The Effect of Electoral Institutions on Tort Awards

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We argue that partisan elected judges have an incentive to redistribute wealth from out-of-state defendants (nonvoters) to in-state plaintiffs (voters). We first test the hypothesis by using cross-state data. We find a significant partisan effect after controlling for differences in injuries, state incomes, poverty levels, selection effects, and other factors. One difference that appears difficult to control for is that each state has its own tort law. In cases involving citizens of different states, *federal judges decide disputes by using state law*. Using these diversity-of-citizenship cases, we conclude that differences in awards are caused by differences in electoral systems, not by differences in state law.

1. Introduction

Politicians are not neutral maximizers of the public good; they respond to incentives, just like other individuals. A clear understanding of political behavior requires, therefore, an understanding of incentive structures. Yet with few exceptions this insight has not been applied to those politicians

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we call judges. The lack of attention is surprising, since judicial incentive structures differ widely in the United States and thus provide an ideal testing ground for economic theories of politics. One important division occurs across the states. State court judges are elected in 23 states and are appointed in 27. Of the 23 elected states, ten use highly competitive partisan elections, whereas in the remainder judges run on nonpartisan ballots. A second division occurs between federal and state judges. Federal judges are appointed and have life tenure, whereas, as just noted, many state court judges are elected and, with the exception of superior court judges in Rhode Island, none have life tenure. We argue that in cases involving corporate defendants with out-of-state headquarters, elected judges, particularly partisan elected judges, have an incentive to grant larger awards than other judges. We test the partisan election hypothesis, using both of the divisions discussed above.

We first test the partisan election hypothesis by comparing cases in partisan elected states with cases in states using other selection mechanisms. We control for other influences that might differ across the states. Furthermore, we use data on settlements to control for the selection effect (Priest and Klein, 1984). One difference across the states, which appears difficult to control for, is that each state has its own body of tort law. It might be thought that the effect of selection mechanisms cannot be distinguished from the effect of tort law because, for example, only Alabama judges apply Alabama law. We take advantage of a peculiar aspect of American federalism to make this distinction. In cases involving citizens of different states, aptly called diversity-of-citizenship cases, *federal judges apply state law* to decide disputes. Diversity-of-citizenship cases, therefore, provide an ideal natural experiment. Do appointed and politically insulated federal judges make the same decisions as elected state judges when both apply the same law?

In section 2 we discuss the partisan election hypothesis. Section 3 introduces our estimation procedure and section 4 presents our cross-sectional results. Section 5 tests for the partisan election effect by using data on federal diversity-of-citizenship cases.

2. The Partisan Election Hypothesis

The dominant methods of judicial selection are partisan elections, nonpartisan elections, gubernatorial appointment, legislative election, and merit plans. The "merit plan," however, is gubernatorial appointment from a slate of candidates put forward by a nominating commission. Furthermore, the governor typically appoints at least some members of the nominating commission. The governor also plays an important role in legislative election, which is used in only three states (Connecticut, South Carolina, and Virginia). The main categories are thus partisan elections, nonpartisan elections, and appointed systems.

Elected judges must cater to the demands of the voters, and they must seek campaign funds from interested parties. Appointed judges by contrast do not need to answer to the voters in competitive elections, nor do they need to raise significant campaign funds. Furthermore, terms in nonelected states tend to be longer than terms in elected states, on average 21%–27% longer for general and supreme courts, respectively (Hanssen, 1999). Nonelected judges are also more secure than elected judges; they are returned to the bench—through reappointment or a retention election—more often than elected judges.[1] Appointed judges are thus more insulated from direct political pressure than elected judges and will tend, therefore, to be more independent (Dubois, 1990; Hanssen, 1999; Posner, 1993, p. 41).

In a partisan election state judges run under a party banner, just as other politicians. In a nonpartisan elected state, judges do not run under banners and are required by law to be independent of party. Elections tend to be more competitive in partisan than in nonpartisan states. Although judicial elections in nonpartisan states are more competitive than retention elections, they are still not very competitive. Many judges run unopposed, and when they are opposed few are defeated. Partisan elections tend to be contested more often, and, as a result, voter turnout is higher. Incumbents

1. Many judges in appointed states maintain their office by running in a retention election. These elections are *unopposed* elections in which the judge is either voted up or down. Hall and Aspin (1987) find that retention elections return the incumbent to office 98.8% of the time. Carbon (1980) points out that retention elections were designed to create lengthy judicial tenures and to insulate judges from the public. Retention elections also insulate appointed judges from pressures from the governor. Since retention elections are essentially perfunctory, we define states that use initial appointment followed by retention elections as appointed states.

are defeated more regularly than in nonpartisan elections (Dubois, 1979; Glick, 1983). Of elected states, ten use partisan elections.[2]

2.1. Previous Research into Judicial Electoral Systems

Judicial selection mechanisms are the subject of a large literature in political science, law, and judicial studies. The dominant approach in these studies has been sociological. The sociological approach posits that judicial outcomes are a function of judicial characteristics like race, sex, education, and wealth. If selection mechanisms have an effect on outcomes, they must do so, according to this view, by selecting for different types of judges. A large literature has tested whether judicial elections or appointments bring more minorities, women, conservatives, and so forth, to the bench or whether the ABA ratings of appointed judges are higher or lower than those of elected judges. Almost unanimously, this literature concludes that selection mechanisms have no significant effects on any judicial characteristics (see, for example, Alozie, 1990; Flango and Ducat, 1979; and Glick and Emmert, 1987; and the reviews of the literature in Baum, 1995; and Stumpf and Culver, 1992). In contrast to the sociological approach, we hypothesize that selection mechanisms affect outcomes through incentives even if they have little or no effect on measurable judicial characteristics.[3] Our hypothesis is thus framed and tested directly in terms of outcomes—in our case, awards in personal injury cases.

In Tabarrok and Helland (1999) we used a sample of 7,642 trial awards to compare awards in partisan elected states, nonpartisan elected states and nonelected states. We found that the average award in a case involving an out-of-state defendant was much higher in partisan elected states than in nonpartisan elected states or nonelected states. Furthermore, we could not reject the hypothesis that awards were the same in nonpartisan elected and

2. The states with partisan elections are Alabama, Arkansas, Illinois, Mississippi, New York, North Carolina, Pennsylvania, Tennessee, Texas, and West Virginia. For more details on our classification of electoral systems, see the Book of the States and the discussion in Tabarrok and Helland (1999). Our conclusions are robust to reclassification of any states with significant mixing of elected and nonelected elements (e.g., New York has a mixed system).

3. The discovery that sociological characteristics do not differ across selection mechanisms strengthens our conclusion that the primary independent variable is the incentive structure. Ashenfelter, Eisenberg, and Schwab (1995) find that sociological characteristics of judges are of no help in predicting outcomes.

nonelected states. We thus concentrate on the difference in awards between partisan elected and other selection systems (which we call nonpartisan systems).

In this article, we take advantage of a peculiar aspect American federalism to test the partisan election hypothesis. If a citizen of Texas sues a citizen of Oklahoma, both citizens have the option of having the case heard in federal court (limitations are described in greater detail below). Since federal judges are unelected and have life tenure, we expect that there will be significant differences between awards in cases decided by federal judges and awards in cases decided by state judges, even when the federal judges apply state law. We discuss diversity-of-citizenship cases and our test procedure at greater length below.

Most cases are settled rather than tried, and tried cases represent a nonrandom selection of disputes. To find the true effect of partisan elections on awards, we use a large data set of 52,545 observations of trial awards and 22,455 observations of settlements to control for any differences in the types of disputes that go to trial in partisan and nonpartisan states.[4] We also control for any differences in the winning disputes in partisan versus nonpartisan states.

2.2. Why Might Selection Mechanisms Matter?

In this section we outline two theories for why judicial selection mechanisms might have an effect on trial awards. Elections may cause judges to curry the favor of plaintiffs who are more often voters than are defendants, and they may cause judges to seek campaign contributions from lawyers interested in larger awards. The theories are not necessarily mutually exclusive. Our goal in this work is to show that how judges are elected has a large and statistically significant impact on tort awards, rather than to pinpoint the exact cause of this impact.

Judges in elected states must cater to the demands of voters. Plaintiffs typically sue in the state in which they live, so most plaintiffs are voters. Defendants, however, are often corporations headquartered in other

4. Tabarrok and Helland (1999) do not control for selection effects. The data set used in this work is deeper, as well as longer, than that used in our earlier study. In our earlier article, control variables such as poverty rates were measured at the state level. Here, all of our control variables are case specific or measured at the level of the county in which the trial takes place.

states or even other countries.[5] Plaintiffs, therefore, will tend to be more politically powerful than out-of-state defendants, especially in states with elected judiciaries. Richard Neely, a retired West Virginia supreme court judge, made this point frankly: "As long as I am allowed to redistribute wealth from out-of-state companies to injured in-state plaintiffs, I shall continue to do so. Not only is my sleep enhanced when I give someone's else money away, but so is my job security, because the in-state plaintiffs, their families, and their friends will reelect me Neely" (1988, p. 4). And, Neely continues, "[I]t should be obvious that the in-state local plaintiff, his witnesses, and his friends, can all vote for the judge, while the out-of-state defendant can't even be relied upon to send a campaign donation" (1988, p. 62). Redistributing wealth from out-of-state defendants to in-state plaintiffs is a judge's way of providing constituency service.[6]

A second explanation for the partisan electoral effect focuses on interest groups and campaign contributions. Just like politicians in the legislative branches of government, elected judges must raise significant amounts of campaign funds in order be elected and reelected. In the aggregate, campaign funds may not bias politicians much one way or the other. For every politician who accepts funds from big business there is another who accepts funds from big labor. Campaign funds, however, are more likely to bias the judiciary. The judiciary affects interest groups from across the political spectrum, but the interest groups do not know which of the thousands of judges will rule in their particular case. (And once a judge

5. Clermont and Eisenberg (1996) examine whether the federal courts are biased against foreign corporations.

6. Judges may understand the negative impact that excessively generous trial awards can have on insurance costs, wages and employment, and economic growth. Nevertheless, state judges have little to gain from more restrained interpretations of liability law. A judgment in favor of a defendant enriches an out-of-state corporation but has little effect on national employment and even less effect on in-state employment or wages. The gains from restrained interpretation of liability laws are external to the state judges who interpret those laws. But the benefits of liberal judgments, in votes and campaign contributions, accrue directly. Similarly, voters have few incentives to demand changes in liability law that primarily benefit out-of-state corporations. The median voter, therefore, is likely to support judges who redistribute income to in-state plaintiffs. Other observers have also noted that elected judges are easier to influence than appointed judges. Herman Wrice, the founder of an antidrug citizen's group in the Mantua section of Philadelphia, notes that "[I]n a city where judges are elected, a few members of Mantua Against Drugs assembled in the court room can add thousands of dollars to the price of bail" (quoted in Benson, 1998, p. 124).

has been assigned to a case it is usually too late to engage in effective lobbying.) A pharmaceutical company, for example, has an interest in liability law but it does not know when or where it might be sued, let alone the judge who will preside over the case. The random assignment of judges to cases means that the most consistent contributors to judicial campaigns are trial lawyers.

Unlike other participants, trial lawyers engage in repeated interactions with the same judges and so have the most incentive to make campaign contributions. Posner (1996, p. 39), for example, points out that "the local trial bar is invariably the major source of campaign contributions to judicial candidates." At a given moment some trial lawyers are working for the plaintiff and others for the defense. Nevertheless, in general, all trial lawyers are interested in larger awards. Larger awards mean larger fees, whether one works for the plaintiff or the defense. Consider two judges who rule in the plaintiff's favor equally often but one of whom tends to be more generous in the granting of awards. Defense and plaintiff's lawyers will both prefer that the more generous judge be elected, because generous judges increase the demand for both plaintiff and defense lawyers. Judges who grant large awards will find fund raising easier than their more "stingy" colleagues. Thus, even if every judge applies the law with no consideration whatsoever for political factors, we can expect that over time generous judges will be selected for in states with an elected judiciary.

The campaign-contribution theory implies that awards in general should be higher in partisan elected states. To reach the conclusion that awards against out-of-state defendants will be especially high, we need the supplementary hypothesis that local defendants (voters) will discipline judges who raise in-state awards. In-state defendants may be able to counter the campaign contributions of trial lawyers through their votes, but no such counter is available to out-of-state defendants. Thus the elasticity of awards against out-of-state defendants (with respect to lawyer campaign contributions) is larger than the elasticity of awards against local defendants.

Each of these theories focuses on judicial incentives or characteristics. Judges, however, directly decide only a small minority of tort cases. Nevertheless, judges have significant control over the trial outcome. Judges must interpret the law for juries, instruct the juries, allow or disallow objections, rule on motions and countermotions, limit or not limit the

lawyers to certain theories of liability and damages, and so on. Our thesis does not require that partisan elected judges make blatantly biased rulings. All the thesis requires is that, compared to other judges, partisan elected judges make marginal changes in rulings which tend in the direction of supporting larger awards.

Since almost all personal injury cases are jury trials, we cannot absolutely rule out the hypothesis that juries in states that elect their judges through partisan elections are especially likely to grant large awards against out-of-state defendants. Nevertheless, three pieces of evidence (plus Occam's razor) suggest that the explanation for our results lies in judges, not juries. First, the limited evidence from judge trials is consistent with the jury evidence. Second, we control for the most obvious characteristic that might affect jury awards, poverty rates of the jury pool. Although we find that local poverty does increase awards, it is not responsible for the partisan electoral effect. Third, if juries were responsible for our results, we would also expect to see higher awards against out-of-state defendants in partisan elected states in cases presided over by federal judges (most tort trials are jury trials in both the federal and state courts). We show, however, that when federal judges are presiding, awards against out-of-state defendants are not significantly higher in partisan elected states compared to nonpartisan states.

3. Exploratory Data Analysis: Partisan versus Nonpartisan State Court Cases

The data on torts was extracted from Jury Verdict Research's Personal Injury Verdicts and Settlements on CD-ROM.[7] Data from trials are drawn directly from court records. Using an extensive survey of lawyers, JVR also collects data on settlements. Our data set contains information on 52,545 trials and 22,455 settled cases.[8] The data set spans all of the

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

8. The data set originally contained two extreme outliers, awards of 4.25 and 5 billion. We eliminated these outliers from all computations.

Table 1. Expected Awards and Win Rates by Trial Category

	Expected Total Award	Win Rate	Trials
All	$332,285	0.556	52,545
Product liability	$1,457,984	0.427	2,134
Medical malpractice	$598,096	0.328	6,147
Auto	$159,734	0.656	24,856
Premises liability	$162,975	0.45	7,916

50 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 means for the total award and the win rate broken down by various categories of case. The breakdown is similar to that found in other data sets.

The data set contains the name of the defendant, which may be either a business or an individual, but it does not give an address for the defendant. Nor do we have addresses for the plaintiffs, all of whom are individuals. We were able to assign an in- or out-of-state classification for each business defendant in our sample by using the COMP database to locate the headquarters of each business. The COMP database contains information on over 140,000 private and public firms. We were able to locate the headquarters of most firms in our sample. We assumed that any firm that we could not find in the database (e.g., Alex's Muffler Shop) was headquartered locally, in essence, in the state in which the trial occurred. We were not able to locate the residences of the individual plaintiffs or defendants in our sample and by default assumed that each individual resides in the state in which the trial occurs. Although it is possible to sue in a state different from the one in which one resides, it is rare because inconvenient.[9]

In Table 3 we perform a simple difference-in-means test by regressing the total award on a constant and four dummy variables, partisan out, partisan in, nonpartisan out, and nonpartisan in. (Descriptive statistics for Table 3 variables can be found in Table 2). Partisan out denotes trials in partisan states with out-of-state business defendants; the other variables are defined similarly. The coefficient on the constant term is the average

9. We removed all class action suits from our sample both because it is difficult to code for injuries in these cases and because plaintiffs in these suits may come from many states.

Table 2. Descriptive Statistics: Difference in Means

Difference-in-Means Variables	Means (St. Dev.)
Total award	333,292
Partisan out	0.0129
Partisan in	0.0812
Nonpartisan out	0.0392
Nonpartisan in	0.1788

award in non-business cases. The coefficients on the other variables are the differences between cases of that type and the average nonbusiness case.

In partisan states the average award against an out-of-state business defendant is $936,190, which is $527,740 larger than the average award against an in-state business defendant ($936,190 − $408,450). In nonpartisan states, the average award against an out-of-state business defendant is only $272,780, which is only $138,730 larger than the average award against an in-state business. The difference partisan out − nonpartisan out measures the total "partisan effect." Awards against out-of-state businesses are $663,410 higher in partisan than in nonpartisan states. The difference is statistically significant at the (far) greater than 1% level, ($F[1,52540] = 16.31$ with $p = .0001$). Our preliminary evidence supports the hypothesis that awards against out-of-state businesses are significantly higher in states with partisan elections than in states that use other selection mechanisms.

The total partisan effect, partisan out − nonpartisan out, combines a partisan out-of-state effect and a partisan business effect. Awards against out-of-state firms in partisan elected states may be higher than in similar cases in nonpartisan states because awards are higher against out-of-state firms in partisan states (the partisan out-of-state effect) or because awards against businesses in general are higher in partisan states (the partisan business effect). The two effects can be decomposed. The partisan out-of-state effect is measured by (partisan out − partisan in) − (nonpartisan out − nonpartisan in). By subtracting out awards against in-state businesses, we control for any increase in awards against businesses in general in partisan elected states, thus isolating the partisan out-of-state effect. The partisan out-of-state effect has the value $393,690 ($F[1,52540] = 4.84$ and $p = .027$). The partisan business effect is measured by (partisan in − nonpartisan in) and has a value of $269,720

Table 3. Difference in Means, Medians, 75th Percentiles, Judge Only

	Total Award (All Awards)	**Medians (Pos. Awards)**	**75th Percentile (Pos. Awards)**	**Judge Cases Only (Pos. Awards)**
Constant	$252,540*	$34,000	$164,194	$204,838*
Nonbusiness cases	(19,524)	[20,033]	[20,033]	(25,914)
Partisan out	$936,190*	$116,942	$800,000	$319,908***
	(143,800)	[320]	[320]	(178,696)
Partisan in	$408,450*	$102,505	$526,668	$16,801
	(60,090)	[2,256]	[2,256]	(81,869)
Nonpartisan out	$272,780*	$79,577	$341,638	$115,143
	(84,070)	[1,261]	[1,261]	(132,549)
Nonpartisan in	$138,730*	$88,572	$367,708	$15,529
	(43,003)	[5,423]	[5,423]	(70,453)
Number of cases	52,545	29, 293	29,293	3,712
Differences in differences				
Partisan out − nonpartisan out	$936,190 - 272,780 = 663,410*$	$116,942 - 79,577 = 37,365*$	$800,000 - 341,638 = 458,362*$	$319,908 - 115,143 = 204,765$
	$p = .0001$	$p = .0084^{nb}$	$p = .0000^{nb}$	$p = .5$

Notes: In the total-awards regression, awards are expressed as differences from the average nonbusiness award (constant). In the median and 75th percentile columns, the awards are expressed as exact values, not as differences from nonbusiness cases. OLS standard errors are in parentheses in the total award column. Number of observations in each category are in square brackets in the other columns. The notation nb means the *p*-values in the median and 75th percentile columns were calculated using the bootstrap method.

*Significant at the .01 level.

***Significant at the .1 level.

($F[1,52540] = 15.7801$, $p = .0001$). Awards against businesses in general are larger in partisan than in nonpartisan states, but most of the partisan effect is due to a bias against *out-of-state* business defendants.

Trial awards are highly right-skewed, and most of the partisan electoral effect comes from an increase in the right-hand tail of the distribution of awards. The second and third columns of Table 3 present the median award and the award at the seventy-fifth percentile. Median awards in cases with out-of-state defendants are $37,365 larger in partisan elected states than in nonpartisan states. The difference is statistically significant at the greater than 1% level. As the percentile increases, the difference in awards between partisan and nonpartisan states increases. At the seventy-fifth percentile awards against out-of-state defendants are $458,362 larger in partisan elected states than in other states.

The fourth column of Table 3 presents results considering only judge-decided trials. The judge and jury samples are not directly comparable since the sample of cases going to trial before a judge are quite different from those going to trial before a jury (Helland and Tabarrok, 2000). In particular, judges deal with the types of cases likely to generate low awards (e.g., premises liability and auto cases) in much greater proportion than juries. As a result, the mean award in judge trials is well below the mean jury award. Judge trials are also quite rare in personal injury lawsuits: more than 90% of these trials are before juries. We should not expect, therefore, that the judge and jury results be similar. Nevertheless, when the defendant is out-of-state the mean award in partisan states is over $200,000 higher than the mean award in nonpartisan states. The difference between the two awards, however, is not statistically significant at conventional levels, probably because the sample size is so small (there are only 60 out-of-state defendants in partisan states and only 111 in nonpartisan states). In the remainder of the article we take advantage of our large data set by focusing on the combined judge and jury sample (results do not change in a jury-only sample).

Although suggestive, these difference in means and medians raise the question whether the larger awards in partisan states are caused by differences in the electoral system or by some other differences that are merely correlated with differences in the electoral system. In the following section we refine the difference-in-differences analysis by adding variables to control for a variety of other potential influences. Also, to properly account

for selection effects, we model the process that transforms a dispute into a trial into a winning case.

Our data set has descriptive information on the victim's injury. We code this information into nine exclusive and exhaustive variables: Llife, major injury, minor injury, emotional distress, rape, sexual assault, sexual harassment, bad faith, and wrongful termination. Llife is the expected years of life left in a case involving a death. The remaining injury variables are dummy variables. If the victim suffered a permanent injury such as loss of limb, brain damage, or blindness, "major" is set equal to one. Minor injuries are those that are (potentially) temporary—for example, broken arms, broken legs, concussions, or wounds. Emotional distress indicates cases in which the victim suffered emotional or psychological injuries. Rape, sexual assault, and sexual harassment are self-explanatory. Bad faith cases are those in which a plaintiff sues an insurance company for denying a claim. The injury in bad faith cases is the denial of the claim, not a physical injury. In a wrongful termination case, the plaintiff sues his ex-employer for wrongful dismissal. Together these variables control for the severity of the plaintiff's injury. To prevent perfect collinearity with the intercept term, we suppress wrongful termination.

We also include case type variables and a number of legal variables that may affect liability. A dummy variable, "weak joint and several," is set equal to one if the state has created significant exceptions to the joint and several liability rule (many states have eliminated the rule in product liability cases and weakened it in other types of cases) and 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. 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. In states with an evidence standard, the defendant's behavior must "clearly and convincingly" be shown to have exhibited "reckless disregard" or "malice" for punitive damages to be awarded. In states with bifurcated trials, punitive damages claims may be considered separately from compensatory claims. Since a bifurcated trial usually occurs only at the request of the defendant, we expect that bifurcated trials will reduce awards. "No punitive" is a dummy

variable set equal to one if the state "prohibits" punitive damages.[10] Punitive cap is set equal to one if the state in which the trial occurs caps punitive damages either absolutely or relative to compensatory damages (for example, punitive damages cannot exceed compensatory damages by more than three times). We expect that weakening the joint and several rule will decrease awards and thus have a negative sign, whereas noneconomic cap, collateral source, evidence standard, bifurcated trial, no punitive, and punitive cap will all reduce compensatory or punitive damages and thus have negative signs.

Anecdotal and statistical evidence indicates that jury awards are higher the higher the local poverty rate is (Helland and Tabarrok, 2001). Juries and judges from poor regions are perhaps more likely to favorably regard wealth redistribution from large corporations to poorer plaintiffs. The poverty rate of the county in which the trial occurs is included as an explanatory variable to test for this possibility. Poverty is thus the poverty rate of the pool from which the jury is drawn. We expect that higher poverty rates will increase awards.

The test variables partisan out, partisan in, nonpartisan out, and nonpartisan in, are as described above. Descriptive statistics on all variables can be found in Table 4. Also, as described above, for one to observe an award, the case must have failed to settle and the plaintiff must have won the case.

To properly control for selection effects, we need to model the process by which a case settles (goes to trial) and wins (loses). Settlement decisions depend upon (among other variables) expectations about what will happen should the case go to trial. To model settlements, therefore, we must estimate expected awards. To create estimates of the expected award and its variance, we estimate the model in two stages. In the first stage we estimate each of the model's equations to create for each case a "shadow award" and a "shadow probability" of winning. The shadow variables are estimates for each case of what would happen if that case went to trial. In the second stage we reestimate the model by using the shadow variables as estimates of plaintiff and defendant expectations. In effect, the

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

Table 4. Descriptive Statistics: State-Award Regression

	Mean (St. Dev.)
Total award	10.85
	(2.189)
Expected years of life left	0.2108
	(0.8694)
Major injury	0.1084
Minor injury	0.7547
Emotional distress	0.0465
Rape	0.0017
Sexual assault	0.0058
Sexual harassment	0.0008
Bad faith	0.0089
Product liability	0.0315
Medical malpractice	0.0690
Auto	0.5585
Premises liability	0.1226
Weak joint and several liability	0.2506
Noneconomic cap	0.2181
Collateral sources	0.4720
No punitive	0.0057
Punitive cap	0.5472
Evidence standard	0.3088
Bifurcated trial	0.1778
Poverty	0.1287
	(0.0562)
Partisan out	0.0109
Partisan in	0.0769
Nonpartisan out	0.0431
Nonpartisan in	0.1853

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.[11]

Unobserved sources of variation in the settlement and win decisions could be correlated with unobserved sources of variation in the award equation. Correlation of errors will cause coefficient estimates in the award equation to be biased. To control for any correlation of errors between the settlement and award equation or the win and award equation we

11. An extended discussion of an estimation procedure similar to ours can be found in the pioneering work of Danzon and Lillard (1982).

use Heckman's (1979) procedure. Results from the settlement and win probit are used to construct inverse Mill's ratios that are then included as explanatory variables in the award equation.[12]

As it turns out, controlling for selection does not greatly influence the variables of special interest concerning the partisan hypothesis. A lengthier description of our estimation procedure and full description of each intermediary equation and the results from that equation can be found in Helland and Tabarrok (1999). We turn now to the final award equation.

4. Results from the Award Equation

Our discussion of the results, to be found in Table 5, will focus on a few illustrative variables rather than pedantically mentioning each in turn. The dependent variable is the natural log of the total award. All non–dummy variables are also in natural logs.

The injury variables are significant and of the expected sign. For comparison purposes, the mean dollar award conditional on winning is $599,000, while the median dollar award is $48,604 (the exponential of the mean log award is close to the median dollar award). If the victim died with an expected 40 years of life remaining (i.e., at approximately age 35) the mean dollar award increases to $2,920,000 and the median dollar award increases to $237,200. Alternatively expressed, if the victim dies at approximately age 35, the award increases by 437% ($e^{.45591*\mathrm{Ln}(40)} - 1$). A major injury increases awards by 179% ($e^{1.0286} - 1$) and a minor injury decreases awards by 50% ($e^{-0.696} - 1$). Awards are approximately double (103% higher) in product liability cases than in otherwise similar cases. A closely related puzzle is that awards in auto cases are about half the size of awards in otherwise similar cases. Thus, a plaintiff is rewarded much more highly if he loses his arm in a lawnmower accident (product liability) than if he loses the same arm in an auto accident. These results suggest a deep pockets effect, although other explanations are possible. Awards could be higher in product liability

12. Heckman (1979) shows that if the error terms in the respective probit equations and the award equation are distributed bivariate normal, then including the inverse Mill's ratios as above will allow the coefficients on the remaining explanatory variables to be estimated consistently.

Table 5. State Regression Results

	Trial Award (A)
Constant	13.128*
	(1.3084)
Expected years of life left	0.45591*
	(0.02967)
Major injury	1.0286*
	(0.091558)
Minor injury	−0.69608*
	(0.085231)
Emotional distress	−1.0132*
	(0.095743)
Rape	1.5684*
	(0.27086)
Sexual assault	1.2839*
	(0.16177)
Sexual harassment	−0.26084
	(0.39584)
Bad faith	−0.18688
	(0.13875)
Product liability	0.71113***
	(0.38531)
Medical malpractice	0.73897
	(0.71503)
Auto	−0.66701**
	(0.28065)
Premises liability	−0.16022
	(0.30666)
Weak joint and several liability	0.006289
	(0.054663)
Noneconomic cap	−0.38883*
	(0.04349)
Collateral sources	0.36266*
	(0.022871)
No punitive	0.14863
	(0.14293)
Punitive cap	−0.32897*
	(0.022675)
Evidence standard	0.24964*
	(0.02590)
Bifurcated trial	−0.071488
	(0.48386)
Poverty	0.94045*
	(0.19452)
Partisan out	0.70742*
	(0.10477)
Partisan in	0.47967*
	(0.045607)

Table 5. Continued

	Trial Award (A)
Nonpartisan out	0.35693*
	(0.05665)
Nonpartisan in	0.35481*
	(0.0338)
IMR settle	−1.2063*
	(0.053352)
IMR win	−0.082936
	(1.8428)
Number of cases	29,238

*Significant at the greater than .01 level.
**Significant at the greater than .05 level.
***Significant at the greater than .1 level.

cases, for example, because these cases are more difficult to detect than auto accidents.

The greater the local poverty rate, the higher the award, holding all else equal. The poverty variable is highly statistically significant ($p = .00001$) and also economically meaningful. Moving a case from a county with an average poverty level to a county with a poverty level one standard deviation above the mean raises the expected award by 5% (about $32,500 at the mean). Since the distribution of poverty is highly right-skewed, it would not be difficult in most states to find a county with a poverty level two or three times higher than the mean.

The legal variables are not all significant or of the expected sign. Weakening the joint and several rule appears to have no effect on awards. Caps on damages due to pain and suffering reduce awards on average by 32%. We expected collateral sources and evidence standard to have negative signs, but they are both statistically significant with positive signs: they raise awards by 36% and 24%, respectively. States with larger awards may be more likely to weaken the collateral-sources rule and enact evidence standards. Endogeneity problems may thus prevent accurate estimation of the effect of these variables in a cross-section regression. (Since we include the legal variables only in order to control for factors, other than electoral systems, that cause differences in awards across the states, the difficulty in interpretation is not material to our primary results.) As expected, caps on punitive damages reduce awards (by 28%), as do bifurcated trials (−6.8%).

Our primary hypothesis concerns the electoral variables, partisan out, partisan in, nonpartisan out, and nonpartisan in. Awards against out-of-state businesses are 42% larger in partisan than in nonpartisan states ($e^{0.70742-0.35693} - 1$). Put differently, moving an otherwise average case with an out-of-state defendant from a nonpartisan to a partisan state raises the expected award by $362,988. The partisan effect is statistically significant at a greater than 1% level.[13] It is worth emphasizing that the $362,967 partisan election effect exists after controlling for a wide variety of potential differences in cases across the states, including differences in injuries, income levels, and major laws. The coefficients on nonpartisan out and on nonpartisan in are almost identical, which suggests that there is little or no penalty against out-of-state businesses in nonpartisan states. In contrast, the coefficient on partisan out is larger than that on partisan in, and both coefficients are larger than their nonpartisan counterparts. The evidence, therefore, suggests that in partisan elected states awards against businesses are higher than in other states; awards against out-of-state businesses are especially high.[14]

As noted earlier, we can break the partisan effect into partisan out-of-state and partisan business effects. The partisan out-of-state effect is measured by (partisan out − partisan in) − (nonpartisan out − nonpartisan in). The partisan out-of-state effect accounts for $230,092 of the $362,988 total partisan effect. The remaining $132,897 is accounted for by the partisan business effect. As we found in the simple difference-of-means estimates, awards are higher in partisan elected states both because awards against businesses are higher and because awards against out-of-state businesses are especially high.[15]

In Table 6 we test the robustness of the partisan electoral effect. In the first column we run the same regression as earlier but without any selection effects (for clarity we present only the electoral variables). We find that awards against out-of-state businesses are 31% larger in partisan

13. The F-test for the restriction (partisan out = nonpartisan out) is $F[1, 29209] = 10.5046$, with $p = .0014$.

14. We cannot reject the hypothesis that nonpartisan out = nonpartisan in, $F[1,29209] = 0.0191$, with $p = .8605$. The restriction (partisan out = partisan in) has $F[1,29209] = 4.2963$ with $p = .0360$.

15. The restriction (partisan out − partisan in) − (nonpartisan out − nonpartisan in) has $F[1,29209] = 3.1432$, with $p = .0725$. The restriction (partisan in = nonpartisan in) has $F[1, 29209] = 10.9615$, with $p = .011$.

Table 6. Robustness Tests

Variable	No Selection Effects	State Fixed Effects[a]	Business Cases Only	Prod./Med. Only
Partisan out	0.679449*	0.70156*	0.429695*	0.9075*
	(0.104679)	(0.10250)	(0.108813)	(0.2800)
Partisan in	0.464236*	0.33804*	0.210822*	−0.1203
	(0.04318)	(0.045359)	(0.051442)	(0.1283)
Nonpartisan out	0.409429*	0.43537*	02011229	0.1928
	(0.0549311)	(0.053965)	(0.0587857)	(0.1572)
Nonpartisan in[b]	0.412717*	0.43185*		0.1576
	(0.030204)	(0.030457)		(0.1015)
Number of cases	29,238	29,238	9,245	2,929
Differences in differences				
Partisan out − nonpartisan out	$(e^{0.679-0.409}-1)=0.309^{*}$ $p=.0191$	$(e^{0.701-0.435}-1)=0.304^{**}$ $p=.0189$	$(e^{0.429-0.02}-1)=0.50^{*}$ $p=.000419$	$(e^{0.907-0.192}-1)=1.0^{**}$ $p=.0160$

Note: OLS corrected standard errors in parentheses.

[a]OLS standard errors were used in this regression (see text).

[b]Nonpartisan in was suppressed in the business cases only regression to prevent perfect collinearity with the intercept.

*Significant at the greater than .01 level.

**Significant at the greater than .05 level.

***Significant at the greater than .1 level.

than in nonpartisan states ($e^{0.6794499-0.409429} - 1$). The difference is statically significant at the just over the 1% level ($F[1,29212] = 5.4965$, $p = .0191$). Since the partisan electoral effect is robust to the exclusion of selection effects, none of the details of our estimation technique, such as our creation of the expected award variables, are driving our results.

As a second robustness test we add state-specific fixed effects to the win and award equations. Award and win rates do appear to vary somewhat across the states, but the variation is orthogonal to the electoral variables. In this regression we estimate that awards against out-of-state businesses are 30% greater in partisan than in nonpartisan states ($e^{0.70156-0.43537} -$ 1). The difference is statistically significant at just over the 1% level ($F[1,29167] = 5.4190$, $p = .0189$).[16]

The reference case in our earlier regressions was a nonbusiness case. The partisan electoral effect is estimated on the basis of cases with business defendants because only in these cases can we easily identify in-state and out-of-state defendants. We include nonbusiness cases in our regressions because we are interested in the coefficients of some nonelectoral variables like poverty and because the inclusion of nonbusiness cases improves the estimates of the nonelectoral variables. Better estimates of the nonelectoral variables in turn allows for better estimation of the electoral variables. In the third column we estimate the model by using business cases only to show that this restriction is not driving our results. Using business cases only, we find that awards in partisan states with out-of-state defendants are 50% larger than awards against out-of-state defendants in nonpartisan states ($e^{0.429695-0.020112} - 1 = 0.50$). The difference is statistically significant at a greater than 1% level ($F[1,9218] =$ 12.44, $p = .000419$).

Our fourth robustness test restricts the sample to cases of special interest, product liability, and medical malpractice cases. Again we find that awards against out-of-state defendants are much higher in partisan states than in nonpartisan states ($e^{0.9075-0.1928} - 1 = 1.04$). The difference is statistically significant at a greater than 5% level ($F[1,2914] = 5.80$, $p =$ .0160).

16. The state-level fixed effects made it very difficult to compute the heteroscedastic consistent var-covariance matrix, so we relied for this F-test only on OLS standard errors.

5. Diversity-of-Citizenship Cases

The Constitution (Art. III, Sec. 2[1]) gives the federal courts the power to decide controversies between citizens of different states. Historically, federal diversity jurisdiction was supported by out-of-state businesses that feared they would be disadvantaged in pro-plaintiff or pro-debtor state courts (Friendly, 1928). Today lawyers continue to cite out-of-state and antibusiness bias as one reason for removing cases to federal court (Miller, 1992). For over a century federal judges decided diversity-of-citizenship cases according to federal common law. The Supreme Court, however, overturned this rule in the 1938 case *Erie R.R. v. Tompkins*. Since 1938 diversity cases have been decided on the basis of state law.[17]

Even when federal judges apply state law, comparing federal and state cases is problematic because of multiple sample selection problems. Cases that go to federal court are not a random selection of state cases. Clearly, diversity-of-citizenship cases require that the plaintiff be suing a citizen of another state. In addition, during the period of interest in order to bring a diversity case to federal court, the plaintiff must have claimed damages of at least $50,000 (the minimum amount in controversy). Other differences in the sample of cases going to federal court may be unobserved. Furthermore, we have to be careful to allow settlement behavior to differ in the two samples. Posner (1996) suggests, for example, that the federal courts are more predictable than the state courts. If the variance of the outcome is lower in federal courts, then, *ceterius paribus*, the probability of settling should be higher and thus a different sample of cases go to trial in federal courts than in state courts.

Our strategy for controlling for these issues is twofold. Most importantly, we do not *directly* compare state cases and federal cases. Instead we follow our earlier differences-in-differences methodology. We compare awards in cases where federal judges apply the law of partisan elected states with awards in cases where federal judges apply the law of nonpartisan states; this gives us the "federal difference" $(\text{partisan} - \text{nonpartisan})_{\text{fed}}$. Using a similar sample of cases (cases involving out-of-state businesses), we create the "state difference" $(\text{partisan} - \text{nonpartisan})_{\text{state}}$. If all of the

17. The definitive source for diversity-of-citizenship law is Wright (1994). Posner (1996) and Lieberman (1992) give short overviews.

partisan election effect is due to differences in the law of torts in partisan elected states, then the federal and state difference should be equal, in essence, $(\text{partisan} - \text{nonpartisan})_{\text{fed}} - (\text{partisan} - \text{nonpartisan})_{\text{state}} = 0$. If the partisan election effect is due to partisan elected judges interpreting essentially the same law differently (than judges in other states) then the state difference should be much larger than the federal difference. The advantage of the differences-in-the-differences method is that it measures exclusively the partisan election effect, thus controlling for any other differences in federal and state cases.

The second part of our strategy for controlling sample selection problems uses the Heckman (1979) two-step method also discussed above. Essentially we add another level of selection, the forum choice, to our earlier model. The sample of cases is all cases involving out-of-state businesses. Each of these cases could potentially go to either federal or state court. As noted, the plaintiff must claim at least $50,000 in damages. In our sample this constraint is unlikely to bind since $50,000 is low relative to the mean amount awarded, which is just under one million dollars. (Furthermore, the plaintiff need only claim $50,000; the plaintiff is not penalized if the actual award is less than $50,000.) A probit is used to estimate the determinants of going to federal court. A settle, win, and award equation is estimated for cases that go to federal court, and a settle, win, and award equation is estimated for cases that go to state court. Thus, we allow for different settle, win, and award decisions in the two samples. To control for unobserved variation in the forum choice, settle, and win equations, which might be correlated with the error in the award equation, we compute forum, settle, and win inverse Mill's ratios, which are included as explanatory variables in the award equations. As before, details concerning our estimation procedure and the results from intermediary regressions, including the forum-choice equation, can be found in Helland and Tabarrok (1999).

We are primarily interested in the award equations, and in particular we wish to compare $(\text{partisan} - \text{nonpartisan})_{\text{state}}$ and $(\text{partisan} - \text{nonpartisan})_{\text{fed}}$.[18] We define partisan as a dummy variable equal to one if

18. Results from the federal and state settlement and win equations are available from the authors upon request.

Table 7. Descriptive Statistics: Diversity-Jurisdiction Regressions

	State Means (St. Dev.)	Federal Means (St. Dev.)
Total award	11.467	12.18337
	(2.1455)	(1.7898)
Expected years of life left	0.1862	0.2769
	(0.8074)	(0.9545)
Major injury	0.1439	0.1247
Minor injury	0.6868	0.5435
Emotional distress	0.0458	0.0781
Bad faith	0.0458	0.0290
Product liability	0.1632	0.1712
Medical malpractice	0.0080	0.0105
Auto	0.3434	0.1677
Premises liability	0.2071	0.1844
Weak joint and several liability	0.3137	0.2098
Noneconomic cap	0.1830	0.2704
Collateral sources	0.4505	0.3363
No punitive	0.0033	0.0114
Punitive cap	0.2429	0.4012
Evidence standard	0.2429	0.2098
Poverty	0.1340	0.1296
	(0.0552)	(0.0327)
Partisan	0.2165	0.3459

the case in question took place in a state with partisan elected judges.[19] $(\text{partisan} - \text{nonpartisan})_{\text{state}}$ is thus equal to the coefficient on partisan in the state regression and $(\text{partisan} - \text{nonpartisan})_{\text{fed}}$ is equal to the coefficient on partisan in the federal regression. Results on the award regression are presented in Table 8. (Descriptive statistics can be found in Table 7.)

19. We use the location of the federal court to deduce the state law that the court is using to decide the case. It is possible that a case adjudicated in a federal court in state A is decided according to the law of state B. In our sample of cases, personal injury cases in which an individual sues a corporation, this is unlikely to occur. In over 99% of these types of cases the plaintiff (an individual) resides in the state in which the trial takes place. Furthermore, the traditional common law rule is that the law of the state where the injury occurred is the law to be applied. Since overwhelmingly most personal injuries occur in the state in which the plaintiff resides, the traditional rule strongly suggests that the law of the state in which the trial takes places is the ruling law. In some states the courts analyze the respective interests of the states to decide the law to be applied. Prime among the determinants a court will use to deduce a state's "interests," however, is the place of the injury (and the residence of the parties to the dispute). For more details see Posner (1998) and the Restatement of the Law Second, Conflict of Laws (American Law Institute).

Table 8. Diversity Jurisdiction Regressions

	State Trial Award (SA)	Federal Trial Award (FA)
Constant	23.468	9.7548***
	(22.065)	(5.2284)
Expected years of life left (Llife)	0.73887	0.35046**
	(0.59406)	(0.17548)
Major injury	0.77818*	0.69981**
	(0.294)	(0.27889)
Minor injury	−0.27835***	0.052187
	(0.27671)	(0.33046)
Emotional distress	−0.056708	−0.83598
	(0.47335)	(0.96127)
Bad faith	−0.51513	0.082754
	(0.31419)	(0.32952)
Product liability	7.9794	0.031906
	(9.9903)	(1.1737)
Medical malpractice	11.105	0.29501
	(14.793)	(1.83)
Auto	−3.9189	−2.5172
	(7.5580)	(2.1438)
Premises liability	−0.59280	−1.6337**
	(0.38107)	(0.67213)
Weak joint and several liability	0.0949	0.23572
	(0.29431)	(0.21536)
Noneconomic cap	−0.18340***	0.084092
	(0.11036)	(0.16731)
Collateral sources	0.38053*	0.085827
	(0.074358)	(0.13338)
No punitive	0.11385	0.31302
	(2.4729)	(0.56295)
Punitive cap	−0.26556*	−0.21347
	(0.10113)	(0.23972)
Evidence standard	0.42125*	0.14723
	(0.10398)	(0.18421)
Number of defendants	0.96580*	
	(0.18248)	
Poverty	1.4331***	0.91642
	(0.78213)	(1.6908)
Partisan	0.20568***	0.12339
	(0.12411)	(0.15279)
IMR FC	4.1657*	2.047
	(1.1981)	(1.977)
IMR T	−0.7701*	1.4230
	(0.26566)	(1.4518)
IMR W	−21.617	−1.0549
	(33.099)	(6.0838)
Number of cases	2,120	1,139

*Significant at the .01 level.
**Significant at the .5 level.
***Significant at the .1 level.

Table 9. Robustness Tests

Coefficient on Partisan	With Restricted Nonelectoral Coefficients	With Circuit/ Regional Dummies
State trial award	0.24708**	0.43398*
	(0.10043)	(0.14127)
Federal trial award	0.13941	0.16609
	(0.10416)	(0.19766)

Note: OLS equations show corrected standard errors in parentheses (see text).
*Significant at the .01 level.
**Significant at the .05 level.

Most importantly, partisan has a coefficient of 0.20568 (statistically significant at the 10% level) in the state regression but not statistically significant coefficient of 0.12339 in the federal regression. Awards are thus larger in partisan elected states when state judges are deciding cases, but not when nonelected federal judges with life tenure are deciding cases. Moving an otherwise average case from a nonpartisan to a partisan state in the state courts raises the expected award by 23%, or $233,157, evaluated at the mean of the federal sample. Moving a case from a nonpartisan to a partisan state in the federal courts, however, does not systematically increase the award.

In Table 9 we perform two robustness tests. The diversity jurisdiction regressions have much smaller sample sizes than our earlier cross-state regression. Some of the regression coefficients in the diversity regressions are clearly not good estimates of the population parameters. The coefficient on medical malpractice in the state diversity regression (SA), for example, is 11, much larger than in the state regressions and far larger than is reasonable. The estimate is, of course, appropriate for the sample, but there are only 17 medical malpractice trials in the state diversity regression, and it so happens that these few trials resulted in large awards that are not representative of the population. We are almost entirely interested in the coefficient on partisan, however, so imprecision in the estimation of control variables is not necessarily disturbing. To improve efficiency, however, we performed the following analysis. We restricted the beta coefficients in the diversity-jurisdiction equation to have the same values as is in the earlier state regression, with the exception of the endogenous sample selection parameters, a constant, and partisan, which were left unrestricted. If the beta coefficients from the state regression—which are

well estimated because of the large sample size—are better estimates of the true betas than the unrestricted betas from the small-sample diversity-jurisdiction equation, then imposing these restrictions will improve the efficiency of estimation of the unrestricted parameters. Results on partisan from the restricted regression are also presented in the first column of Table 9. Partisan has a statistically significant (at the 5% level) coefficient of 0.24708 in the state regression and a statistically insignificant coefficient of 0.13941 in the federal regression. Thus, improving the efficiency of the estimates strengthens the conclusion that awards are larger in states with partisan elected judges when state judges make decisions, but not when federal judges make decisions using the same set of laws. The estimates from the unrestricted and restricted state diversity equations suggest that awards are 21% to 28% higher in partisan states with out-of-state defendants than in other states. Evaluated at the mean of the state diversity sample, awards are higher by between $233,157 and $286,169.

Observers of the judicial process have long argued that federal circuit courts differ in their interpretations of the law (for a review see Rowland and Carp, 1996). We add circuit dummies to the award and win equations to control for any systematic differences in awards across federal circuits. Since circuits often overlap with regions, we also include the same set of dummies in the state regression. When we control for circuit-regional effects we find that the coefficient on partisan in the federal regression is virtually unchanged; it remains small and statistically insignificant. The coefficient on partisan in the state regression, however, increases in size and statistical significance. The coefficient suggests that awards against out-of-state businesses may be as much as 54% larger in partisan than in nonpartisan states. Thus we continue to find that awards in cases with out-of-state defendants are larger in partisan elected states when state judges are deciding cases, but not when nonelected federal judges with life tenure are deciding cases.[20]

20. As was noted earlier, since tort trials in both state and federal courts are primarily before juries, our failure to find a partisan-electoral effect in the federal data suggests that judges, not juries, are responsible for the state effect.

6. Conclusions

Judges respond to incentives just like other politicians. Understanding judicial behavior, therefore, requires an understanding of incentive structures. In ten states, judges are elected on competitive partisan ballots. Partisan elected judges must cater to their constituents, and they must raise campaign funds in order to be elected. We hypothesized that these forces would increase awards in partisan elected states relative to other states, particulary awards against out-of-state businesses. The evidence, both from the cross-state regressions and from diversity of citizenship cases, strongly supports the partisan election hypothesis. In cases involving out-of-state defendants and in-state plaintiffs, the average award (conditional on winning) is $362,988 higher in partisan than in nonpartisan states; $230,092 of the larger award is due to a bias against out-of-state defendants, and the remainder is due to generally higher awards against businesses in partisan states.

Awards might be higher in partisan elected states because of differences in the law in those states or because of differences in the judicial incentive structure (of course these possibilities are not exclusive, for differences in the law could be caused by differences in the incentive structure.) To test these alternative possibilities we compared awards in cases decided by unelected, lifetime-tenured federal judges with awards in cases decided by state judges, when both apply state law. More precisely, we compared the difference in awards in partisan and nonpartisan states in cases decided by federal judges with the difference in awards in partisan and nonpartisan states when cases were decided by state judges. We found that awards were higher in partisan elected states only when the cases were decided by state judges. Our evidence suggests, therefore, that the primary reason awards are higher in partisan elected states is not differences in law across the states, but rather that partisan elected judges decide cases differently than judges selected in other ways.

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