Runaway Judges? Selection Effects and the Jury

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Reports about runaway jury awards have become so common that it is widely accepted that the U.S. jury system needs to be "fixed." Proposals to limit the right to a jury trial and increase judicial discretion over awards implicitly assume that judges decide cases differently than juries. We show that there are large differences in mean awards and win rates across juries and judges. But if the types of cases coming before juries are different from those coming before judges, mean award and win rates may differ even if judges and juries would make the same decisions when faced with the same cases. We find that most of the difference in judge and jury mean awards can be explained by differences in the sample of cases coming before judges and juries. On some dimensions, however, there remain robust and suggestive differences between judges and juries.

1. Introduction

The American civil jury is on trial. It has been charged with being biased in favor of the plaintiff, subject to emotion rather than reason, inaccurate in its understanding of law, and wildly unpredictable. Evidence of jury bias, in the form of the anecdote, is found regularly on the pages of the *Wall Street Journal* and in the popular press [see e.g., Adler (1994)]. Anecdotes, however, almost invariably focus attention on the atypical rather than the typical, and are thus misleading.[1] Furthermore, anecdotes, even if accurate, miss the point if judges would have made the same decisions in the same circumstances. Realistic reform requires that we compare alternative institutions, all of which may be imperfect. If judges and juries decide cases similarly, then the charges leveled against the jury are moot since the judge is the primary alternative

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1. In other cases the anecdotal evidence is just wrong. For example Vidmar (1997) points out that the well known case of a women who was awarded damages because a CAT scan destroyed her psychic abilities had in fact suffered permanent brain damage due to an allergic reaction to a contrast dye and collected damages because of her inability to work. Her job merely happened to be a psychic.

decision maker. Only if judges decide cases differently do restrictions on civil juries have any hope of achieving their aims. It is therefore important to bring the available evidence to bear on this fundamental question; do trial judges reach systematically different decisions than juries?

In Section 2 we survey the literature on judge versus jury trials. In Section 3 we discuss our dataset and present data on mean awards and win rates across judge and jury trials. The fact that the average jury award is much larger than the average judge award is the point of departure for the remainder of the article. How much of this difference may be explained by differences in the sample of cases coming before judges and juries? We answer this question first by asking how juries would have decided the sample of cases going to judges and then by asking how judges would have decided the sample of cases going to juries. We ask both questions in three stages, progressively controlling for larger sets of independent variables and more sophisticated error structures across selection and award equations. We find that we can explain three-quarters to two-thirds of the difference in mean awards across judges and juries, thus demonstrating that most of the difference in mean awards is due to sample differences and not to different attitudes or decision processes across judges and juries. Nevertheless, 25–33% of the differences in mean awards is unexplained. In Section 6 we examine the corollary question, "Holding the sample of cases constant, in what respects do judges decide cases differently than juries?" A direct comparison of judge and jury award equations reveals small but significant differences in judge and jury decision processes. Although our focus is on awards, we also examine differences in win rates and ask how much of the judge/jury difference is explained by sample selection and how much by differing decision processes. In Section 7 we offer some concluding remarks.

2. Judges Versus Juries

Are juries out of control? Compared to whom? The usual answer has been "yes," at least compared to judges. In particular, juries in personal injury torts are often accused of compensating sympathetic accident victims even when the defendant has not committed a tort. The North Carolina Hospital Association, for example, claimed that

> Often awards have little relationship to the seriousness of injury. There is no way to predict how a jury will rule on a particular set of facts . . . Often awards bear no relationship to economic losses . . . today juries often make awards regardless of the "fault" of anyone—out of sympathy for an injured person . . . too often

> juries appear to award on [the] basis of emotion as opposed to facts and/or realistic evaluation of case circumstances.[2]

Bernstein (1996) agrees, calling juries "a disaster for the civil justice system" because they "undermine certainty, are incompetent to decide complex cases, and often base their decisions on illegitimate factors." In England, Canada, and Australia, Bernstein notes pointedly, "judges alone handle personal injury cases." Bernstein and others argue that judges are better than juries at evaluating complicated evidence (a factor in many medical malpractice and product liability trials), they are less likely to be swayed by emotion, and are more likely to closely follow the principles of tort law. Tort reforms therefore typically try to limit the jury's discretion by imposing limits on the amounts that juries may award for pain and suffering, to give one example. More generally, opponents of the current tort system point out that compared with the rest of the world the American reliance on the jury is anachronistic and should be curtailed.[3]

Perceived differences between juries and judges are not limited to critics of the tort system. Practitioner's handbooks on trial law, for example, often suggest that, "As a general rule, most plaintiffs with highly charged cases want a jury in the hope that the jury will be swept away in a tide of emotion and award large damages" (Izard, 1998). Juries are also said to be preferable when the case does not rest on complex facts or legal technicalities and when the plaintiff is a "little guy" relative to the defendant [see, e.g., Haydock and Sonsteng (1991) and Izard (1998)].

One would expect lawyer perceptions of the trial process to be reasonably accurate, so it's quite surprising that the academic literature on judges versus juries does *not* find a large difference in decision making. In their classic study, *The American Jury*, Kalven and Zeisel (1966) surveyed the judges who presided over some 4000 civil *jury* trials. In 78% of the trials, the presiding judges would have ruled the same as the juries had it been up to them. This rate of agreement is comparable to the rate of agreement among different experts of all kinds (e.g., scientists doing peer review, physicians diagnosing patients, etc.) and, of importantance, it is comparable to the rate of agreement among different judges (Diamond, 1983).[4] When Kalven and Zeisel found disagree-

2. Reported in United States General Accounting Office (U.S. GAO), *Report to Congressional Requesters*, *Medical Malpractice: Case Study in North Carolina* (Dec. 1986). This and many other similar quotations can also be found in Vidmar (1997).

3. Schuck (1993) reviews a number of jury reform proposals. Bernstein (1996) is particularly antagonistic toward juries.

4. Of interest, this rate of agreement is almost identical to the appeals courts affirmation rate of trial verdicts (81%). See Clermont and Eisenberg (1999).

ment among judge and jury it was just about as likely that the judge found liability and the jury did not as the reverse.[5]

Most of the studies of judge/jury differences rely on hypothetical questions—judges are asked what they would have done *if* they had been responsible for deciding the case—or they rely on artificial experiments. Almost no research has been done using nonsurvey data on judge and jury outcomes. The first systematic effort to look at this question using litigation data was by Clermont and Eisenberg (1992). Clermont and Eisenberg compare win rates and awards in a sample of federal civil trials. They find that win rates often differ significantly across the trial forum and not always in ways predicted by the critics of the jury system—in some types of cases plaintiff win rates are higher in judge trials than in jury trials. Clermont and Eisenberg are primarily interested in explaining why judge trials are more prevalent in some areas of litigation than in other areas. In particular, they focus on the puzzle of why plaintiffs predominantly choose jury trials even in case categories where judge win rates are significantly higher than jury win rates. They suggest that a combination of selection effects and misperceptions might explain the data. We also offer some comments on this issue below.

3. Mean Awards and Win Rates

To test whether judges and juries decide cases similarly we use a large dataset that includes data on settlements as well as trial outcomes. The data were extracted from Jury Verdict Research's (JVR's) Personal Injury Verdicts and Settlements on CD-ROM.[6] Data from trials are drawn directly from court records. Using an extensive survey of lawyers, JVR also collects data on settlements. Our dataset contains information on 59,304 trials, and 27,429 settled cases.[7] The dataset spans all 50

5. The Kalven and Zeisel results are supported by other research showing that judges and juries reach similar decisions in similar cases and that juries appear to respond to information in reasonable ways. A number of articles in Litan (1993) make this point, see especially Lempert (1993:235) who writes, "The weight of evidence indicates that juries can reach rationally defensible verdicts in complex cases [and] that we cannot assume that judges in complex cases will perform better than juries . . . " The literature on the quality of jury decision making is reviewed in Hans and Vidmar (1986); see also Clermont and Eisenberg (1992).

6. JVR markets their data to lawyers who are seeking to ascertain the value of their cases by comparing them with similar cases. In other words, lawyers use JVR data to create rational expectations of case outcomes. The JVR dataset is the largest and most extensive dataset on state court records currently extant. In our estimation the dataset is of much higher quality (in terms of accuracy, missing records, size, and extent of coverage) than most government-generated datasets.

7. The dataset originally contained two extreme outliers, awards of $4.25 and $5 billion. We eliminated these outliers from all computations. We also eliminated all class action suits. Thus the injured party in every case in our sample is an individual. Some cases have multiple defendants.

Table 1. Judge / Jury Differences (All Trials)

	Juries	Judges	Two-Sided p-Value on Difference[c]
Win rate	56.67%	67.73%	0.000
Mean award[a]	$696,149	$218,629	0.000
Median award[a]	$74,879	$17,279	0.000
Mean of log awards	11.24	10.02	0.000
Standard deviation of log awards	2.188	1.853	0.000
(Dollar equivalent)[a, b]	($603,156)	($121,885)	
Number of trials	53,335	5969	

[a] Conditional on a plaintiff win.
[b] Since dollar awards are not normally distributed, the standard deviation of dollar awards is not informative. The standard deviation of log awards has meaning, however, because log awards are well approximated by a normal distribution. To convert a standard deviation in logs back to a dollar figure we evaluate at the mean of the log awards.
[c] The p values for the difference in win rates, means, and standard deviations are two-sided and were computed using standard tests available in any text (e.g., Aczel 1996). The difference in medians test was computed using a Monte Carlo method with 5000 replications.
Source: JVR.

states. The earliest cases were tried in 1988 and the most recent cases date from 1996. All award amounts are corrected for inflation by conversion into 1996 dollars.

Table 1 presents data on win rates, mean and median awards, the log standard deviation of awards, and the number of trials in each category (Table 1 does not include data on settlements). At first glance, the table appears to support the claims of jury reformers that the jury is biased toward the plaintiff. The mean award in a case before a jury is more than twice as large as the mean award in a case before a judge. Contrary to the conventional wisdom, however, the win rate before judges is significantly higher than the win rate before juries. The higher judge win rate, however, does not fully offset the higher awards before juries—the expected award is higher before a jury than a judge.

The median award before a jury is significantly higher than the judge median. The higher jury mean is thus not simply an artifact of the occasional astronomical award before a jury. In both judge and jury cases the mean award is well above the median award, suggesting a strongly right-skewed distribution. Figure 1 is a kernel density estimate of log awards in judge and jury trials.[8] Since the kernel density estimate for log awards is approximately normal, the distribution of dollar awards is approximately lognormal. The density function for jury awards clearly

8. We use a biweight kernel with smoothing parameter optimized on the assumption that the underlying data are normally distributed [see Silverman (1986) for more information on kernel estimation]. The use of other kernels and/or smoothing parameters does not materially affect the results.

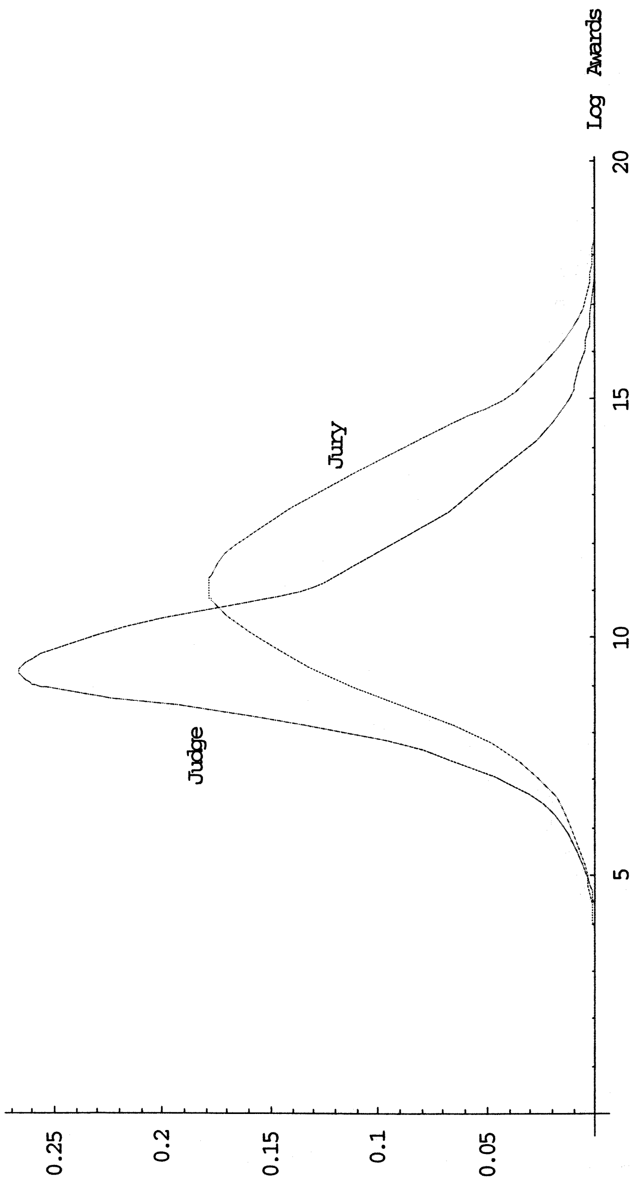

Figure 1. Kernal estimation of all cases: judge and jury.

has a larger mean and standard deviation than that for judge awards. Aside from the higher win rate in judge trials, the raw data appears to support the case for jury reform.

4. The Importance of Sample Selection

4.1 Sample Selection and Awards

The average judge award is 31% of the average jury award. How much of this difference can be explained by differences in the sample of cases appearing before judges and juries? To answer this question we examine potential sources of different judge/jury samples and ask how much of the difference in average awards can be explained by sample variation if the null hypothesis of no difference in judge and jury decision processes is true. We examine three sources of potential sample variation: 1) case categories, 2) injuries and other variables, and 3) unobserved sample selection effects.

It is well known that awards in product liability and medical malpractice cases are much larger than in premises liability and auto injury cases.[9] Column one of Table 2, for example, shows a regression of log awards (in jury trials) on these four case categories. Evaluated at the mean log award, awards are approximately $180,000 and $187,000 larger than average in product liability and medical malpractice cases and $25,000 and $100,000 lower than average in premises liability and auto cases, respectively.

Product liability cases and medical malpractice cases are comparatively rare; they make up 4.7% and 7.3% of jury trials, respectively. Premises liability and auto injury cases are much more common; these case categories account for 15.2% and 47.5% of our sample of jury trials, respectively (the remainder are miscellaneous torts). (Descriptive statistics on all variables can be found in Table A1 in Appendix A.) If product liability and medical malpractice cases are proportionately a larger part of the judge sample than the jury sample, this could explain why the average jury award is so much larger than the average judge award. In fact this is the case, the high-award case types of product liability and medical malpractice make up only 1.51% and 1.58% of judge trials, respectively, while the low-award case types of premises liability and auto injury account for 9% and 64.9% of the judge sample, respectively. To establish the importance of this source of variation we ask, "If juries had decided the cases that actually went to judges how much lower would the average award have been?"[10] Using the coeffi-

9. Tabarrok and Helland (1999) and Helland and Tabarrok (1999) show that awards are higher in product liability and medical malpractice cases than in other cases even after controlling for injuries.

10. Later we report what judges would have done had they decided the cases that actually went to juries.

Table 2. Award Regressions

	OLS Jury	OLS Jury
Constant	11.747***	11.61***
	(.023)	(.069)
Number of defendants		.423***
		(.051)
Expected years of life left		.272***
		(.02)
Major injury		.633***
		(.071)
Minor injury		−.995***
		(.065)
Emotional distress		−1.16***
		(.0769)
Bad faith		−.134
		(.113)
Male		.327***
		(.218)
Premises liability	−.386***	−.101***
	(.038)	(.037)
Medical malpractice	1.311***	.815***
	(.049)	(.47)
Product liability	1.255***	.652***
	(.0579)	(.055)
Auto	−1.27***	−.915***
	(.0285)	(.0299)
Poverty		2.22***
		(.197)
Joint and several liability		.091*
		(.049)
Noneconomic cap		−.528***
		(.028)
Collateral sources		.322***
		(.218)
No punitive		−.081
		(.129)
Punitive cap		−.106
		(.022)
Evidence standard		.18***
		(.024)
Number of cases	30,226	30,226

*, **, *** Significant at > 0.1, > 0.05, and > 0.01 levels, respectively.
Standard errors in parentheses.

cients from Table 2, we find that if juries had decided the sample of cases going to judges, the average award would have been 63% of the average jury award. Thus just over half of the difference in average judge and jury awards can be explained solely by differences in the sample of four case categories going to judge and jury trial.[11]

We now add injuries, differences in tort law across the states, the number of defendants, and local poverty rates to the list of variables that may lead to different judge/jury samples. Our dataset has descriptive information on the victim's injury. We code this information into six variables. Five of the variables—major injury, minor injury, emotional distress, bad faith, and wrongful termination—are dummy variables. Major injury is set equal to one if the victim suffered a permanent injury such as loss of a limb, brain damage, or blindness. Minor injuries are those that are (potentially) temporary, for example, broken arms, broken legs, concussions, or wounds. A pianist might consider a broken finger a major injury if recovery was not 100% complete. We do not know all of the specifics of a case so we cannot control for potential miscodings of this type; nevertheless any coding errors will be uncorrelated with our other independent variables. Emotional distress indicates cases in which the victim suffered emotional or psychological injuries. Bad faith cases are those in which an insurance company is sued for refusing to pay a claim. Wrongful termination is set equal to one when the plaintiff claims a wrongful termination of employment. To prevent perfect collinearity with the intercept term we suppress wrongful termination. We also include a sixth variable, the expected years of life left in a case in which the victim died. We calculated the expected years of life left using the age at death and actuarial tables which control for age and sex. We do not have data on lost wages, but we do include a dummy variable set to one when the victim was a male, on the theory that average wage losses are higher for males than females. Together these variables control for the severity of the plaintiff's injury.

In addition to injuries, we include a number of legal variables that may affect liability. Under the joint and several rule, any defendant can be liable for a plaintiff's entire injury regardless of the relative contribution of that defendant to the injury. Some states have modified the joint and several rule to limit the liability of some defendants (e.g., a defendant responsible for less than 50% of the injury may not be assessed more than his relative contribution). Joint and several is set

11. Under the null hypothesis we initially expect judge and jury awards to be the same. The "unexplained" difference is thus $100\% - 31\% = 69\%$. If we can explain $100 - x$ of this difference, then the ratio of the unexplained to the explained is $(100 - x)/(100 - 31)$. Note that when $x = 31$, 100% of the difference is explained and when $x = 100$, as would have been the case if the sample of case types going to judge and jury trial were the same, then none of the difference is explained. Letting $x = 63$, we have that 53% of the difference in averages is explained by differences in case types.

equal to one if the state has modified the rule and if there is more than one defendant. Noneconomic cap is set equal to one if state law puts a cap on damages due to pain and suffering or other noneconomic losses. Punitive cap and no punitive control for states that cap punitive awards or prohibit them altogether.[12] Evidence standard is set equal to one if the state requires that "malicious intent" be *proven* for punitive damages to be recoverable. Under the collateral sources rule, payments to the plaintiff from a third party (i.e., insurance) are not deducted from damages due from the defendant. If collateral sources is set equal to one the collateral sources rule is weakened so that some offset is allowed.[13]

The variable poverty measures the percentage of the population in poverty in the county in which the trial occurs. Helland and Tabarrok (1999) find that higher rates of poverty in the trial county are significantly associated with larger awards. The number of defendants is included as another control variable that may affect the size of awards. The descriptive statistics for the independent variables are given in Appendix A.

Column 3 of Table 2 shows the impact of these variables on awards. As before, awards are higher than average in product liability and medical malpractice trials and lower than average in premises liability and auto trials. Also, as expected, awards are higher than average in cases involving deaths and major injuries and lower than average in cases involving minor injuries, emotional distress, or bad faith contracting. Limitations on joint and several awards tend to raise awards, a result the opposite of that expected, but the effect is small and not statistically significant at the 5% level. Caps on noneconomic awards and punitive awards appear to reduce awards as intended; in both cases the effect is highly statistically significant. Evidence standards, however, do not appear to lower awards. Awards also tend to be larger in states where the collateral sources rule is weakened, perhaps because juries increase awards if they think insurance payments will later be deducted.[14] Trials with multiple defendants appear to generate larger awards than otherwise similar trials.[15] Finally, the higher the poverty rate the county

12. No state prohibits punitive damages absolutely and completely. Punitive damages are prohibited in New Hampshire, for example, except where explicitly allowed for by statute.

13. The American Tort Reform Association (ATRA) home page (http://www.atra.org) contains information on tort reform legislation by state.

14. Some of these results may be subject to endogeneity problems—perhaps states with above average punitive damage awards are more likely to pass evidence standards than other states—so we cannot make definitive conclusions about the effect of various laws. "Reduced form estimates," however, are all we need in order to examine the role of sample effects in explaining differences in average judge and jury awards.

15. There are no class action suits in our sample.

in which the trial occurs (the jury pool), the greater the award.[16] (We look at marginal effects in more detail further below.)

If trials before judges tend to involve fewer deaths or major injuries than trials before juries or if they tend to occur in richer counties or in states which cap pain and suffering awards, then differences in the sample could explain differences in the average award. Taking into account all of these possible sources of variation we find that if the judge sample had been tried before a jury, the average award in the judge sample would have been 56% lower than the average jury award. Case type variables alone already suggested that the average award in the judge sample would be 63% lower than the jury average. Injuries, differences in tort law, the number of defendants, and local poverty rates do not therefore greatly increase our ability to explain the difference in judge and jury average awards.[17] Using case types and all of the additional variables we are able to explain approximately 62% of the difference in average awards, $(100 - 56)/(100 - 31) = 0.63$.

4.2 The Heckit Model

As noted above, one potential problem with estimating the effect of the independent variables on trial awards is that awards do not represent a random sample. To be awarded damages before a jury, for example, at least one of the defendants must have requested a jury trial, the case must not have been settled, and the plaintiff must have won at trial. Unless the sample selection is controlled for, the parameter estimates may be biased because unobserved sources of variation in the forum, settlement, and win decisions could be correlated with unobserved sources of variation in the award equation. To account for this sample selection we estimate probit models for the forum, settlement, and win decisions and then use Heckman's (1979) procedure to control for any correlation of errors between each of these decision equations and the award equation. For tractability we assume that the errors of the probit equations are uncorrelated with each other.

The award at trial is thus estimated by,

$$\log(award) = X_v \beta_v + \lambda_J \beta_{\lambda_J} + \lambda_W \beta_{\lambda_W} + \lambda_T \beta_{\lambda_T} + \varepsilon,$$

where log(award) is the trial award, X_V are the variables described above, λ_i, $i = J, T, W$ are inverse Mill's ratios, β_{λ_i}, $i = J, T, W$ are the

16. We also included a specification with poverty and poverty squared. The high correlation of these variables made interpretation more difficult than with the simpler specification used in the text, but the comparison across judge and juries was similar.

17. The case category variables explain a larger fraction of the judge/jury difference than the "injury" set of variables, regardless of the order in which variables are added. Since the marginal explanatory power does vary, however, with the order in which variables are added, the total explanatory power is the more important result.

vector of coefficient estimates from the decision equations (see below), and ε is the error term.

The coefficients are estimated by ordinary least squares. The least squares covariance matrix will, however, be biased because the disturbance term in the award equation is, by construction, heteroscedastic. The correct asymptotic covariance matrix is

$$\text{var}[\beta_1, \beta_2, \ldots \beta_k] = [X_V^{*\prime} X_V^*]^{-1} \left[X_V^{*\prime}(\sigma I - \Pi) X_V^* + \sum_{i=1}^{3} Q_j \right] [X_V^{*\prime} X_V^*]^{-1},$$

where

$$X_V^* = [X_V | \lambda_J | \lambda_w | \lambda_T],$$
$$\Pi = \text{diag}(\pi_1 \ldots \pi_n),$$
$$\pi_i = \beta_{\lambda_J}^2 \delta_J + \beta_{\lambda_T}^2 \delta_T + \beta_{\lambda_W}^2 \delta_W,$$
$$\delta_j = \lambda_j(\lambda_j + \gamma_j X_j)$$
$$\Sigma_j = \text{asymptotic covariance matrix for estimates of } [\beta_i]$$
$$\Delta_j = \text{diag}(\delta_i \ldots \delta_n)$$
$$Q_j = \beta_{\lambda_i}^2 (X_V^{*\prime} \Delta_j X_j) \Sigma_j (X_j' \Delta_j X_V^*),$$
$$j = J, T, \text{ and W}, \quad \text{and}$$
$$\sigma^2 = (1/n) e'e - (1/n) \Sigma_j \pi_i.$$

There is one remaining complication even after selection effects have been controlled for; plaintiffs and defendants decide whether or not to settle a dispute based in part upon expectations of the trial award. Decisions about whether to pursue a judge or jury trial are also likely to be based in part on expectations of future outcomes. To account for these considerations we use a two-stage procedure. In the first stage we run through each of the equations to create for each case a shadow trial award, a shadow probability of winning, and a shadow probability of going to jury trial. We then reestimate the model in the second stage using the shadow variables as estimates of plaintiff and defendant expectations. In effect, the first-stage estimates use all of the independent variables in a given equation as instruments for the shadow variables (structural variables) in the second stage.[18] The estimation procedure is depicted in Figure 2.

18. An estimation procedure similar to that described here was first used in Danzon and Lillard (1982).

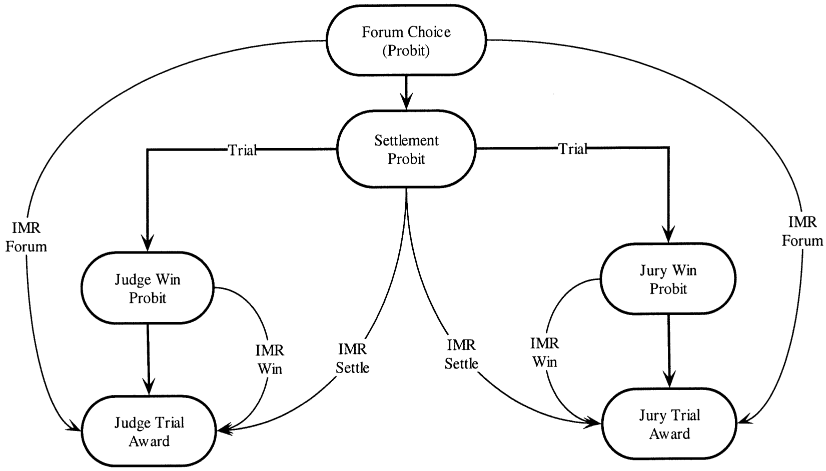

Figure 2. Estimation procedure.

4.3 The Selection Effects

4.3.1 Forum Selection. The first decision equation to estimate is the choice of judge or jury trial. We model the decision to choose a jury rather than a judge as a function of the default forum, the relative costs of each forum, expected differences in the judge and jury award, and certain case characteristics.

In every state, both the defendant and the plaintiff have a right to a jury trial in just about any case involving money damages.[19] In many states, however, a bench trial is the default. In these states, if the plaintiff or defendant want a jury trial it must be requested, often in writing within a short period of time after filing or responding to a complaint. The default rule will determine the forum if both the plaintiff and defendant are indifferent to forum or if one of the parties wants a jury trial but doesn't realize that it must be specifically requested. We define the dummy variable, default, to be one if the default forum is a judge trial and zero otherwise. We expect that default will reduce the probability of a jury trial and thus will have a negative sign.

The expected cost of each forum is proxied by the expected time from filing to decision before a judge and jury. Cases scheduled to be decided by a judge typically reach court and are tried faster than cases before juries. We modeled the duration of time to decision using a sample of 36,896 cases tried before a jury and 5496 cases tried before a judge. Included within our model are injury variables (death, major, minor etc.), case types (product liability, medical malpractice, etc.), the number

19. In most states the right to a jury trial is protected by the state constitution, but in some it is based only on statute.

of defendants, and as a measure of the state court queue, the number of filings per judge by state. We found that a model of duration based on the logistic hazard function fit the data well. The results of the model are presented in Table B1 in Appendix B. Subtracting the expected time to a judge decision from the expected time to a jury decision creates the time difference variable. We expect that as the costs (time to decision) of a jury trial increase the probability of selecting a jury trial will diminish.[20]

If the expected award in a judge trial exceeds the expected award in a jury trial, the defendant will request a jury trial. On the other hand, if the expected award in a judge trial is less than the expected award in a jury trial, the plaintiff will request a jury trial. To account for this symmetry we define defense request to be equal to the expected judge award minus the expected jury award if the difference is positive, and if the difference is negative we define plaintiff request to be the expected jury award minus the expected judge award. We expect both defense request and plaintiff request to be positive.

The number of defendants is included because *any* defendant can request a jury trial. Thus we might expect that the probability of a jury trial will increase with the number of defendants. Alternatively, cases with a large number of defendants may be especially complex and potentially time consuming, thus plaintiffs and defendants may agree to a judge trial to save court costs. Finally, a dummy variable for cases involving auto accidents are included, as these cases tend to be more symmetrical on a number of important dimensions than other cases. Both defendants and plaintiffs are typically individuals in auto cases, for example, and this may make jury bias against defendants less likely than when the defendant is a business (other auto case symmetries are discussed in the results section).

4.3.2 The Decision to Proceed to Trial. We model the decision to settle using a model based on Gould (1973), Posner (1973), Priest and Klein (1984), and others.[21] The settlement model suggests that the settlement decision is a function of the variance of plaintiff and defendant's prediction errors, the expected award, risk, court and settlement costs, and stake asymmetry. We proxy for each of these factors using the following variables. As noted above, we create for each case a shadow probability and a shadow award. We proxy for prediction error by the

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20. Surveys indicate that both plaintiffs and defendants prefer shorter time to trials (see, e.g., Miller, 1992). It is sometimes argued that defendants want longer times to trial to avoid paying damages. We find this theory dubious as longer times to trial increase everyone's costs and plaintiff lawyers are sure to correct damage measures for inflation and interest. Since either the defendant or plaintiff can request a jury trial, however, this possibility does not change the expected sign or interpretation of our results.

21. Cooter and Rubinfeld (1989) review the literature.

variance of the shadow probability, $p(1-p)$. The shadow award proxies for the expected judgment amount, and we measure risk as the variance of the expected award, $p(1-p)X^2$, where X is the shadow award and p is the shadow probability. Court costs are again proxied by the expected time to trial weighted by the probability of a judge or jury trial.[22] We expect that the longer the expected time to trial the greater the expected court costs and thus the greater the incentive to settle. We include the number of defendants as a proxy for settlement costs. If holdout and bargaining problems when defendants must allocate damages among themselves increase the difficulty of reaching a settlement, trials will become more likely the greater the number of defendants. Alternatively, the cost per defendant falls for any given compensatory claim and thus, if the defendants can agree on an allocation, settlement costs may fall with more defendants.

In product liability and medical malpractice cases the award to the plaintiff in the event the plaintiff wins may underestimate the cost to the defendant. A loss in one product liability case may generate further lawsuits, and a loss in a medical malpractice case might mean further scrutiny of the defendant doctor from, say, a hospital board, and may even cause a loss of operating rights. We include product liability and medical malpractice dummies to account for these effects.

In addition to the factors suggested directly by the model, we include several other variables. Nonpecuniary elements may enter into a plaintiff's bargaining efforts if a death, particularly a child's death, is involved in the dispute. Defendants may also be more likely to settle these types of cases if a trial would generate negative publicity. To control for possible nonpecuniary elements in bargaining, we include two variables, a dummy variable labeled child (set equal to one if a child died), and the expected number of years of life left in cases involving an adult death.

Kornhauser and Revesz (1994; see also Donohue 1994) show that the joint and several liability rule, under which any one defendant is liable for the damages of all, can change the probability of settlement. Whether the probability of settlement increase or decreases, however, depends on the correlation of the defendants probabilities of winning at trial. As the correlation between the defendants probabilities of success at trial increases, the probability of settlement decreases. We include a variable called joint and several which is equal to one in states which have *weakened* the joint and several rule so that liability of some defendants (e.g., a defendant responsible for less than 50% of the injury) may not be assessed more than his relative contribution. Joint and several could be either positively or negatively signed.

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22. That is, Pr(judge trial) $*$ expected time until trial before a judge $+$ Pr(jury trial) $*$ expected time until trial before a jury.

Lawyers paid on a contingency fee basis are willing to settle for lower amounts than their clients because the lawyers, not the clients, bear most of the costs of a trial (Miller 1987; Thomason 1991). When information is imperfect, lawyers may convince plaintiffs to settle even when a better-informed plaintiff would prefer to go to trial. In some states contingency fees are capped, limited, or court reviewed, while in others any fee agreed upon by the plaintiff is acceptable. No limit is a dummy variable set equal to one in states with no limits on contingency fees. We expect that settlements will be more likely in states that have no limits on contingency fees.

Our sample of cases underrepresents settlements and overrepresents trials as compared to population proportions. To rebalance our sample the settlement equation is estimated using the weighted exogenous sample maximum likelihood estimator (WESML) of Manski and Lerman (1977).[23] In our application, the WESML is essentially a weighted probit model where the weights are equal to population proportions divided by sample proportions. A number of studies have found that approximately 10% of tort cases go to trial; we therefore use 10% as our estimate of the population proportion of trials to settlements.[24,25]

4.3.3 Plaintiff Win Equation. The probability that the plaintiff wins is estimated separately for judge and jury trials using a probit model. To account for different decision standards we include dummy variables for the case types, medical malpractice, product liability, auto, and premises liability. Life expectancy is included to account for any differences in the probability of winning a case in which a death was involved. Some states allow a "products defense" in product liability cases. A typical products defense might allow a defendant to claim that the product, say a knife, was "inherently dangerous" and thus injuries from ordinary use do not impose liability on the defendant. Products defense is a dummy variable set equal to one in product liability cases in states allowing a products defense.

4.3.4 Heckit Results. The results of the selection equations are presented in Table 3. Since we have discussed the results from a similar

23. The WESML is applied in a problem similar to ours by Boyes, Hoffman, and Low (1989).

24. In their survey of the literature Cooter and Rubinfeld (1989:1070) note, "A typical finding is that 10 disputes settle out of court for every one that is tried." Using one month of data from 33 courts, the National Center for State Courts (1994) finds that approximately 5% of tort cases go to trial. Using data on 2996 torts in federal court Waldfogel (1995) finds an average trial rate of 18.7%. Danzon and Lillard (1983) find that 12% of medical malpractice cases go to trial.

25. Our results are robust with respect to varying the weights in the WESML estimator.

Table 3. Forum Choice, Trial, and Win Sequential Probit Results

Variable	Forum Choice Probit	Trial Probit	Jury Win Probit	Judge Win Probit	LR Test χ
Constant	1.873*** (.051)	−5.968*** (.704)	.227*** (.011)	.283*** (.033)	2.54
Expected years of life left if defendant died		−.033*** (.008)	−.0025 (.006)	−.089*** (.019)	18.879***
Product liability		.346*** (.0418)	−.231*** (.034)	−.5** (.128)	4.18*
Medical malpractice		1.332*** (.107)	−.595*** (.02)	−.936*** (.093)	13.26***
Auto	−1.02*** (.045)		.19*** (.014)	.66*** (.042)	114.55***
Premise liability			−.265*** (.017)	−.399*** (.054)	5.656**
Joint and several liability		.144*** (.026)			
No limit on contingency fees		−.059*** (.012)			
Products defense			−.125*** (.046)	−.045 (.207)	.141
Number of defendants	−.251*** (.0181)	−.195*** (.0269)			
Child		−.547*** (.0137)			
Time difference	−.575*** (.053)				
Default	−.271*** (.0164)				
Plaintiff request	.181*** (.0154)				
Defendant request	.773*** (.025)				
Expected time to trial[a]		−.628*** (.106)			
Expected award		.21 *** (.023)			
Variance of expected award (risk)		34.69*** (2.48)			
Var p		−.0238*** (.003)			
Number of cases	59,304	86,733	53,335	5969	

*, **, *** Significant at > 0.1, > 0.05, and > 0.01 levels, respectively.
Asymtotic standard errors in parentheses.
[a] The weighted average of expected time until a jury trial and expected time until a trial before a judge.

settlement equation at length elsewhere (see Helland and Tabarrok, 1999) we will mention only a few variables briefly. All of the variables in the model are highly statistically significant and their signs are as expected. The sign on var P is positive, indicating, as the Priest–Klein model predicts, that the more uncertain the trial outcome (win/lose) the greater the probability of going to trial. Settlements are more likely (trials less likely) in states with no limits on contingency fees. Cases involving a death are more likely to settle than other cases. Higher expected court costs (as measured by expected time to trial) result in more settlements (a lower probability of going to trial).

We now examine in more detail the choice of forum equation, the win equations, and the award equations. The descriptive statistics are given in Table A1 in Appendix A.

4.3.5 Forum Choice Results. Juries are selected in 90% of the personal injury cases in our sample. If judges and juries grant similar awards—as our results indicate—why are most personal injury trials held before juries? Two reasons help to explain this. First, both the plaintiff and the defendant have the right to a trial by jury, so bench trials occur only when *both* the plaintiff and the defendant prefer a judge. Assume that there are no differences in the costs of trying a case before a judge or jury, and assume that judges and juries are unbiased in the sense that if every case were to be tried before both a jury and a judge, then half of the time the jury would grant a larger award to the plaintiff and half of the time the judge would grant a larger award. Let both plaintiffs and defendants have rational expectations about judge and jury bias. If the errors in plaintiff and defendant expectations are independently distributed, then 75% of trials will be jury trials. As the correlation of defendant and plaintiff errors increases, the probability of a jury trial increases. When errors are perfectly correlated so that whenever the plaintiff believes the jury to be in his favor the defendant agrees (i.e., thinks a judge would be in the defendant's favor) and vice versa, then 100% of trials will be jury trials. Thus it is not difficult to reconcile an observation of 90% jury trials, even if there are no differences in awards between judges and juries. If juries are slightly biased toward plaintiffs (or defendants, although most observers suggest this is not the case) then the reconciliation is *a fortiori*.

A second reason for the predominance of jury trials is that if plaintiff lawyers think that juries are biased, then jury trials will predominant even if judges and juries grant identical awards on average. Note that if judges and juries are unbiased, then rationality does *not* require lawyer perceptions to match reality since there is no cost to false perceptions. Furthermore, since win rates and mean awards differ significantly by case type, false lawyer perceptions can easily drive average awards and win rates in judge and jury trials far apart. It's possible that a "perceptions equilibrium" may arise in which average awards and win rates are driven in just such a way that the false perceptions of lawyers appear to be verified in the data. Although we find some differences in judge and jury decisions, false lawyer perceptions seemingly verified by, on average, low judge awards and high jury awards may be responsible for some of the predominance of jury trials in personal injury cases. If false and self-reinforcing perceptions are responsible for forum choice decisions, then we would expect to see quite different and arbitrary forum choices across different classes of cases. Clermont and Eisenberg (1992) examine forum choice across contract, personal property torts, fraud, personal injury, and other case types and find that a model of false and sometimes self-reinforcing lawyer perceptions is the best explanation for the data.

Given a set of perhaps false baseline perceptions, forum choice decisions made on the margin will still be rationally responsive to other considerations. We find, for example, that in states where the default trial is a judge trial, jury trials are 4.9% less likely than in other states. The coefficient on time difference is also significant and negative. A one standard deviation increase in the time to jury trial relative to a judge trial decreases the probability of a jury trial by 2.4%. Both plaintiff request and defense request are positive, which indicates that the plaintiff chooses a jury trial when the expected jury award rises above the expected judge award, and the defendant chooses a jury trial when the expected jury award falls below the expected judge award. Defendants appear to be slightly more sensitive to differences in the expected award across forums than are plaintiffs. If the expected jury award rises above the expected judge award by $1000, the probability of a jury trial increases by 1.6%, but if the expected jury award falls below the expected judge award by $1000, the probability of a trial increases by 2.7%.

Adding a second defendant reduces the probability of a jury trial by 3%. This is consistent with the theory that both plaintiffs and defendants prefer judges in more complex cases. Finally and importantly, auto cases are 18% less likely to be tried before a jury than nonauto cases.[26] An important aspect of auto cases is that they are more symmetrical than most other cases. Auto cases often occur between two individuals (rather than between an individual and a business), both of whom are injured and neither of whom has much deeper pockets than the other. Each of these aspects of symmetry makes jury bias less likely. Thus in auto cases both defendant and plaintiff should be more likely to

26. For dummy variables (d) we calculate the exact difference between the probability of jury trial when $d = 0$ and when $d = 1$ when all other variables are at their means. For continuous variables we calculate marginal effects using the derivative at the mean of all variables.

take advantage of faster judicial decision making by accepting a bench rather than a jury trial.

4.3.6 Jury Win Equation. Results from the jury win equation are presented in column 4 of Table 3. Win rates are significantly lower than average in product liability, medical malpractice, and premises liability cases, lower in states and cases in which a products defense is applicable, and higher than average in auto cases. Win rates do not appear to differ from average in death cases. The results for the judge equation are given in column 5 of Table 3. Results are similar in sign in judge trials, except death cases before judges significantly reduce the chances of winning and product defense rules have no impact. We discuss differences between judge and jury win rates at greater length in Section 5.

4.3.7 Award Equation. The award equation found in column 1 of Table 4 is similar, although not identical to the OLS equation. Of importance, the inclusion of the inverse Mill's ratios, which control for unobserved correlation of the error terms across the selection and award equations, significantly increases the fraction of the difference in judge and jury means that can be explained by differences in the sample. If the sample of cases actually decided by a judge had instead been decided by a jury, the average award in that sample would have been 47.1% lower than the average award in the jury sample. Thus 77% of the difference in the average judge and jury award can be explained by differences in the sample of cases appearing before judges and juries $(100 - 47.1)/(100 - 31) = 0.766$.

5. What Would Judges have Done with Trials that Went to Juries?

We have far more observations on jury trials than on judge trials, so the jury equation is better estimated. Since the jury equation is better estimated we can get better estimates of what juries would have done with the judge sample than what judges would have done with the jury sample. Nevertheless, the latter question is also interesting and is not equivalent to the former. Suppose that juries receive cases of type A and judges receive cases of type B. It's possible that juries would treat every type B case just as judges would, but that judges would treat type A cases quite differently than juries.

We use the same three sets of variables as above. Recall that the actual jury mean is 3.18 times as high as the actual judge mean. Using only case type variables, we find that if judges had tried the cases that were actually tried by juries, the mean award would have been 1.58 times as high as the actual judge mean.[27] Case type variables alone

27. We only report equation results for the most inclusive judge equation, given in column 2 of Table 4. Other results are available from the authors upon request.

Table 4. Award Equation Results for Heckit Estimation

	3-Level Heckit Jury	3-Level Heckit Judge	*F* test
Constant	10.96***	15.72***	1.96
	(1.15)	(3.19)	
Number of defendants	.66***	.666***	.001
	(.0606)	(.162)	
Expected years of life left	.35***	.322	.0009
	(.026)	(.275)	
Major injury	.862***	−.58*	16.44***
	(.0792)	(.34)	
Minor injury	−.926***	−1.65***	4.8**
	(.0715)	(.324)	
Emotional distress	−1.06***	−1.92***	5.49**
	(.0843)	(.354)	
Bad faith	−.013	−.966*	3.37*
	(.123)	(.518)	
Male	.325***	.157**	4.05**
	(.024)	(.08)	
Premises liability	−.726**	−.079	.361
	(.289)	(1.3)	
Medical malpractice	−.693	1.89	.563
	(.681)	(3.38)	
Product liability	−.112	1.03	.398
	(.327)	(1.78)	
Auto	−.336*	−.555	.016
	(.193)	(1.71)	
Poverty	3.03***	−2.64***	40.15***
	(.233)	(.86)	
Joint and several liability	−.055	−.02	.198
	(.0568)	(.16)	
Noneconomic cap	−.479***	−.189*	6.04**
	(.032)	(.113)	
Collateral sources	.250***	.0042	4.14**
	(.024)	(.118)	
No punitive	−.152	.186	.315
	(.144)	(.586)	
Punitive cap	−.146***	−.07	.749
	(.024)	(.084)	
Evidence standard	.106***	.173	.313
	(.027)	(.116)	
IMR trial mode	−1.00***	.399	20.3***
	(.172)	(.258)	
IMR settle	−.866***	−1.73***	13.5***
	(.0623)	(.228)	
IMR win	3.41**	−1.53	.779
	(1.75)	(5.33)	
Number of cases	30,226	4043	34,269

*, **, *** Significant at > 0.1, > 0.05, and > 0.01 levels, respectively.
Correct standard errors in parentheses — see text.

therefore explain 26.6% of the difference in mean awards [(158 − 100)/(318 − 100) = 26.6]. Using our second set of variables, which adds injury and law variables, we find that if judges had decided the sample of cases going to juries, awards in that sample would have been 2.08 times as high as the actual judge mean. Thus our second set of variables increases the explanatory power to approximately 50% [(218 − 100)/(318 − 100) = 49.5]. Adding the sample selection effects, we find that if judges had decided cases which actually went to juries, awards in that sample would have been 2.36 times as high as in the actual judge sample. Thus our most comphrensive set of variables is able to explain 62.5% of the difference in mean awards.

Although we find that three-quarters to two-thirds of the differences in mean awards is due to sample differences, there is still a significant unexplained difference in mean awards.

6. Comparing the Decision Process of Judges and Juries

6.1 Awards

A more detailed examination of the judge and jury equations sheds light on where differences in decision making ocurr.[28] Column 2 of Table 4 contains a judge award equation comparable to the Heckit model for juries in column 1. In column 3 we give F tests of the difference in coefficient values across the two equations. The F tests indicate that there are systematic differences between judges and juries in the impact that various factors have on awards. Bearing in mind that the judge equation is not as well estimated as the jury equation and that some of the coefficient values in the judge equation appear implausible, we can gain some insights by comparing the judge and jury coefficients.

Juries appear to be more sympathetic to injured plaintiffs than are judges. Holding the sample constant, juries give larger awards than judges for every injury category, with the exception of expected years of life left, for which no significant judge/jury differences are found.

Caps on damages for pain and suffering (noneconomic caps) cause a greater decline in awards when the case is decided by a jury than when the case is decided by a judge. The greater effectiveness of caps on juries is also consistent with the evidence on injuries discussed above. If juries grant larger awards than judges for pain and suffering when they are allowed to do so, it follows that juries rather than judges will be constrained by caps. Since judges grant fewer large pain and suffering awards to begin with, we find that caps on judges are "less effective"

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28. The lower bounds of what can be explained by differences in samples are 77% and 62.3%, since the inclusion of more variables, even random ones, would allow more of the difference to be explained. We are confident, however, that the additional explanatory power of any further variables is low. This specification includes two variables added to an earlier specification at the request of referees. The two additional variables raised the explanatory power by less than 2%.

(because they are less necessary). The collateral resources rule also has a different impact on juries than judges; it increases the award in jury trials but has no effect on trials before a judge. Again this effect is consistent with a story in which juries neutralize a weakening of the collateral sources rule by topping awards up, while judges, perhaps out of greater respect for the law, do not try to offset the law's intended effect.

The most robust difference between judges and juries arises in the impact of local poverty. A one standard deviation increase in the local poverty rate raises jury awards by $22,913, but causes a slight reduction in judge awards of $3394 (evaluated at the means). Poverty was included in the awards regression under the hypothesis that less affluent juries might be more responsive to income redistribution via the courts. Under this reasoning we would expect poverty to affect jury awards but not judge awards. Although statistically significant, the negative effect of poverty on judge awards is relatively small, and thus our results are consistent with the theory that less affluent juries are more sympathetic to plaintiffs.

The influence of local poverty on juries is the most important explanation for the "unexplained" difference in average awards. If poverty had no affect on juries, that is, if the coefficient on poverty in the jury equation were zero, then we could have explained 100% of the difference in average awards on the basis of sample differences. In other words, if poverty had no influence on jury awards, juries would have given the same average award to the judge sample of cases as judges actually gave.

Two of the selection terms differ across judges and juries. Not surprisingly, the coefficient on the inverse Mill's ratio generated by the trial forum equation is different, which suggests that the sample of cases going to juries and judges is different. More interesting is the fact that the inverse Mill's ratio for settlement has a different effect for jury trials than for judge trials. The data suggest therefore that settlement behavior is different depending on whether the case is scheduled to be decided by a judge or a jury. Unfortunately we are unable to investigate this effect in detail since we do not have data on whether settled cases were scheduled to be decided by a judge or jury.[29]

6.2 Win Rates — Sample Selection or Differences in Decision Processes?

We turn now to a more complete discussion of win rates across judges and juries. The average win rate in jury cases is 56.67% and in judge cases is 67.73%. Using the coefficients for the jury win equation in column 4 of Table 3, we can estimate what the win rate would have

29. Put differently, we are forced by data limitations to assume that cases are settled before the trial forum is decided upon.

been if the sample of cases going to judges had instead been decided by juries—60.04%. Sample selection can thus explain about 30% of the difference in judge and jury win rates [(60.04 − 56.67)/(67.73 − 56.67) = 0.3].

Since most of the difference in win rates appears not to be caused by sample selection, there may be significant differences in win decision processes across judges and juries. Using a likelihood ratio (LR) test, given in column 6 of Table 3, we can compare the coefficients from the jury and judge win equations given in columns 4 and 5 of Table 3. The test rejects at the 10% level or greater the null hypothesis of identical judge and jury win coefficients for every variable except products defense and the constant.

Marginal effects from the jury and judge win equations are presented in Table 5. Significantly almost all of the marginal effects run in the *opposite* direction to that of the average win rate. The average win rate is higher for judges than for juries, but this is almost entirely due to the higher win rate of auto cases before judges than before juries. Consistent with the anecdotal evidence, plaintiffs with product liability and medical malpractice cases are more likely to win before juries than before judges (although these cases are harder to win than the average in both forums).

7. Discussion

Bernstein (1996) argues that in an ideal world juries would be "eliminated" for civil trials. He continues, that unfortunately this would be unconstitutional in most states. As a result, the most "important measure that legislatures can take to eliminate the pernicious effects of civil juries is to remove the issue of damages from the jury and put it in the hands of judges." Our results show that such a reform would have a smaller effect on awards than Bernstein and other tort reformers imagine.

There is some truth, however, to the views of the tort reformers. Juries do grant systematically larger awards to injured plaintiffs than judges. Juries also appear to be more receptive to "redistribute the wealth" arguments than judges. In particular, juries drawn from pools

Table 5. Marginal Effects, Judge and Jury Win Equations

	Jury	Judge
Expected years of life left	0.09%	−3.1%
Product liability	−9.1%	−19.7%
Medical malpractice	−23.3%	−35.5%
Auto	7.4%	23.1%
Premises liability	−10.5%	−14.8%
Product defense	−4.9%	−1.5%

with high poverty rates grant systematically larger awards than judges and than juries drawn from more affluent regions. Win rates in product liability and medical malpractice cases are higher before juries than judges. The differences in judge and jury decision making we have discovered, however, explain only one-quarter to one-third of the difference in average award rates across judges and juries. Three quarters to two-thirds of the difference in average awards is due not to differences in decision making but to differences in the sample of cases appearing before judges and juries. The difference in average awards across judges and juries gives a very misleading picture of what would happen if the United States followed the rest of the world and shifted decision making from the judge to the jury.

Tort reformers often point the finger of blame for high awards on juries, but the revolution in product liability and medical malpractice law which has occurred over the past 40 years has been a product not of juries but of judges (Epstein 1980; Priest 1991). If juries have granted large awards in class action suits, it is the judges who have rewritten the law to enable those suits to be brought, often on the flimsiest of evidence. From this perspective, it's not surprising that judges grant similar awards to juries—the judges are leading the charge.

Appendix A: Descriptive Statistics

Table A1. Descriptive Statistics

Variable	Mean	Std. Dev.
Jury trial awards		
Log(jury award)	11.24	2.187
Number of defendants	.2336	.4055
Expected years of life left	.2433	.9215
Major injury	.1132	.3169
Minor injury	.7268	.4456
Emotional distress	.05102	.22
Bad faith	.0126	.1117
Male	.51	.5
Premises liability	.152	.3591
Medical malpractice	.0729	.26
Product liability	.04744	.2126
Auto	.4752	.4994
Poverty	.1281	.05512
Joint and several liability	.2366	.42499
Noneconomic cap	.1908	.3929
Collateral sources	.4983	.5
No punitive	.0711	.08404
Punitive cap	.52	.4996
Evidence standard	.322	.4674
Judge trial awards		
Log(judge award)	10.027	1.853
Number of defendants	.245	.41094

Continued

Table A1. *Continued*

Variable	Mean	Std. Dev.
Judge trial awards		
Expected years of life left	.156	.77
Major injury	.121	.326
Minor injury	.766	.4237
Emotional distress	.0493	.2164
Bad faith	.0866	.09268
Male	.48	.5
Premises liability	.0933	.2909
Medical malpractice	.0151	.122
Product liability	.0158	.125
Auto	.649	.4773
Poverty	.138	.0583
Joint and several liability	.203	.4022
Noneconomic cap	.231	.421
Collateral sources	.273	.446
No punitive	.0047	.0684
Punitive cap	.606	.489
Evidence standard	.262	.4396
Forum choice		
Forum choice (jury = 1)	.899	.3008
Auto	.4197	.4935
Number of defendants	.2332	.4092
Time difference	1.009	.2321
Default (judge = 1)	.229	.4204
Plaintiff request	.455	.659
Defendant reguest	.662	.792
Trial equation		
Does the case go to trial (yes = 1)	.683	.465
Product liability	.0474	.212
Medical malpractice	.0955	.294
Expected time to trial	6.663	.157
Number of defendants	.239	.4153
Child	.434	.496
Expected years of life left	.288	1.003
Joint and several liability	.225	.417
No limit on contingency fees	.47	.499
Jury win equation		
Plaintiff win at a jury trial (yes = 1)	.567	.4955
Product liability	.057	.2315
Auto	.407	.4913
Medical malpractice	.116	.3203
Premises liability	.178	.3822
Expected years of life left	.2705	.9663
Products defense	.0287	.1671
Judge win equation		
Plaintiff win at a judge trial (yes = 1)	.677	.468
Product liability	.028	.165
Auto	.534	.499
Medical malpractice	.0426	.202
Premises liability	.141	.348
Expected years of life left	.2171	.906
Products defense	.01	.0998

Appendix B: Duration Results

Table A2. Time to Trial Results

	Logistic Hazard Model Jury	Logistic Hazard Model Judge
Constant	7.15***	5.109***
	(.419)	(.273)
Death	.133***	.073
	(.023)	(.154)
Major injury	.143	−.245*
	(.213)	(.135)
Minor injury	.03	−.14
	(.02)	(.126)
Emotional distress	−.097	−.153
	(.023)	(.146)
Premises liability	−.005	.204***
	(.104)	(.077)
Medical malpractice	.211***	.292**
	(.119)	(.124)
Product liability	.212***	.327**
	(.016)	(.14)
Log(number of defendants)	.046***	.173***
	(.004)	(.026)
Auto	−.164***	−.42***
	(.009)	(.054)
Number of cases filled per judge in the state	−.067***	.1**
	(.005)	(.034)
Number of cases	36,896	5496

*, **, *** Significant at the > 0.1, > 0.05, and > 0.01 levels, respecvtively.
Asymptotic standard errors in parentheses.

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