3-1-2002

Juries, Judges, and Punitive Damages: An Empirical Study

Theodore Eisenberg
Cornell Law School, ted-eisenberg@lawschool.cornell.edu

Neil LaFountain
National Center for State Courts, nlafountain@ncsc.dni.us

Brian Ostrom
National Center for State Courts, bostrom@ncsc.dni.us

David Rottman
National Center for State Courts

Martin T. Wells
Cornell University, mtw1@cornell.edu

JURIES, JUDGES, AND PUNITIVE DAMAGES: AN EMPIRICAL STUDY

Theodore Eisenberg, Neil LaFountain, Brian Ostrom, David Rottman & Martin T. Wells†

This Article, the first broad-based analysis of punitive damages in judge-tried cases, compares judge and jury performance in awarding punitive damages and in setting their levels. Data covering one year of judge and jury trial outcomes from forty-five of the nation's largest counties yield no substantial evidence that judges and juries differ in the rate at which they award punitive damages or in the central relation between the size of punitive awards and compensatory awards. The relation between punitive and compensatory awards in jury trials is strikingly similar to the relation in judge trials. For a given level of compensatory award, there is a greater range of punitive awards in jury trials than in judge trials. The greater spread, however, produces trivially few jury awards that are beyond the range of what judges might award in similar cases.

† Theodore Eisenberg is the Henry Allen Mark Professor of Law, Cornell University; Brian Ostrom and David Rottman are Project Co-Directors of the Civil Trial Court Network Project at the National Center for State Courts (NCSC); Neil LaFountain is Court Research Analyst at the NCSC; and Martin Wells is Professor of Statistics, Department of Social Statistics, and Elected Member of the Law Faculty, Cornell University. The research that the NCSC conducted was supported by grant number BJS-96-BJ-CX-K012 from the U.S. Bureau of Justice Statistics (BJS). The points of view presented are those of the authors; they do not necessarily represent the policies or views of the BJS or the NCSC. We thank Kevin M. Clermont and Jeffrey J. Rachlinski for their comments.

Introduction

Concerns about juries dominate punitive damages reform debates. Some observers suggest that allowing judges, not juries, to set punitive award levels will improve civil justice.[1] Others regard the juror's role as a strength of the system or question reducing jurors' power without evidence of judges' superiority.[2] Judge-jury differences, however, often are illusory or exaggerated—a fact that might give pause to juries' critics.[3] Misperceptions about juries help explain trial win rates and appellate outcome patterns.[4]

---

1 *See* Reid Hastie & W. Kip Viscusi, *What Juries Can't Do Well: The Jury's Performance as a Risk Manager*, 40 Ariz. L. Rev. 901, 916 (1998); Paul Mogin, *Why Judges, Not Juries, Should Set Punitive Damages*, 65 U. Chi. L. Rev. 179 (1998); David Schkade et al., *Deliberating About Dollars: The Severity Shift*, 100 Colum. L. Rev. 1139, 1173 (2000); Cass R. Sunstein et al., *Assessing Punitive Damages (with Notes on Cognition and Valuation in Law)*, 107 Yale L.J. 2071, 2078 (1998).

2 *See* Marc Galanter & David Luban, *Poetic Justice: Punitive Damages and Legal Pluralism*, 42 Am. U. L. Rev. 1393, 1439 (1993); Richard Lempert, *Juries, Hindsight, and Punitive Damage Awards: Failures of a Social Science Case for Change*, 48 DePaul L. Rev. 867 (1999); *cf.* Michael L. Rustad, *How the Common Good is Served by the Remedy of Punitive Damages*, 64 Tenn. L. Rev. 793, 844–45 (1997) (arguing that punitive damages are not random or arbitrary, but predictable).

3 *See* Kevin M. Clermont & Theodore Eisenberg, *Trial by Jury or Judge: Transcending Empiricism*, 77 Cornell L. Rev. 1124, 1151–56 (1992); Chris Guthrie et al., *Inside the Judicial Mind*, 86 Cornell L. Rev. 777, 827 (2001) (finding that "judicial decision making . . . is influenced by . . . coguitive illusions"); Neil Vidmar, *The Performance of the American Civil Jury: An Empirical Perspective*, 40 Ariz. L. Rev. 849, 868–70, 884–85 (1998); Neil Vidmar & Jeffrey J. Rice, *Assessments of Noneconomic Damage Awards in Medical Negligence: A Comparison of Jurors with Legal Professionals*, 78 Iowa L. Rev. 883, 896 (1993); Roselle L. Wissler et al., *Decisionmaking About General Damages: A Comparison of Jurors, Judges, and Lawyers*, 98 Mich. L. Rev. 751, 812 (1999); *cf.* Richard A. Posner, *An Economic Approach to the Law of Evidence*, 51 Stan. L. Rev. 1477, 1500–02 (1999) (asserting that there is only slight evidence of differences).

4 *See* Kevin M. Clermont & Theodore Eisenberg, *Appeal from Jury or Judge Trial: Defendants' Advantage*, 3 Am. L. & Econ. Rev. 125 (2001) [hereinafter Clermont & Eisenberg, *Appeals I*]; Kevin M. Clermont & Theodore Eisenberg, *Plaintiphobia in the Appellate Courts: Civil Rights Really Do Differ from Negotiable Instruments*, 2002 U. Ill. L. Rev. (forthcoming April) [hereinafter Clermont & Eisenberg, *Appeals II*]; Clermont & Eisenberg, *supra* note 3.

Misperceptions about juries and punitive damages are especially strong. Contrary to popular belief, juries rarely award such damages,[5] and award them especially rarely in products liability and medical malpractice cases.[6] Rather, juries tend to award punitive damages in intentional misconduct cases.[7] When juries do award punitive damages, they do so in ways that relate strongly to compensatory awards.[8]

---

5 *E.g.*, Thomas A. Eaton et al., *Another Brick in the Wall: An Empirical Look at Georgia Tort Litigation in the 1990s*, 34 GA. L. REV. 1049, 1094 (2000) (finding that "punitive damages currently are not a significant factor in personal injury litigation in Georgia"); Theodore Eisenberg et al., *The Predictability of Punitive Damages*, 26 J. LEGAL STUD. 623, 633–37 (1997) (summarizing studies on the decision to award punitive damages); Neil Vidmar & Mary R. Rose, *Punitive Damages by Juries in Florida:* In Terrorem *and in Reality*, 38 HARV. J. ON LEGIS. 487, 487 (2001) (reporting that, in Florida, the "frequency of punitive damages was strikingly low").

6 *E.g.*, Eisenberg et al., *supra* note 5, at 635–37 (summarizing studies of jury trial outcomes in these case categories); Deborah Jones Merritt & Kathryn Ann Barry, *Is the Tort System in Crisis? New Empirical Evidence*, 60 OHIO ST. L.J. 315, 388 (1999) (finding no punitive awards in medical malpractice or products liability cases in a twelve-year period in Franklin County, Ohio); Vidmar & Rose, *supra* note 5, at 487 (reporting in a Florida study that, "with the exception of asbestos cases, punitive damages were almost never given in products liability cases"); J. CLARK KELSO & KARI C. KELSO, AN ANALYSIS OF PUNITIVE DAMAGES IN CALIFORNIA COURTS, 1991–2000, at 8 tbl.3 (reporting eleven products liability punitive damages awards in ten *years* of California litigation), *available at* http://12.2.169.205/government_law_and_policy/publications/ccglp_pubs_punitive_ damages_report.pdf (last visited Jan. 22, 2002). Kelso and Kelso do not separately report on medical malpractice claims, perhaps because there were too few punitive awards to warrant separate reporting of that case category.

7 *See* Rustad, *supra* note 2, at 809.

8 Eisenberg et al., *supra* note 5, at 637–39, 647–52; Theodore Eisenberg & Martin T. Wells, *The Predictability of Punitive Damages Awards in Published Opinions, the Impact of* BMW v. Gore *on Punitive Damages Awards, and Forecasting Which Punitive Awards Will Be Reduced*, 7 SUP. CT. ECON. REV. 59 (1999) [hereinafter Eisenberg & Wells, *Predictability on Appeal*]; Theodore Eisenberg & Martin T. Wells, *Punitive Awards After* BMW, *a New Capping System, and the Reported Opinion Bias*, 1998 WIS. L. REV. 387, 388–89 [hereinafter Eisenberg & Wells, *Reported Opinions*]; Jonathan M. Karpoff & John R. Lott, Jr., *On the Determinants and Importance of Punitive Damage Awards*, 42 J.L. & ECON. 527, 543 (1999); Erik K. Moller et al., *Punitive Damages in Financial Injury Jury Verdicts*, 28 J. LEGAL STUD. 283, 300 n.52 (1999); Vidmar & Rose, *supra* note 5, at 501 tbl.3 (finding that twelve years of Florida jury punitive awards had a median punitive-compensatory ratio of 0.7 to 1). Even Alabama's much-maligned system of punitive damages awards is less extreme than is widely believed. *See* Moller et al., *supra*, at 333–34 (finding that Alabama juries in financial injury cases award punitive damages in amounts smaller than and at rates similar to other states in study, but that punitive awards constitute a high proportion of amounts awarded and are a high multiple of compensatory damages).

Schkade et al., *supra* note 1, at 1142 n.12, report an unpublished study "finding that compensatory and punitive damage awards are random in sexual harassment cases." The *Columbia Law Review* kindly furnished us with the manuscript on which this claim is based, and which provides citations to the cases involved in the study. Cass R. Sunstein & Judy M. Shih, Damage Awards in Sexual Harassment Cases (June 23, 1998) (unpublished manuscript, on file with authors). The randomness claim appears to depend on the extreme influence of a handful of observations. The mass of cases in the study show a statistically significant relation between punitive (log) and compensatory (log) awards. In addition, the study used published opinions, which tend to overstate punitive award levels and the ratio of punitive awards to compensatory awards, *see* Eisenberg & Wells, *Reported Opinions*, *supra*, at 413.

Substantial evidence about juries' punitive damages performance has led some critics to shift from criticizing the absolute nature of juries' performance to arguing that, however well juries perform, judges will perform better.[9] Evaluating the propriety of reduced jury authority requires knowledge of judges' performance in punitive damages cases. Unstudied axioms relevant to a shift toward judges are that judges and juries systematically differ in (1) their inclinations to award punitive damages and (2) the levels of awards.

This Article explores these axioms with actual case data. Data covering one year of judge and jury trial outcomes from forty-five large trial courts, comprising nearly nine thousand trials, yield no evidence that judges and juries differ significantly in their rates of awarding punitive damages, or in the relation between the size of punitive and compensatory awards. Our primary results are descriptive and our primary claim is a negative one—the *absence* of evidence that judges and juries behave substantially differently. While it is tempting to make the positive assertion that judges and juries behave similarly, they do not see the same streams of cases and we cannot be sure how they would behave if they did. Nevertheless, the absence of a significant difference in the cases is noteworthy. Conventional wisdom about judges and juries—especially the claim that juries systematically impose punitive damages "in a random and capricious manner"[10]—would find more support if substantial differences emerged.

The issues addressed here transcend the usual debate about optimizing rules of civil adjudication. In academia, criticizing punitive damages has become *the* civil justice cause of recent years.[11] Outside academia, punitive damages reform is one of the main battlegrounds in the larger tort reform struggles.

Part I of this Article describes the data and reports descriptive statistics about punitive damages award frequencies and levels. Part II describes a model of the decision to award punitive damages and reports empirical results of tests of the model—tests that find no meaningful judge-jury differences. This Part also discusses the implications of case routing and case selection for interpreting the empirical results. Part III addresses the relation between punitive and compensatory damages awards and again finds no substantial difference between judge and jury adjudication.

---

9 *See* Schkade et al., *supra* note 1, at 1167–73; Sunstein et al., *supra* note 1, at 2142–45.

10 W. Kip Viscusi, *Why There Is No Defense of Punitive Damages*, 87 GEO. L.J. 381, 395 (1998).

11 *See, e.g.*, Daniel Kahneman et al., *Shared Outrage and Erratic Awards: The Psychology of Punitive Damages*, 16 J. RISK & UNCERTAINTY 49 (1998); Mogin, *supra* note 1; A. Mitchell Polinsky & Steven Shavell, *Punitive Damages: An Economic Analysis*, 111 HARV. L. REV. 869 (1998); Schkade et al., *supra* note 1; Sunstein et al., *supra* note 1.

I
Background and Descriptive Data

The Civil Trial Court Network (CTCN), a project of the National Center for State Courts and the U.S. Bureau of Justice Statistics (BJS), obtains data directly from state court clerks' offices. It covers state courts of general jurisdiction in a random sample consisting of forty-five of the seventy-five most populous counties in the United States.[12] The seventy-five counties include approximately 33% of the 1990 U.S. population and the forty-five counties actually sampled account for about 20% of the population.[13] The first CTCN trial data were limited to jury trials and covered fiscal year 1991–92.[14] New data, using the same stratified sampling methodology, cover judge and jury trials in calendar year 1996.[15] The 1996 data's inclusion of judge trials allows study of differences between judge and jury trial characteristics. The data about each case include the subject matter, locale, prevailing party, type of litigants, and compensatory and punitive damage award levels.[16]

The CTCN data are the most representative sample of state court trials in the United States. With direct access to court clerks' offices and approximately one hundred trained coders recording the data, the information gathered does not depend on litigants or third parties to report and code cases. Such self-reporting in jury verdict reporters can lead to sample bias that overstates plaintiff win rates and award levels.[17] In several counties, CTCN case lists obtained from clerks' offices were compared with local jury verdict reporters. The

---

12 For a summary of the data and methodology, see Bureau of Justice Statistics, U.S. Dep't of Justice, Bulletin No. NCJ-173426, Civil Trial Cases and Verdicts in Large Counties, 1996, at 1 (Sept. 1999) [hereinafter BJS 1996]. For a more complete description of the data from the related 1992 study, see Bureau of Justice Statistics, U.S. Dep't of Justice, Special Report No. NCJ-153177, Tort Cases in Large Counties 1, 6 (Apr. 1995). The BJS used a two-stage stratified sampling technique described in the 1992 special report. *See id.*

13 *See* BJS 1996, *supra* note 12, app. B, at 20 (showing sampled counties' 1996 populations); Theodore Eisenberg et al., *Litigation Outcomes in State and Federal Courts: A Statistical Portrait*, 19 Seattle U. L. Rev. 433, 434 (1996).

14 *See* Bureau of Justice Statistics, U.S. Dep't of Justice, Civil Justice Survey of State Courts, 1992 (Inter-Univ. Consortium for Political and Soc. Research No. 6587, 3d ed. 2001).

15 *See* BJS 1996, *supra* note 12, at 17. For sampling purposes, the seventy-five counties were divided into four strata. *See id.* The 1996 data are from Bureau of Justice Statistics, U.S. Dep't of Justice, Civil Justice Survey of State Courts, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2993, 2d ed. 2001) [hereinafter 1996 Survey], *available at* http://www.icpsr.umich.edu.

16 *See* BJS 1996, *supra* note 12.

17 *See* Eisenberg et al., *supra* note 5, at 641 n.53; Merritt & Barry, *supra* note 6, at 324–26 (finding serious bias in commercial verdict reporter samples); Moller et al., *supra* note 8, app. at 335 (reporting reasonable levels of confidence in the jury verdict reporters used but acknowledging some possible bias).

jury verdict reporters sometimes failed to include approximately half the cases the CTCN found.

The CTCN data include all completed trials in thirty-six counties and a random sample of trials in nine counties.[18] In eight of the sampled counties, well over half the 1996 trials are included in the data. Appendix Table 1 lists summary statistics by county. In Harris County, Texas, which includes Houston, 352 judge and jury trials were randomly sampled out of approximately 1,500 trials, with jury trials somewhat more heavily sampled than judge trials. This is the only county in which sampling might distort the actual number of punitive awards in judge and jury trials.[19] The analyses reported here include Harris County, but we report throughout the Article whether inclusion of Harris County materially affects results.[20]

A. Trial Outcomes Generally

Table 1 presents summary statistics of the variables in this study. Panel A's two rows show, for both compensatory and punitive damages, the mean and median awards in thousands of dollars and the number of plaintiff awards, all reported separately for jury and judge trials. Panel A's first row shows that the mean compensatory award in jury trials with an award is $1.047 million and the mean compensatory award in judge trials with an award is $152,000. The medians are also substantially different; the jury trial median is $45,000 while the judge trial median is $25,000. For both compensatory and punitive awards, panel A confirms the conventional wisdom that jury awards are higher than judge awards. But substantial non-random case routing occurs. Amounts plaintiffs demand in jury-tried cases are significantly higher than those they demand in judge-tried cases.[21] In addition, tort trials tend to be routed to juries[22] while judges tend to adjudicate contract

---

18 For a description of the sampling used, see BJS 1996, *supra* note 12, at 17–18. The statements in text are based on the authors' analysis of the data.

19 In Harris County, sampling may have led to an extraordinary proportion of judge-tried punitive damages cases. *See* BJS 1996, *supra* note 12, app. E, at 23 (reporting sixty-seven bench trials with punitive awards, which exceeds the sum of bench trial punitive awards in all other counties in the sample). The sixty-seven trials reported reflect the county-level sampling. Twelve actual punitive awards were found in Harris County.

20 Unless otherwise noted, simple descriptive tables use unweighted data. In regression analyses, unless otherwise noted, data are weighted to reflect the sampling in nine counties, and standard errors and significance levels are adjusted to reflect the stratified nature of the sample and the clustering of data at the county level. Our regression analyses account for the weighted sampling design in the data collection as well as the county-level clustering and stratification based on sampling patterns.

21 *See* Clermont & Eisenberg, *supra* note 3, at 1177 app. B; Theodore Eisenberg & Kevin M. Clermont, *Trial by Jury or Judge: Which Is Speedier?*, 79 JUDICATURE 176, 180 (1996).

22 *See* Clermont & Eisenberg, *supra* note 3, at 1141 tbl.4; Eisenberg et al., *supra* note 13, at 443 tbl.4. Panel C's last two columns show that only three percent of medical malpractice trials were before judges.

TABLE 1: SUMMARY STATISTICS OF JURY AND JUDGE TRIAL OUTCOMES AND AWARD PATTERNS

	Mean		Median		Number of Plaintiff Awards		Number of Trials	
A. Award levels (thousands of dollars) & frequencies	Jury	Judge	Jury	Judge	Jury	Judge	Jury	Judge
Compensatory damages	1,047	152	45	25	3,001	1,375	6,429	2,295
Punitive damages	1,870	547	50	30	121	55	121	55
	Proportion of Trials		Proportion of Trials Won by Plaintiffs		Proportion Plaintiff Wins with Punitive Award			
B. Plaintiff/defendant status								
Individual only vs. individual only	.370	.269	.479	.607	.035	.053	2,391	618
Individual only vs. government	.075	.052	.456	.271	.024	.000	485	118
Individual only vs. corporation	.390	.273	.490	.576	.047	.067	2,512	627
Individual only vs. hospital	.082	.020	.310	.511	.031	.000	526	45
Individual & nonind. vs. ind. only	.007	.014	.568	.606	.000	.050	44	33
Individual & nonind. vs. corporation	.019	.027	.557	.683	.059	.047	122	63
Nonindividual vs. individual only	.017	.126	.491	.745	.039	.023	110	290
Nonindividual vs. corporation	.041	.220	.541	.690	.050	.009	266	507
C. Case categories								
Motor vehicle tort	.394	.096	.549	.636	.008	.007	2,532	220
Premises liability	.180	.056	.383	.504	.011	.108	1,159	129
Products liability: asbestos	.018	-	.549	-	.016	-	116	0
Products liability: other	.022	.007	.307	.706	.071	.083	140	17
Intentional tort	.032	.029	.546	.627	.214	.238	205	67
Medical malpractice	.116	.011	.237	.280	.006	.000	747	25
Professional malpractice	.012	.017	.397	.475	.069	.000	74	40
Slander/libel	.007	.006	.362	.385	.177	.000	47	13
Other negligence	.045	.027	.500	.516	.021	.031	292	62
Fraud	.027	.071	.578	.586	.150	.126	173	162
Seller plaintiff	.033	.290	.668	.773	.028	.006	214	665
Buyer plaintiff	.036	.102	.468	.645	.119	.046	234	234
Employment discrimination	.020	.017	.484	.282	.213	.091	126	39
Other employment dispute	.014	.036	.567	.537	.157	.093	90	82
Rental/lease agreement	.013	.087	.506	.685	.073	.007	81	200
Tortious interference with contract	.010	.023	.667	.519	.098	.111	63	52
Other contract	.010	.044	.594	.485	.079	.041	64	102
Other real property	.011	.081	.403	.366	.214	.017	72	186
Totals			.473	.618	.040	.039	6,429	2,295

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001) cases. Plaintiffs tend to prevail in judge-tried cases significantly more often than in jury-tried cases. Table 1's "Totals" row shows that plaintiffs have an overall win rate of 62% in judge trials and 47% in jury trials.[23]

Panel B reports award frequencies for each litigant pair; panel C does the same for each case category. Every case, regardless of the number of plaintiff or defendant types, is assigned one of four plaintiff and defendant designations: hospital, corporation, government, or individual. For cases involving more than one plaintiff or defendant type, the case is assigned to the type appearing first in the above listing. For example, a case with a hospital defendant is characterized as a hospital defendant case even if the case also included business, individual, and government defendants.[24] The litigant characterization hierarchy tries to capture the most salient characteristic of the litigants, though the hierarchy cannot assure capture of the most salient litigant characteristic in each case. We further refine litigant pair categories to identify cases in which only individuals sued other individuals and cases in which only individuals sued corporations, and we also create some smaller related categories.

B. Punitive Damages Award Patterns

Given a plaintiff victory at trial, punitive award decisionmaking can be divided into two separate decisions: the decision to award punitive damages, and the decision setting the level of the punitive award.

1. *Frequency of Punitive Awards*

With respect to punitive damages award frequency, Table 1's two largest litigant pair categories—"individual only vs. individual only" and "individual only vs. corporation" (reported in panel B's first and third rows)—account for over 75% of the trial outcomes. Both categories show a higher rate of punitive damages awards in judge trials than in jury trials. Panel B's fifth and sixth numerical columns show that when individuals sue individuals, punitive damages are awarded in 3.5% of the jury trials won by plaintiffs and in 5.3% of the judge trials won by plaintiffs. However, the difference is not statistically sig-

23 This finding is consistent with federal court data in products liability and medical malpractice cases, Clermont & Eisenberg, *supra* note 3, app. A, at 1175, but is more broad based in these state court cases. Panel C shows that in nearly all tort categories—including motor vehicle tort, premises liability, intentional tort, medical malpractice, professional malpractice, slander/libel, other negligence, and fraud—plaintiffs prevailed at a higher rate in judge trials than in jury trials. Either judges are more sympathetic to plaintiffs than juries or plaintiffs route their weaker cases to juries, probably based on the belief that juries will be biased toward harmed plaintiffs. When that belief turns out to be a misperception, the observed pattern of win rates results. *Id.* at 1174.

24 BJS 1996, *supra* note 12, at 3 n.3.

nificant.[25] The single highest rate of punitive damages awards occurs when individuals sue corporations in judge trials; punitive damages awards occur in 6.7% of the successful plaintiff cases in this category. These simple descriptive statistics provide no evidence that juries are more inclined than judges to award punitive damages.

Panel C also shows that punitive damages award rates vary more across case categories than they do across litigant pairs. Intentional tort, fraud, and employment cases[26] are sizeable case categories in which punitive award rates are much higher than the four percent average for all plaintiff trial wins. Fraud and intentional tort cases yield punitive awards at a high rate regardless of whether the adjudicator is judge or jury, and together they account for about one-third of all punitive awards. Employment cases show a noticeably higher rate of punitive awards before juries than before judges.[27]

Prior CTCN data and other data confirm geographical variation in award frequencies.[28] The new CTCN data replicate this finding with substantial intercounty differences in punitive award rates.[29] These differences exist whether one considers only judge-tried cases or only jury-tried cases. But we are interested in the relation between the frequency of punitive awards and the mode of trial—judge or jury. Even if judges and juries award punitive damages at different rates, one still wants to know whether that difference varies across the forty-

---

25 By statistical convention, the hypothesis being tested is called the null hypothesis. GEORGE W. SNEDECOR & WILLIAM G. COCHRAN, STATISTICAL METHODS 64 (8th ed. 1989). Significance levels (also called *p*-values) are the probability of rejecting the null hypothesis when it is true. That is, the significance levels provide an inverse measure of the likelihood that the difference in punitive damages award rates shows a real difference rather than mere random variation. The smaller the significance level, the more surprised one would be to observe the difference if the tested hypothesis were true. *See id.* at 64–66. By arbitrary convention, results that are significant at or below the 0.05 level are described as statistically significant. *See* THE EVOLVING ROLE OF STATISTICAL ASSESSMENTS AS EVIDENCE IN THE COURTS app. A, at 196–97 (Stephen E. Fienberg ed., 1988). Throughout this Article, we use the term significant in the statistical sense of significance level. For "individual only vs. individual only," the judge-jury punitive award rate difference is significant at the 0.126 level. For "individual only vs. corporation," the judge-jury punitive award rate difference is significant at the 0.175 level. The insignificance of these *p*-values (both being noticeably higher than 0.05) does not depend on the inclusion of Harris County, Texas. *See supra* note 19.

26 Vidmar and Rose report similar results for Florida jury trials, except they find that motor vehicle cases involving impaired or reckless drivers constitute the leading source of punitive awards. Vidmar & Rose, *supra* note 5, at 494, 495 tbl.2. For evidence of the dominance of employment cases as a source of punitive damages in Florida federal court, see *id.* at 491 n.14.

27 Combining the two employment categories yields a difference in judge-jury punitive award rates that is significant at the 0.062 level. *See supra* note 25.

28 Eisenberg et al., *supra* note 5, at 640–41; *accord* STEPHEN DANIELS & JOANNE MARTIN, CIVIL JURIES AND THE POLITICS OF REFORM 69–72 (1995); ERIK MOLLER, TRENDS IN CIVIL JURY VERDICTS SINCE 1985, at 33 (1996).

29 BJS 1996, *supra* note 12, app. E, at 23; *infra* Appendix Table 1.

five different counties observed in the study. Statistical analysis reveals that one cannot reject the hypothesis of no county-level effect on the relation between frequency of punitive damages awards and mode of trial.[30]

The numbers in Table 1 allow us to calculate the percent of punitive award cases decided by judges. Judges gave 55 of the 176 (31.3%) punitive damages awards.[31] Given the overwhelming focus on jury punitive awards in the literature and policy debate, this share is surprisingly high.[32] A detailed study of Georgia tort cases confirms the surprisingly high fraction of punitive awards made by judges.[33]

2. *The Relation Between Punitive and Compensatory Awards*

With respect to punitive award levels, Figure 1 shows the relation between punitive and compensatory awards in cases with punitive awards. Figure 1 is a scatterplot of punitive damages (log) and compensatory damages (log), with judge (bench) and jury trials separately indicated by "B" and "J," respectively. The logarithmic scales are used because, as is often the case with award amounts, linear scales do not

---

30 The common method for testing such a hypothesis is to calculate the significance level of the common odds ratio. *See* DOUGLAS G. ALTMAN, PRACTICAL STATISTICS FOR MEDICAL RESEARCH 270–71 (1991). The test for a common odds ratio across counties gives an exact p-value of p=0.331. For each county, we construct a table with two rows and two columns. The rows consist of judge and jury trials. The columns represent cases in which no punitive award was given and cases in which a punitive award was given. This yields forty-five 2 × 2 tables, one for each county, the entries of which can be computed from the Appendix Tables. One can think of each county's 2 × 2 table as exploring the difference between judge and jury punitive award rates *within* the county, a question we explore for all counties combined in the regression models that follow. We test whether the odds ratio for these forty-five 2 × 2 tables is equal across the forty-five counties. Rather than using the Mantel-Haenszel procedure, *see id.*, we apply a test based on the conditional maximum likelihood estimate of the common odds ratio. A problem in directly applying the test is that many of the 2 × 2 tables are sparse (i.e., have cell counts of zero or one) and the asymptotic p-values may not be reliable. However, we use the results of Robert L. Strawderman & Martin T. Wells, *Approximately Exact Inference for the Common Odds Ratio in Several 2 × 2 Tables*, 93 J. AM. STAT. ASS'N 1294 (1998), to compute the exact p-value, thereby eliminating the sparseness problem.

31 The figures reported in Table 1 yield slightly different results due to rounding. For judges, (2,295 trials) × (0.618 proportion won by plaintiffs) × (0.039 proportion plaintiff wins with punitive award) = 55.3. For juries, (6,429 trials) × (0.473 proportion won by plaintiffs) × (0.040 proportion plaintiff wins with punitive award) = 121.6.

32 The 31.3% judge-trial share may understate the prominence of judge-awarded punitive damages. Accounting for the sampling methods used, the proportion of punitive awards that are made by judges is 44%, and judges award punitive damages in about 5.5% of successful trials compared to 4.4% for juries. But excluding Harris County, Texas, *see supra* note 19, gives judge trials a 25.7% share of all punitive damages awards, with judges giving punitive awards in 3.2% of plaintiff wins and juries giving punitive awards in 4.0% of plaintiff wins.

33 Eaton et al., *supra* note 5, at 1094 (finding that judges awarded punitive damages more frequently than juries).

reveal the relation between the variables.[34] Figure 1 also shows the best-fitting simple linear regression lines for judge and jury trials.[35] The lines, which best describe the relation between compensatory and punitive awards, are similar in slope and intercept. We defer detailed statistical analysis of judge-jury differences to Parts II and III. The punitive-compensatory patterns in Figure 1 are similar to those in earlier studies,[36] and the pattern in judge-tried cases is consistent with the pattern in jury-tried cases.

Figure 1 shows that the simple log transformation that describes the relation between punitive and compensatory jury awards also describes judge awards. Both compensatory and punitive awards are approximately log-normally distributed for judges and juries.[37] This result addresses a concern about jury punitive awards—that a logarith-

34 For discussion of the need for logarithmic scales in other sets of punitive damages awards, see, for example, Eisenberg et al., *supra* note 5, at 638 figs.2–3, app. at 661 figs.A1–A2.

35 Simple regression models of punitive damages (log) as the dependent variable and compensatory damages (log) as the explanatory variable, when run separately for judge and jury trials, yield the following results (the intercepts are not statistically significant in either equation):

	Compensatory (log) coefficient	Standard error	Intercept	*r*-squared	*N*
Judge trials	.883	.099	–.155	.674	54
Jury trials	.930	.130	–.395	.459	119

These models account for the sample stratification, weighting, and clustering by county. Models that combine the trial modes and use more explanatory variables are described in Table 3 *infra*. The greater dispersion around the regression line in jury trials, as evidenced by Figure 1 and the lower *r*-squared for jury trials, is discussed in Part III.C *infra*.

36 *E.g.*, Eisenberg, et al., *supra* note 5, at 638 fig.3; Moller et al., *supra* note 8, at 300 n.52.

37 A test of the hypothesis that jury trial punitive awards (log) are normally distributed with the observed sample mean and standard deviation yields p=0.418. The same test of judge trial punitive awards (log) yields p=0.140. Thus, for both judges and juries, one cannot reject the hypothesis that punitive awards are log-normally distributed. In cases that yield positive punitive awards, a test of the hypothesis that jury trial compensatory awards (log) are normally distributed with the observed sample mean and standard deviation yields p=0.986. The same test of judge trial compensatory awards (log) yields p=0.093. *See* Eisenberg et al., *supra* note 5, at 638 figs.2–3 (indicating that the logarithm of punitive damages awards is close to being normally distributed, and showing the relationship between compensatory and punitive damages). In addition, one can firmly reject the hypotheses that the untransformed punitive and compensatory awards are normally distributed. A scatterplot of punitive awards in judge trials against compensatory awards without the log transformation is uninformative, as is a plot of untransformed punitive awards against the log of compensatory awards.

Implicitly we are assuming that the punitive damages are distributed as a log-normal distribution. A random variable has a log-normal distribution if its natural logarithm has a normal distribution. The log-normal distribution is common in financial modeling. For instance, the celebrated Black-Scholes pricing formula assumes that the evolution of the price stochastic process follows a geometric Brownian motion. Hence the individual prices are log-normally distributed. *See* Fischer Black & Myron Scholes, *The Pricing of Options and Corporate Liabilities*, 81 J. POL. ECON. 637, 644 (1973).

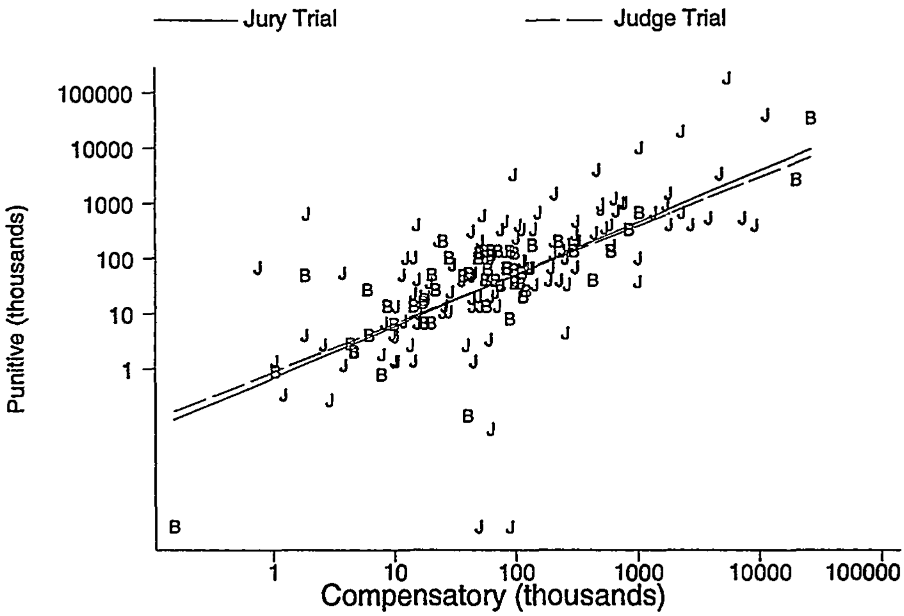

FIGURE 1. Punitive and compensatory damages by judge or jury trial status. The figure displays both a scatterplot and the best fitting regression lines.

mic scale is needed to reveal the relation between punitive and compensatory awards, and that the scale understates variation in punitive awards.[38] The same logarithmic transformation illuminates the relation between punitive and compensatory awards in judge trials.

3. *The Punitive-Compensatory Ratio*

Figure 1 shows no judge-jury difference in the relation between punitive and compensatory awards. To summarize that relation in a single number to facilitate further comparison, we use punitive damages (log) divided by compensatory damages (log) to construct the ratio of the two damages measures.

Table 2 reports statistics summarizing the punitive-compensatory ratio in judge and jury trials. The table's last row shows that the means, medians, and standard deviations do not differ significantly across trial modes. A statistical test of the difference between the entire distributions does not allow us to reject the hypothesis that they are the same.[39] The 95% confidence intervals for the means of the ratios are narrow for both judge and jury trials. For judge trials the interval ranges from 0.882 to 0.981. For jury trials, the interval ranges

[38] *See* Brief of Certain Leading Business Corporations as Amici Curiae in Support of Petitioner at 11 n.14, Cooper Indus., Inc. v. Leatherman Tool Group, Inc., 532 U.S. 424 (2001) (No. 99-2035); Schkade et al., *supra* note 1, at 1146 n.31; Sunstein et al., *supra* note 1, at 2076 n.21; Viscusi, *supra* note 10, at 386.

[39] The significance level is p=0.508.

from 0.907 to 0.983, slightly narrower than the judge trial range, presumably because of the greater number of jury trials. Surprisingly, the median punitive-compensatory ratio without transforming to logs is higher in judge trials than in jury trials. This ratio also does not significantly differ between judge and jury trials.[40]

TABLE 2: PUNITIVE-COMPENSATORY AWARD RATIO BY TRIAL MODE

Trial Mode	Mean Ratio	Median Ratio	Standard Deviation	Untransformed Median Ratio	Number of Cases
Judge trial	.931	.948	.181	.578	54
Jury trial	.945	.945	.211	.497	119
Significance of judge-jury difference	.675	.987	.200	.924	

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)

Note: The ratio is the ratio of punitive award (log) to compensatory award (log) in each case with an award of both. Untransformed ratio is the ratio of punitive award to compensatory award. The significance of means difference is based on a t-test; the significance of medians differences are based on a Mann-Whitney test; the significance of standard deviations difference is based on an F-test.

The untransformed mean ratio is greater in jury trials. But this statistically insignificant difference is a consequence of a handful of awards, most of which are known to have been reduced or are justifiable. Follow-up study of the most extreme punitive-compensatory ra-

40 The significance level is p=0.924. A significance test of the entire untransformed ratio distributions yields p=0.425. The untransformed median ratio allows for comparison with Moller et al., *supra* note 8. In a study of financial injury cases, the Moller study reports a median punitive-compensatory ratio in jury trials of 1.4. *Id.* at 303 tbl.5. The 1991–92 CTCN data, limited to jury trials, have an untransformed punitive-compensatory ratio of 0.757. The substantially higher median in the Moller study may be a consequence of sample bias in jury verdict reporters. *See supra* notes 17–18 and accompanying text. The Moller study also shows some stability in the untransformed punitive-compensatory ratio. Its median does not vary in a statistically significant manner across jurisdictions in insurance cases or real property cases, or across plaintiff types in insurance cases. *Id.* at 310 tbl.9, 313 tbl.11, 318 tbl.16. The median punitive-compensatory ratio is significantly different across jurisdictions in employment cases, *id.* at 315 tbl.13, and across party types in a residual category of contract cases; *id.* at 324 tbl.21. It is marginally significantly different across jurisdictions in a residual category of contract cases. *Id.* at 322 tbl.19.

The absence of statistically significant effects is probably not a consequence of sample size. Perfectly executed studies may fail to reveal socially important differences "simply because the sample sizes are too small to give the procedure enough power to detect the effect." STANTON A. GLANTZ, PRIMER OF BIOSTATISTICS 178 (4th ed. 1997). The power of a test refers to its likelihood of detecting an effect of a specified size at a specified significance level. *Id.* at 177–78. If a test is not very powerful, the likelihood of detecting the effect is small. A power calculation shows that to be 80% confident of detecting a 10% difference in the ratios' means, at a significance level of 0.05, requires 104 jury trials and 48 judge trials. The sample size exceeds these numbers. This power calculation employs the standard deviations observed in Table 2 and hypothesizes means that are 0.9 and 1.0. The sample is also large enough to detect a statistically significant difference in means of about 0.07 with 70% confidence.

tios[41] suggests the distortion introduced by relying on extreme awards without further inquiry. Five of seven cases with punitive-compensatory ratios greater than ten were clearly justifiable or later resulted in a reduced punitive award. One case with an extreme ratio involved sexual abuse of a child by a sports coach and led to a $2.6 million punitive damages award that no one expected to actually be paid.[42] Another extreme case involved a $300,000 punitive award against a wealthy car dealer who had failed to disclose a vehicle's history of defects and had rolled back a car's odometer by nearly 40,000 miles.[43] The largest award was substantially reduced due to a then-existing cap on punitive damages.[44] One case with an extreme ratio is known to have been reduced but the amount of reduction is not known, and another case's punitive award was reduced by 90% to reflect that the plaintiff was 90% at fault.[45] No information was found on two of the cases.

Figure 2 presents the distributions of the punitive-compensatory ratio for judge trials and for jury trials.[46] Although Table 2 shows that the differences between the distributions are not statistically significant, the jury trial punitive-compensatory ratios are more "spread" than the judge trial punitive-compensatory ratios. Both the left and right tails of the jury trial distribution are larger than those of the judge trial distribution. We address this greater spread in Part III.C.

Together, Table 1's descriptive statistics, Figure 1, and Table 2's and Figure 2's analyses of punitive-compensatory ratios show that judge-jury rates of awards do not differ substantially and that their levels of punitive awards bear a similar relation to levels of compensatory awards. The remainder of this Article employs more comprehensive statistical methods to further probe the absence of evidence of judge-jury differences.

41 Three cases (one judge-tried and two jury-tried) involved zero compensatory awards so the punitive-compensatory ratio is not amenable to numerical analysis. *See infra* note 104.

42 Margaret Zack, *Man Awarded $2.6 Million for Coach's Abuse, but Probably Won't Receive Much*, STAR TRIB. (Minneapolis–St. Paul), May 15, 1996, at B1.

43 Draeger Martinez, *Gamel, Firm Ordered to Pay "Lemon" Damages*, FRESNO BEE, Oct. 22, 1996, at B1.

44 *Verdict Reduced*, NAT'L L.J., Apr. 8, 1996, at A11 (punitive award reduced from $138 million to $64.8 million, against three gas company defendants in an explosion case involving twenty plaintiffs).

45 E-mail from Neil LaFountain to Theodore Eisenberg (Aug. 2, 2001, 09:27:34 EST) (on file with Theodore Eisenberg).

46 Figure 2 employs what are known as kernel density estimates to portray the ratios' distributions. The more traditional way of portraying univariate statistical distributions, the histogram, can be thought of as a kind of kernel density estimator. Kernel density estimates have the advantages of being smooth (they do not require the box-like bins of histograms) and of being independent of bin location, a choice that can profoundly shape the appearance of a histogram. For discussion of kernel density estimation, see B.W. SILVERMAN, DENSITY ESTIMATION FOR STATISTICS AND DATA ANALYSIS 34–94 (1986).

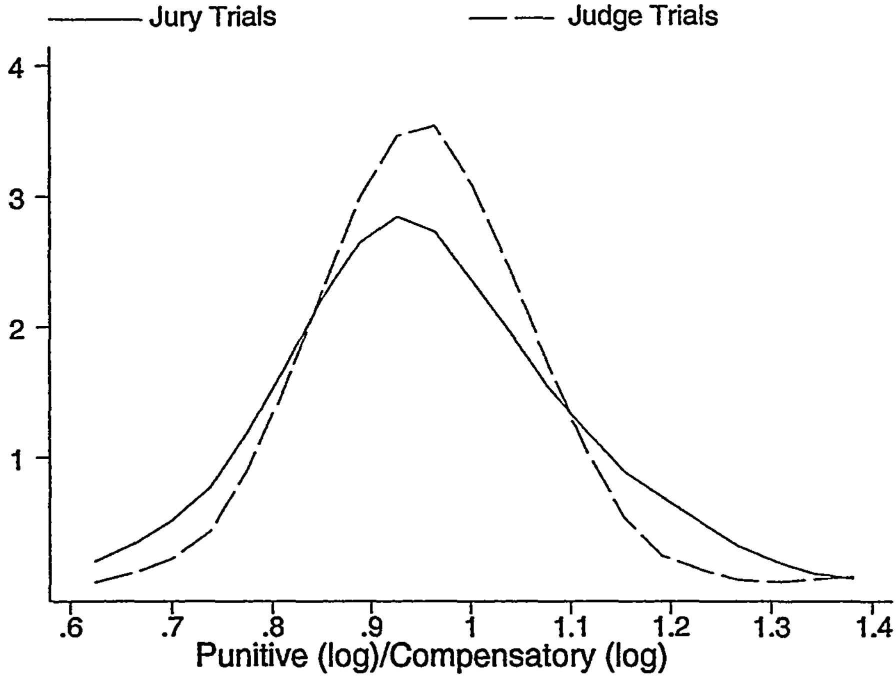

FIGURE 2. Kernel density estimates of the ratio of punitive and compensatory damages in cases containing both kinds of awards, by judge or jury trial status. Jury trials show greater "spread" than judge trials.

II
THE DECISION TO AWARD PUNITIVE DAMAGES

Differences in types of cases routed to judges and juries suggest the need for analysis beyond Table 1's simple comparison of punitive award rates. It may be that cases with punitive damages potential tend to be routed to one decisionmaker. For example, punitive damages tend to be awarded more frequently in financial injury cases,[47] which judges tend to receive more often than juries. Such case routing may mask a judge-jury difference, rendering Table 1's similar rates of awarding punitive damages misleading. A more detailed analysis should help account for routing of cases by subject area and other factors. Multivariate regression works to segregate the effects of various factors, such as case category.[48]

47 Moller et al., *supra* note 8, at 284.

48 Multivariate regression is a statistical technique that quantifies the influence of each of several factors (independent variables) on the phenomenon being studied (dependent variable). *See generally* MICHAEL O. FINKELSTEIN & BRUCE LEVIN, STATISTICS FOR LAWYERS 323–467 (1990) (discussing regression models); DAVID W. HOSMER & STANLEY LEMESHOW, APPLIED LOGISTIC REGRESSION 31–90 (2d ed. 2000) (describing and applying logistic regression models). Because the dependent variable is dichotomous (because pu-

Preliminarily, one must consider whether the sample is large enough to detect meaningful differences between judge and jury punitive award rates, even if they do exist.[49] Specifying what degree of difference is meaningful is a subjective judgment. Suppose one accepts that a meaningful difference is a 3% punitive award rate in plaintiff victories in judge trials compared to a 5% punitive award rate in plaintiff victories in jury trials. Both assumed rates are reasonably close to Table 1's observed rate of punitive awards in about 4% of plaintiff trial wins. Using these rates, there is almost a 90% chance of detecting such a difference, or a larger difference, at a statistically significant level.[50]

A. Modeling the Decision to Award Punitive Damages

According to a simple model of the decision to award punitive damages presented in our earlier work, the decision is linked to the nature of the defendant's behavior.[51] The best available proxy for such behavior is the type of case. On average, for example, one expects defendant behavior to have been worse in an intentional tort case than in a case involving mere negligence. Table 1 and prior empirical work confirm this pattern.[52] Therefore, in modeling the decision to award punitive damages, we include dummy variables (variables equal to zero or one) for each case category and expect case categories involving intentional misbehavior to yield the highest punitive award rates, as Table 1 suggests. This also helps control for the routing of cases between judges and juries based on subject areas.

A second behavior-related measure is the level of the compensatory award. Other things being equal, we regard behavior that causes more harm as worse than behavior that causes less harm and as deserving of more severe punishment.[53] We use the compensatory damages awarded in each case to approximate the degree of harm caused.

---

nitive damages either are or are not awarded), we use logistic regression. *See* FINKELSTEIN & LEVIN, *supra*, at 447–52.

49 Professor Viscusi's effort to detect interstate differences in accident deterrence based on whether states allow punitive damages was limited by, *inter alia*, sample size (the number of states). *See* Theodore Eisenberg, *Measuring the Deterrent Effect of Punitive Damages*, 87 GEO. L.J. 347, 349 (1998). Viscusi acknowledges this limitation. Viscusi, *supra* note 10, at 390. However, erroneous characterization of states' punitive damages rules also compromised the effort. Eisenberg, *supra*, at 348–49; Eisenberg & Wells, *Predictability on Appeal*, *supra* note 8, at 73 n.45. Viscusi notes that he relied on published sources. Viscusi, *supra* note 10, at 391.

50 *See supra* note 40.

51 Eisenberg et al., *supra* note 5, at 644–47.

52 *See id.* at 645 tbl.4.

53 *E.g.*, Jeffrey S. Parker & Raymond A. Atkins, *Did the Corporate Criminal Sentencing Guidelines Matter? Some Preliminary Empirical Observations*, 42 J.L. & ECON. 423, 448–49 (1999).

Evidence exists that jurors react differently to corporate and individual defendants.[54] Table 1 reports summary statistics for several litigant pairs, including two of particular interest: actions by individuals only versus individuals only, and actions by individuals only versus corporations. We include dummy variables for these and for Table 1's other litigant pairings. We use statistical models that account for the clustering of the sample at the county level.[55]

B. Empirical Results

Because the decision to award punitive damages is dichotomous, we employ logistic regression analysis.[56] The dependent variable is whether the plaintiff received a punitive damages award. The sample consists of over 4,300 trials with plaintiff victories. The explanatory variables include dummy variables for case categories and litigant characteristics, and a continuous variable for the size of the compensatory award (log). The explanatory variables of particular interest are a jury trial dummy variable, which assesses whether juries are more likely than judges to award punitive damages, and an interaction term consisting of the product of the jury trial dummy variable and the compensatory award. The interaction term monitors whether, as compensatory awards increase, juries are more likely than judges to award punitive damages.

Table 3 reports two models. The first includes the above-described explanatory variables. The second limits the explanatory variables to three: the compensatory award, the jury trial dummy variable, and the interaction term.

The first and third numerical columns show a variable's "odds ratio" or "odds multiplier," a way of expressing the size of a variable's influence on whether punitive damages were awarded.[57] For the case

54 *E.g.*, Valerie P. Hans & William S. Lofquist, *Jurors' Judgments of Business Liability in Tort Cases: Implications for the Litigation Explosion Debate*, 26 LAW & SOC'Y REV. 85 (1992); Robert J. MacCoun, *Differential Treatment of Corporate Defendants by Juries: An Examination of the "Deep-Pockets" Hypothesis*, 30 LAW & SOC'Y REV. 121, 140 (1996). But this difference does not necessarily translate into juror hostility toward corporations. *See* Richard Lempert, *Why Do Juries Get a Bum Rap? Reflections on the Work of Valerie Hans*, 48 DEPAUL L. REV. 453, 455 (1998). Jurors may react especially negatively to misbehavior by insurance companies. *See* Eisenberg & Wells, *Predictability on Appeal*, *supra* note 8, at 67, app. at 85 tbl.1 (showing that the median of punitive-compensatory damages ratios are highest in insurer misbehavior cases); Valerie P. Hans & Nicole Vadino, *Whipped by Whiplash? The Challenges of Jury Communication in Lawsuits Involving Connective Tissue Injury*, 67 TENN. L. REV. 569, 580–82 (2000). But juries generally are skeptical about plaintiffs' claims. *See id.* at 572–77.

55 A fixed-effects model does not yield materially different results, but we had to drop several hundred observations due to lack of within-site variation.

56 *See supra* note 48.

57 In multivariate logistic regression, each estimated coefficient provides an estimate of the corresponding variable's effect on the logarithm of the odds of the dependent variable, adjusting for all other variables included in the model. The odds multiplier is ob-

TABLE 3: LOGISTIC REGRESSION RESULTS: PUNITIVE DAMAGES AWARDED

	Full Model		Parsimonious Model	
Variable	Odds Ratio	Significance	Odds Ratio	Significance
Compensatory damages (log)	1.371***	0.000	1.302***	0.000
Jury trial dummy	1.660	0.419	0.956	0.934
Jury trial × log of compensatory interaction	0.821*	0.014	0.904	0.139
Plaintiff/defendant status dummy variables				
Individual only vs. individual only (ref. category)				
Individual only vs. government	0.294**	0.007		
Individual only vs. corporation	0.891	0.734		
Individual only vs. hospital	0.798	0.635		
Individual & nonind. vs. individual only	0.244	0.220		
Individual & nonind. vs. corporation	1.177	0.732		
Nonindividual vs. individual only	0.736	0.570		
Nonindividual vs. corporation	0.264**	0.003		
Case category dummy variables				
Products liability (reference)				
Motor vehicle tort	0.131**	0.001		
Premises liability	0.862	0.865		
Products liability: asbestos	0.700	0.784		
Intentional tort	5.130*	0.010		
Medical malpractice	0.206	0.191		
Professional malpractice	0.421	0.344		
Slander/libel	3.095	0.184		
Other negligence	0.484	0.400		
Fraud	2.909*	0.048		
Seller plaintiff	0.320	0.094		
Buyer plaintiff	2.461	0.158		
Employment discrimination	4.272*	0.025		
Other employment dispute	2.397	0.072		
Rental/lease agreement	0.484	0.422		
Tortious interference with contract	2.234	0.128		
Other contract	1.058	0.939		
Other real property	1.410	0.612		
Model significance	.0000		.0000	
Pseudo r-squared	.187		.023	
Number of cases	4,332		4,376	

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)

Note: Dependent variable is whether a punitive damage award was made. The sample is limited to cases won at trial by plaintiffs. Models account for clustering at the county level and the stratified sampling pattern.

* $p < .05$
** $p < .01$
*** $p < .001$

category variables and the litigant pair variables, the odds multiplier is the amount by which the odds of a case having a punitive damages award should be multiplied compared to a reference category, holding all other variables constant. For the case categories, Table 3 shows that "products liability" is the reference category. An odds multiplier of 1.0 for a case category (the "other contract" category, for example, has an odds multiplier close to 1.0) indicates that, compared to products liability cases, the case category does not change the odds of receiving a punitive award. An odds multiplier greater than 1.0 indicates case categories with chances of a punitive award that are greater than the chances in products liability cases. An odds multiplier of less than 1.0 indicates case categories with chances of a punitive award that are less than the chances in products liability cases. For example, Table 3 shows that the odds multiplier for motor vehicle tort cases (0.131) is substantially less than 1.0 and thus that, compared to the reference category (products liability), motor vehicle cases are much less likely to end with a punitive award. Similarly, the large odds ratio for intentional tort cases (5.130) indicates that they are unusually likely to have a punitive award.

The odds of a punitive damages award should be distinguished from the probability of a punitive damages award, even though the terms "odds" and "probability" are often used interchangeably in informal conversation. For example, Table 3 shows that the intentional tort case category has an odds multiplier of 5.130. Assume that the odds of a punitive damages award (based on all of the other factors about a case) are 1:1, corresponding to a 50% probability of a punitive award. The odds multiplier of 5.130 means that the classification of the case as one of intentional tort changes the odds of a punitive damages award from 1:1 to 5.130:1, corresponding to a probability of punitive damages award of approximately 84%.[58]

The interpretation of the compensatory damages variable, which is continuous, differs from the interpretation of the case category and litigant pair variables, which are dichotomous. For the compensatory damages variable, the odds multiplier represents the effect of a unit increase in the variable. For example, an increase in the compensatory award (log) from 1.0 to 2.0 would require multiplying the odds of a punitive award by 1.37. The second and fourth columns in Table 3

tained by taking the anti-log of the regression coefficient. HOSMER & LEMESHOW, *supra* note 48, at 50; J. SCOTT LONG, REGRESSION MODELS FOR CATEGORICAL AND LIMITED DEPENDENT VARIABLES 79–82 (1997).

58 This description of the marginal effect of case category is slightly oversimplified. In the logistic model, the effect of a change in one variable on the dependent variable depends on the values of the other variables. LONG, *supra* note 57, at 82. For a set of "average" values for the other variables, the presence of an intentional tort increases the probability of a punitive damages award by 0.082.

show significance levels for each odds multiplier: the probability that the observed result would occur by chance.

In the full model, the jury trial dummy variable's odds multiplier is greater than one, but is not statistically significant. The effect changes direction (from greater than one to less than one) in the simpler of the two models. One cannot reject the hypothesis that juries are no more likely than judges to award punitive damages. The odds multiplier on the interaction term, consisting of the jury trial dummy variable and the compensatory award, is less than one and is statistically significant in one model and of borderline statistical significance in the simpler model. Thus, judges, more than juries, display a tendency to award punitive damages as the stakes increase.[59]

Table 3 confirms Table 1's evidence that certain classes of tried cases—intentional tort, fraud, and employment cases—are more likely than other classes of tried cases to lead to punitive awards. The odds multipliers for these variables are all large and statistically significant. These results are similar to findings from the 1991–92 CTCN data.[60]

Another noteworthy result in Table 3 is the absence of support for the belief that adjudicators are more likely to award punitive damages against corporate defendants. The odds multiplier for the "individual only vs. corporation" dummy variable is less than 1.0. Compared to the reference category of cases involving "individuals only vs. individuals only," plaintiffs are more likely to obtain punitive awards against individual defendants than against corporate defend-

59 The special sampling characteristics of Harris County, Texas, *supra* note 19, led us to run Table 3's full model with dummy variables for Harris County judge and jury trials. Each of the dummy variables is positive and significant, indicating that both Harris County judges and juries are more likely to award punitive damages than the reference category, which consists of judge trials in other counties. Harris County judges are significantly more likely to award punitive damages than Harris County juries. In this model, both the interaction term and the jury trial dummy variable become significant, but in opposite directions. The jury trial dummy variable's coefficient is 1.002 and the jury trial dummy compensatory damages interaction term's coefficient is –0.154. This suggests that juries are more likely to award punitive damages at the low end of compensatory awards, up through about $670,000, at which point judges become more likely to award punitive damages. In a model that excludes the interaction term, the jury trial dummy variable is positive, but not statistically significant.

60 Eisenberg et al., *supra* note 5, at 645 tbl.4. The low rate of punitive awards in cases involving government defendants is consistent with the common rule that punitive damages cannot be awarded against the government. *See, e.g.*, ARIZ. REV. STAT. ANN. § 12-820.04 (West 1992) (no punitive damages against public entity or employee); FLA. STAT. ANN. § 768.28(5) (West Supp. 2002) (government immune from punitive damages); HAW. REV. STAT. ANN. § 662-2 (Michie 1995) (same); IND. CODE ANN. § 34-13-3-4 (Michie 1998) (same); MINN. STAT. ANN. § 3.736(3) (West 1997) (same); *id.* § 466.04(1)(b) (West 2001) (no punitive damages against municipalities); MO. REV. STAT. § 537.610(3) (2000) (no punitive damages against state and its public entities); WIS. STAT. ANN. § 893.80(3) (West Supp. 2001) (same). The data include some cases in which the government is a defendant, presumably because other parties also are defendants.

ants, though the effect is not statistically significant. Because businesses' concern focuses especially on juries' tendency to award punitive damages, we have run a model similar to the large model in Table 3, but adding an interaction term to explore the differences between judges and juries. That model, not reported here, includes an interaction term equal to the product of the jury trial dummy variable and the individual only vs. corporation dummy variable. The odds multiplier for the interaction term is less than one and is statistically significant.[61] Thus, compared to judges, our models suggest that, conditional on a case being tried, juries are significantly less likely than judges to award punitive damages in cases involving individual plaintiffs and corporate defendants.

C. Interpreting the Results in Light of Other Recent Studies

The absence of significant difference between judge and jury decisions to award punitive damages is consistent with experimental evidence comparing groups of adjudicators. Researchers report finding "a remarkable consensus in the judgments of individual jurors, made on a rating scale, about a series of personal injury cases."[62] They also report strong agreement among sets of juries when judging punishment[63] and a strong correlation between the egregiousness of behavior and the decision to award punitive damages.[64] These experiments do not involve judges. But judges, like jurors, are people responding to stimuli. If many individual judgments reflect a "remarkable consensus," some of that consensus ought to survive when the individual judgments are those of judges. As Guthrie, Rachlinski, and Wistrich state, "Judges, it seems, are human."[65]

Our results are less consistent with judge-jury punitive damages experiments of Hastie and Viscusi. They report that mock jurors were much more likely than judges to impose punitive sanctions in a hypo-

61 Coefficient = –1.182; $p = 0.011$. The coefficient is –0.905 (corresponding to an odds multiplier of less than 1.0) and the significance level is 0.050 in a model that includes dummy variables for Harris County, Texas judge and jury trials. *See supra* note 19.

62 Schkade et al., *supra* note 1, at 1141; *cf.* Wissler et al., *supra* note 3, at 815 (concluding that, although high unpredictability exists when examining award patterns of individual jurors, jurors as a group "produce considerably more stable and predictable outcomes").

63 Schkade et al., *supra* note 1, at 1152 & tbl.3.

64 *See* Michelle Chernikoff Anderson & Robert J. MacCoun, *Goal Conflict in Juror Assessments of Compensatory and Punitive Damages*, 23 LAW & HUM. BEHAV. 313, 326–28 (1999). *But see* Reid Hastie et al., *Juror Judgments in Civil Cases: Effects of Plaintiff's Requests and Plaintiff's Identity on Punitive Damage Awards*, 23 LAW & HUM. BEHAV. 445 (1999) (finding that mock jurors award punitive damages when courts found them unwarranted).

65 Guthrie et al., *supra* note 3, at 821; *cf.* Richard A. Posner, *What Do Jndges and Justices Maximize? (The Same Thing Everybody Else Does)*, 3 SUP. CT. ECON. REV. 1, 4 (1993) ("By treating judges and Justices as ordinary people, my approach makes them fit subjects for economic analysis; for economists have no theories of genius." (footnote omitted)).

thetical railroad accident case.[66] Similarly, in experiments involving airplane repair scenarios, Viscusi reports evidence that "judges are much more willing than jurors . . . to refrain from imposing punitive damages."[67]

Unlike the classic Kalven and Zeisel judge-jury study, these researchers did not study actual judges and juries. Kalven and Zeisel's questionnaires to presiding judges in about 4,000 actual civil jury trials in the 1950's yielded data showing a 78% agreement between judge and jury on liability.[68] In the cases of disagreement, there was a very small tendency of juries to favor plaintiffs.[69] And the rate of agreement, although not 100%, is high compared to other human endeavors.[70]

The vast difference between experimental punitive damages results and real case evidence of absence of disagreement between judges and juries is worth addressing. It is theoretically possible that real-world judges and juries could so substantially agree on liability, as Kalven and Zeisel found,[71] and yet, as the recent experimental evidence might suggest, so massively disagree on the decision to award punitive damages.[72] But experiments searching for substantial disagreement between judges and juries on punitive damages might try to replicate the real world rate of agreement between judges and juries about liability. Otherwise, a risk exists that the experiments' judge-jury difference in awarding punitive damages results from an experimental design that artificially inflates the differences.

---

66 Hastie & Viscusi, *supra* note 1, at 916; *see also* Viscusi, *supra* note 10, at 387–88 (finding that judges "perform better than jurors with respect to hindsight bias").

67 W. Kip Viscusi, *Jurors, Judges, and the Mistreatment of Risk by the Courts*, 30 J. LEGAL STUD. 107, 115 (2001) (comparing Hastie & Viscusi's mock jurors with judges).

68 HARRY KALVEN, JR. & HANS ZEISEL, THE AMERICAN JURY 63–64 (Univ. of Chi. Press 1971) (1966).

69 *Id.* at 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. *See also* Hans Zeisel, *The American Jury*, *in* THE AMERICAN JURY SYSTEM (FINAL REPORT) 65, 69–70 (Annual Chief Justice Earl Warren Conference on Advocacy in the United States ed., 1977) (discussing analysis of judge and jury disagreement); Marc Galanter, *The Civil Jury as Regulator of the Litigation Process*, 1990 U. CHI. LEGAL F. 201, 204–05 (reporting more recent polls supporting similar results).

70 As previous research has concluded:

> 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 pbysicians diagnosing patients, and almost as good as the 79% or 80% rate of agreement between judges making sentencing decisions in an experimental setting.

Clermont & Eisenberg, *supra* note 3, at 1153; *see also* Wissler et al, *supra* note 3, at 814 tbl.5 (comparing reliability of various decisionmakers).

71 *See supra* note 68 and accompanying text.

72 *See supra* notes 66–67 and accompanying text.

Unlike Kalven and Zeisel's experiments, the recent experiments do not achieve substantial judge-jury agreement on liability. In an airplane accident scenario, judges and mock jurors were asked whether a firm should undertake a repair that was constructed to be economically inefficient. Judges and experimental jurors disagreed massively on this basic liability question, with nearly 90% of the jurors requiring repair of the plane compared to less than 50% of the judges.[73] The vast judge-jury difference on liability in the new studies calls into question the experimental designs' ability to detect socially meaningful punitive damages differences between judges and juries. If the Kalven and Zeisel and other results[74] are valid, the new experiments may employ scenarios and assumptions that occur too rarely to be of social significance.[75] The airplane scenarios seem especially questionable as evidence exists that punitive awards are unusually rare in such accidents.[76]

---

73 *See* Viscusi, *supra* note 67, at 113 tbl.4, 114 (comparing judges' and juries' results).

74 "[C]onsiderable research indicates that juries are not substantially different from judges." Clermont & Eisenberg, *Appeals I*, *supra* note 4, at 145; *see also supra* note 3 (citing examples of such research). Indeed, "virtually no evidence exists to support the prevailing ingrained intuitions about juries"; instead "the evidence, such as it is, consistently supports a view of the jury as generally unbiased and competent." Clermont & Eisenberg, *supra* note 3, at 1151–52.

75 Nevertheless the newer work would represent a useful addition to the study of differences between judges and juries. Comments about why their findings differ from findings of adjudicator similarity may be helpful. First, the experiments may elicit greater punitive damages rates from jurors because the experimenters offer jurors only punitive damages as a remedy. *E.g.*, Hastie & Viscusi, *supra* note 1, at 905. A punitive award is the only way for the jurors to express disapproval of behavior. Judges would more likely understand that the existence of a compensatory award already expresses disapproval of the behavior.

Second, the experimental scenarios yielding massive judge-jury differences in willingness to award punitive damages do not represent real-world punitive damages cases. In cases before both judges and juries, over 50% of punitive awards are in cases involving fraud, intentional tort, or employment matters. Products liability cases, which seem to be most analogous to the airplane scenario, constitute a trivially small fraction of punitive awards. *See supra* Table 1 and note 6. Railroad crossing cases are not numerous enough to be accounted for in the CTCN data. *See supra* Table 1.

Third, the sample of judges used in the experiments is not representative. The judges were attendees at a law and economics program. Viscusi, *supra* note 67, at 109. Such a group of judges seems more likely to respond based on economic efficiency than a random sample of judges, especially to questions whose "right" answers depend on the ability and willingness to engage in cost-benefit analysis. *See* Guthrie et al., *supra* note 3, at 818 n.201 (noting problems with the sample in the Hastie and Viscusi studies).

76 *See* JAMES S. KAKALIK ET AL., COSTS AND COMPENSATION PAID IN AVIATION ACCIDENT LITIGATION 27 (1988) ("[P]unitive damages were not paid on any of the 2,198 closed [aviation accident] cases in our study; in two cases, they were awarded in trial but reversed on appeal.").

D. Selection Effect Considerations

A remaining issue is the need to account for the nonrandom routing of cases between judges and juries. Interpreting empirical legal findings requires caution about accepting results at face value where strong selection effects may be at work.[77] Table 1 suggests that juries tend to adjudicate large-stakes cases, as measured by both compensatory and punitive damages. Federal data confirm this,[78] as do studies using jury verdict reporter data.[79] This skewed routing of cases to juries cannot conclusively be proven from our data, which lack the ex ante characteristics of the cases. But given perceptions about jurors' greater inclination to award punitive damages, one expects juries to see a sample of cases biased in favor of strong candidates for punitive awards.[80] Looked at differently, a study in which the higher-stakes cases that are more prone to punitive damages are in fact being routed to judges would be more remarkable than any of our findings.

1. *Selection Attributable to Case Routing Between Judge and Jury*

The most realistic assumption is that what are perceived as high-stakes cases with punitive damages potential are more likely to be routed to juries than judges. If plaintiffs select juries for cases with high probabilities of punitive awards, Table 3 may overstate the differences between judges' and juries' tendencies to award punitive damages and could be interpreted as precluding rejection of the hypothesis that judges are more likely than juries to award punitive damages. If judges saw the same group of high-punitive-probability cases that we believe juries see, a model similar to that depicted in Table 3 might reveal a greater tendency by judges to award punitive damages. Table 3's interaction term already suggests that judges are more likely than juries to award punitive damages as compensatory awards increase.[81]

Another possible routing effect relates to the perceived greater reliability of judges compared to juries. Plaintiffs may regard judges

---

77 *E.g.*, Kevin M. Clermont & Theodore Eisenberg, *Do Case Outcomes Really Reveal Anything About the Legal System? Win Rates and Removal Jurisdiction*, 83 CORNELL L. REV. 581, 588–91 (1998).

78 Clermont & Eisenberg, *supra* note 3, at 1162–65. Conditional on plaintiff winning at trial, separate logistic regressions of the mode of trial (jury or judge) for each case category yield positive coefficients on the compensatory award (log) level. The other explanatory variables used are dummy variables for the litigant characteristics.

79 KELSO & KELSO, *supra* note 6, at 21; Eric Helland & Alexander Tabarrok, *Runaway Judges? Selection Effects and the Jury*, 16 J.L. ECON. & ORG. 306, 323–24 (2000).

80 *Cf.* Helland & Tabarrok, *supra* note 79, at 325–27 & tbl.4, app. A, at 330 (finding differences in samples of cases routed to judges and juries).

81 Separate logistic regression of judge-tried cases and jury-tried cases confirm this finding. The coefficient for compensatory damages (log) is noticeably higher in the judge-tried cases' regression.

as more reliable, and thereby opt for trial before judges in their strongest cases on the merits.[82] Similarly, the plaintiff with a strong punitive damages case may opt for a judge trial to avoid the less reliable jury because the jury might refuse to grant a punitive award in a case that merits an award. If such routing occurs, then one might observe judges awarding punitive damages at a greater rate than juries and defendants opting for jury trials when the case for punitive damages is weakest. Absent some affirmative evidence of such routing, we are skeptical that it systematically occurs. Perceptions that juries are more likely to award punitive damages, and more likely to award more in punitive damages, are widespread.[83] Indeed, until the CTCN data were gathered, the surprisingly prominent role of judges in the punitive damages area was, we suspect, unknown. Systematically seeking the reliable judge to obtain a punitive damages award, when judges are not perceived as even awarding punitive damages, seems an unlikely effect. But we cannot eliminate this effect based on our data, and it could mask real judge-jury differences.

2. *Selection Attributable to Case Settlement*

Because juries are believed to be more unpredictable than judges, especially in high-award cases,[84] defendants may choose to settle cases that have high probabilities of large punitive awards. It is more difficult to account for this aspect of case selection.[85] Trials in cases in which jurors' propensity to award punitive damages is strongest may never be observed. Juries are viewed as so much wilder than judges that they only rarely get to act in those cases in which their behavior would be expected to be wildest.[86]

---

82 *See supra* note 23. This effect, applied symmetrically to plaintiffs and defendants, should lead to all trials being jury trials because the party with the weaker case should opt for the adjudicator perceived to be less reliable. Yet we observe many judge trials, perhaps because parties differ in their evaluation of the merits of the same case.

83 Clermont & Eisenberg, *supra* note 3, at 1127–28.

84 *See* Helland & Tabarrok, *supra* note 79, at 306–08.

85 *See* Thomas Koenig, *The Shadow Effect of Punitive Damages on Settlements*, 1998 WIS. L. REV. 169, 171–72 (noting that debate has shifted to this "shadow effect," and finding that fear of runaway damages is exaggerated); Herbert M. Kritzer & Frances Kahn Zemans, *The Shadow of Punitives: An Unsuccessful Effort to Bring It into View*, 1998 WIS. L. REV. 157, 157–59.

86 *See, e.g.*, David Segal, *Tag-Team Lawyers Make Businesses Blink*, WASH. POST, Nov. 12, 1999, at A1. Studies funded by industry or tort reform groups, which find evidence that punitive damages are often requested, conclude that the frequency of such requests poses a serious problem. *See* KELSO & KELSO, *supra* note 6, at 5 n.3; JOHN H. SULLIVAN, NEW STATE DATA CONFIRMS [sic] RUNAWAY ABUSE OF PUNITIVE DAMAGES (surveying incidence of punitive damage demands in various jurisdictions), http://www.cjac.org/research/punitivedamages.pdf (last visited Jan. 22, 2002). A study that followed up on the tallying of claims for punitive damages by studying whether awards were in fact obtained suggests the need for caution in interpreting the effect of punitive damages claim rates. The study found that 3,763 of 25,561 (14.7%) tort cases filed in Georgia state courts from 1994 to 1997 involved claims for punitive damages. These 3,763 cases yielded 15 punitive damages

The CTCN data's pattern should partially alleviate this concern. That judges award punitive damages as frequently as juries is surprising and has implications for the dogma of juror bias. Because judges award punitive damages at rates similar to juries, the simplest description of the data, admittedly ignoring case routing, is that juries and judges behave similarly, as Kalven and Zeisel found.[87] Even if they do not behave similarly, jurors' shadow effect on settlements cannot be based on the observed pattern of judge and jury trial outcomes. The presumed settlement behavior may be based on misperception.[88]

Second, available data suggest that there is no hidden mountain of settlements involving punitive damages masking the ability of trial studies to detect punitive damages' true effects. Karpoff and Lott studied eleven years (1985–96) of lawsuits in which plaintiffs sought punitive damages from publicly traded businesses.[89] They found that settlement amounts (including both punitive and nonpunitive damages) constituted a near-trivial portion of awards paid, and that trial verdicts dwarfed settlements in both number and amount.[90] They also found no statistically significant abnormal stock returns and concluded that "the data from our sample do not support" the hypothesis that settlements shaped by punitive damages comprise the main effect of punitive damages.[91]

The CTCN data allow further study of the settlement effect, albeit not as directly as the Karpoff-Lott study. The existence of statutory caps on punitive damages in several states allows for an empirical test of the settlement aspect of case selection. In states that cap punitive

---

awards, with judges disproportionately making the awards, not juries. Eaton et al., *supra* note 5, at 1060, 1094. Sullivan, for example, reports high rates of claims for punitive damages but does not report the resulting yield in actual punitive awards. *See* SULLIVAN, *supra*.

87 *See* KALVEN & ZEISEL, *supra* note 68, at 63–64.

88 If defendants are rational economic actors, knowledge of the sober pattern of punitive awards should lead them to be less averse to pressing potentially damaging cases to trial. Punitive awards are rare; "bet-the-company" cases are also rare. The settlement selection effect could affect some potentially large cases, but might not be expected to influence the pattern observed in the mass of cases. Furthermore, plaintiffs may play on defendants' fears of juries to increase their settlement demands to the point where defendants must strongly consider taking a chance on trial.

89 Karpoff & Lott, *supra* note 8, at 534–35.

90 *See id.* at 537.

91 *Id.* at 562. But serious doubts exist about the representativeness of the Karpoff-Lott sample, and further study of this issue is warranted. Their data show 94 settlements and 1,595 plaintiff verdicts. *Id.* at 538 tbl.2. This settlement-verdict ratio suggests that the vast majority of plaintiff recoveries are via verdicts rather than settlements, which seems to contradict virtually every other study of civil litigation. In addition, their finding that 310 of 352 products liability verdicts were for plaintiffs, *see id.*, contradicts all other studies of products liability litigation, as does their report that, across all lawsuit types, plaintiff verdicts were 85% of all verdicts. *See id.* Either suits seeking punitive damages against publicly traded corporations are shockingly different from other lawsuits or some further explanation of their data is needed.

awards, plaintiffs' threat to go to the jury should not intimidate defendants into settling nearly as much as in states that do not cap awards. If defendants are only gambling against a punitive award of two or three times the compensatory award—the typical cap—the pressure to avoid the jury substantially diminishes. Therefore, in states that cap punitive awards we might expect defendants to press to jury trial potential punitive cases that they would settle in states without caps. The percentage of jury trials ending in punitive awards increases, but defendants know that the punitive award is substantially less likely to bankrupt them because of the caps. If defendants settle enough cases to call into question the implications of studies limited to trials, this effect should be substantial.[92]

To explore this possibility, we introduce a dummy variable equal to one in states that cap punitive awards. The CTCN sample includes counties in twenty-one states, and several of these states cap punitive awards.[93] Connecticut, Florida, Georgia, Illinois, Indiana, Texas, and Virginia did so for all or part of the period covered by the CTCN

---

92 For a model of the effect of caps on awards on settlement rates, see Linda Babcock & Greg Pogarsky, *Damage Caps and Settlement: A Behavioral Approach*, 28 J. LEGAL STUD. 341, 368 (1999). The authors report experimental evidence that a cap on damages has a positive and marginally significant effect on settlement rates. *Id.* The caps they assess differ from punitive damages caps because punitive damages caps do not cap the total award. The compensatory award is still unlimited in most states in most classes of cases. The risk averseness of defendants who settle rather than take a chance before a jury without a cap on punitive awards suggests that imposing a punitive cap could promote trial rather than settlement of cases with large potential punitive awards. One study reports experimental evidence that capping punitive damages awards causes jurors to inflate compensatory awards and summarizes other studies on the subject. Edith Greene et al., *The Effects of Limiting Punitive Damage Awards*, 25 LAW & HUM. BEHAV. 217 (2001).

93 *E.g.*, CONN. GEN. STAT. ANN. § 52-240b (West 1991) (capping punitive damages at twice compensatory damages in products liability cases); FLA. STAT. ANN. § 768.73(1) (West Supp. 2001) (generally capping punitive damages at three times compensatory damages); GA. CODE ANN. § 51-12-5.1(g) (2000) (capping punitive damages at $250,000 in some tort actions); IND. CODE ANN. § 34-51-3-4 (Michie 1998) (capping punitive damages at the greater of three times the amount of compensatory damages or $50,000); N.J. STAT. ANN. § 2A:15-5.14 (West 2000) (capping punitive damages at five times the amount of compensatory damages or $350,000, whichever is greater); TEX. CIV. PRAC. & REM. CODE ANN. § 41.008 (Vernon 1997) (capping punitive damages at two times the amount of economic damages plus the greater of either $200,000 or an amount equal to any noneconomic damages found by the jury, not to exceed $750,000); VA. CODE ANN. § 8.01-38.1 (Michie 2000) (capping punitive damages at $350,000).

Illinois capped punitive damages at three times compensatory for other than "healing art or legal malpractice cases." 735 ILL. COMP. STAT. ANN. 5/2-1115.05(a) (West Supp. 2001). But the provision, which was effective March 9, 1995, was invalidated when the entire act of which it was a part was struck down on state constitutional grounds in *Best v. Taylor Machine Works*, 689 N.E.2d 1057 (Ill. 1997). Ohio capped punitive damages as of January 27, 1997, but the provision was struck down by the Ohio Supreme Court. State *ex rel.* Ohio Acad. of Trial Lawyers v. Sheward, 715 N.E.2d 1062, 1090–91 (Ohio 1999).

sample.[94] We set the dummy variable equal to one for cases decided at a time when state law imposed caps on punitive awards.

Strong settlement selection effects may exist if this dummy variable correlates with punitive damages award frequency in jury trials. Table 4, however, shows that this is not the case. Limiting the sample to jury trials won by plaintiffs, Table 4 shows that the punitive award rate was lower in states with caps than in states without caps. If one includes jury trials won by defendants, the rates of punitive awards in capping states remain lower. Regression analysis of jury trial punitive awards using models similar to those in Table 3 yields insignificant results for the capping dummy variable. We thus find no evidence that caps which reduce the risk of ruinous punitive damages liability affect the rate of punitive awards in jury-tried cases.[95]

There are, of course, other possible explanations for the absence of effect. Capping and noncapping states might differ in ways that our models do not capture. Studying punitive award patterns within capping states before and after enactment of punitive award caps might be revealing.[96]

---

94 Connecticut, Georgia, Indiana, and Virginia capped punitive damages throughout the period covered by the CTCN sample. *See supra* note 93. Texas enacted meaningful caps during the period, *see supra* note 93, so not all Texas sample cases are subject to caps. Texas earlier capped punitive damages at the greater of four times the compensatory award or $200,000, but excepted intentional torts and malicious behavior. 1987 Tex. Gen. Laws ch. 2, § 2.12. This earlier cap affected one Harris County, Texas jury trial in our sample, *see Verdict Reduced, supra* note 44, but the uncapped amount is used in the data. Florida changed the way it capped during the sample period. We have repeated our analysis excluding Florida cases with no material change in results. New Jersey enacted caps effective for cases filed after October 27, 1995, *see supra* note 93, too late in the CTCN period to materially affect results. Illinois imposed caps early in 1995 but they were invalidated in 1997, *see supra* note 93, and defendants might not yet have come to rely on them to a degree sufficient to affect results. We have repeated the analysis treating Illinois as an uncapped state without material effect on the results.

95 We do find a positive, significant correlation between the existence of caps and punitive awards in judge-tried cases. However, this effect is not robust to the exclusion of Harris County, Texas. *See supra* note 19. The insignificance of the jury trial effect is not sensitive to inclusion of Harris County. The increasing use of statutory caps on punitive damages awards raises the question whether a variable should account for the existence of caps. Except for an earlier capping statute that later affected a Texas award, *see supra* notes 44, 94, all of the capping systems in the states studied here either took effect after the decisions were rendered or imposed limits that were not reached in any case subject to them. Thus, the caps did not directly affect punitive award levels even if they might have affected settlement behavior. Knowledge of a cap, however, might affect an adjudicator who knows of its existence even if an award does not exceed the cap. *See* Greene et al., *supra* note 92; Hastie et al., *supra* note 64.

96 *See* Albert Yoon, *Damage Caps and Civil Litigation: An Empirical Study of Medical Malpractice Litigation in the South*, 3 AM. L. & ECON. REV. 199 (2001) (studying the effect of Alabama's cap on medical malpractice awards).

TABLE 4: PUNITIVE AWARD FREQUENCY BY STATES' CAPPING STATUS (JURY TRIALS ONLY)

	Jury Trials Won by Plaintiffs		All Jury Trials	
	No Punitive Award	Punitive Award	No Punitive Award	Punitive Award
States without caps	1,996 (95.73%)	89 (4.27%)	4,456 (98.04%)	89 (1.96%)
States with caps	924 (96.65%)	32 (3.35%)	1,853 (98.30%)	26 (1.70%)
Significance of difference in punitive award rates	.228		.484	

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)

III
THE RELATION BETWEEN PUNITIVE DAMAGES AND COMPENSATORY DAMAGES

This Part shifts the focus from the decision to award punitive damages and studies instead the level of punitive damages, given a punitive award. All studies confirm that juries' punitive damages awards correlate strongly with compensatory awards.[97] We now further assess Figure 1's indication that a similar relation exists in cases tried by judges, and Figure 2's and Table 2's indications that the punitive-compensatory ratio does not vary between judge and jury trials.

A. Modeling the Level of Punitive Damages Awards

Given a decision to award punitive damages, what should determine their level? Previous work outlines a model in which the most important influences on the punitive award level are the harm caused, the egregiousness of the misbehavior, and the amount needed to accomplish the goals of punishment or deterrence.[98] We therefore discuss the model only briefly here.

The punishment meted out in a punitive award ought to relate to the harm the defendant caused. We tend to regard behavior as worse when it causes more damage. The compensatory component of the damages award is a measure of the harm the defendant has caused. Thus, punitive damages ought to correlate positively with compensatory damages.[99] We therefore include the compensatory award level (log) in our model of punitive damages.

The punishment-deterrent rationale underlying punitive awards suggests that the level of punitive award ought to relate to the defen-

97 *See* Eisenberg et al., *supra* note 5, at 650–51.

98 *See id.* at 637–40.

99 This harm-measuring use of compensatory damages in setting a punitive award is distinct from the use of compensatory damages to compensate victims. *Id.* at 639.

dant's financial circumstances,[100] though the theoretical soundness of considering defendants' wealth is debated in the law and economics literature. We found evidence of this effect in the 1991–92 CTCN data.[101] We therefore include dummy variables representing the litigant pairs summarized in Table 1 in the punitive damages model.

Independent of the harm caused, the defendant's degree of misbehavior ought to influence the punitive award level. Table 3 shows that case categories are useful proxies for punishable behavior. Fraud and intentional tort, which correlate positively with the decision to award punitive damages, represent intentional misbehavior. Given a decision to award punitive damages, however, a case's subject area should become less relevant to the award level.[102] "Intentional or egregious wrongdoers are more serious wrongdoers whether they produce products, publish newspapers, harm patients, or defraud contractual partners. They are united more by the nature of their wrongdoing than by the subject area of their wrong."[103] We thus expect, as found in the 1991–92 CTCN data, the subject matter of a case to play less of a role in explaining the size of punitive damages awards than it plays in explaining the decision to award punitive damages.

To these previously studied influences on punitive awards, we add variables to represent the adjudicator. As in our decision-to-award models, a jury trial dummy variable assesses whether the intercept for punitive awards changes in jury trials compared to judge trials. An interaction term between the jury trial dummy variable and the compensatory award (log) assesses whether the slope of the punitive-compensatory line changes in jury trials compared to judge trials. The direction and significance of the coefficients for these variables are of primary interest here.

B. Results

Table 5's first numerical column reports the results of the above model, in which the punitive award (log) is the dependent variable in a sample of 171 cases in which plaintiffs won a nonzero punitive award at trial.[104] This model includes all the variables and shows a strong, significant correlation between the compensatory award and the puni-

100 *See id.* at 639–40.

101 *See id.* at 648 tbl.5.

102 *Id.* at 630.

103 *Id.*

104 Table 5 excludes three punitive damages cases in which the compensatory award was zero. For discussion of punitive awards in cases with zero or nominal damages awards, see Eisenberg et al., *supra* note 5, at 629–30. For a notable such case, see *Tronzo v. Biomet, Inc.*, 236 F.3d 1342 (Fed. Cir. 2001), a patent infringement case that, after a remand and partial reversal on appeal, resulted in a $20 million punitive award and a $526 compensatory award.

tive award. As was the case for the 1991–92 CTCN data,[105] the compensatory award is the most powerful predictor of the punitive award. The compensatory award alone explains about half the variance in the punitive award. If the compensatory award is a rational measure of harm caused—and evidence exists to suggest that it is[106]—one can also use the compensatory award to partly control for differences across judge-jury cases, especially the greater tendency of higher stakes cases to go to juries.[107]

Table 5's third model changes the dependent variable from the punitive award (log) to the ratio of the punitive and compensatory award (logs), as described in Table 2 and Figure 2. From one perspective, this is the variable of primary interest. Holding other factors constant, do judges and juries differ in the amount of punitive damages awarded per unit of compensatory damages? Except for the change in the dependent variable, the third model is the same as the first model. The litigant pair categories and case categories, reported in the Appendix,[108] behave about the same as in the first model. The model's explanatory power is substantially lower because the punitive-compensatory ratio is so unvarying.[109]

The two jury trial variables are not statistically significant. In the first model, the jury trial dummy variable is negative and statistically insignificant.[110] The interaction term is positive but also statistically insignificant.[111] None of the models support the hypothesis that

105 Eisenberg et al., *supra* note 5, at 648 tbl.5.

106 *See* Merritt & Barry, *supra* note 6, at 364–65; Frank A. Sloan & Chee Ruey Hsieh, *Variability in Medical Malpractice Payments: Is the Compensation Fair?*, 24 LAW & SOC'Y REV. 997, 1025 (1990); Vidmar, *supra* note 3, at 868–70.

107 Other variables, reported in Appendix Table 2, yield results generally similar to those in the 1991–92 data but with some differences worth noting. The coefficient on the dummy variable "individual only vs. corporation" is positive, as was the most analogous variable in the 1991–92 data, and their 95% confidence intervals overlap. Eisenberg et al., *supra* note 5, at 648 tbl.5. But the effect is statistically significant in the 1991–92 data and is not statistically significant in the 1996 data.

Case category effects initially appear to be stronger in the 1996 data than in the 1991–92 data, as evidenced by significant differences between the reference case category, products liability, and many other case categories. But using products liability as the reference category tends to overstate the importance of case category effects because it has only one punitive award, and that award is relatively high. Using motor vehicle tort, a more middle-of-the-road case category with respect to punitive award levels, shows that only products liability cases, which are higher, and slander/libel cases, which are lower, differ at the 0.05 level from motor vehicle cases. A Bonferroni correction to the test of the hypothesis that all case category coefficients are jointly zero yields p=0.175. *See* JASON C. HSU, MULTIPLE COMPARISONS: THEORY AND METHODS 175–80 (1996) (discussing abuses in multiple comparisons). Case category remains a weak force in explaining the level of punitive awards.

108 *See infra* Appendix Table 2.

109 *See supra* Table 2.

110 p=0.726.

111 p=0.918.

TABLE 5: REGRESSION MODELS OF PUNITIVE DAMAGES LEVELS

Variable	Dependent Variable		
	Punitive Damages (log)	Punitive Damages (log)	Punitive-Compensatory Ratio (logs)
Compensatory damages (log)	0.854***	0.883***	0.002
	(0.144)	(0.096)	(0.019)
Jury trial dummy	-0.360	-0.240	0.036
	(1.047)	(0.959)	(0.126)
Jury trial × log of compensatory	0.029	0.056	-0.014
	(0.225)	(0.198)	(0.026)
Model significance	.0030	.0000	.0022
r-squared	.621	.523	.196
Number of cases	171	173	171

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)

Note: Models account for clustering at the county level and the stratified sampling pattern. Robust standard errors are in parentheses. Plaintiff/defendant status dummy variables and case category dummy variables, included in the first and third models, are reported in Appendix Table 2.

* $p < .05$

** $p < .01$

*** $p < .001$

judges and juries differ in the way they set levels of punitive awards or in the amount of punitive damages awarded per unit of compensatory damages. The other variables in the fuller model—compensatory award, case category dummies, and litigant pair dummies—provide substantial control for the known principal differences between cases routed to juries and judges. That is, judges and juries are known to see cases with different stakes and with different distributions of case categories.[112] The insignificance of the judge-jury differences survives when we account for selectivity and is not sensitive to inclusion of Harris County, Texas.[113]

C. The Greater Spread in the Jury Regression Models

The preceding section addresses the question whether there is a mean effect between judges and juries. The regressions assume that

112 *See supra* notes 21–23, 51–52 and accompanying text.

113 *See supra* note 19 and accompanying text. We accounted for selectivity by using two Heckman models in which the decision to award punitive damages is estimated jointly with the level of damages for those cases in which punitive damages are awarded. In both models the selection equation included the jury trial dummy variable, the compensatory award, the litigant pair dummy variables, and the case category dummy variables. In both models we accounted for county-level clustering and for the sampling pattern used. In one model, the punitive award level equation used the jury trial dummy variable, the interaction term, the compensatory award, and the litigant pair dummy variables. In the second model, the punitive award level equation included the case category dummy variables instead of the litigant pair dummy variables.

the variability in the judge and jury regression models is the same. This section considers how juries and judges differ not in their mean behavior, but in their variability.

We study variability by analyzing the residuals of Table 5's two full regression models, those reported in the table's first and third numerical columns. Experimental research suggests that the regression models should be less able to explain jury awards than judge awards.[114] This weaker explanatory power should manifest itself in greater departures from the predicted regression values. Residuals measure the difference between predicted values and observed values of a regression model, and should be higher in jury-tried cases. Because we are interested in both larger positive and larger negative residuals, and residuals have a mean equal to zero, we analyze the absolute value of the residuals.

Table 6 presents the results. For Table 5's first model, the residuals in jury-tried cases are substantially and significantly higher than the residuals in judge-tried cases. This is consistent with Fignre 1's scatterplot, which shows more jury-tried cases than judge-tried cases are distant from the best-fitting regression line, and which explains more of the variance in judge-tried cases than jury-tried cases.[115] It is also consistent with studies finding greater variability in jury awards.[116] But the significance of the difference in residuals is sensitive to the inclusion of Harris County, Texas, the county with the least complete sample of trials.[117] Excluding that county leaves the mean and median differences in judge-jury residuals far from significant.[118] In Table 5's third model, which uses the punitive-compensatory ratio as the dependent variable, differences between judge and jury residuals are at most marginally significant, even with the inclusion of Harris County. The statistically insignificant, or marginally significant, spread is consistent with Figure 2 and Table 2's analysis of the punitive-compensatory ratio. Jury punitive-compensatory ratios are more spread out than judge ratios but not dramatically so.

Because the hypothesis we explore is the absence of meaningful judge-jury differences, conservative data analysis counsels us to emphasize the model that maximizes the differences. That is, to include

---

114 *See* Wissler et al., *supra* note 3, at 783–87.

115 *See supra* note 35.

116 Wissler et al., *supra* note 3, at 783–87. For debate and evidence about whether the group deliberating process of juries will increase or decrease awards, and increase or decrease their variability, see Shari Seidman Diamond et al., *Juror Judgments About Liability and Damages: Sources of Variability and Ways to Increase Consistency*, 48 DEPAUL L. REV. 301 (1998); Guthrie et al., *supra* note 3, at 827; Stephan Landsman et al., *Be Careful What You Wish For: The Paradoxical Effects of Bifurcating Claims for Punitive Damages*, 1998 WIS. L. REV. 297, 322–23; Schkade et al., *supra* note 1; and Wissler et al., *supra* note 3, at 801–04.

117 *See supra* note 19 and accompanying text.

118 p=0.567 (mean) and p=0.355 (median).

TABLE 6: ANALYSIS OF ABSOLUTE VALUE OF RESIDUALS FROM REGRESSIONS REPORTED IN TABLE 5

Trial Mode	Mean	Median	Number of Cases
A. Dependent variable=punitive damages (log)			
Judge trial	0.870	0.626	53
Jury trial	1.347	1.023	118
Significance of judge-jury difference	.032	.024	
B. Dependent variable=punitive/compensatory ratio (logs)			
Judge trial	0.095	0.065	53
Jury trial	0.126	0.091	118
Significance of judge-jury difference	.171	.091	

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)

Note: The significance of means difference is based on a t-test; the significance of medians difference is based on a Mann-Whitney test.

Harris County and to use the model with punitive damages (log) as the dependent variable instead of the ratio-based model. In addition to analyzing judge-jury differences using residuals, one can explore how many jury trial punitive damages awards are beyond what judges might have awarded. Jury trials within the expected range of judge awards cannot be viewed as serious impediments to financial planning. If judges gave all punitive awards, defendants would still have to plan on awards in this range.

How many jury punitive awards could be considered, with traditional statistical certainty, to be beyond the range of likely awards in judge-tried cases? To answer this question we calculate how many jury awards fall within the 95% prediction band of the judge awards. Figure 3 explores this topic through the same scatterplot as Figure 1, but with two new lines superimposed on the data. The central line in the figure is the regression line that best fits the data in judge-tried cases. The lines above and below that line, labeled "Upper 95% prediction line" and "Lower 95% prediction line," respectively, are the upper and lower 95% prediction lines for judge-tried cases. These lines provide a way to identify punitive awards by juries or judges that bear an extreme relation to compensatory awards.[119] Scatterplot points above the upper 95% line are extreme in the sense that they represent punitive awards that are above the judge-tried case upper 95% prediction line. Scatterplot points below the lower 95% line are extreme in the sense that they represent punitive awards that are below the judge-tried case lower 95% prediction line.

119 *See* Eisenberg & Wells, *Predictability on Appeal*, *supra* note 8, at 61, 81.

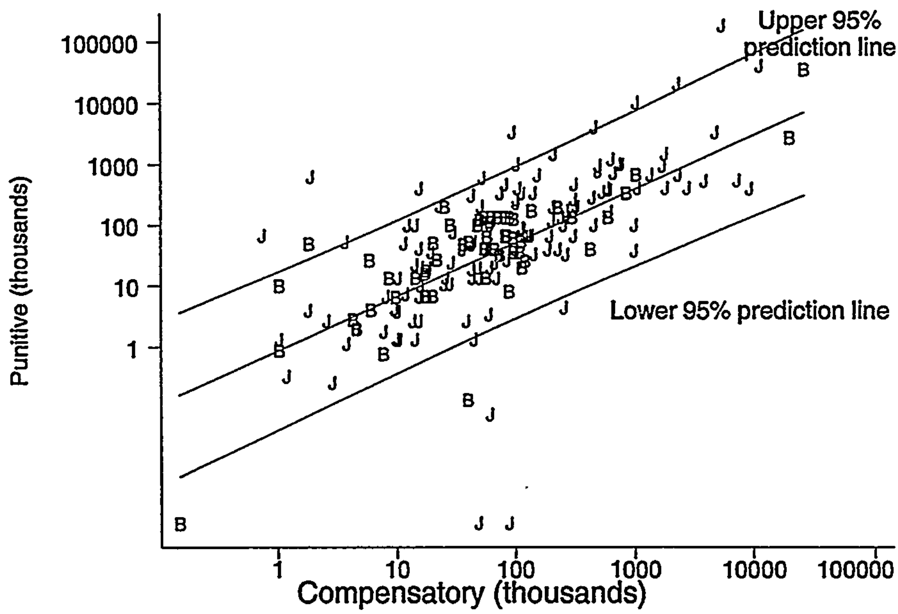

FIGURE 3. Punitive and compensatory damages by judge or trial status. The scatterplot reproduces the scatterplot in Figure 1. The lines summarize the relation between punitive and compensatory damages in judge trials. The top and bottom lines represent the upper and lower 95 percent prediction bands for the relation between punitive and compensatory awards in judge trials only. The center line represents the best-fitting regression line for judge trials only.

The figure shows seven cases above the upper 95% prediction line. Six of those cases are jury trials and one is a judge trial.[120] The figure also shows that seven awards are extremely low in that they fall below the lower 95% prediction line. Five of these are jury trials and two are judge trials. We combine the high and low outlying groups to consider whether jury trials are more likely than judge trials to produce extremely high or low awards. Three of 53 judge awards are either high or low compared to 11 of 118 jury awards. This difference is not statistically significant.[121] We cannot reject the hypothesis that jury-tried cases are no more likely than judge-tried cases to lead to an extreme award, *as measured by the mass of judge-tried cases.*

But even these differences in award patterns overstate the difference between judge and jury awards that one should expect to survive appeal. All but one of the cases that fall above the upper 95% prediction line have punitive awards that exceed ten times the compensatory award. Most such awards are known to have been justifiable or re-

120 Note that three of the high punitive awards occupy the extremely low end of the compensatory award range. One does not expect a strong correlation between punitive and compensatory awards in that range. *Supra* note 104.

121 p=0.553.

duced.[122] A study that successfully explains appellate outcomes in punitive award cases shows that such awards are unlikely to survive appellate review.[123] After the appellate process, we would expect not more than about one or two awards above the upper 95% prediction line to survive.

So in one year of data covering a substantial fraction of the nation's litigation, in the model that most emphasizes judge-jury difference, we find trivially few cases with punitive-compensatory ratios greater than ten, and some of these are not extreme given their facts.[124] We expect a small handful of juries' punitive awards to survive appeal that exceed the range of what judges might have awarded. The greater spread in jury awards thus produces few outcomes that are beyond what one expects had all cases been tried to judges. Substantial reform to ameliorate such awards may be a solution in search of a problem. To the extent there is a problem, a cap on punitive damages of ten times the compensatory award would solve it.[125]

D. Selection Effect Considerations

Accounting for what we know about case selection suggests even greater similarity in judge-jury decisionmaking. Case routing between judges and juries based on expected compensatory award size almost certainly exists.[126] Juries tend to see high-award cases, and the greater award potential of these cases creates greater potential variance. Thus, greater variability in the jury punitive-compensatory relation is to be expected. The difference in spread may result from differences

---

122 *Supra* notes 42–45 and accompanying text.

123 Eisenberg & Wells, *Predictability on Appeal, supra* note 8, at 80–83. More generally, appellate courts are especially likely to reverse jury trial verdicts for plaintiffs. *See* Clermont & Eisenberg, *Appeals I, supra* note 4, at 141–44; Clermont & Eisenberg, *Appeals II, supra* note 4, at 2.

124 *See, e.g., supra* note 42 and accompanying text.

125 Jury critics fail to address another method to reduce variance in punitive awards. Floors as well as caps reduce variability. With respect to low-end punitive-compensatory ratios, a system of minimum punitive damages awards would decrease variability. For example, a system that requires punitive awards to at least equal compensatory awards would reduce the variance in punitive award patterns. Critics' failure to propose floors suggests that reducing variability may be less of a goal than reducing high awards. Imposing a system of floors has a cost, namely, the loss of the ability to show mercy in punishing by imposing a low punitive award. This practice may be expected when the wrongdoer is in a precarious financial condition. The level of corporate criminal fines is significantly and negatively correlated with corporate-defendant bankruptcy. Cindy R. Alexander et al., *Regulating Corporate Criminal Sanctions: Federal Guidelines and the Sentencing of Public Firms*, 42 J.L. & ECON. 393, 412 tbl.5 (1999). We may have to tolerate a tradeoff between allowing mercy and increased variability. *See* Stephen P. Garvey, *"As the Gentle Rain from Heaven": Mercy in Capital Sentencing*, 81 CORNELL L. REV. 989, 991–92 (1996).

126 *See supra* Part II.D. Such routing is consistent with the finding that high-stakes cases tend to go to juries. Clermont & Eisenberg, *supra* note 3, at 1164–65; Helland & Tabarrok, *supra* note 79, at 323–25.

in the cases judges and juries decide and not from differences in their treatment of those cases. If litigants select juries for higher stakes cases, our results may overstate judge-jury differences. Judges might appear to behave even more like juries if they were seeing the same cases as juries.[127]

Furthermore, real cases introduce variability that further suggests actual differences between judges and juries may be smaller than observed differences. Some extreme punitive awards may result from defendants' strategic choices. Defendants vigorously contest liability for punitive damages. But defendants often fail to introduce evidence to try to minimize the amount of punitive damages before juries,[128] leaving plaintiffs in control of the damage guidance presented to juries. People understandably tend to "anchor" on numbers presented to them.[129] If plaintiffs present large numbers and defendants present no numbers relating to award level, juries that apply the only numbers presented to them, as in the multibillion dollar punitive award in the Texaco-Pennzoil case,[130] should not necessarily be viewed as rendering highly variable awards.

Conclusion

Evidence from one year of trials suggests that substantial change in punitive award patterns would not result from shifting greater responsibility to judges. Juries and judges award punitive damages at about the same rate, and their punitive awards bear about the same relation to their compensatory awards. Jury punitive awards have a bit more spread than judge awards, but the effect is not robust and leads to few jury punitive awards outside the range of what judges are expected to award.

127 To the extent caps on punitive awards lead to trial of high-risk cases for defendants (cases that would likely settle in states without caps), we might expect to see a positive correlation between the capping dummy variable and the size of the punitive award. In models that include the capping variable, the variable is insignificant.

128 *See* Andrew L. Frey & Evan M. Tager, *Punitive Damages*, *in* 3 BUSINESS AND COMMERCIAL LITIGATION IN FEDERAL COURTS § 40.2(g) (Robert L. Haig ed., 1998); Joseph Sanders, *From Science to Evidence: The Testimony on Causation in the Bendectin Cases*, 46 STAN. L. REV. 1, 55–56 (1993).

129 *E.g.*, Hastie et al., *supra* note 64, at 449.

130 THOMAS PETZINGER, JR., OIL & HONOR: THE TEXACO-PENNZOIL WARS 403–04 (1987); Theodore Eisenberg, *Damage Awards in Perspective: Behind the Headline-Grabbing Awards in* Exxon Valdez *and* Engle, 36 WAKE FOREST L. REV. 1129, 1153–54 (2001) (noting possible effect of defense trial strategy on punitive award levels).

Appendix Table 1: Trial Wins and Punitive Damages Awards by County and Trial Mode

County	Proportion of Trials Won by Plaintiffs		Proportion Plaintiff Wins with Punitive Award		Mean Nonzero Punitive Award		Number of Trials	
	Jury	Judge	Jury	Judge	Jury	Judge	Jury	Judge
Maricopa, AZ	.567	.615	.047	.000	183,000	n/a	263	26
Pima, AZ	.620	.718	.042	.071	72,500	95,200	79	39
Alameda, CA	.429	.778	.000	.000	n/a	n/a	70	10
Contra Costa, CA	.455	.455	.083	.100	18,090	1	55	22
Fresno, CA	.458	.519	.105	.071	151,554	25,182,770	48	27
Los Angeles, CA	.498	.589	.138	.026	775,736	53,000	249	130
Orange, CA	.425	.599	.099	.072	1,896,999	46,246	219	162
San Bernadino, CA	.347	.615	.000	.000	n/a	n/a	49	13
San Francisco, CA	.512	.636	.047	.048	1,175,667	100,000	125	33
San Jose, CA	.438	.579	.032	.136	2,500	69,751	73	38
Ventura, CA	.394	.457	.077	.095	239,333	28,359	99	46
Fairfield, CT	.690	.714	.000	.250	n/a	13,641	42	7
Hartford, CT	.476	.639	.000	.059	n/a	9,010	42	83
Dade, FL	.636	.483	.053	.000	78,316	n/a	206	29
Orange, FL	.671	.737	.000	.000	n/a	n/a	73	19
West Palm Beach, FL	.657	.877	.015	.000	225,000	n/a	201	81
Fulton, GA	.556	.105	.089	.000	273,325	n/a	81	57
Honolulu, HI	.560	.810	.143	.000	400,000	n/a	25	21
Cook, IL	.443	.643	.015	.029	362,500	50,000	309	56
DuPage, IL	.608	.652	.065	.067	7,500	7,500	102	46
Marion, IN	.568	.788	.000	.019	n/a	2,500	44	66
Jefferson, KY	.593	.636	.088	.036	84,513	5,000	138	44
Essex, MA	.404	.375	.000	.000	n/a	n/a	57	8
Middlesex, MA	.286	.600	.061	.333	81,250	26,700	119	5
Norfolk, MA	.234	.000	.000	n/a	n/a	n/a	64	3
Suffolk, MA	.312	.643	.042	.000	175,000	n/a	77	14
Worcester, MA	.220	.556	.000	.000	n/a	n/a	50	9
Oakland, MI	.384	.556	.000	.000	n/a	n/a	146	45
Wayne, MI	.468	.565	.010	.000	1,364	n/a	218	23
Hennepin, MN	.426	.638	.022	.027	1,258,455	100	216	58
St. Louis, MO	.419	.737	.048	.107	79,000	186,932	148	38
Bergen, NJ	.517	.588	.067	.053	151,298	38,241	147	68
Essex, NJ	.342	.333	.000	.000	n/a	n/a	120	12
Middlesex, NJ	.319	.810	.033	.176	11,500	79,339	191	21
New York, NY	.516	.716	.019	.019	10,158,333	124,651	306	74
Cuyahoga, OH	.458	.688	.034	.036	59,000	7,500	190	80
Franklin, OH	.568	.615	.060	.000	191,667	n/a	88	26
Allegheny, PA	.408	.693	.025	.000	205,803	n/a	201	202
Philadelphia, PA	.507	.500	.017	.000	204,933	n/a	364	36
Bexar, TX	.364	.232	.038	.000	33,938	n/a	143	69
Dallas, TX	.430	.515	.039	.000	1,527,100	n/a	298	33
Harris, TX	.349	.669	.062	.110	35,400,250	226,674	189	163
Fairfax, VA	.490	.742	.021	.000	1,500	n/a	198	66
King, WA	.509	.543	.028	.016	104,333	30,000	212	116
Milwaukee, WI	.579	.686	.011	.017	200	20,000	152	88
Totals	.473	.617	.040	.039	1,869,898	547,161	6,486	2,312
Significance	.000						.000	

Source: Bureau of Justice Statistics, U.S. Dep't of Justice, Civil Justice Survey of State Courts, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)
Note: The sample size in the two right-hand columns represents the total number of trials. The frequency counts for the other columns should be computed using these numhers.

APPENDIX TABLE 2: REGRESSION MODELS OF PUNITIVE DAMAGES LEVELS

Variable	Dependent Variable		
	Punitive damages (log)	Punitive damages (log)	Punitive-compensatory ratio (logs)
Compensatory damages (log)	0.854***	0.883***	0.002
	(0.144)	(0.096)	(0.019)
Jury trial dummy	−0.360	−0.240	0.036
	(1.047)	(0.959)	(0.126)
Jury trial × log of compensatory	0.029	0.056	−0.014
	(0.225)	(0.198)	(0.026)
Plaintiff/defendant status dummy variables			
Individual only vs. individual only (reference)			
Individual only vs. government	−0.655		−0.061
	(1.958)		(0.182)
Individual only vs. corporation	0.214		0.033
	(0.321)		(0.036)
Individual only vs. hospital	1.121*		0.110*
	(0.494)		(0.046)
Individual & nonind. vs. individual	−0.855		−0.083
	(1.027)		(0.104)
Individual & nonind. vs. corporation	0.441		0.012
	(0.414)		(0.045)
Nonindividual vs. individual only	−0.699		−0.062
	(0.588)		(0.063)
Nonindividual vs. corporation	−0.474		−0.041
	(1.007)		(0.088)
Case category dummy variables			
Products liability (reference)			
Motor vehicle tort	−2.342		−0.234
	(1.408)		(0.177)
Premises liability	−2.281		−0.276
	(1.402)		(0.171)
Products liability: asbestos	−1.834		−0.225
	(1.275)		(0.160)
Intentional tort	−2.827*		−0.335*
	(1.309)		(0.162)
Medical malpractice	−2.789		−0.300
	(1.396)		(0.169)
Professional malpractice	−0.946		−0.138
	(1.516)		(0.182)
Slander/libel	−3.954*		−0.410*
	(1.451)		(0.167)
Other negligence	0.241		−0.036
	(1.817)		(0.202)
Fraud	−2.731*		−0.309
	(1.258)		(0.157)
Seller plaintiff	−2.587		−0.282
	(1.522)		(0.173)
Buyer plaintiff	−2.153		−0.257
	(1.371)		(0.166)
Employment discrimination	1.980		−0.234
	(1.270)		(0.158)
Other employment dispute	−3.138*		−0.343*
	(1.349)		(0.162)
Rental/lease agreement	−2.446		−0.290
	(1.266)		(0.158)
Tortious interference with contract	−0.873		−0.142

	(1.459)		(0.170)
Other contract	−1.322		−0.183
	(1.343)		(0.161)
Other real property	−3.333*		−0.370*
	(1.516)		(0.179)
Constant	−2.400	−0.155	1.211***
	(1.502)	(0.499)	(0.187)
Model significance	.0030	.0000	.0022
r-squared	.621	.523	.196
Number of cases	171	173	171

Source: BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CIVIL JUSTICE SURVEY OF STATE COURTS, 1996 (Inter-Univ. Consortium for Political and Soc. Research No. 2883, 2d ed. 2001)
Note: Models account for clustering at the county level and the stratified sampling pattern. Robust standard errors are in parentheses.
*$p < .05$
**$p < .01$
***$p < .001$