *supra* note 10, at 1579, 1581-82; Priest & Klein, *supra* note 13, at 5, 24-29, 40; *cf.* Donald Wittman, *Dispute Resolution, Bargaining, and the Selection of Cases for Trial: A Study of the Generation of Biased and Unbiased Data*, 17 J. LEGAL STUD. 313, 341-45 (1988) (more complete modeling). *See generally* Kathleen Engelmann & Bradford Cornell, *Measuring the Cost of Corporate Litigation: Five Case Studies*, 17 J. LEGAL STUD. 377 (1988) (studying asymmetrical stakes more generally); Samuel R. Gross & Kent D. Syverud, *Getting to No: A Study of Settlement Negotiations and the Selection of Cases for Trial*, 90 MICH. L. REV. 319 (1991) (broadening consideration to include parties' strategic behavior). The differential stakes analysis can be applied to the litigants' different estimates of the size of judgment and to different attitudes toward risk.

[21] Analyis suggests various other possible pressures that would, at most, lead to minor differences in judge/jury win rates. For example, differences in ability to assess decisionmakers could lead to departures from 50% success. If defendants' attorneys are systematically better at forecasting case outcomes than plaintiffs' attorneys, win rates at trial should reflect this. *See* Wittman, *supra* note 20, at 325-27. But this difference, too, should fade when the comparison is between win rates in the two different trial modes. For this factor to translate into differences between judge trials and jury trials, the superior attorneys must enjoy their edge in a way that differs between the two kinds of trials. For example, defendants' attorneys must enjoy a stronger edge in assessing judge trials than they do in assessing jury trials. Yet one suspects that most factors that lead to attorney superiority at assessment, such as experience, education, analytical ability, and resources, apply to both modes of trial.

ent degrees regarding judges and juries, then different judge/jury win rates would result.[22]

Another possible source of difference in judge/jury win rates involves differential routing of strong or weak cases to the judge or the jury. But this source is all too easy to exaggerate. After all, routing is just the sort of effect that the selection effect should obliterate. Nevertheless, under certain conditions a skewed flow of cases toward judge and jury could positively affect the win rates, although under other conditions its effect could be negligible or even opposite. Under any plausible conditions, however, the effect of skewed routing should be relatively minor.[23]

Second, the central thrust of selection effect theory has been to model which cases settle before trial and which cases go to trial, not to model the choice between judge and jury trial. Perhaps a more complete theory would forecast substantial differences in judge/jury win rates.

Real dissimilarities exist between the settlement decision and the choice of adjudicator. In deciding whether to settle or litigate, both parties can benefit from a decision to settle by saving litigation costs, and trial results only when the parties substantially disagree in expectations. The choice of adjudicator differs. Either side can opt for a jury trial and will do so when that choice increases its expected return. One side would almost always seem to benefit from the choice of jury trial. A judge trial would result only when its cost savings overwhelmed any party's benefit from jury trial, the parties' differing preferences or perceptions lead them both to see judge trial as more favorable, or the interested party has inadvertently waived jury trial.

Although the choice between judge and jury undoubtedly differs from the choice between settling and litigating, we doubt that it does so in a way that undermines misperception as a powerful

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. 22 *Cf. id.* at 328 (proof of similar point). We later discuss a presumably lesser effect of differing impediments to settlement. *See infra* text accompanying note 93.

23 *See* Priest & Klein, *supra* note 13, at 19-22. *But cf.* Wittman, *supra* note 20, at 334 (critical nature of heteroscedastic error assumption). Similarly secondary and indeterminate, but offsetting, sources of difference in judge/jury win rates lie in such possible jury characteristics as greater cost of trial, *see infra* note 59 and accompanying text, and greater generosity in award, *see infra* note 70 and accompanying text. *Compare also id.* at 334-35 (unpredictability of award size tends to increase win rate) *with id.* at 335-36 (large awards imply lower win rate).

In subsequent analysis, we use the underlying insight of the widely accepted, strong version of selection effect theory described in the text. If that theory is flawed, so that judge and jury win rates do not tend toward equality in the absence of differential misperception, our explanation would still proceed along similar lines. The explanation would be simpler to state, however, because we could rely more on the routing process and less on the settlement process.

source of difference. Given the process of choosing an adjudicator, a consistently and pronouncedly differential routing of strong or weak cases to the judge or jury does not suggest itself. Moreover, once the cases are separated into judge and jury cases, if the parties mutually and accurately perceive judge/jury differences, the settlement process, as in basic selection effect theory, should tend to reduce, though not necessarily eliminate, differences in win rates in the residues of cases reaching trial. The data confirm this, for in the vast majority of case categories there are relatively small differences in judge/jury win rates.[24] But in some categories of jury cases, where parties misperceive jury tendencies, the settlement process that normally tends to produce a comparable win rate could be distorted in a way that produces a widely variant win rate. In judge cases too, misperceptions could distort the settlement process, once the parties have agreed on a judge trial for one reason or another.

II
DATA ON OUTCOMES IN JUDGE AND JURY TRIALS

Differences in win rates between judge and jury trial thus might be consistent with either the popular or the academic view. Each view has a core prediction worth testing. The popular view forecasts greater personal-injury tort success before juries. The academic view expects to observe little difference in success between the two trial modes. Does either view reflect reality?

Data gathered by the Administrative Office of the United States Courts enable us to compare the outcomes of judge trials and jury trials for cases tried in federal court. When any civil case terminates in federal district court, the court clerk files a form with the Administrative Office containing information about the case.[25] The form includes data regarding the subject matter and jurisdictional basis of the case, the amount demanded, the dates of filing and termination, the procedural progress of the case at termination, including whether it was tried before judge or jury, and, when a judgment was entered, who prevailed and any amount awarded in damages. The Administrative Office form contains many subject matter categories, including branches of tort, contract, civil rights, and other areas of law. The data used here cover the fiscal years 1979 through 1989.[26]

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24   *See infra* text accompanying note 39.
25   *See* ADMINISTRATIVE OFFICE OF THE U.S. COURTS, XI GUIDE TO JUDICIARY POLICIES AND PROCEDURES transmittal 64, at II-19 to -28 (Mar. 1, 1985). For a complete description of Administrative Office data, see INTER-UNIVERSITY CONSORTIUM FOR POLITICAL AND SOCIAL RESEARCH, FEDERAL COURT CASES: INTEGRATED DATA BASE, 1970-1987, ICPSR 8429 (2d ed. Winter 1989) [hereinafter ICPSR].
26   The Administrative Office began recording who prevailed in 1979. ICPSR, *supra* note 25, at 15 (civil codebook). Data after 1989 are not yet available to us.

Strengths and weaknesses of the Administrative Office data are noted elsewhere.[27]

The Administrative Office data allow for several different approaches to determining whether there are differences between judge and jury trials and to deciding what factors might explain those differences. One can study different win rates before judges and juries, different award patterns before judges and juries, and differences in the rate at which the parties select judge and jury trials. Relationships among these factors also are of interest. With several possible points of reference, there is no single obviously "correct" comparison with which to begin the analysis. Ultimately, we will explore all of these factors and others. We begin with plaintiff trial win rates for two reasons. First, it is a highly visible, comprehensible factor about which popular and academic views generate predictions. Second, win rates so far have played the leading role in empirical studies of the selection effect.[28]

Appendix A shows win rates at judge and jury trials, aggregated for all ninety-four federal districts for eleven fiscal years in every category of cases.[29] To be precise, the win rate is the fraction of plaintiff wins among judgments for either plaintiff or defendant entered after full trial. Initial examination of the data shows that neither popular nor academic predictions are uniformly true.

The notion that all plaintiffs need to do in any type of case is "get to the jury" is thoroughly discredited, at least in its simplistic sense. Plaintiffs lose a fair number of trials, including jury trials. In fact, plaintiffs enjoy greater success before judges than before juries in three major tort categories—product liability (personal injury), medical malpractice, and motor vehicle. In only two large personal-injury categories—Federal Employers' Liability Act (FELA) and marine—is there a significantly higher win rate before juries than before judges. The most striking differences are in the product liability and medical malpractice categories.

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27 James A. Henderson, Jr. & Theodore Eisenberg, *The Quiet Revolution in Products Liability: An Empirical Study of Legal Change*, 37 UCLA L. REV. 479, 518-22 (1990); Stewart J. Schwab & Theodore Eisenberg, *Explaining Constitutional Tort Litigation: The Influence of the Attorney Fees Statute and the Government as Defendant*, 73 CORNELL L. REV. 719, 723 (1988). One important limitation is the inability to distinguish among kinds of claims within the Administrative Office categories. *See, e.g.*, PATRICIA M. DANZON, MEDICAL MALPRACTICE: THEORY, EVIDENCE, AND PUBLIC POLICY 38-39 (1985) (type of injury in medical malpractice cases relates to chance of winning).

28 *E.g.*, Eisenberg, *supra* note 14; Priest & Klein, *supra* note 13.

29 The data we report here are limited to case categories that had at least one reported full trial during the period covered. Within the categories, data based on termination reports should reflect any adjustment of jury verdict by the trial judge, but not any effect of appeal. On the generally small effect of appeal upon completed trials, *see* Galanter, *supra* note 8, at 224-27.

If the popular view founders on plaintiffs' frequently greater success in judge trials, the academic view founders on the sharp differences in win rates within categories. The parties' settlement behavior does not lead to sending cases toward judges and juries in a way that equalizes win rates. There are large differences between win rates in many case categories, including highly visible areas such as product liability and medical malpractice. One possible explanation for these results is that judges and juries receive different mixes of cases because parties misperceive judge/jury differences. Before examining possible explanations, however, it is necessary to screen the case categories to identify classes of cases in which there truly is a choice between judge trials and jury trials.

A. Refining the Data

First, study should be limited to case categories with reasonable numbers of cases tried before both judges and juries. Many case categories have few judge trials or few jury trials.[30] Differences in numerically small categories are too tenuous to warrant serious statistical inquiry.

Second, one must focus on case categories in which equitable relief does not play a prominent role,[31] because parties have no right to jury trial in cases seeking solely equitable relief.[32] Differences between judge and jury win rates are meaningful only when the parties have a real choice between the two kinds of trial. To illustrate, the case category called jobs in Appendix A shows an extremely low win rate in judge trials, 0.20, and a higher win rate in jury trials, 0.39. The jobs category is dominated by cases brought under Title VII of the Civil Rights Act of 1964, which did not authorize jury trials.[33] The difference in judge/jury win rates may or may not reveal that Title VII cases are more difficult to win than other jobs cases. It does not, however, reveal differential treatment of Title VII cases by judges and juries, because juries did not resolve such cases. We therefore exclude case categories in which equitable relief is prominent.[34]

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[30] *See infra* Appendix A. In fact, none of the categories so eliminated had few judge trials but a substantial number of jury trials.

[31] *See* Eisenberg, *supra* note 10, at 1596-97.

[32] KEVIN M. CLERMONT, CIVIL PROCEDURE 102 (2d ed. 1988).

[33] THEODORE EISENBERG, CIVIL RIGHTS LEGISLATION 949 (3d ed. 1991).

[34] For each category of cases, we examined the percentage of plaintiffs' judgments after full judge trial that resulted in money awards (in contrast to equitable relief). When more than about 90% of the awards were money awards, a level below which there was a clear break in the data, we included the case category in the analysis. We also eliminated the category of marine contract cases. The paucity of jury trials there evidences the legal reality that judicial money awards result from the absence of a jury

Third, other kinds of cases do not offer a true choice between judge and jury trial. When the United States is a defendant, usually no jury right exists.[35] For example, in a contract or tort action brought under the Tucker Act or Federal Tort Claims Act, statute dictates trial by judge.[36] Medical malpractice cases against the Veterans' Administration, for a more specific example, do not present the parties with a true choice between judge and jury trial. We thus eliminate cases in which the United States is a defendant.

Eliminating numerically small categories, categories with a prominent equitable component, and federal-government-as-defendant cases leaves the contract and tort categories and cases shown in Table 3. Table 3 thus isolates the sizable case categories that most clearly lead to a choice between judge and jury trial.[37]

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trial right, unlike the situation in marine torts. *See* 9 CHARLES A. WRIGHT & ARTHUR R. MILLER, FEDERAL PRACTICE AND PROCEDURE § 2315 (1971).

Within each of the retained categories, we compared the subset of cases in which the plaintiff clearly requested monetary relief with the subset of cases for which the datum of amount demanded was missing. If the subset of cases with missing demands was significantly associated with a higher percentage of judge trials, as in the category of other fraud, we took the absence of an amount demanded to be a possible indicator of equitable relief and so eliminated that subset in the particular category. Accordingly, we retained the cases with missing demands in seven of the categories, which kept the number of cases higher but perhaps included some cases in which the jury right derived solely from a counterclaim. We analyzed the data for the effects of this retention decision. None of our major findings would be affected by excluding all cases with missing demands.

[35]   *See id.* § 2314.

[36]   28 U.S.C. §§ 1346, 2402.

[37]   For greater expressiveness in Table 3, we change the category names of other contract, other personal property damage, and other fraud to "general contract," "torts to personal property," and "fraud."

For purposes of Table 3, and the analysis hereafter, we group the Administrative Office's eight different product liability categories into a single category, "product liability." We do not wish to mask the substantial differences in win rates across product liability categories, which are revealed by Appendix A. We are not confident, however, that lawyers checking boxes on the civil cover sheet form, which is how cases are categorized in the Administrative Office's system, usefully distinguish among the different product liability categories. An alternative approach would be to examine only the "purest" product liability category, personal injury-product liability, which numerically dominates all the other product categories combined. Limiting the analysis to this single product liability category would lead to no substantial difference in results.

It should also be noted that in 1985 in the Southern District of Ohio, defendants won several hundred Bendectin cases in one joint jury trial. *See* Henderson & Eisenberg, *supra* note 27, at 519 n.159. This giant trial depresses the jury win rate in product liability cases by several percent, an effect that is offset by the amalgamation of the non-personal-injury product liability cases with a higher jury win rate into our single artificial product liability category. Thus, the results presented here are the consequence of neither the new aggregate product category nor the unusual Bendectin litigation.

TABLE 3

Outcomes in Sizable Categories Involving Clear Judge/Jury Choice

| | Judge Trial | | Jury Trial | | |
	N (completed)	Win Rate	N (completed)	Win Rate	Win Ratio
General					
Airplane Personal Injury	75	.53	324	.62	0.85
Federal Employers' Liability	94	.62	1168	.72	0.86*
Assault, Libel, Slander	86	.43	339	.49	0.88
Marine Personal Injury	889	.57	1455	.64	0.89***
Other Personal Injury	516	.51	2769	.49	1.04
General Contract	3979	.69	2458	.66	1.05
Torts to Personal Property	732	.63	458	.59	1.07
Torts to Land	203	.66	149	.60	1.10
Negotiable Instruments	643	.81	173	.73	1.11*
Fraud	308	.67	265	.59	1.14
Motor Vehicle	590	.69	3633	.60	1.15***
Prod-Med					
Product Liability	456	.48	3648	.28	1.71***
Medical Malpractice	64	.50	732	.29	1.72***

* Judge/jury win rates differ at .05 level; ** at .01 level; *** at .001 level

B.   Preliminary Observations

Table 3 introduces a single number, the "win ratio," to repre-
sent the relationship between judge trial win rate and jury trial win
rate. The win ratio is the judge trial win rate divided by the jury trial
win rate. The win ratio is greater than one when plaintiffs enjoy a
higher win rate before judges than before juries; the ratio equals
one when there is no difference between judge and jury win rates;
and it is less than one when plaintiffs enjoy a higher win rate before
juries than before judges. Using the win ratio to illuminate the suc-
cess patterns in the surviving case categories reveals several notable
results.

First, six of the thirteen categories have win rates in judge and
jury trials that differ significantly at the .05 level.[38] In two of the
categories, product liability and medical malpractice, the win rate
differences are both significant and large. These two distinctive cat-
egories, which we shall for brevity sometimes term "prod-med," ob-
viously warrant special explanatory efforts.

Second, the differences in win rates do not follow a pattern pre-
dictable from either the popular or the academic view. If the popu-
lar view draws support from the greater plaintiff win rate in jury
trials for FELA and marine personal-injury cases, it must confront
the opposite relationship in motor vehicle, product liability, and
medical malpractice cases. The academic view suggests that win ra-

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[38]   *See supra* note 11 (discussing statistical significance).

tios should be near one, and so must confront ratios substantially greater than one, as in the prod-med categories.

Third, there is a high positive correlation between rates of success in judge trials and rates of success in jury trials. Plaintiffs tend to do well before judges in the same case categories in which they tend to do well before juries.[39]

This brief examination detects differences in judge/jury win rates worth trying to explain. The differences defy simple groupings, such as tort or contract. Yet the individual case categories clearly do matter. For example, limiting consideration to categories with significantly different win rates, the range of win ratios in tort personal-injury cases spans from 0.86 for FELA cases to 1.72 for medical malpractice cases. Plaintiffs thus range in winning from doing 14% worse before judges than before juries in FELA cases to faring 72% better in medical malpractice cases. No explanation that fails to account for individual case categories is likely to be persuasive.[40]

III
POSSIBLE EXPLANATIONS WITHIN THE OUTCOME DATA

Case categories are of central importance. Indeed, one might conclude that they are *all* that matter. That is, win ratios may be a function of the case category in a way such that little else, except the unobserved innards of the case category, will help explain the different win ratios. Yet some possible explanations deserve exploration before conceding that win ratios depend heavily on factors extrinsic to the available data. This section investigates these explanations, ultimately rebutting the natural suspicions that considering either the size of awards or local variations will resolve the mysteries of differing win ratios, but in the process deriving an important understanding of those mysteries.[41]

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[39] This result corresponds with an earlier result showing a strong positive correlation between success rates on pretrial motion and success rates at trial. Theodore Eisenberg, *The Relationship Between Plaintiff Success Rates Before Trial and at Trial*, 154 J. ROYAL STAT. SOC'Y ser. A, pt. 1, at 111 (1991).

Excluding product liability and medical malpractice cases, the ordinary least squares regression line that best fits the judge and jury win rates is quite close to the line representing equal win rates. The regression line that includes product liability and medical malpractice cases suggests greater variation between the judge and jury win rates. These two categories, which also stand out in Table 3, differ from the others mostly in their low win rates before juries.

[40] *See generally* Marc Galanter, *Case Congregations and Their Careers*, 24 LAW & SOC'Y REV. 371 (1990). While recognizing that we are working with the crude Administrative Office categorization, see *supra* note 27, we believe that the categories are fairly distinctive in terms of perceived and real judge/jury differences.

[41] Other possible explanations within the outcome data, such as the individual or corporate status of the parties, likewise generate some interesting insights, such as that a

A. Preliminary Steps

A more complete view of litigation accounts not only for whether plaintiffs obtain judgment, but also for the size of any judgment received. The Administrative Office data allow calculation of the mean recovery for judge and jury trials in each case category.[42] The mean recovery is the average amount awarded in those cases in the refined data set that show both a judgment for plaintiff after full trial and a positive dollar recovery. Appendix B gives, for the thirteen categories under study, mean as well as median awards, stated using the consumer price index in terms of 1989 dollars. To explore the possible relationship among size of awards, win rates, and the litigants' choice between judge and jury trials, we introduce several additional measures of judge/jury trial relationships.

We are less interested in the absolute size of recoveries in judge and jury trials than in the relationship between the sizes of recoveries in the two kinds of trials. For example, huge recoveries in product liability cases are not of interest here unless the amounts differ between judge and jury trials. Using the mean recoveries, the "mean recovery ratio" is defined as the plaintiff's mean recovery in judge trials divided by the plaintiff's mean recovery in jury trials.

One can use the concept of actual return to account for both the likelihood of winning and the size of recovery. For each mode of trial in each case category, the actual return is the win rate times the mean recovery. Using the computed actual returns, the "actual return ratio" is defined as the plaintiff's actual return in judge trials divided by the plaintiff's actual return in jury trials.

Finally, the "percent judge trials" is the percent of trials that are judge trials.[43] The "percent jury trials" is one hundred minus the percent judge trials.

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suit between corporations is significantly more likely to be tried by a judge, but they do not explain away our basic findings. *See also* Milo Geyelin, *Study Suggests Jurors Are Not Anti-Business*, WALL ST. J., Apr. 20, 1992, at B1 (jurors erroneously perceived to be anti-corporate).

[42] The maximum dollar recovery, and amount demanded, that the Administrative Office data can accommodate is $9,999,000. This limitation has the effect of trimming the impact on the mean of the atypically large case. We prominently use mean recovery to facilitate the calculation of return, while recognizing that for other purposes the median award is a less distorting and more useful measure. *See* Deborah R. Hensler, *Researching Civil Justice: Problems and Pitfalls*, LAW & CONTEMP. PROBS., Summer 1988, at 55, 57-58.

For further use of amount data and analysis of more sophisticated relations between win rates and awards, see *infra* notes 94-99 and accompanying text.

[43] The numbers of trials used to construct the percent judge trials column in Table 4 differ from the numbers used to compute win rates. Unlike Table 3, this new column includes cases in which a mode of trial was commenced even though the trial was not finished or an unambiguous judgment ultimately was not entered. Thus, for example, settlements and other dispositions during trial appear in the percent judge trials statis-

These new variables offer several possibilities for exploring the differences in judge/jury data. They also provide testable hypotheses of both popular and academic impressions of judge/jury differences.

As already noted, stereotypical views of juries, not accounting for selection effect, forecast win ratios below one in most tort categories, reflecting juries' perceived greater sympathy for injured plaintiffs facing large or powerful institutions. Stereotypical views of juries similarly forecast that mean recovery ratios and actual return ratios will be less than one in most tort categories, reflecting juries' perceived greater generosity and plaintiffs' power to insist upon trial by jury. Popular perceptions about jury trials also suggest that percent judge trials will be low for most tort categories.

Economic theory predicts that any seemingly telling pattern in win ratios will tend to disappear as mean recoveries enter the picture. The party choosing the mode of trial will choose the mode that maximizes that party's expected return. For example, the plaintiff will prefer a judge trial not when the win ratio exceeds one, but when the win rate times mean recovery from a judge trial exceeds the analogously calculated return from a jury trial. Economic theory thus suggests that if plaintiffs' preference dominates the choice of trial mode, percent judge trials should increase as the actual return ratio increases.

B. Explaining the Data by Using Size of Awards

Table 4 presents the win ratio, mean recovery ratio, and actual return ratio for all thirteen case categories. When focusing solely on win ratios, only the product liability and medical malpractice categories showed large differences from 1.0. The addition of the mean recovery and actual return ratios leads to a less clear grouping of case categories. The frequency with which the parties select judge trials provides the cleanest basis for dividing the cases. This statistic for percent judge trials divides the case categories into personal-injury and non-personal-injury groupings. Personal-injury litigants overwhelmingly opt for jury trials. Non-personal-injury litigants usually are content with judge trials.[44] The stereotypical view is correct here.

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tics but do not appear in the win rate statistics. *See infra* Appendix B, where N(commenced) gives the number of judge and jury trials used to compute percent judge trials and, incidentally, where N(recovery) gives the number of judge and jury trials used to compute mean and median recoveries.

[44] As measured by the percentage of judge trials, the marine category does not fit comfortably with either the personal-injury or the non-personal-injury categories. We suspect that, because of the vagaries of admiralty law and Administrative Office categori-

TABLE 4

Percent Judge Trials, Win Ratio, Mean Recovery Ratio
& Actual Return Ratio

	% Judge Trials	Win Ratio	Mean Recovery Ratio	Actual Return Ratio
Personal-Injury				
Federal Employers' Liability	8.9	0.86*	1.22	1.05
Medical Malpractice	9.7	1.72***	1.03	1.78
Product Liability	12.1	1.71***	0.77	1.32
Motor Vehicle	14.7	1.15***	1.29	1.48
Other Personal Injury	15.4	1.04	1.03	1.07
Assault, Libel, Slander	20.1	0.88	0.12	0.10
Airplane Personal Injury	23.6	0.85	0.92	0.78
Marine Personal Injury	34.9	0.89***	1.27	1.13
Non-Personal-Injury				
Fraud	52.4	1.14	0.68	0.77
Torts to Land	56.5	1.10	0.35	0.39
General Contract	58.7	1.05	0.72	0.75
Torts to Personal Property	61.4	1.07	0.90	0.97
Negotiable Instruments	75.8	1.11*	0.63	0.70

* Judge/jury win rates differ at .05 level; ** at .01 level; *** at .001 level

With few exceptions the division between personal-injury and non-personal-injury helps describe the data. More personal-injury categories show significant differences[45] in win rates between judge and jury trials. Only the personal injury categories show a large variation in win ratio from category to category. The mean recovery ratios in personal-injury categories tend to be higher than the mean recovery ratios in the non-personal-injury categories, as do the actual return ratios. The other stereotypical views, then, are incorrect.

Figure 1 combines the organizing features of Tables 3 and 4 to reveal a seemingly telling pattern. The tables' two different groupings interact to yield three super-categories, or groups, of cases: (1) prod-med, (2) personal injury other than prod-med, which we shall call general-personal-injury, and (3) non-personal-injury. These groups reveal a sharp division in win ratios between prod-med and all the other categories, as well as a sharp division in percent judge trials between non-personal-injury and all the others.[46] Figure 1's plot of percent judge trials against win ratios, however, shows no correlation between mode of trial and rate of success.

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zation, this category contains many cases in which there was no true choice between judge trial and jury trial. *See supra* notes 34, 37.

[45] *See supra* note 11 (discussing statistical significance).

[46] In general, data from state courts making the necessary distinctions are not widely available. *See, e.g.,* NATIONAL CENTER FOR STATE COURTS, STATE COURT CASELOAD STATISTICS: ANNUAL REPORT 1989. The existing data, however, do exhibit a similar dichotomy in percent judge trials between tort and contract. FLANGO ET AL., *supra* note 7, at 53, 130-39. Moreover, among tort cases, a useful distinction on judge/jury questions

Percent Judge Trials vs. Win Ratio
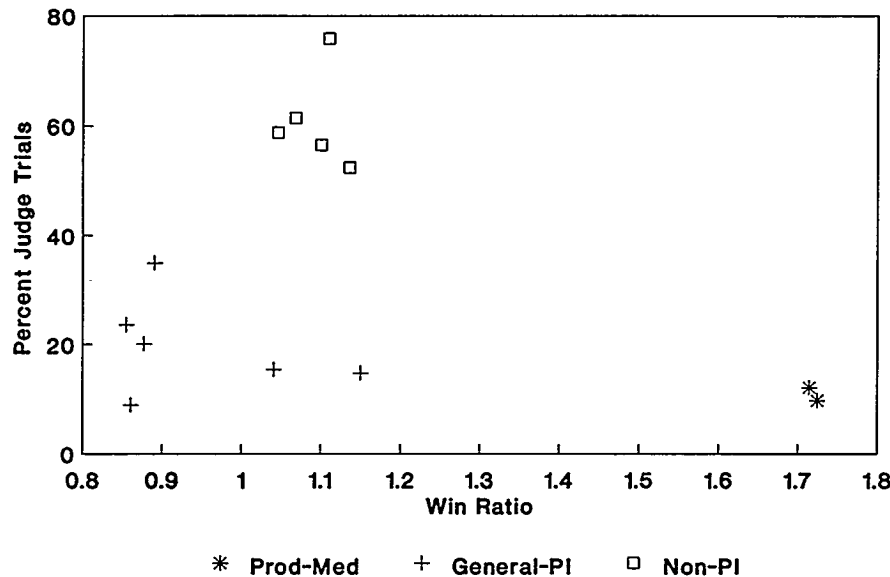
* Prod-Med   + General-PI   □ Non-PI
Source: Administrative Office Data, 1979-1989
FIGURE 1

Economic theory raises the question whether mode of trial correlates with actual return. Does the party choosing the trial mode do so in a way that reflects knowledge of observed judge/jury performance? Percent judge trials exhibits a personal-injury/non-personal-injury dichotomy, which is consistent with the view that plaintiffs' preference dominates the choice of trial mode. Therefore, the theorist would expect plaintiffs to opt more heavily for jury trials as their actual return before juries relatively increases. But in fact their actual return before juries is relatively highest in non-personal-injury cases, where plaintiffs shun juries. Figure 2 more generally shows an inverse relationship rather than the expected direct relationship between percent judge trials and actual return ratio. This is surprising.

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lies between prod-med cases and others. *See* David B. Rottman, *Tort Litigation in the State Courts: Evidence from the Trial Court Information Network*, St. Ct. J., Fall 1990, at 4, 15-16.

Some researchers are beginning to generate more pertinent state court data. Results to date appear to conform to ours. *E.g.*, Brian J. Ostrom & David B. Rottman, Does Plaintiff and Defendant Status Matter?: Verdicts and Awards in Tort Litigation, at 3, 5-6 (1991) (unpublished manuscript) (in 762 tort cases tried during three-month period in 27 state courts, there were few judge trials (15%), but plaintiffs won more often before judges (70%) than before juries (54%)).

**Percent Judge Trials vs.
Actual Return Ratio**

Source: Administrative Office Data, 1979-1989

FIGURE 2

In sum, using size of awards does not provide a simple explanation for the data. Instead, it forcefully suggests that something other than knowledge of observed economic benefits is driving the choice between judge and jury.

C. Explaining the Data by Using Local Variations

The differences between judge and jury trials may not be amenable to a single national explanation for all case categories. Much interesting work by legally oriented social scientists emphasizes the importance of local legal cultures.[47] We can explore whether local explanations play a role in our problem by addressing two questions. First, after accounting for the fact that cases are tried in dozens of different federal districts, do differences in judge/jury win rates remain? Second, can local quantitative factors, such as delay and adjudicator's receptivity, explain the choice between judge and jury trial?

Both questions explore whether the dominance of the case categories in explaining outcomes and choices of trial mode can be

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[47]   *E.g.*, Stephen Daniels, *Caseload Dynamics and the Nature of Change: The Civil Business of Trial Courts in Four Illinois Counties*, 24 LAW & SOC'Y REV. 299 (1990); Richard Lempert, *Docket Data and "Local Knowledge": Studying the Court and Society Link over Time*, 24 LAW & SOC'Y REV. 321 (1990); Frank W. Munger, Jr., *Commercial Litigation in West Virginia State and Federal Courts 1870-1940*, 30 AM. J. LEGAL HIST. 322 (1986).

muted or eliminated. Assessing local influence requires testing the influence of local factors on the judge/jury data, while accounting for a case's subject matter category. The need to account simultaneously for local and categorical charactertistics suggests use of multivariate regression-like analysis.

1. *Win Rate Differences and Locale*

To address the first question, consider a model in which the dependent variable is whether plaintiff or defendant prevails after full trial; the independent variables used to try to explain the dependent variable are whether the trial was before judge or jury and a series of variables identifying each United States District Court. A separate logistic regression equation is run for each case category (product liability, medical malpractice, and so on).[48] For present purposes, these thirteen regressions[49] do not lead to results importantly different from those in Table 3. The product liability, medical malpractice, and marine personal-injury equations all show significant judge/jury effects that correspond to those in Table 3. In the motor vehicle category, controlling for districts eliminates the significance of the judge/jury variable as a predictor of winning, but a fair number of districts contribute to that result; no small handful of strikingly variant districts explains the significance of the motor vehicle finding in Table 3. Thus, the most noteworthy differing win rates are not a consequence of local variation.

In most of the cases in this study, federal courts apply the substantive law of the state in which they sit.[50] Conceivably, an unusual number of cases decided under the law of a single state might have been the source of judge/jury differences. This local analysis, however, also suggests that win rate differences are not the consequence of a few unusual legal regimes.

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[48] Multivariate regression is a statistical technique that quantifies the influence that each of several factors has on the phenomenon being studied. A notable recent use of multivariate regression in the law was the attempt to prove that the race of both the accused and the victim played a disproportionate role in death penalty decisions in Georgia. *See* DAVID C. BALDUS ET AL., EQUAL JUSTICE AND THE DEATH PENALTY (1990). This attempt was rejected by the Supreme Court. McClesky v. Kemp, 481 U.S. 279, *reh'g denied*, 482 U.S. 920 (1987). For more about regression analysis, see MICHAEL O. FINKELSTEIN & BRUCE LEVIN, STATISTICS FOR LAWYERS ch. 12 (1990); DAVID W. HOSMER & STANLEY LEMESHOW, APPLIED LOGISTIC REGRESSION (1989).

Because the dependent variable in this model is dichotomous (judgment for plaintiff or defendant), we use logistic regression in lieu of ordinary least squares regression. *See* FINKELSTEIN & LEVIN, *supra*, at 448.

[49] The results are available from the authors.

[50] Erie R.R. v. Tompkins, 304 U.S. 64 (1938); Klaxon Co. v. Stentor Elec. Mfg. Co., 313 U.S. 487 (1941).

2. *Trial Mode and Local Factors*

The queues for judge trials and jury trials undoubtedly differ among districts, and plaintiffs hoping to receive funds quickly may choose the shorter queue. The choice between judge and jury trial therefore may reflect whether one mode of trial leads to terminating a case more quickly than the other mode. The parties' choice of trial mode also should reflect how they believe judges and juries will treat them. For example, the plaintiff who believes local juries are more hostile than local judges in product liability cases will not opt for a jury trial.

To pursue these possible explanations for choice of trial mode, both of which assume that plaintiffs' preference dominates the choice, consider a model in which the dependent variable is the mode (judge or jury) in commenced trials and the independent variables are the calendar year of termination, the thirteen case categories, and two variables used to capture local differences in delay and receptivity between judge trials and jury trials. For each district we compute separate mean times from filing to termination for judge and jury trials in each of three groups of cases: prod-med, general-personal-injury, and non-personal-injury.[51] To quantify the difference in time to termination between judge trials and jury trials, we construct a variable, "local time ratio," by dividing the mean time for judge trials by the mean time for jury trials. If this ratio exceeds one, the local mean time to termination is greater in cases tried before judges than in cases tried before juries.[52] We also compute a "local win ratio" for each of the three groups of cases. Using the local and other variables, a logistic regression of 40,026 cases

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[51] The three groups are preferable to the thirteen individual case categories because many districts lack enough cases in each of the thirteen categories to yield meaningful local means. The local mean times derive from cases in the refined data set that show a judgment for plaintiff or defendant after full trial and also show filing and termination dates.

[52] For the period of this study, judge-tried cases tended to consume more total time, as opposed to actual trial time, than jury-tried cases. This result should surprise many commentators who routinely assume the opposite. *E.g.*, RICHARD A. POSNER, ECO-NOMIC ANALYSIS OF LAW 549 (3d ed. 1986); Leon Sarky, *Civil Juries, Their Decline and Eventual Fall*, 11 LOY. L. REV. 243, 255-56 (1963); *see* GORDON BERMANT ET AL., PRO-TRACTED CIVIL TRIALS: VIEWS FROM THE BENCH AND THE BAR 43-45 (1981). Possible explanations for our result include that the judge-tried cases in the refined data set are more complex and time-consuming than the jury-tried cases, that the judge trial queue overall contains more cases and more complex cases than the jury trial queue, and that trial and eventual decision by judge is more prone to procrastination than the jury process. *See* Leighton Bledsoe, *Jury or Nonjury Trial—A Defense Viewpoint*, 5 AM. JUR. TRIALS 123, 141-42 (1966) (delay in judicial decision); Prentice H. Marshall, *A View from the Bench: Practical Perspectives on Juries*, 1990 U. CHI. LEGAL F. 147, 155-56 (same); William J. Palmer, *On Trial: The Jury Trial*, 20 F.R.D. 65, 78 (1958) (same).

involving a choice between judge and jury trial yields the results presented in Table 5.

The first numerical column shows a variable's "odds multiplier," a way of expressing the size of a variable's influence.[53] For the case category variables, the odds multiplier is the amount by which the odds of having a jury trial in an average case should be multiplied if the case is in that category, holding all other variables constant.[54] An odds multiplier of 1.0 indicates that the variable's presence does not change the odds of having a jury trial. An odds multiplier greater than 1.0 indicates that the variable's presence, holding other variables constant, increases the chance of a jury trial. An odds multiplier of less than 1.0 indicates that the presence of the factor reduces the chance of a jury trial. For example, the 3.70 odds multiplier for medical malpractice cases shows that, compared to an average category, medical malpractice cases are much more likely to be tried before juries than before judges.

The interpretation of the year, local time ratio, and local win ratio variables differs from the interpretation of the case category variables. For these continuous variables, the odds multiplier represents the effect of a unit increase in the variable. For example, an increase in the local time ratio from 1.0 to 2.0 would require multiplying the odds of a jury trial by 1.31.

The second column in Table 5 shows the significance level of the coefficients associated with each of the independent variables. Roughly speaking, they represent the probability that the observed result would occur by chance.[55] Thus, for all of the variables, there is only a small probability that one would observe these results by chance.[56]

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[53] In multivariate logistic regression, each estimated coefficient provides an estimate of the corresponding variable's effect on the logarithm of the dependent variable's odds, adjusting for all other variables included in the model. The odds multiplier is obtained by taking the anti-log of the regression coefficient. HOSMER & LEMESHOW, *supra* note 48, at 58.

[54] The odds of a jury trial should be distinguished from the probability of a jury trial, even though the terms "odds" and "probability" often are used interchangeably in informal conversation. For example, medical malpractice has an odds multiplier of 3.70. Assume that the odds of jury trial (based on all of the other factors about a case) are 1:1, corresponding to a 50% probability of jury trial. The odds multiplier of 3.70 means that the presence of medical malpractice as the case category changes the odds of jury trial from 1:1 to 3.70:1, corresponding to a probability of jury trial of 79%.

[55] *See supra* note 11. For discussion of significance levels of regression coefficients, see FINKELSTEIN & LEVIN, *supra* note 48, at 352-54, 451.

[56] One can measure the correctness of the complete regression model's estimate of jury trial odds by the correctness of classifications based on the estimate. FINKELSTEIN & LEVIN, *supra* note 48, at 451; HOSMER & LEMESHOW, *supra* note 48, at 146. That is, how often does the regression model correctly classify a case as tried by judge or jury? The classification table below shows that the model predicts correctly in about 76% of the cases. For cautions about using classification tables to measure how well a model fits the

Intuitively, the local time ratio variable should be greater than one, indicating that when judge-tried cases take longer than jury-tried cases, plaintiffs are more likely to opt for a jury trial. In contrast, the expectation is that the local win ratio variable will be less than one, indicating that as plaintiffs win relatively more frequently before judges, they will be less likely to demand a jury trial.

TABLE 5

Logistic Regression Results
Choice of Trial Mode

Dependent Variable = Jury Trial (1), Judge Trial (0)
(Deviation coefficients used for case categories)

Variable	Odds of Jury Trial Multiplier	Signif.
year	1.02	.000
case categories		
Negotiable Instruments	.14	.000
General Contract	.32	.000
Torts to Land	.34	.000
Airplane Personal Injury	1.41	.000
Assault, Libel, Slander	1.75	.000
Federal Employers' Liability	4.54	.000
Marine Personal Injury	.83	.000
Motor Vehicle	2.52	.000
Other Personal Injury	2.45	.000
Medical Malpractice	3.70	.000
Fraud	.41	.000
Torts to Personal Property	.28	.000
Product Liability	2.61	.000
local time ratio (judge/jury)	1.31	.000
local win ratio (judge/jury)	1.10	.000
constant	.39	.006

data, see HOSMER & LEMESHOW, *supra* note 48, at 147. We therefore also present below other standard measures of the model's goodness-of-fit to the data.

TABLE 6

	Chi-Square	df	Signif.
-2 Log Likelihood	40504.668	40010	.000
Model Chi-Square	9119.305	15	.000
Goodness of Fit	39941.877	40010	.000

Observed	Predicted Judge	Jury	Correct
Judge	8017	4431	64.40%
Jury	5339	22239	80.64%
		Overall	75.59%

Table 5 shows the observed values of both variables. The odds multiplier of the local time ratio, as expected, is greater than 1.0, as plaintiffs increasingly opt for jury trials over judge trials. Surprisingly, the odds multiplier of the local win ratio is also significantly greater than 1.0, which is in the direction opposite to intuitive expectations. Plaintiffs are probably not consciously flocking to juries as they observe greater success before judges. So, again, analysis suggests something other than observed economic benefits is driving the choice between judge and jury.[57]

The most important result embodied in Table 5 is the continued vitality of the individual case categories. As suggested by percent judge trials in Table 4, the mode of trial largely depends on the individual category. Table 5 suggests that this effect survives consideration of local phenomena.[58]

IV
POSSIBLE EXPLANATIONS BEYOND THE OUTCOME DATA

We cannot explain our surprising data set—and ultimately why civil litigants choose either judge or jury trial—by our data on case outcomes. With all statistical analysis completed, the data still look odd. The data's explanation must lie beyond, in real or supposed differences between judge and jury operating on the various case categories. And so we must look beyond, at past theoretical and empirical work on judge/jury differences.

Surely, some differences do affect choice of trial mode. Considering trial by jury, the actual trial itself takes longer and costs more,[59] and this must affect choice. Similarly, a jury trial requires

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[57] Other models using variables to represent local mean recovery ratios and local actual return ratios do not improve on the model presented in the text. Instead, the explanation seems to be parties' misperceptions of judge/jury differences, which should produce an inverse relationship between percent judge trials and win ratio.

[58] Table 5 also shows a significant trend over time in favor of jury trials. Controlling for case categories and local effects, there is a growing tendency to opt for jury trials over judge trials. Interestingly, our data from 1970 through 1978 exhibit an opposite trend. Data going back to 1940 show that the percent judge trial statistic oscillates. WOLF HEYDEBRAND & CARROLL SERON, RATIONALIZING JUSTICE 109 (1990); *see also* Galanter, *supra* note 8, at 258 (reporting jury trial percentages in 1961, 1970, 1980, and 1988).

The pattern of win ratios displays no noticeable time trend, looking generally at all thirteen case categories. In product liability cases, for a specific example, the win rate before judges exceeds the win rate before juries fairly steadily over the years, although the win ratio seems to be increasing since the mid-1980's. Theodore Eisenberg & James A. Henderson, Jr., *Is the Quiet Revolution in Products Liability Reflected in Trial Outcomes?*, CORNELL L.F., July 1990, at 2, 3. A recently widening difference in win rates also appears in medical malpractice cases, but not in the other categories.

[59] *See* JAMES S. KAKALIK & RANDY L. ROSS, COSTS OF THE CIVIL JUSTICE SYSTEM 37, 40, 68, 73 (1983); HANS ZEISEL ET AL., DELAY IN THE COURT 71-81 (2d ed. 1978).

special litigation skills of the attorney,[60] and the relative advantage that this difference creates will influence each side's choice of trial mode.

Yet the patterns of our data on percentages of judge and jury trials, win rates, and mean recoveries seem to rest on more substantial differences between judges and juries that would affect their decisions. Moreover, lawyers focus on the direct effect on outcome when explaining their choices. A leading trial practice book, in listing the factors that should guide choice, begins with (1) the nature of the factual issue (choose jury trial if your case rests on emotional or sympathy issues), (2) the type of client (opt for jury if the client has an appealing personality or relatively plays the role of the little person), and (3) the nature of the legal theory (stick with judge if the case depends on a legal technicality).[61] Lawyers suppose that by learning the differences between judge and jury in terms of outcome under current operational methods, and then choosing accordingly, they can greatly increase their chances of winning or of winning big.

A. Perceptions

More generally, lawyers' longstanding perceptions of substantial judge/jury differences seem to center on a view of the jury as biased and incompetent, relative to the judge. The jury's bias might apply willfully or unwillfully either in an uneven view of the evidence or in ignoring legal restrictions on reaching a verdict, and it normally works in favor of the plaintiff or the little person on liability and toward generosity on damages. The jury's incompetency also infects both the factfinding process and the duty to apply the law according to its terms. On the one hand, the supposed bias skews the results in a predictable direction, of which counsel may take advantage. On the other hand, the supposed incompetency makes the outcome unpredictable, especially if the case is complex.[62]

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[60] *See* Bruce G. Merritt, *Does a Business Ever Want a Jury?*, LITIGATION, Spring 1990, at 27, 29.

[61] ROGER HAYDOCK & JOHN SONSTENG, TRIAL 78, 81 (1990). Other factors listed therein are efficiency of bench trial, litigation skills of the opponent, and characteristics of the assigned judge, as well as preferences of the client and standard of review on appeal.

[62] *E.g.*, Merritt, *supra* note 60. The trial practice literature purveying such perceptions is extensive. *E.g.*, 1 FRED LANE, LANE'S GOLDSTEIN TRIAL TECHNIQUE § 9.02 (3d ed. 1984); RICHARDSON R. LYNN, JURY TRIAL LAW AND PRACTICE 27-32 (1986); THOMAS A. MAUET, FUNDAMENTALS OF TRIAL TECHNIQUES 23-24 (2d ed. 1988); JOSEPH R. NOLAN, TRIAL PRACTICE 14 (1981); Orville Richardson, *Jury or Bench Trial? Considerations*, TRIAL, Sept. 1983, at 58. Nevertheless, the perceptions are not unanimously held, and some commentators attempt to correct or invert the admittedly prevailing views. *E.g.*, Bledsoe, *supra* note 52; Jim Sullivan & Paul E. McMeans, *Judge or Jury*, LITIGATION, Summer

For the most part policymakers share this view of the jury. A so-called elitist tradition, which included Alexander Hamilton, Erwin Griswold, and Warren Burger, has successfully secured the high ground in the jury debates. It has belittled an institution that seemed to deserve belittling. The jury has its defenders in an egalitarian or even anti-judicial tradition dating from Thomas Jefferson. Even these defenders, however, have at times conceded, although downplayed, the jury's bias or incompetency, while resting their defense of the jury on political grounds.[63]

Judge Jerome Frank, by a telling method, set the prevailing perceptions into a larger scheme:

> I have said that, supposedly, the task of our courts is this: To make reasoned applications of legal rules to the carefully ascertained facts of particular law-suits. You will recall my crude schematization of the alleged nature of the process—$R \times F = D$—i.e., the Rules times the Facts equals the Decision. Where, in that scheme, does the jury fit in?
>
> . . . .
>
> . . . Longenecker, in a book written by a practical trial lawyer for practical trial lawyers, says: "In talking to a man who had recently served for two weeks on juries, he stated that in one case after they had retired to consider the verdict, the foreman made a speech to them somewhat like this: 'Now boys, you know there was lying on both sides. Which one did the most lying? The plaintiff is a poor man and the defendant is rich and can afford to pay the plaintiff something. Of course the dog did not hurt the plaintiff much, but I think we ought to give him something, don't you?' There were several 'sures'; we thought the plaintiff might have to split with his lawyers, so we gave him a big verdict." A case is reported in which the jurors explained their verdict thus: "We couldn't make head or tail of the case, or follow all the messing around the lawyers did. None of us believed the witnesses on

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1984, at 18; *see also* Andrew Wolfson, *Publication Shows Juries in Jefferson Not Generous*, LOU-ISVILLE COURIER-J., Nov. 28, 1988, at A1 (*available in* LEXIS, Nexis Library, Papers File) (reporting bar's surprise when new local jury verdict reporter revealed 50% win rate and low average award).

Of course, perceptions about judges in general or the assigned judge in particular influence choice too. For example, seeing the judge as an affirmatively hostile adjudicator for certain kinds of claims might push the plaintiff in pursuit of a neutral adjudicator toward the jury. Nevertheless, perceptions of jury trial appear to be the most prevalent motivator in choosing between judge and jury.

[63] *See generally* JOHN GUINTHER, THE JURY IN AMERICA at xiii-xiv (1988); Paul D. Carrington, *The Seventh Amendment: Some Bicentennial Reflections*, 1990 U. CHI. LEGAL F. 33, 34-42; Galanter, *supra* note 8, at 205-09, 245-51. In the policy debate, some maintain that the jury is equal or superior in performing the truth function. *E.g.*, LLOYD E. MOORE, THE JURY: TOOL OF KINGS, PALLADIUM OF LIBERTY 231-55 (2d ed. 1988); Palmer, *supra* note 52, at 78-80.

either side, anyway, so we made up our minds to disregard the evidence on both sides and decide the case on its merits." ...

    ....

Are jurors to blame when they decide cases in the ways I've described? I think not. In the first place, often they cannot understand what the judge tells them about the legal rules. ... It is inconceivable that a body of twelve ordinary men, casually gathered together for a few days, could, merely from listening to the instructions of the judge, gain the knowledge necessary to grasp the true import of the judge's words. ... The jurors usually are as unlikely to get the meaning of those words as if they were spoken in Chinese, Sanskrit, or Choctaw. ...

    ....

Suppose, however, that the jurors always did understand the *R*'s. Nevertheless, often they would face amazing obstacles to ascertaining the *F*'s. ...

    ....

This is no laughing matter. For prejudice has been called the thirteenth juror, and it has been said that "Mr. Prejudice and Miss Sympathy are the names of witnesses whose testimony is never recorded, but must nevertheless be reckoned with in trials by jury." ...

    ....

... [T]he general-verdict jury trial renders absurd the conventional description of the decisional process—the $R \times F = D$. To my mind a better instrument than the usual jury trial could scarcely be imagined for achieving uncertainty, capriciousness, lack of uniformity, disregard of the *R*'s, and unpredictability of decisions.[64]

B. Realities

Perceptions such as these are undoubtedly important in determining which cases go to jury trial and which to the judge.[65] Nevertheless, these perceptions' basis in reality is much more problematic. Indeed, one of the more remarkable lessons that empirical study has to offer the law is that virtually no evidence exists to support the prevailing ingrained intuitions about juries. In fact, existing evidence is to the contrary.

Admittedly, not much effective empirical work exists on the quality of the jury's performance, and there is even less on juries' performance as compared to that of judges. Studies on broad ques-

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[64] JEROME FRANK, COURTS ON TRIAL 110, 114, 116, 118, 122-23 (1949) (no footnotes omitted); *see* Skidmore v. Baltimore & O.R.R., 167 F.2d 54 (2d Cir.) (Frank, J.), *cert. denied,* 335 U.S. 816 (1948).

[65] *See* Galanter, *supra* note 8, at 227 (discussing key role of "jury knowledge").

tions regarding the jury are difficult to do, and correspondingly shaky to interpret.[66] But the evidence, such as it is, consistently supports a view of the jury as generally unbiased and competent, or at least so compared to a judge. The fact that jury and judge show a high degree of agreement is better supported.[67]

Research, for example, indicates that the strength of the trial evidence is the most important determinant of the verdict.[68] Evaluated over the run of cases, juries are good factfinders.[69] More specifically, research does not support a view of the jury as overly generous on awards,[70] frequently ignoring the law,[71] or institutionally unable to handle complex cases.[72] Related research indeed suggests that a jury could even outperform a judge, because the judge

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[66] *See* GUINTHER, *supra* note 63, at xvii-xxviii (stressing that the fundamental limitation arises from researchers' lack of access to real jurors); MACCOUN, *supra* note 6, at 18-23; Robert J. MacCoun, *Experimental Research on Jury Decision-Making*, 244 SCIENCE 1046 (1989).

[67] Of the various contesting standards for evaluating the jury's performance, comparison to a judge's performance seems the uniquely appropriate one for our purposes. *See generally* MOLLY SELVIN & LARRY PICUS, THE DEBATE OVER JURY PERFORMANCE 57-65 (1987) (discussing several different standards). *But cf.* Marianne Constable, *What Books About Juries Reveal About Social Science and Law*, 16 LAW & SOC. INQUIRY 353 (1991) (generally criticizing contemporary jury studies for focusing on the "law of the officials").

[68] *See* GUINTHER, *supra* note 63, at 101-02; SALLY M.A. LLOYD-BOSTOCK, LAW IN PRACTICE 48 (1989); MACCOUN, *supra* note 6, at 19-22; Valerie P. Hans, *The Jury's Response to Business and Corporate Wrongdoing*, LAW & CONTEMP. PROBS., Autumn 1989, at 177, 184, 194; Christy A. Visher, *Juror Decision Making: The Importance of Evidence*, 11 LAW & HUM. BEHAV. 1 (1987). "Indeed, the power of evidence is so well recognized by jury researchers that when studying processes other than evidence, they must calibrate the evidence to be moderate so that it leaves some variance to be influenced by the variables under study." MICHAEL J. SAKS & REID HASTIE, SOCIAL PSYCHOLOGY IN COURT 68 (1978).

[69] *See* VALERIE P. HANS & NEIL VIDMAR, JUDGING THE JURY 113-48, 245-47 (1986) (summarizing the literature); REID HASTIE ET AL., INSIDE THE JURY 230 (1983) (mock jury study); SAUL M. KASSIN & LAWRENCE S. WRIGHTSMAN, THE AMERICAN JURY ON TRIAL: PSYCHOLOGICAL PERSPECTIVES 207-16 (1988) (summarizing the literature and also attributing any jury shortcomings more to defective legal procedures than to jury's intrinsic nature); RITA J. SIMON, THE JURY: ITS ROLE IN AMERICAN SOCIETY 49-71 (1980) (summarizing empirical studies).

[70] *See* GUINTHER, *supra* note 63, at 169-72; *Edith Greene, On Juries and Damage Awards: The Process of Decisionmaking*, LAW & CONTEMP. PROBS., Autumn 1989, at 225, 246; *see also* Stephen Daniels & Joanne Martin, *Myth and Reality in Punitive Damages*, 75 MINN. L. REV. 1 (1990) (analyzing jury awards of punitive damages). At most, the research suggests that on average a jury would give an award 20% higher than would a judge. Harry Kalven, Jr., *The Dignity of the Civil Jury*, 50 VA. L. REV. 1055, 1065 (1964); *see* HANS & VIDMAR, *supra* note 69, at 161.

[71] *See* GUINTHER, *supra* note 63, at 224-27; HANS & VIDMAR, *supra* note 69, at 160-63; HASTIE ET AL., *supra* note 69, at 231-32; JAMES P. LEVINE, JURIES AND POLITICS 182-83 (1992). *Compare* Cecil et al., *supra* note 7, at 748-49 (jurors' difficulty in understanding instructions) *with id.* at 763-64 (reasons for diminished effect on verdicts).

[72] *See* GUINTHER, *supra* note 63, at 208-13; HANS & VIDMAR, *supra* note 69, at 250-51; KASSIN & WRIGHTSMAN, *supra* note 69, at 125-27; Cecil et al., *supra* note 7, at 733-36, 750-64; Peter W. Sperlich, *The Case for Preserving Trial by Jury in Complex Civil Litigation*, 65 JUDICATURE 394, 413-14 (1982).

is also human and groups typically outperform individuals by virtue of superiority in such tasks as recall of facts and correction of errors.[73]

The classic work in this area by Kalven and Zeisel addressed reliability (the ability to treat like cases alike) rather than the validity or correctness of jury decisionmaking. Their questionnaires to presiding judges in some 4000 actual civil jury trials in the 1950's yielded data showing a 78% agreement between judge and jury on liability.[74] When judge and jury disagreed, they exhibited no distinct pattern other than the juries' very small tendency to favor plaintiffs relative to judges.[75]

When compared to other human decisionmakers, the rate of agreement is more impressive than it first appears. This 78% agreement rate is better than the rate of agreement between scientists doing peer review, employment interviewers ranking applicants, and psychiatrists and physicians diagnosing patients, and almost as good as the 79% or 80% rate of agreement between judges making sentencing decisions in an experimental setting.[76] So although theory

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[73] *See* MacCOUN, *supra* note 6, at 22-23; Phoebe C. Ellsworth, *Are Twelve Heads Better than One?*, LAW & CONTEMP. PROBS., Autumn 1989, at 205, 205-07; *cf.* HASTIE ET AL., *supra* note 69, at 230 (group of jurors will outperform individual juror).

[74] HARRY KALVEN, JR. & HANS ZEISEL, THE AMERICAN JURY 63-64 (2d ed. 1971), *critically reviewed by* John Kaplan, Book Review, 115 U. PA. L. REV. 475 (1967); Michael H. Walsh, *The American Jury: A Reassessment*, 79 YALE L.J. 142 (1969). *See generally* Valerie P. Hans & Neil Vidmar, The American Jury *at Twenty-Five Years*, 16 LAW & SOC. INQUIRY 323 (1991).

[75] KALVEN & ZEISEL, *supra* note 74, at 63-64. The jury but not the judge found for the plaintiff in 12% of the cases, while the jury but not the judge found for the defendant in 10% of the cases:

JURY

		P	D
JUDGE	**P**	47	10
	D	12	31

See also Zeisel, *supra* note 7, at 69-70 (analyzing result); Galanter, *supra* note 8, at 204-05 (reporting more recent polls supporting similar result).

[76] *See* Michael J. Saks, *Enhancing and Restraining Accuracy in Adjudication*, LAW & CONTEMP. PROBS., Autumn 1988, at 243, 246-48 (based on work by Shari S. Diamond).

Data from the Netherlands suggest that even at the highest level of civil law systems, where elite experts neutrally render advisory opinions before high court judges decide civil cases, the rate of agreement is modest. The First Chamber of the Netherlands'

plausibly suggests some judge/jury differences—such as that juries, because of a need for compromise to produce a unanimous verdict, would tend to give plaintiffs more wins but less money[77]—the significance of any such differences seems to fade in actuality. Apparently, judge trial and jury trial combine to operate a decisionmaking system that is, at least in one sense, highly reliable.[78]

Much of the recent research has focused on jury verdicts, revealing trends such as the average award markedly increasing in re-

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highest court, the Hoge Raad, decides private civil law cases. JOHAN F. BRUINSMA, CASSATIERECHTSPRAAK IN CIVIELE ZAKEN (Cassation Proceedings in Private Law Cases) 155 (1988). Before deciding, the justices receive advisory opinions from a branch of the Attorney General's Department attached to the Hoge Raad, which is independent of the Ministry of Justice and is not charged with representing the government's position in Hoge Raad cases. *Id.* at 157-58. The Hoge Raad and this branch of the Attorney General's Department are regarded as the two most elite legal offices in the Netherlands. *Id.* at 155.

In the period from January 1, 1982, to July 1, 1983, covering 445 cases, the Hoge Raad disagreed with the advice of the attorneys general in 16% of the cases. *Id.* at 75. A study of cases decided from 1970 to 1974 found disagreement in 23.4% of the cases. *Id.* at 74. The explanation for the later, lower disagreement rate seems to be the increasing specialization by subject-matter of the attorneys general. *Id.* A breakdown of the later period's cases confirms this, as the rate of disagreement with the advisory opinions substantially increased (to 25%) for less experienced, less specialized attorneys general. *Id.*

Interpretation of these results depends on at least three additional observations. First, the Hoge Raad has no discretionary jurisdiction. Unlike the United States Supreme Court, it must decide all cases presented to it. *Id.* at 155. This prevents the court from filling its docket with cases selected for their difficulty, resulting in an expectation of greater than normal disagreement. Second, the court's jurisdiction reaches only questions of law, *id.*, so the noise of factual disputes does not inflate the rate of disagreement. Third, to the extent the Hoge Raad is deferential to the Attorney General's recommendations, the 16% disagreement rate understates the true disagreement rate.

Results from another jurisdiction also shed some light on the rate of judge/jury disagreement. In Japan from 1928 to 1943, in serious criminal cases, juries of twelve private citizens heard cases along with three trial judges and rendered advisory verdicts by majority vote. In 94.78% of the 460 cases, the judges accepted the jury's recommendation. Takashi Maruta, Jury Competence: The Japanese Case—An Analysis of the Criminal Jury Trials in Japan, 1928-1943, at 3-4 (1991) (unpublished manuscript).

[77] *See* GUINTHER, *supra* note 63, at 96-97. *But see* ROBERT E. KEETON, TRIAL TACTICS AND METHODS 302 (2d ed. 1973) (suggesting that the judge is more inclined to compromise than the jury).

[78] The encouraging results persist in category-specific research. *See, e.g.*, DANZON, *supra* note 27, at 42 (court system, including juries, is far from arbitrary in medical malpractice cases); Thomas B. Metzloff, *Resolving Malpractice Disputes: Imaging the Jury's Shadow*, LAW & CONTEMP. PROBS., Winter 1991, at 43 (jury's usual competence and fairness shown by empirical study of medical malpractice verdicts and insurer's pretrial evaluations); Neil Vidmar, *Medical Malpractice Juries*, DUKE L. MAG., Summer 1991, at 8, 12 (in experimental setting, jurors gave lower median awards than arbitrators).

cent years.[79] Even if accurate,[80] these observations do not bear on the issue of jury performance versus judge performance. Judges, who go wholly unobserved in this research, might be responding similarly to similar forces. For a specific example, Figure 3 presents from our data, in millions of 1989 dollars, the mean recovery in judge and jury trials of successful product liability cases over the calendar years covered by this study.[81] The similarity of the recovery trends suggests that the explanation for trends in awards lies somewhere other than in peculiarities of the jury system.[82]

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[79] *E.g.*, MARK A. PETERSON, CIVIL JURIES IN THE 1980s: TRENDS IN JURY TRIALS AND VERDICTS IN CALIFORNIA AND COOK COUNTY, ILLINOIS (1987) (also recounting previous studies by the RAND Institute for Civil Justice); *see* Michael Chiorazzi et al., *Empirical Studies in Civil Procedure: A Selected Annotated Bibliography*, LAW & CONTEMP. PROBS., Summer 1988, at 87, 152-57.

[80] Such research, *see, e.g.*, MICHAEL G. SHANLEY & MARK A. PETERSON, COMPARATIVE JUSTICE: CIVIL JURY VERDICTS IN SAN FRANCISCO AND COOK COUNTIES, 1959-1980, at 79-88 (1983) (discussing methodology), rests on relatively few cases from a few localities, *see* Galanter, *supra* note 8, at 223-24, and draws data only from possibly biased jury verdict reporters, *see id.* at 236-37, 249. *See also* Daniels, *supra* note 4, at 300-04 (problems with data); Hensler, *supra* note 42, at 56-57 (misinterpretation of data).

[81] The virtues of the product liability example include this category's substantial number of cases. Also, it is an area where the especially vivid results of jury verdict research, *see* Daniels, *supra* note 4, at 297-300, commonly lead to policy recommendations, *e.g.*, VISCUSI, *supra* note 9, at 87-116. Nevertheless, Figure 3 is representative of other categories, so that related ones such as other personal injury and diverse ones such as general contract exhibit similar patterns.

[82] The recovery data are difficult to interpret. First, judges and juries are deciding different sets of cases. However, the predominant selection effect here would seemingly be to lower the mean recovery before judges relative to the mean recovery before juries. *See infra* notes 94-95 and accompanying text. Second, as explained *supra* note 42, the data trim the highest product liability awards. However, awards in excess of $9,999,000 in then current dollars are very rare, comprising only one or two awards in each year except 1984-1986. Moreover, their proportional occurrence in judge and jury trials is roughly equivalent. Third, one must keep median recoveries in view. Over the years, however, judge/jury median recoveries here show the same general pattern as mean recoveries.

Mean Recovery vs. Termination Year:
Product Liability Cases

— Judge Trials   ·-+-· Jury Trials
Source: Administrative Office Data, 1979-1989
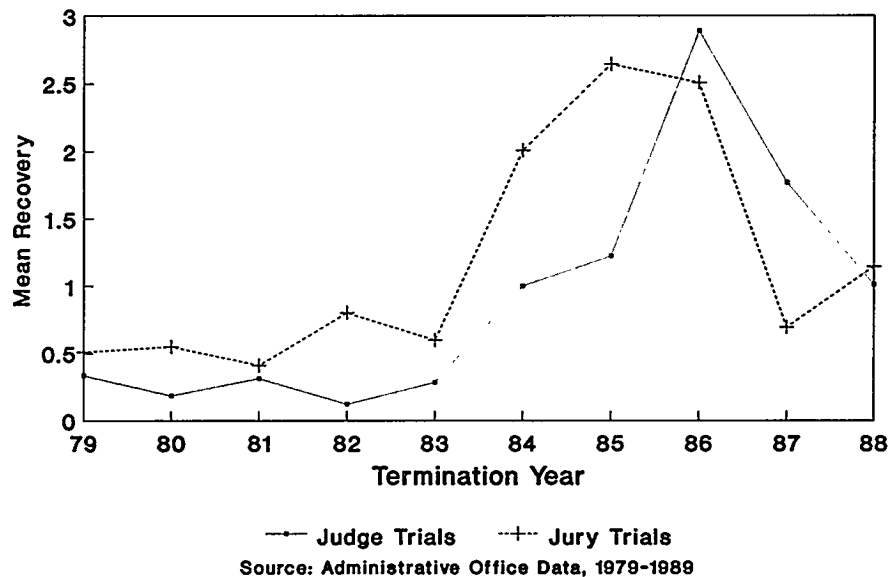
FIGURE 3

C. Misperception as Explanation

Our intention is not to survey accurately the lawyers' perhaps ill-founded perceptions of juries and judges, or to resolve or even detail the considerable uncertainties concerning actual judge/jury differences. Instead, our chosen task is simply to explore whether an explanation for our data could plausibly lie in these supposed or real differences.

As we have noted already, perceptions of the jury's bias and incompetency do not directly translate into the observed patterns of the data. Supposed judge/jury differences may have a role to play, but it is a subtle one. Also, as noted above in regard to actual differences, any such differences that create an advantage for one side or merely increase uncertainty for both should be bargained out, resulting in approximately equal win rates before judge and jury. Real judge/jury differences might exist, but theoretically they will not substantially reveal themselves in our data on win rates if the parties know about the differences.

Misperceptions of judge/jury differences must therefore constitute the truly potent explanation for our data. These misperceptions could comprise both real but unperceived differences and imagined but nonexistent differences. We can rule out some sweep-

ing generalizations. For example, any thought that all lawyers see the jury as consistently pro-plaintiff founders on data showing that the jury has not by any stretch become the exclusive trial mode. Nonetheless, one could formulate more refined misperceptions to explain the data plausibly, although one must ultimately be prepared to meet the theoretical objection that such misperceptions should tend to undergo correction eventually after litigants repeatedly observe the misperceptions' consequences.

Here is a prime example of drawing on misperception to explain the data. Consider the folkloric view of the judge as more reliable on liability, and of the jury as biased in favor of the plaintiff or the little person and biased toward generosity on damages for unliquidated claims. This persistent folklore would lead to the prediction that personal-injury cases would flow predominantly to jury trial, while the other cases such as contract would predominantly go to judge trial. Indeed, one would expect the preferences to be strong enough to overwhelm any effect the type of adjudication would have on settlement rate.[83] Thus the preferences should show up in percentage of judge trials. In fact, our data confirm this prediction with remarkable clarity: the percentage of judge trials is uniformly low for personal-injury cases and uniformly high for other cases.[84]

The special significance of this example is its suggestion that the litigants' perceptions driving the data may crystallize around groups of case categories, such as the group of non-personal-injury cases. Within groups, real judge/jury differences may be going unperceived, which would also affect the data. Along those lines—persistent misperceptions that exaggerate any judge/jury differences between broad groups of case categories or that ignore small differences within groups—we shall seek the most plausible explanation of the data.

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[83] *See generally* George L. Priest, *The Role of the Civil Jury in a System of Private Litigation,* 1990 U. CHI. LEGAL F. 161, 195-99.

[84] Other factors might also contribute to this phenomenon. For example, the personal-injury grouping tends also to correspond to those case categories in which prejudgment interest is unavailable in some jurisdictions. *See* DAN B. DOBBS, HANDBOOK ON THE LAW OF REMEDIES 165-74 (1973); CHARLES T. McCORMICK, HANDBOOK ON THE LAW OF DAMAGES 213-27 (1935); JOHN F. O'CONNELL, REMEDIES IN A NUTSHELL 64-65 (2d ed. 1985). As our data show that judge-tried cases consume more time from filing to termination, *see supra* note 52, plaintiffs in this group would have an added incentive to opt for a jury. *Cf.* STEPHEN J. CARROLL, JURY AWARDS AND PREJUDGMENT INTEREST IN TORT CASES (1983) (juries might implicitly award prejudgment interest).

V

OUR EXPLANATION

The data make one thing clear: more is at work here than judge and jury treating a similar flow of cases similarly. Instead, either the judge and the jury act differently as adjudicators or the parties differentially filter the cases flowing to them, or both. This realization suggests an analytic approach, represented by Figure 4. Thus, the data clearly preclude the upper-left box as an explanation.

Discussions of the data with colleagues have shown that on their own they intuit this framework. When confronted with, say, the fact that prod-med exhibits very high win ratios, colleagues elect one or the other of the framework's dimensions. For example, using some real-world insight to turn the popular view on its head, some have formulated a version of what we shall call Stab #1: "Judges and juries are different. Juries tend to blame the victims.[85] So naturally plaintiffs win more often before judges."

Speaking more generally, we can place this stab in the upper-right box: judge and jury treating an undifferentiated case mix differently. By itself this is a sorely inadequate explanation. First, any such stab initially appears implausible as a general explanation because it runs against the popular view. Second, existing empirical observations do not support the contention that judges and juries are in fact so very different, even if one could theorize why they would be different in the required ways. Finally, explaining why litigants have failed to adjust to such judge/jury differences would be difficult; plaintiffs should be less enamored of jury trials and defendants should embrace such trials.

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[85] On this human tendency under some conditions, see Aaron Abbott & Frank Heckman, *Juries and Child Death Cases*, CASE & COM., Nov.-Dec. 1989, at 3, 6-8.

Analytic Framework

Treatment

	same	different
same	cannot explain any differences in data	Stab #1
different	Stab #2	X

(left axis: **Cases**)

FIGURE 4

Other colleagues infuse an academic notion to formulate Stab #2: "Parties send a different case mix to judge and jury. Only in weak cases does the plaintiff opt for the crazy jury. So naturally plaintiffs win more often before judges."

This stab lies in the lower-left box: judge and jury similarly treating a differentiated mix of cases. Although this approach is more sophisticated, Stab #2 is still subject to three basic rebuttals, which upon reflection also apply to Stab #1. First, if in strong cases the plaintiff seeks the sanity of judge trial, then why does the defendant not seek the less reliable jury trial? Indeed, given that each side in our adversary system is seeking an advantage and is able to demand a jury, Stab #2 would lead to the inaccurate conclusion that virtually every trial should be a jury trial. Second, if parties do send a different case mix to judge and jury, then why does settlement bargaining not offset the difference, leaving for trial cases of similar strength? Indeed, accepting the academic view fully would lead to the conclusion that, even if they hear very different sets of cases, both judge and jury trial should exhibit approximately the same equilibrium win rate, which is again not accurate. Third, if this stab at an explanation is valid, then why do the data for the other case categories not exhibit the same phenomenon? The low win ratios that typify other personal-injury categories offer a strong rebuttal.

Nevertheless, these stabs at explaining the results hold promise. They are probably both at work, but they need more rigor, which will unavoidably complicate them. The entire case-filtering process requires more detailed consideration to determine how it yields, despite the selection effect, a residue of judge-tried cases different from the residue of jury-tried cases. We need to consider the decisional process that might involve some differences in treatment by judge and jury. Throughout we shall emphasize the essential role of misperception in producing the observed results. Moreover, we must treat the different case categories separately to account for the odd patterns of the data.

Thus, we focus on the lower-right box in Figure 4: a differentiated mix of cases going to judge and jury, with the parties largely imagining judge/jury differences, but also with judge and jury unperceivedly acting slightly differently as adjudicators. We shall first describe the general mechanism of filtering and deciding tried cases. Next, recognizing that the case characteristics that trigger a jury request differ for a personal-injury case and a non-personal-injury case, we shall explain how parties alter their case-mix filter according to case group, as well as how adjudicators might alter their decisional behavior according to category; we shall also suggest why the settlement process does not obliterate the effect of these processes in our data. Finally, we shall use our explanation to draw lessons from the now expressive data.

A. Mechanism Generalities

Once a dispute becomes a lawsuit by filing the complaint with the court, the mechanism that produces our data goes through three phases. The initial phase is the *routing process* by which the parties choose the mode of trial. Either side can opt for jury trial, but the requesting party must do so promptly.[86] Plaintiffs' preference might dominate this choice of trial mode in practice, because defendants might, with fair consistency, be content with judge trial.[87] Nevertheless, legally the two sides have an equal choice. The choice likely relies heavily on popular perceptions of judge/jury differences. The party's urge might be to avoid the jury's supposed pro-plaintiff bias and its incompetence, or to seek a trait of that sort. The result of the routing process may be a judge/jury case mix differentiated on the basis of strength on the liability issue.

Next, consider the *settlement process*. Bargaining often occurs before case filing and might extend into the trial and beyond. Our

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[86]   *See* Fed. R. Civ. P. 38, 39.
[87]   *See* Merritt, *supra* note 60, at 27.

concern here, however, is with the role of bargaining in further filtering the case mix to produce the residues of judge-tried cases and jury-tried cases. The academic view has much to say on this process. It suggests that parties should bargain out known judge/jury differences, which would tend to leave residues of cases with approximately equal win rates. And most case categories other than prodmed exhibit similar win rates. Misperceptions, however, could impede the equalizing tendency. Thus, the routing and settlement processes would work together to filter the case mix, possibly resulting in very different sets of cases reaching decision before judge and before jury.

Finally, there is the *decisional process*. In reality, judge and jury do not appear to operate very differently. Although they probably show some differences, and although the parties probably do not perceive some of these differences, the primary explanatory effect likely lies in the case-mix filter operating through the routing and settlement processes.

For the purposes of our explanation, the key process of the three is clearly the settlement process. The critical insight is that this process operates under parties' misperceptions of judge/jury differences. Misinformed bargaining produces the odd results in the data. Indeed, we could make the unrealistic assumption that the parties randomly choose between judge and jury trial, so that the routing process has no impact on the data. That is, we need not rely on assumptions about routing, such as that only the plaintiff chooses mode of trial or that any hypothesized filtering effect results from the routing process. Likewise, we could make the unrealistic assumption that judge and jury are identical adjudicators, so that the decisional process has no impact. That is, we need not rely on assumptions of unperceived judge/jury differences. Instead, our critical contention is that, somewhere along the line from dispute to judgment, misperceptions lead to a set of unsettled cases reaching decision by the judge that produces a win rate different from the jury win rate.

B. Case-Category Particulars

Explaining how parties alter the mix of cases reaching judges and juries requires identifying factors that make judge or jury attractive or repellent. The data suggest that these factors vary across case categories. Plaintiffs might prefer jury trial in some categories out of fear of judicial hostility or out of belief in favorable jury bias. They may prefer judge trial in some cases because they are more confident of victory there than before a jury that they view as less reliable or as maintaining a bias that could turn on them in a factu-

ally unappealing case. By varying the degree to which such factors in combination apply to particular case categories, we can develop an explanation for the observed results.

1. *Product Liability and Medical Malpractice Group*

The story in our data that most loudly calls for explanation concerns the hot areas of tort law. For product liability [88] and medical malpractice,[89] the plaintiff does far better before judge than before jury, especially as compared to the other categories. Figure 5 compares the win rates in prod-med with the average of the win rates in the other categories. Surprise turns to mystery when we add the datum that the parties nevertheless select judge trial in only about 10% of product liability and medical malpractice trials.

Win Rates in Judge & Jury Trials:
Product, Medical & Other
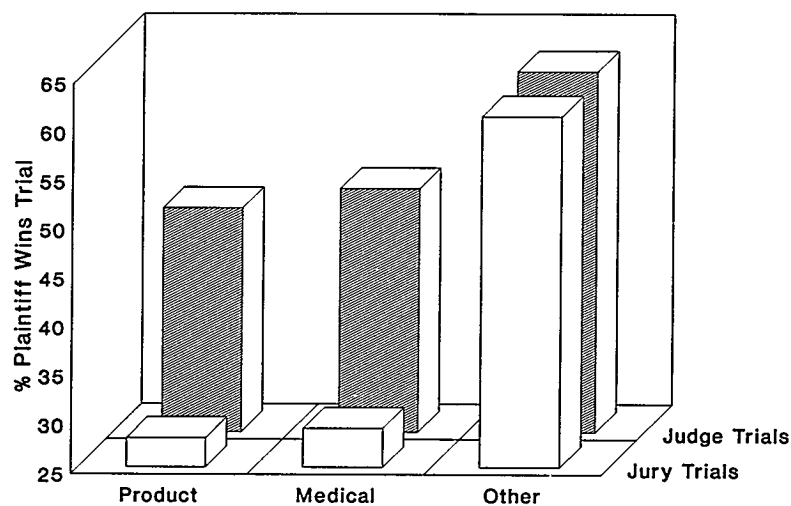
Source: Administrative Office Data, 1979–1989

FIGURE 5

Because prod-med cases often involve controversial claims and big stakes,[90] we explain this strangeness by theorizing that the par-

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[88]   *See generally* Henderson & Eisenberg, *supra* note 27.
[89]   *See generally* DANZON, *supra* note 27.
[90]   If one combines judge and jury cases, product liability and medical malpractice cases have by far the two highest mean recoveries of all thirteen categories considered here. *See infra* Appendix B; *see also* Daniels & Martin, *supra* note 4, at 340-42 (state data showing product liability and medical malpractice jury cases have higher median verdicts than other tort categories). Indeed, recoveries are much greater in prod-med than in other personal-injury cases such as motor vehicle, even controlling for injury and other case characteristics. *See* DANZON, *supra* note 27, at 53-57.

ties, who might not view juries as biased favorably, nevertheless see judges in general (or the assigned judge in particular) as consistently more hostile to these kinds of claims on liability and conservative on damages. Therefore, the defendant would fairly uniformly prefer the judge, and the plaintiff would prefer a jury[91]—except in those few cases, very strong on liability, where the risk-averse plaintiff sees the jury's unpredictability as just too great a risk to run. This perception of relative judicial hostility, combined with a view of the jury as a "noisy" decisionmaker, would create a very skewed case-mix filter. Thus, the resulting data should reflect a much better win rate for judge than for jury, while nevertheless jury trials would far outnumber judge trials.[92]

An additional filtering effect could be at work in big-stakes prod-med cases. The jury's perceived (or, for that matter, real) variability on damages could be impeding settlement in very weak jury cases by causing the parties to value the claim differently. The plaintiff foresees the possibility of a mega-verdict, and so values the case highly enough to preclude settlement. The plaintiff's premium may derive from the occasional risk-preference of a plaintiff (or more likely that of the plaintiff's contingent-fee attorney), from over-optimism on expected return caused by high-end possibilities, or even from an economically irrational pot-of-gold mentality.[93] The end effect would be to lower the jury win rate.

Thus, by some filtering devices operating during the routing and settlement processes, a stronger residue of cases meet judge

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[91]	This appears to be true even in medical malpractice cases. *See* 1 DAVID W. LOUISELL & HAROLD WILLIAMS, MEDICAL MALPRACTICE ¶ 11.26 (1991). To the extent such considerations vary between medical malpractice and product liability cases, we would have to change the details of our explanation, but we could do so within the same general explanatory mechanism.

[92]	*See infra* notes 100-01 and accompanying text (criminal analogy). Given that a party can choose the mode of trial, the party might strategically try to choose in a way that sends the adjudicator a favorable signal, saying in effect that "I chose you as adjudicator because the strong side usually chooses you." In civil cases, signalling is in the abstract impossible: if a jury is chosen, then the jury must act naively as it does not know which side chose jury trial, or at least does not know the significance of the signal; if a judge is chosen, then both sides sent the same signal. However, as the choice of mode of trial becomes in reality more predominantly a one-sided choice by the plaintiff, some signalling to the judge becomes possible. This would nonetheless be a subtle signal in a complex setting. Signalling would also have offsetting effects on win rate as, mindful of the signal, the plaintiff alters the flow of cases to judge trial while the judge offsettingly alters decision on those cases. Moreover, the settlement process should take the signal into account and thereby obliterate its effects on win rate. Therefore, we can with justification overlook the secondary effects of signalling. Indeed, for like reasons we can overlook strategic behavior of the judge, such as any attempt to reward the plaintiff for waiving jury trial.

[93]	*See* U.S. DEP'T OF HEALTH, EDUC. & WELFARE, REPORT OF THE SECRETARY'S COMMISSION ON MEDICAL MALPRACTICE 33-34 (1973).

trial than jury trial. A sharp difference in win rates results. Figure 6 summarizes the processes for prod-med cases.

Case Flow
Product Liability & Medical Malpractice

As in all pi cases, trust in attributes of judge leads to sprinkling of judge trials	**Filed Disputes**	As in all pi cases, belief in more favorable jury leads jury trial mode to dominate
Routed to Judge Trial 10% of cases	expectation of judge bias and jury incompetence skews routing process	**Routed to Jury Trial** 90% of cases
Settlement Process	similar misperceptions skew settlement process	**Settlement Process**
Residue of Tried Cases win rate • 0.5	dissimilar win rates observed because of case-mix filtering	**Residue of Tried Cases** win rate • 0.3

FIGURE 6

We further theorize as to mean recovery ratios in prod-med cases. First, for any group of tried cases, one might expect that a higher win rate pushes toward a lower mean recovery.[94] Second,

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[94] Initially, regarding the decision to pursue a disputed claim, one might expect such an inverse relationship to exist in any group of disputes. *See* Wittman, *supra* note 13, at 188. The settlement process theoretically should not remove it in the subset of tried cases. *See* Wittman, *supra* note 20, at 335-36. Additionally, regarding the split into judge and jury trials, one would expect, all else being constant, that the inverse relationship would survive.

the bigger cases tend to go to the jury rather than the judge.[95]
Third, the judge may in fact be somewhat more restrained in award-
ing damages than the jury.[96] All else being constant, these three
pressures suggest that the few prod-med cases going to judges,
while resulting heavily in plaintiffs' wins, will produce relatively
small damages awards.

All else is not constant, however, in selection effect or in the
judge/jury choice, as this Article shows by Tables 4 and 5. The in-
verse relationship thus remains at most a tendency frequently ob-
scured by other pressures.[97] Some of those other pressures might
be rather specific to the case category. For example, one might ex-
pect a more direct relationship of win rate and mean recovery in
medical malpractice cases, because the very size of injury might im-
ply a greater chance of malpractice.[98] The normal inverse relation-
ship between win rate and mean recovery, which would prevail in
product liability cases, would therefore disappear in medical mal-
practice cases.[99] This in turn would mean a higher mean recovery
ratio for medical malpractice than for product liability, which is in
fact true.

Our data support all these suppositions: prod-med cases have
very low percentages of judge trials, very high win ratios, and con-
trasting mean recovery ratios. Moreover, familiar data from serious

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[95]   Plaintiffs might simply seek to avoid the more expensive jury in smaller cases. *See
supra* note 59 and accompanying text. Also, the plaintiffs' perception of jury generosity
in potentially big cases should create resistance to any urge toward judge trial, causing
the filtering mechanism to work most effectively in sending smaller cases to the judge.

Regardless of explanation, we can support the statement in the text. The Adminis-
trative Office data show for most cases the plaintiff's demand for damages in dollars.
We can calculate a mean demand for those cases in the refined data set that show both
an amount demanded and a judgment for plaintiff or defendant after full trial. Appen-
dix B gives the mean demands for judge and jury trials, as well as N(demand), which is
the number of judge and jury trials used to compute those mean demands. We could
then add to our statistics a "mean demand ratio" for each case category, defined to be
the mean demand in judge trials divided by the mean demand in jury trials. Suffice it to
say that for all but one (other personal injury) of the thirteen case categories, the mean
demand ratio is less than one, indicating that demands are larger in jury trials.

The mean demand ratio for product liability is 0.34 and for medical malpractice is
0.92. Interestingly, the mean demand ratios are almost uniformly lower in the non-
personal-injury categories (averaging 0.58) than in the general-personal-injury catego-
ries (averaging 0.77).

[96]   *See supra* note 70.

[97]   *See* Priest, *supra* note 13, at 237-40. Moreover, the use of regression to control
for mean recovery, as suggested by Wittman, *supra* note 20, at 336-37 (semble), does not
substantially diminish the overall negative impact of jury trial on win rate.

[98]   *Compare* DANZON, *supra* note 27, at 38 (the more severe the injury in medical mal-
practice cases, the more likely it was caused by negligent behavior) *with* VISCUSI, *supra*
note 9, at 52-54 (contrary product liability data).

[99]   Support for this lies in a comparison of the relationships between overall win
rate in fully tried cases in the refined data set (without the distorting Bendectin cases
described *supra* note 37) and the amount demanded in those tried cases:

criminal cases reassuringly provide a confirmatory analogy to our data. Accused felons effectively get to choose the mode of trial and overwhelmingly choose trial by jury, at a rate in the neighborhood of 80%. Yet they fare far better before judges, with their conviction rate being about 50%, compared with a jury conviction rate of about 80%.[100] The best explanation involves the assumption that the many guilty defendants seek the jury's supposedly noisy processing, while the innocent few prefer the supposedly more reliable judge trial. Thus, although criminal defendants might perceive the judge as a less favorably inclined adjudicator, the judge's small flow of cases is skewed to yield a lower conviction rate.[101]

—*— Medical Malpractice   —□— Product Liability
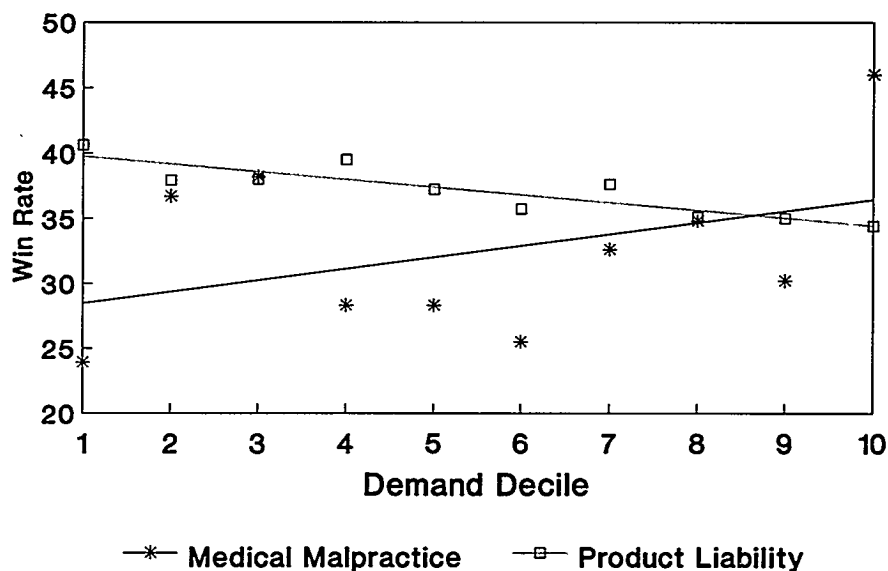
FIGURE 7

Incidentally, a direct relationship between overall win rate and the amount demanded, as for the medical malpractice category, characterizes the general-personal-injury group, except for FELA and assault-libel-slander. An inverse relationship characterizes the non-personal-injury group other than torts to personal property.

Adding the mean demand ratio, *supra* note 95, to the picture provides some support for our explanation. A substantial departure of the mean demand ratio from the usual background tendency to give big cases to the jury and small cases to the judge suggests that different sets of cases are reaching judge and jury. In the medical malpractice category, where large claims correlate with strong cases, the relatively high demand ratio and the direct relationship shown by this graph imply that the strong cases are reaching the judge. Similarly, in the product liability category, where small claims correlate with strong cases, the very low demand ratio and the inverse relationship again imply that the strong cases are reaching the judge. The consistent feature, and the explanatory mechanism, seems to be filtering on the basis of the case's strength on the liability issue.

100 *See* Gerald D. Gay et al., *Noisy Juries and the Choice of Trial Mode in a Sequential Signalling Game: Theory and Evidence*, 20 RAND J. ECON. 196, 206-09 (1989).

101 *Id.* at 198, 211 (innovative analysis of criminal data); *cf.* Harold W. Elder, *Trials and Settlements in the Criminal Courts: An Empirical Analysis of Dispositions and Sentencing*, 18 J.

2. *General-Personal-Injury Group*

For personal-injury categories other than prod-med, the data follow a more predictable pattern. Indeed, the challenge instead becomes explaining why categories such as FELA, assault-libel-slander, airplane personal injury, and marine personal injury are ordinary enough not to share the extreme characteristics of product liability and medical malpractice. We theorize that in these less controversial and more traditional tort categories the parties generally see the judge as neutral, rather than hostile to claimants. Meanwhile, the parties' perception of the jury as usually biased favorably toward these kinds of claims, especially as to amount of damages, overcomes most fear of jury noise. In contrast to the prod-med group, where the perception of jury incompetence works to skew the case-mix filter, here the operative force is the perception of jury bias.

Accordingly, the defendant would prefer the judge, and the plaintiff would usually opt for the jury. As a result, we should observe a low percentage of judge trials.[102] Indeed, where the jury's pro-plaintiff bias is seen as especially strong, as in FELA cases, the percentage of judge trials should be particularly low.

As to differential case mix, we would not expect pronounced effects, so that the win ratio would not depart far from one. In other words, we should observe, despite the huge number of cases in our sample, a substantial equivalency of result before judge and jury. Nevertheless, a small difference should result from the following process. Although some plaintiffs still might abandon the usually favorable jury for the more reliable judge in strong cases, other plaintiffs would prefer a more dispassionate judge in cases where unappealing facts might alienate a jury. More generally, the plaintiff would abandon the jury wherever judicial neutrality seems important. If, however, the judge disappoints relative to expectations—that is, if the judge turns out to be less neutral or more like the jury than expected—then the settlement process based on such expectations will yield a case mix skewed to produce a win ratio somewhat below one.

Note that here the desire for a favorable judge draws the plaintiff in some cases to judge trial, rather than a fear of jury noise driving the reluctant plaintiff to a relatively hostile judge in very strong cases. Accordingly, the mix of these general-personal-injury cases routed to the judge differs from the mix of prod-med cases routed

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LEGAL STUD. 191 (1989) (initial attempt to apply selection effect theory to criminal process).

[102] *See supra* text accompanying note 84.

to the judge. Also, the settlement process proceeds on different assumptions. Indeed, the settlement process alone explains the low win ratio for these general-personal-injury categories, while the routing process, shaped in part by the greater belief in jury generosity on damages, might help segregate strong prod-med cases for judge trial. The end result of these distinctions is a marked divergence in win ratio between the general-personal-injury group and the prod-med group.

We would not expect mean recovery ratios in the general-personal-injury categories to follow the fairly consistent pattern of win ratios. Here the cases filtered for judge and jury trial would be subject to conflicting pressures on mean recovery.[103] The mean recovery ratio thus should show no distinctive pattern.

Again, our data bear out all these predictions: a small percentage of judge trials, a win ratio around 0.9, and an erratic mean recovery ratio for the categories of FELA, assault-libel-slander, airplane personal injury, and marine personal injury.

The only pronounced oddity in the personal-injury data comes in the categories of motor vehicle and other personal injury, where the win ratios are respectively 1.15 and 1.04. Here the explanation might be that these categories share the characteristics of the two subgroupings of personal-injury categories—prod-med, on the one hand, and FELA, assault-libel-slander, airplane personal injury, and marine personal injury, on the other hand. This is plausible for the category of other personal injury, where its catch-all nature would imply shared characteristics. For motor vehicle cases, the plaintiff might see the judge as hostile to run-of-the-mill state law cases clogging federal court dockets but not to the premises of motor vehicle liability, and might see the jury as a more neutral and hence normally a relatively more favorable adjudicator deciding between two similarly situated parties. Some plaintiffs might turn to the judge in fairly strong cases out of fear of the jury's incompetent unpredictability and trust in the judge's fundamental neutrality, an effect we do not observe substantially in the traditional tort categories because of the plaintiffs' perception that pro-plaintiff jury bias works advantageously even in strong cases. Other plaintiffs might turn to the judge in occasional cases with the view that the jury there might be more anti-plaintiff than the judge, an effect we do not observe substantially in the hot tort categories because of the plaintiffs' perception that anti-plaintiff judicial hostility prevails across the board. That is, plaintiffs might send the judge both fairly strong cases to

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[103] Some of the pressures on mean recovery ratio discussed above, but not the others, could tend to raise the mean recovery ratio above one. *See supra* notes 94-99 and accompanying text.

avoid jury noise and also occasional cases to seek a supposedly more favorable adjudicator. Alternatively, while the judge in general-personal-injury cases might disappoint expectations, so might the jury in motor vehicle cases, just as the jury apparently does in prod-med cases.[104] Under either explanation, the result would be a win ratio falling between the ratios for the two other subgroupings of personal-injury categories.

3. *Non-Personal-Injury Group*

For this group of categories—negotiable instruments, torts to personal property, torts to land, general contracts, and fraud cases—our data are even less surprising. We again theorize that the parties generally see the judge as a neutral adjudicator. In contrast to the general-personal-injury group, however, here the plaintiff sees the jury as much less consistently biased favorably toward the plaintiff, with less room to play generously with damages.[105]

Accordingly, the defendant still might prefer the judge, but so might the plaintiff, especially in the cases, strong on liability, where the plaintiff seeks the perceived reliability of judge trial. The plaintiff would abandon the judge only where the jury appears to be the more favorable adjudicator—but if the jury disappoints, the settlement process will produce a relatively lower jury win rate. The powerfully explanatory notion here seems analogous to that for the general-personal-injury group: occasional abandonment of the usual adjudicator (here the judge, rather than the jury) for the adjudicator expected to be more favorable, a settlement process proceeding on the basis of those expectations, and disappointment of those expectations so that the win ratio departs slightly from one.

Thus, we should observe a high percentage of judge trials. We again would not expect pronounced effects on win ratio. But the mix of cases reaching the judge should be relatively strong on liability, and inversely that mix should be relatively small on mean recovery.[106] Therefore, the win ratio should consistently be somewhat above one, while the mean recovery ratio should remain well below one.

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104   *See* JOHN G. FLEMING, THE AMERICAN TORT PROCESS 111-12 (1988) (arguing that concern about insurance premiums has come to dampen jury's pro-plaintiff bias in motor vehicle cases). On the substantial role of the insurance industry in generating public perception of a prod-med crisis since the mid-1980's, see Daniels, *supra* note 4; and on the slightly dampening effect of such a perception on jury generosity, see Greene et al., *supra* note 7.

105   Informal surveys show little difference in perceptions of success before judges and before juries in contract cases. In each of the three surveys, *supra* note 12, one cannot reject the hypothesis of no perceived difference. Many respondents predicted a 50% win rate for both judge and jury in contract cases.

106   *See supra* notes 94-99 and accompanying text.

Our data conform: a high percentage of judge trials, a win ratio around 1.1, and a low mean recovery ratio for the non-personal-injury categories.

4. *Intra-Group Effects*

Intuition suggests that the parties' perceptions driving the case-mix filter operate mainly at a gross level. Parties tend to stereotype in terms as gross as, say, all five non-personal-injury categories as one group. Our data are consistent with that intuition. An overall constancy of data points exists within each of the three large groups—rather remarkable constancy as to percent judge trials, fair constancy as to win ratios, and some constancy as to mean recovery ratios. In other words, parties seem to be sending a similar mix of cases to the judge and to the jury in each of the categories within any group.

Nonetheless, small differences in win ratio exist within each group. To the extent that these do not come from differences in case mix or random fluctuations, they reflect small but real judge/jury differences between categories. Our data could suggest, for example, that judge trial is a slightly more favorable mode of trial in negotiable instruments cases than is judge trial in other contract cases, as compared to the jury.

Possibly, parties' altering category-by-category the case mix going to two virtually identical modes of trial could be the sole explanation of the patterns of our data. Yet it is hard to believe that judge and jury exhibit absolutely no systematic but unperceived differences as adjudicators. Moreover, it would be hard to hypothesize the misperceptions that would drive the required case-mix filter if the difference in win ratio were significant. For example, it would be mildly strange for settlement to give the weak negotiable instruments cases to the jury and the strong ones to the judge more selectively than other contract cases. Thus, differences in case mix are probably not the sole explanation, and actual judge/jury differences have a minor explanatory role.

5. *Summary*

Returning to the analytic framework to summarize, we believe that persistent misperceptions exaggerating any judge/jury differences and varying between broad groups of case categories skew the case mix and thus largely explain the patterns of our data. But persistent misperceptions that ignore small but real judge/jury differences within those groups are probably also at work to help explain the finer variations in our data. Therefore, our explanation lies in the lower-right box of the framework in Figure 8.

Explanatory Framework
Treatment

|	same	different
same	cannot explain any differences in data	cannot explain the differences between groups
different	might not explain the differences within groups	explains big and small differences in data

(Cases)

FIGURE 8

Having located anew our explanation, we now formulate it expressly. Judges and juries exhibit, across case categories, some differences as adjudicators. Parties use their intense perceptions of differences to choose between judge and jury trial, but mainly differentiate on the gross level of broad groups of categories. At that level they exaggerate any judge/jury differences, and so send varying percentages and very different mixes of cases to the two modes of trial. Within groups, the parties tend to ignore finer judge/jury differences, and so generally fail to differentiate the case mix at this secondary level.

After so invoking the routing and decisional processes, we must face the question of why the key settlement process does not obliterate in our data the effects of our explanation. After all, if the parties accurately perceive any judge/jury differences, then, once they choose the mode of trial, their knowledgeable bargaining should leave on both the judge and the jury calendars a residue of unsettled cases exhibiting approximately the same equilibrium win rate. We know that this result does not eventuate, so therefore the parties must inaccurately perceive the realities of judge and jury trial.

Personal-injury plaintiffs may opt for judge trial in circumstances where they view the judge, relative to the jury, as hostile (prod-med group) or neutral (general-personal-injury group). The judge, however, may turn out on average to be more like the jury than expected, and hence either much less hostile or less neutral as

a relative matter. These misperceptions thus produce win ratios that are respectively very high and moderately low, as the judge performs either much more favorably or less favorably, relative to the jury than expected. Similarly, non-personal-injury plaintiffs may opt for jury, trial when they view the jury as more favorable. The jury may turn out on average to be more like the judge than expected, producing a moderately high win ratio.

The more pronounced effect in the prod-med group may result from the greater degree of misperception. In particular, not only may the judge turn out to be less hostile than expected, but even more so the jury may turn out to be less favorable than expected, as suggested by the unusually low jury win rates in prod-med cases. These two disappointments would whipsaw the win ratio to atypical heights.[107] Further, the exceptional power of the filtering mechanisms to route strong prod-med cases to the judge and weak ones to the jury may accentuate the effect.

There still remains the question of why these misperceptions do not eventually undergo correction, as litigants repeatedly observe the consequences of their misperceptions. For our explanation to work, we need not merely misperceptions of judge/jury realities, but persistent misperceptions.

We believe that persistence of misperceptions is more than plausible in this context. Surprising and longstanding misperceptions about the legal system are not uncommon.[108] In this area, elitist perceptions of a biased and incompetent jury system seem to conform to the natural order of things and can even be comforting. Empirical evidence to the contrary has been slow in accumulating and is still far from overwhelming. The data on trial results have not been widely reported and, as we are about to show, remain frustratingly ambiguous once available. Finally, many lawyers prefer to rely on personal experience and anecdote.[109] All in all, lawyers' misperceptions of judge/jury differences have understandably prevailed for a long time.

C. Lessons from Data

Repeated mention of misperceptions begets thoughts of lessons to draw from the data. There are a few.

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[107] *Compare supra* note 58 (noting possible time trend in prod-med cases) *with supra* note 104 (suggesting possible cause).

[108] *See* Theodore Eisenberg & James A. Henderson, Jr., *Inside the Quiet Revolution in Products Liability*, 39 UCLA L. REV. 731 (1992); Theodore Eisenberg & Stewart J. Schwab, *What Shapes Perceptions of the Federal Court System?*, 56 U. CHI. L. REV. 501 (1989); Henderson & Eisenberg, *supra* note 27, at 541; Wolfson, *supra* note 62. *See generally* THOMAS GILOVICH, HOW WE KNOW WHAT ISN'T SO (1991).

[109] *See generally* Galanter, *supra* note 8, at 227-51.

The biggest lesson is one of humility. Practitioners and policy-makers who believe that plaintiffs as a group always do better before juries are wrong. Academicians who predict that judge and jury win rates equalize are wrong. We can say that both prevailing intuition and prevailing theory will find in the data no direct evidence in support.

On the more mundane or practical level, however, lessons become more elusive. The reason for ambiguity lies, of course, in rigorous selection effect theory. For example, because they hear different sets of cases, one cannot conclude from the very high win ratio in the product liability and medical malpractice group that the judge is more favorable toward plaintiffs than the jury. Instead, the high win ratio probably reflects only the selection of tremendously strong cases for the judge, a set on which jury verdicts might have been no different from, or possibly more favorable than, judicial decisions. Once one realizes that the parties are altering the case-mix input to judge and jury trials, one cannot with great confidence draw lessons from the output.

We can, however, construct the most plausible explanation of the data, as we have tried to do in this Article. We have formulated likely perceptions of judge/jury differences, surmised that these are exaggerations of a reality in which judge/jury similarities dominate, and demonstrated how these persistent misperceptions would generate the major part of the data's patterns. A minor explanatory role falls to unperceived real judge/jury differences.

If one accepts our explanation, lessons start to emerge. Returning to the same example of product liability and medical malpractice, one could conclude that the jury is less of an advantage for plaintiffs, and the judge less of a disadvantage, than the parties think. That realization should affect the terms of settlement.[110] Moreover, if only one side comes to that realization, that side could manipulate the judge/jury choice to its bargaining advantage.[111]

Furthermore, if we are correct that to some degree judge and jury are in actuality unperceivedly different in handling similar cases, additional lessons emerge. For example, if judge trial is a relatively more favorable mode of trial in negotiable instruments cases than is judge trial in other contract cases, then litigants have something to learn.

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[110] *See* GUINTHER, *supra* note 63, at 44.

[111] Litigants' most valuable lessons lie, of course, in the settlement process. Trials are rare events, and so lessons on whether to try a case to judge or jury will matter to few persons. Ubiquitous bargaining, however, proceeds in the shadow of trial results, making their interpretation worthy of study. *See* Galanter, *supra* note 8, at 209-12, 227-32, 241-42, 257.

Even if the specifics of our explanation are incorrect, we believe we have fixed upon a dominant explanatory mechanism: parties' persistent misperceptions of judge/jury differences, with the consequence of a case selection reaching judge trial that is different from the case selection reaching jury trial.

CONCLUSION

Our data on outcomes in judge and jury trials hold surprises, confounding both intuitionists and theorists. Plaintiffs as a group do not always fare better before juries, nor do they fare equally as well before judges and juries. Reality is much more complicated, and certainly neither litigants in weighing the choice between judge and jury nor policymakers in weighing drastic proposals to restrict that choice should proceed on the simplistic prevailing views of reality.

The data most prominently show that in product liability and medical malpractice cases, the plaintiffs win far more frequently in the few judge trials than in the many jury trials. This discovery should alter prevailing views. Therefore plaintiffs should learn to waive jury trial, while defendants should finally begin flocking to the anti-plaintiff jury.

Wrong! Few would accept that direct inference, and rightly so. It flies in the face of not only every time-honored stereotype but also all well-established facts. It is nevertheless representative of a variety of easy explanations of the data. The choice is between a direct, implausible conclusion of that sort and an indirect, complex alternative. Recognition of such a stark choice, while driving this Article's pursuit of the alternative conclusion, should also induce the necessary tolerance for sometimes seemingly thin motivations, zigzagging explanations, and ambiguous effects.

The most plausible explanation of the data would acknowledge that judges and juries exhibit some differences as adjudicators. However, parties, through their lawyers, perceive big differences and so send varying percentages and very different mixes of cases to the two modes of trial. Persistent misperceptions of category-based judge/jury differences allow certain effects of actual adjudicator differences and of differential case selection to survive the settlement process and so generate the patterns of our data. Uncovering the operative misperceptions—especially those that imagine nonexistent differences but also those that ignore real differences—can lead to lessons profound and practical.

APPENDIX A
Plaintiff Win Rate in Judge Trials and Jury Trials
Federal Court Cases: 1979-1989

		Judge Trial		Jury Trial	
	Admin. Off. Code	N (unrefined)	Win Rate	N (unrefined)	Win Rate
CONTRACT					
Insurance	110	1783	.50	2301	.56
Marine	120	1362	.68	105	.70
Miller Act	130	402	.76	56	.80
Negotiable Instruments	140	655	.80	176	.72
Recovery of Overpayments & Enforcement Judg.	150	140	.80	18	.72
Recovery of Medicare Overpayments	151	9	.56	3	1.00
Recovery of Defaulted Student Loans	152	33	.85	1	1.00
Recovery of Overpayments of Veteran Benefits	153	74	.82	8	.50
Stockholders Suits	160	37	.41	15	.60
Other Contract Actions	190	6431	.65	3795	.66
Employee Relations	191*	2	1.00	0	n/a
Contract Product Liability	195	142	.54	233	.58
REAL PROPERTY					
Land Condemnation	210	4394	.17	569	.22
Foreclosure	220	328	.85	33	.79
Rent, Lease, Ejectment	230	118	.72	21	.62
Torts to Land	240	244	.61	152	.59
Tort - Product Liability	245	18	.22	44	.36
Other Real Property Actions	290	498	.55	95	.60
TORTS-personal injury					
Airplane	310	200	.46	372	.59
Airplane Product Liability	315	41	.34	178	.39
Assault, Libel, Slander	320	171	.41	480	.50
Federal Employers' Liability	330	108	.57	1171	.72
Marine	340	1408	.57	1848	.63
Marine - Product Liability	345	38	.55	29	.34
Motor Vehicle	350	1148	.66	3654	.60
Motor Vehicle - Product Liability	355	45	.51	445	.32
Other Personal Injury	360	2044	.45	3834	.49
Personal Injury - Medical Malpractice	362	565	.52	759	.30
Personal Injury - Product Liability	365	489	.44	3839	.26
Asbestos Personal Injury - Product Liability	368	18	.50	174	.42
TORTS-personal property damage					
Other Fraud	370	574	.63	389	.60
Truth in Lending	371	19	.68	4	.50
Other Personal Property Damage	380	893	.60	470	.59
Property Damage - Product Liability	385	98	.44	238	.45
CIVIL RIGHTS					
Other Civil Rights	440	3275	.32	4350	.36
Voting	441	302	.73	8	.62
Jobs	442	8264	.20	2378	.39
Accommodations	443	186	.48	109	.45
Welfare	444	63	.56	6	0.00
PRISONER PETITIONS					
Vacate Sentence (§ 2255)	510	120	.18	4	.25
Parole Board Review	520*	5	.20	0	n/a
Habeas Corpus	530	1172	.22	29	.21
Mandamus and Other	540	62	.48	10	.10
Prisoner - Civil Rights	550	4309	.12	1617	.17

FORFEITURE & PENALTY

Agriculture Acts	610	41	.37	1	0.00
Food & Drug Acts	620	115	.89	33	.64
Liquor Laws	630	2	1.00	0	n/a
Airline Regulations	650	17	.76	1	1.00
Occupational Safety/Health	660	15	.60	0	n/a
Other Forfeiture & Penalty Suits	690	920	.86	63	.78

LABOR LAWS

Fair Labor Standards Act	710	727	.64	175	.54
Labor/Management Relations Act	720	943	.50	186	.35
Labor/Management Reporting & Disclos. Act	730	160	.42	30	.60
Railway Labor Act	740	67	.30	16	.44
Other Labor Litigation	790	470	.40	317	.44
ERISA	791	560	.56	75	.59

PROPERTY RIGHTS

Copyright	820	316	.76	84	.68
Patent	830	556	.49	206	.62
Trademark	840	550	.69	119	.59

SOCIAL SECURITY

Social Security (before 7/1/78)	860*	39	.28	4	1.00
Medicare Act Part A (42 USC 1395ff(b))	861	16	.19	0	n/a
Black Lung (30 USC 923)	862	6	.67	1	0.00
Disability Insurance (42 USC 405(g))	863	110	.47	12	.50
Supplemental Security Income Title XVI	864	19	.47	2	.50
RSI (42 USC 405(g))	865	2	0.00	1	0.00

OTHER STATUTES

State Re-apportionment	400	22	.77	0	n/a
Antitrust	410	270	.40	473	.53
Bankruptcy Trustee	420*	99	.71	22	.68
Bankruptcy Transfer Rule 915(b)	421*	24	.67	14	.50
Bankruptcy Appeals Rule 801	422	361	.46	18	.44
Withdrawal	423	46	.50	50	.66
Banks and Banking	430	76	.47	37	.57
Interstate Commerce	450	189	.63	40	.67
Deportation	460	28	.21	2	.50
Antitrust - Electrical Equipment	470*	14	.64	25	.60
Selective Service	810	2	0.00	0	n/a
Securities, Commodities, Exchange	850	595	.52	602	.54
Tax Suits	870	1312	.67	404	.55
IRS - Third Party Suits (26 USC 7609)	871*	171	.88	7	.57
Customer Challenge (12 USC 3410)	875	3	.33	1	0.00
Review or Enforcement	880*	1	1.00	0	n/a
Other Statutory Actions	890	1474	.47	401	.61
Agricultural Acts	891	211	.30	15	.73
Economic Stabilization Act	892	8	.38	1	1.00
Environmental Matters	893	243	.55	11	.73
Energy Allocation Act	894	52	.85	4	.50
Freedom of Information Act of 1974	895	54	.33	1	0.00
Appeal of EAJA Fee	900	1	0.00	0	n/a
Constitutionality of State Statutes	950	144	.46	9	.33

LOCAL QUESTION

Domestic Relations	910	242	.95	4	1.00
Insanity	920	31	.94	1	1.00
Probate	930	0	n/a	3	1.00
Other	990	24	.46	12	.50

* Discontinued code

APPENDIX B
Sizable Categories Involving Clear Judge/Jury Choice
Federal Court Cases: 1979-1989

Judge Trial

	N (commenced)	N (recovery)	Mean Recovery*	Median Recovery*	N (demand)	Mean Demand*
General						
Airplane Personal Injury	139	38	0.89	0.11	58	1.89
Federal Employers' Liability	214	52	0.86	0.09	56	0.45
Assault, Libel, Slander	122	35	0.06	0.01	86	1.09
Marine Personal Injury	1350	460	0.76	0.09	889	0.87
Other Personal Injury	788	239	0.85	0.05	516	1.15
General Contract	5469	2493	0.70	0.05	3979	0.60
Torts to Personal Property	1078	400	0.61	0.05	515	0.41
Torts to Land	298	113	0.24	0.03	145	0.70
Negotiable Instruments	838	474	0.70 ·	0.10	456	0.25
Fraud	426	184	0.47	0.02	308	0.72
Motor Vehicle	943	374	0.76	0.05	438	0.89
Prod-Med						
Product Liability	827	198	0.91	0.10	456	1.17
Medical Malpractice	114	26	1.44	0.19	42	1.90

Jury Trial

	N (commenced)	N (recovery)	Mean Recovery*	Median Recovery*	N (demand)	Mean Demand*
General						
Airplane Personal Injury	449	196	0.97	0.13	191	1.95
Federal Employers' Liability	2202	808	0.70	0.13	747	0.88
Assault, Libel, Slander	485	146	0.55	0.05	339	1.63
Marine Personal Injury	2513	889	0.60	0.14	1455	1.07
Other Personal Injury	4325	1265	0.83	0.09	2769	1.11
General Contract	3854	1509	0.97	0.09	2458	1.00
Torts to Personal Property	677	250	0.68	0.05	331	0.62
Torts to Land	229	85	0.67	0.06	96	1.08
Negotiable Instruments	267	111	1.10	0.13	110	0.53
Fraud	387	148	0.69	0.11	265	1.38
Motor Vehicle	5479	2036	0.59	0.06	2723	0.97
Prod-Med						
Product Liability	6005	956	1.18	0.19	3648	3.49
Medical Malpractice	1063	203	1.40	0.16	422	2.06

* Millions of 1989 dollars