The Effect of Electoral Institutions on Tort Awards

Eric Helland* and Alexander Tabarrok**[1]

Abstract

Politicians are not neutral maximizers of the public good, they respond to incentives just like other individuals. We apply the same reasoning to those politicians in robes called judges. We argue that elected judges, particularly partisan elected judges, have an incentive to redistribute wealth from out-of-state defendants (non-voters) to in-state plaintiffs (voters). The partisan electoral hypothesis is tested first using data on 75,000 tort awards from across the states. We control for differences in injuries, state incomes, poverty levels, selection effects and other factors that may cause awards to differ across the states. One difference which appears difficult to control for is that each state has its own body of tort 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 allow us to test whether differences in awards are caused by differences in electoral systems or differences in state law. The evidence from the cross-state regressions and from the 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 42% higher in partisan than in non-partisan states; approximately 2/3 rds of the larger award is due to a bias against out-of-state defendants and the remainder due to generally higher awards against businesses in partisan states.

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* Dept. of Economics, Claremont-Mckenna College, Email: eric_helland@MCKENNA.EDU

** Director of Research, The Independent Institute, Oakland, CA, 94621, Email: ATabarrok@Independent.org. The views expressed here are solely those of the authors and do not necessarily reflect the views of The Independent Institute.

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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 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, 10 use highly competitive partisan elections while in the remainder judges run on non-partisan ballots. A second division occurs between federal and state judges. Federal judges are appointed and have life tenure while, as noted above, 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 One of the paper we discuss the partisan election hypothesis. Section Two of the paper introduces our estimation procedure and presents our cross-sectional results. Section Three tests for the partisan election effect using data on Federal diversity of citizenship cases.

The Partisan Election Hypothesis

The dominant methods of judicial selection are partisan elections, non-partisan 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, non-partisan 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 non-elected states tend to be longer than terms in elected states, on average 21-27% longer for general and supreme courts respectively (Hanssen, 1999). Non-elected judges are also more secure than elected judges; they are returned to the bench - through reappointment or a retention election - more often than are elected judges.[2] Appointed judges are thus more insulated from direct political pressure than are elected judges and will tend, therefore, to be more independent (Hanssen (1999), Posner (1993, 41), and Dubois (1990)).

In a partisan election state judges run under a party banner, just as do other politicians. In a non-partisan 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 non-partisan states. Although judicial elections in non-partisan 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 and incumbents

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[2] 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 length 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 prefunctory we define states using initial appointment followed by retention elections as appointed states.

are defeated more regularly than in non-partisan elections (Dubois 1979, Glick 1983). Of elected states, 10 use partisan elections.[3]

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 etc. 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, Flango and Ducat (1979), Glick and Emmert (1987), Alozie (1990), 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.[4] 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, non-partisan elected states and non-elected 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 non-partisan elected states or non-elected states. Furthermore, we could not reject the hypothesis that awards were the same in non-partisan elected and non-elected states. We thus concentrate on the difference in awards between partisan elected and other selection systems (which we call non-partisan systems).

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[3] 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 non-elected elements (eg. NY has a mixed system).

[4] 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.

In this paper, 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). In these diversity of citizenship cases, *Federal judges decide disputes on the basis of state law.* 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 non-random 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 non-partisan states. [5] We also control for any differences in the winning disputes in partisan versus non-partisan states.

Why Might Selection Mechanisms Matter?

In this section we outline three 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; they may cause judges to seek campaign contributions from lawyers interested in larger awards; and elections may increase judicial diversity in a way in which increases the mean award. The theories are not necessarily mutually exclusive. Our goal in this paper 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 states or even other countries.[6] Plaintiffs, therefore, will tend to be more politically powerful than out-of-state defendants,

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[5] Tabarrok and Helland (1998) do not control for selection effects. The data set used in this paper is deeper as well as longer than that used in our earlier paper. In our earlier paper, control variables such as poverty rates were measured at the state level. In this paper, all of our control variables are case specific or measured at the level of the county in which the trial takes place.

[6] Clermont and Eisenberg (1996) examine whether the Federal courts are biased against foreign corporations.

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, 4)."

And, Neely continues, "it 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 (Neely 1988, 62)." Redistributing wealth from out-of-state defendants to in-state plaintiffs is a judge's way of providing constituency service.[7,8] Maloney, McCormick, and Tollison (1984) make the same argument with respect to regulators. They argue that a vote-maximizing regulator will transfer wealth from out-of-state to in-state consumers by raising prices whenever out-of-state consumers consume a large fraction of industry output. Applying the theory to electricity prices they find that an increase of electricity exports by 10% raises the price of electricity by 4.5%.

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

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[7] 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 judgement 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 judgements, in votes and campaign contributions according to Neely's hypothesis, 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.

[8] Other observers have also noted that elected judges are easier to influence than appointed judges. Herman Wrice the the founder of an anti-drug citizen's group in the Mantua section of Philadelphia notes that "In a city where judges are elected, a few members of Mantua Against Drugs assembed in the court room can add thousands of dollars to the price of bail." Quoted in Benson (1998, 124).

likely to bias the judiciary. The judiciary affects interest groups from across the political spectrum but the interest groups don't know which of the thousands of judges will rule in their particular case. (And once a judge 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 doesn't 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, 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.

A third explanation for why awards in cases with out-of-state defendants are larger in partisan elected states than in other states is that partisan elections bring more extreme judges to the bench. Extreme in this context means having a tendency to give either significantly higher or significantly lower awards than average. Since awards are bounded below by zero, however, a greater variance in award decisions implies a larger mean award. Like the campaign contribution theory, the variance theory implies that awards in general will be higher in partisan elected states. With the supplementary hypothesis that local defendants can discipline judges who grant large awards against local defendants, the theory also implies that awards against out-of-state defendants will be especially high. The variance theory differs from the plaintiff-voter and campaign contribution theories in that the increase in awards is not a direct consequence of partisan elections but a byproduct of higher judicial variance. As noted above, researchers have not found any significant differences in judicial characteristics across electoral systems but easily observable characteristics do not exhaust the many margins on which judges may differ. Moreover, mean judicial characteristics may be similar across systems yet partisan elections still cause large differences in awards if those differences are due to a handful of judges.

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, allowing or disallow objections, rule on motions and counter-motions, limit or not limit the lawyers to certain theories of liability and damages etc. 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 which elect their judges using 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 (see below) from judge trials is consistent with the jury evidence. Second, we control for the most obvious characteristic which 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 below, however, that when federal judges are presiding, awards against out-of-state defendants are not significantly higher in partisan elected states compared to non-partisan states.

Exploratory Data Analysis: Partisan vs Non-Partisan State Court Cases

The data on torts was extracted from Jury Verdict Research's Personal Injury Verdicts and Settlements on CD-ROM.[9] 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.[10] The data set spans all of the 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 either be 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 a majority of the firms in our sample. We assumed that any firm which we could not find in the database (e.g. Alex's Muffler Shop) was headquartered locally, i.e. 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 assume 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 you reside it is rare since inconvenient.[11]

In Table 2 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. 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 award in non-business cases. The coefficients on the other variables are the differences between cases of that type and the average non-business case.

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[9] 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 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.

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

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 non-partisan 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 between the differences (Partisan Out – NonPartisan Out) measures the total "partisan effect." Awards against out-of-state businesses are \$663,410 higher in partisan than in non-partisan states. The difference is statistically significant at the (far) greater than 1% level, ($F[1,52540]=16.31$ with $p=0.0001$). 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 similar cases in non-partisan 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 value \$393,690 ($F[1,52540]=4.84$ and $p=0.027$). The partisan business effect is measured by (Partisan In-Non Partisan In) and has a value of \$269,720 ($F[1, 52540] =15.7801$, $p= .0001$). Awards against businesses in general are larger in partisan than in non-partisan states but the majority of the partisan effect is due to a particular 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. Columns 2 and 3 of Table 2 present the median award and the award at the 75th percentile in all the case types. Median awards in cases with out-of-state defendants are \$37,365 larger in partisan elected states than in non-partisan states. The difference is statistically significant at the greater than 1% level. As the percentile increases, the difference in

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[11] 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.

awards between partisan and non-partisan states increases. At the 75th percentile awards against out-of-state defendants are $458,362 larger in partisan elected states than in other states.

Column 4 of Table 2 presents results looking only at 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, 1999). In particular, judges deal with the types of cases likely to generate low awards (eg. premises liability and auto cases) in much greater proportion than do 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 two hundreds thousand dollars higher than the mean award in non-partisan 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 non-partisan states). In the remainder of the paper 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).

Figure 1 presents a kernel density estimate of the distribution of log awards in cases with out-of-state defendants in partisan and non-partisan states.[12] The density function for partisan states contains less weight in the mid-region and more weight in the right hand tail than does the density for non-partisan awards. Using information from the density one can calculate, for example, that the percentage of cases with awards over one million dollars is twice as high in partisan as in non-partisan states (24% to 12%).[13]

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 which are merely correlated with differences in the electoral system. In the following section we refine the difference in differences analysis

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[12] We use a bi-weight kernel with smoothing parameter optimized on the assumption that the underlying data is normally distributed (see Silverman (1986) and Stine (1996) for more information on kernel estimation.) The use of other kernels and/or smoothing parameters does not materially affect the results.

[13] The kernel density estimate also suggests that there is a greater density of awards in the left hand tail. This provides some evidence for the "extreme" judges hypothesis advanced earlier (which suggested more awards of very high and very low amount in partisan elected states). The data are logged, however, so the awards in the left hand tail are *very much* smaller than awards in the right hand tail. In dollar terms, for example, the crossing point for the partisan and non-partisan densities occurs around $3000. These awards are too small to be of importance. All of the action is in the right hand tail.

to control for a variety of other potential influences. In particular, to properly account for selection effects we model the process which transforms a dispute into a trial into a winning case.

Estimation Procedure-Overview

Plaintiffs and defendants decide whether or not to settle a dispute based upon the costs of going to trial, the size of the expected award, and uncertainty about the expected award among other factors (see below). 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 re-estimate the model 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.[14]

In the last step of the second stage of estimation we regress the total award by case on a number of explanatory variables. A potential problem with this type of regression is that cases which reach trial are not a random sample of all cases and cases which win at trial are not a random sample of tried cases. 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 use Heckman's (1979) two step procedure. Results from the settlement and win probit are used to construct inverse Mill's ratios, $\lambda_i = \phi(X_i\beta_i)/\Phi(X_i\beta_i)$ - where *i=s,w,* ϕ , is the univariate normal p.d.f , Φ, is the univariate normal c.d.f., X_i are the independent variables in the settlement and win equations, and β_i are the coefficient estimates for the respective equations. The award at trial is then estimated by,

$$A = X_A\beta_A + \lambda_S\beta_{\lambda_s} + \lambda_W\beta_{\lambda_w} + \varepsilon_A$$

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[14] An extended discussion of an estimation procedure similar to ours can be found in the pioneering paper of Danzon and Lillard (1982).

where A is the award at trial, X_A are explanatory variables like injuries and case types, β_A is the vector of coefficient estimates, and ε_A is the error term.[15]

The coefficients may be 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 (see Greene (1997)). Corrected standard errors are presented in the regressions which follow.

Figure 2 presents an overview of our estimation procedure. Note that we allow each selection equations to have an error term correlated with subsequent award equations but for tractability we assume that the error terms in the selection equations are uncorellated. Although we present results on the settlement and win processes our focus in this paper is on the effect of partisan elections on trial awards.

The first and second stages of the model are the same except for the inclusion of the shadow variables in the second stage. We thus explain each step of the second stage along with our results.

The Decision to Settle

We model the decision to settle using a model based on Posner (1973, 1996), Gould (1973), Priest and Klein (1984), and others.[16] Assuming risk neutrality the plaintiff's (maximum) ask can be written $A=p_pJ-C_p+S_p$ where p_p is the plaintiff's estimate of the probability of a plaintiff win, J is the judgement to the plaintiff if the plaintiff wins, C_p are court costs and S_p are the cost of a settlement. Similarly, the defendant's maximum offer is $O=p_dJ+C_d+S_d$. A case goes to trial if the plaintiff's ask exceeds the defendant's offer, which with some rearranging can be expressed as (condition one)

$$(P_p-P_d)J>C-S$$
$$\text{Where } C=C_p+C_d \text{ and } S=S_p+S_d$$

On the left hand side of condition one is the 'amount in dispute.' If the plaintiff calculates his expected award (p_pJ) at $500,000 and the defendant calculates an expected award of $400,000 then $100,000 is the amount in dispute. On the left hand

[15] 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.
[16] Cooter and Rubinfeld (1989) review the literature.

side are the cost savings from settlement. If the amount in dispute exceeds settlement savings the case goes to trial otherwise it settles. Holding all else equal, cases are more likely to go to trial the larger is J since an increase in J magnifies the effect of any difference in p_p-p_d.[17] The probability of a trial also increases with reduced settlement savings which may be due either to decreases in the costs of trial or increases in settlement costs.

Asymmetry in judgements can be handled by modifying condition one slightly to:

$$P_pJ_p-P_dJ_d>C-S$$

If the defendant stands to lose more than the plaintiff gains from a trial then $J_p<J_d$. An increase in J_d reduces the amount in dispute and thus the probability of a trial, an increase in J_p has the opposite effect. Although not included in the formal model it is easy to see that if plaintiffs and defendants are risk averse an increase in risk will cause plaintiffs to lower their Asks and defendants to raise their Offers. An increase in risk, therefore, lowers the amount in dispute and increases the likelihood of a settlement (Cooter and Rubinfeld (1989)).

Priest and Klein (1984) formally model the formation of defendant and plaintiff expectations of a plaintiff win. They assume that disputes are drawn from a distribution along which is measured the defendant's culpability or, from the plaintiff's point of view, the case quality. It is usually assumed that the shape of the dispute distribution is Normal although similar results follow from any uni-modal distribution. The decision standard is a cut point, D such that the plaintiff wins every case with quality $Y\geq D$ and loses every case with quality $Y<D$. Defendants and plaintiffs estimate case quality with error which generates differing opinions as to the probability of a plaintiff win. Let Y' be the true case quality then the plaintiff's estimate of case quality is $Y_p=Y'+e_p$ and the defendant's estimate is $Y_d=Y'+e_d$. Priest and Klein assume that the error terms are normally distributed with mean zero and standard deviations σ. The plaintiff's estimate of a win is therefore $p_p=\Pr[Y'\geq D]=\Pr[Y_p-e_p\geq D]=\Pr[e_p\leq Y_p-D]=F((Y_p-D)/\sigma)$ and the defendant's estimate is $p_d=F((Y_d-D)/\sigma)$ where F is the standard Normal CDF. The key to the Priest-Klein model is that $p_p-p_d= F((Y_p-D)/\sigma)- F((Y_d-D)/\sigma)$ will vary according to the true case quality Y', the decision standard D, and the error variance σ. It can be shown,

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[17] The settlement condition may also be written $(P_p-P_d)>(C-S)/J$ which emphasizes that court and settlement costs may be a function of J, the conditional award. If (C-S) increases less than proportionately with an increase in J the probability of a trial increases, if (C-S) increases more than proportionately the probability of a trial decreases. The assumption in the literature has typically been that lawyer contingency fees make (C-S)/J roughly constant.

for example, that p_p-p_d is maximized when Y_p and Y_d are equally spaced around the decision standard. The intuition is that disagreement about case quality generates large disagreements about the probability of a plaintiff win only when the true case quality is near the decision standard.

The settlement model suggests, therefore, that the settlement decision will be a function of the variance of plaintiff and defendant 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 variance of the shadow probability, p(1-p). The shadow award proxies for the expected judgement 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 the shadow probability.

Court costs are proxied by the expected length of time each case takes to go to trial. Time to trial proxies for court costs in two senses. First, court costs increase directly with time to trial because lawyer fees and opportunity costs to the parties increase the longer a case remains active. Second, and more importantly, *the primary causes of longer times to trials are endogenous.* The more difficult and important the case, the longer the time will be spent in discovery and in pre-trial motions and counter-motions. Cases with longer expected times to trial thus tend to be difficult and costly cases. We modeled the duration of time to trial using a sample of 41,986 cases for which we have data on time to trial. Included within our model are injury variables (death, major, minor etc.), case types (product liability, medical malpractice etc.), the number 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.[18] We expect that the longer the expected time to trial the greater are expected court costs and thus the greater is 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

[18] We estimated the following duration model based on the logistic hazard function, time=6.8653+.15304*death+.13334*major+.041541*minor-.10671*emot+.25343*prod+.25639*med+.019945*prem-.19037*auto+.092918*ndef-.033791*file.

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, it may even cause a loss of operating rights. We include product liability and medical malpractice dummies to account for these effects. Business is also included to account for possible stake asymmetry or for any differences in bargaining behavior between businesses and individuals.

In addition to the factors suggested directly by the model, we include several other variables. Non-pecuniary elements may enter into a plaintiff's bargaining efforts if a death, particularly a child's death is involved in the dispute in question. Defendants may also be more likely to settle these types of cases if a trial would generate negative publicity. To control for possible non-pecuniary elements in bargaining we include two variables, a dummy variable labeled Child (set equal to one if a child died) and LLife. LLife is the expected number of years of life left in cases involving an adult death. LLife was computed using actuarial tables and the victim's age.[19]

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 a trial decreases (i.e. the prob. of settlement increases). We include a variable called Weak Joint and Several which is equal to one in cases with multiple defendants in states which have weakened the joint and several rule (for example, a defendant responsible for less than 50% of the injury may not be assessed more than his relative contribution).[20] In a regression with trial as the dependent variable, Weak Joint and Several should be positively signed if defendants' probabilities of success are highly correlated and negatively signed otherwise.

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[19] In computing the expected number of years of life left we controlled only for age at the time of death and not for race, sex or income for which we did not have data.

[20] The web page of the American Tort Reform Association contains information on recent tort reform in the states.

Our sample of cases under-represents settlements and over-represents trials as compared to population proportions. To rebalance our sample the settlement equation is estimated using the weighted exogenous sample maximum likelihood estimator (WESML) due to Manski and Lerman (1977). In our application, the WESML is essentially a weighted Probit model where the weights are equal to population proportions divided by sample proportions.[21] 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.[22]

It should be noted that our purpose in running a trial/settlement equation is to generate an inverse Mill's ratio which will later be used to create consistent estimates of the partisan election effect (see further below). We are less interested in testing any particular model of settlement. Our discussion of results from the trial equation will therefore be brief.

Results from the Trial Equation

Table 5 column 1 shows the results from the trial equation. A positive coefficient indicates a greater probability of going to trial. Column 2 of Table 5 shows the marginal effects (descriptive statistics can be found in Table 3). The non-dummy dependent variables are in natural logs (for variables for which 0 is a possible observation we added one before logging). Most of the variables are statistically significant at the far greater than 1% level. The exceptions are LLife and Number of Defendants neither of which are statistically significant at conventional levels and Expected Award which is significant at just under the 2% level. The sign of VarP, the variance of the probability of winning, is positive. Thus, the more uncertain the trial outcome (win/lose) the greater the probability of going to trial, as the Priest-Klein model predicts. The positive sign on the expected award coefficient is also consistent with Priest-Klein which implies that, holding all else equal, larger cases are more likely to go to trial than smaller cases. All is not equal, however, as larger cases also increase risk. Risk reduces the amount in dispute and induces plaintiffs and defendants to settle rather than go to trial. The risk

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[21] The WESML is applied in a problem similar to ours by Boyes, Hoffman, and Low (1989). Our results are robust with respect to varying the weights in the WESML estimator.

[22] 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.

effect dominates the expected award effect creating a small positive net effect. Figure 3 shows the probability of settling for any probability of winning for expected awards of size $10,000, $100,000, and $10,000,000. The probability of going to trial is maximized at a win probability of .5 because plaintiff and defendant expectations about the outcome are likely to differ most in close cases (a win probability of .5 maximizes the variance of the win/loss outcome). Figure 3 indicates that on average 8.12% of cases with expected awards of $10,000 go to trial and 5.67% of $100,000 cases go to trial. An increase in the expected award of 10 times, therefore, increases the probability of settling by only 2.45%. Figure 3 also shows that 2.41% of cases with expected awards of $10,000,000 reach trial. In particularly close cases, up to 31% of $10,000, 22% of $100,000, and 10% of $10,000,000 cases may fail to settle.

As predicted by the model, higher court costs (measured by time to trial) increase the probability of a settlement. The mean time to trial is 743 days with a standard deviation of 91 days. A one standard deviation increase in the mean time to trial increases the probability of settling by 2.8%.

According to the settlement model higher defendant stakes in product liability and medical malpractice cases should reduce the probability of going to trial. We find, contrarily, that the probability of going to trial is 13% higher in product liability cases and 43% higher in medical malpractice cases than in otherwise similar case.[23] A possible explanation is that the defendant also faces higher costs of settling in medical malpractice and product liability cases. If a settlement in a product liability case, for example, generates as many further lawsuits as a loss at trial then defendants may have very little to gain from settlement (C-S falls reducing the incentive to settle). Or suppose that a settlement in a medical malpractice case exposes the doctor to loss of hospital privileges. If trial costs and settlement costs both rise in product liability and medical malpractice cases we would expect fewer of these cases to go to trial. Trials are also slightly more likely (1.1%) in cases involving businesses.

Trials are 11% less likely when the case involves the death of a child, suggesting that defendants may be more eager to settle in these cases than in otherwise similar cases. Cases involving multiple defendants and weakened joint and several rules are 2.2% more likely to go to trial (less likely to settle) than otherwise similar cases.

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[23] For dummy variables (d) we calculate the exact difference between the probability of trial when d=0 and when d=1 when all other variables are at their means.

Following Kornhauser and Revesz's (1994) argument this suggests that the probability of success against multiple defendants is highly correlated.

The Win Equation

The probability of winning at trial is estimated using a Probit model. To account for different decision standards we include dummy variables for case types, i.e. medical malpractice, product liability, auto, and premises liability. LLife, 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. The variable products defense is a dummy variable set equal to 1 if a state has created some new defenses to product liability suits *and* if the case is a product liability case. A typical products defense might create a presumption of safety when the product complied with Federal and State safety regulations (FDA, FAA etc.). A similar reform creates a defense when the product complied with generally recognized "state of the art" safety design. We expect that a products defense will reduce the probability of a plaintiff win.[24]

Results are found in columns 3 and 4 of Table 5 (descriptive statistics are in Table 3). Perhaps the most interesting result is that plaintiffs are 22% less likely to win medical malpractice cases and 10% less likely to win product liability cases than otherwise similar cases. We found earlier that medical malpractice and product liability cases are less likely to settle than other cases. Together these findings suggest that the decision standard is higher for these types of cases than other types. Automobile cases are about 10% more likely to result in a plaintiff win and premises liability cases are about 10% less likely to result in a plaintiff win, holding all else equal. The ability to use a products defense lowers the probability of a plaintiff win by approximately 4%. A case involving a death with 40 years of expected life remaining is about 1.8% less likely to win than a non-death case. The LLife effect is small which is what we should expect given that LLife was not significant in the settle equation.

The Award at Trial

Our data set has descriptive information on the victim's injury. We code this information into 9 exclusive and exhaustive variables: LLIFE, Major Injury, Minor Injury,

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[24] The American Tort Reform Association has collected data on tort reform legislation by state since 1986. Further details on reform measured by state can be found on the ATRA home page, http://www.aabiz.com/ATRA.

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. As in earlier regressions we include case type variables.

We also include 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. Non-Economic Cap is set equal to one if state law puts a cap on damages due to pain and suffering or other non-economic 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 a 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 only occurs 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.[25] 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 eg. 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 while Non-

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

Economic 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 that indicates that jury awards are higher the higher is the local poverty rate (Tabarrok and Helland, 1999). 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 explantory 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. Also, as described above, to observe an award the case must have failed to settle and the plaintiff must have won the case. To account for the selection effect we estimate the award at trial using inverse Mill's ratios generated from the settlement and win equations.

Results from the Award Equation

Our discussion of the results 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 approx. 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*Ln(40)}$-1). A major injury increases awards by 179% ($e^{1.0286}$-1) and a minor injury decreases awards by 50% ($e^{-.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 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=0.00001) 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, which 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 concern 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 non-partisan states ($e^{.70742-.35693}$-1). Put differently, moving an otherwise average case with an out-of-state defendant from a non-partisan to a partisan state raises the expected award by $362,988. The partisan effect is statistically significant at the greater than 1% level.[26] 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 Non Partisan Out and on Non Partisan In are almost identical which suggests that there is little or no penalty against out-of-state businesses in non-partisan states. In contrast, the coefficient on Partisan Out is larger than that on Partisan In and

[26] The F test for the restriction Partisan Out=Non Partisan Out is F[1,29209]=10.5046 with p=0.0014.

both coefficients are larger than their non-partisan 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.[27]

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)-(Non Partisan Out – Non Partisan 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.[28]

In Table 6 we tests the robustness of the partisan electoral effect. In column one 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 than in non-partisan states ($e^{.6794499-.409429}-1$). The difference is statically significant at the just over 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 non-partisan states ($e^{.70156-.43537}-1$). The difference is statistically significant at the just over 1% level (F[1, 29167] = 5.4190, p = 0.0189).[29]

The reference case in our earlier regressions was a non-business 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 non-business cases in our regressions because we are interested in the coefficients of some non-electoral variables like poverty and because the inclusion of

[27] We cannot reject the hypothesis that Non Partisan Out = Non Partisan In, F[1,29209]=0.0191 with p=.8605. The restriction Partisan Out=Partisan In has F[1,29209]=4.2963 with p=0.0360.

[28] The restriction (Partisan Out – Partisan In) – (Non Partisan Out – Non Partisan In) has F[1,29209]=3.1432 with p=0.0725. The restriction Partisan In = Non Partisan In has F[1,29209]=10.9615 with p=0.011.

non-business cases improves the estimates of the non-electoral variables. Better estimates of the non-electoral variables in turn allows for better estimation of the electoral variables. In column three we estimate the model 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 non-partisan states ($e^{.429695-.020112}-1=.50$). The difference is statistically significant at the greater than 1% level ($F[1,9218]=12.44$, p value=.000419).

Our forth 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 non-partisan states ($e^{.9075-.1928}-1=1.04$). The difference is statistically significant at the greater than 5% level ($F[1,2914]=5.80$, p value=.0160).

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 who feared they would be disadvantaged in pro-plaintiff/pro-debtor state courts (Friendly, 1928). Today lawyers continue to cite out-of-state and anti-business bias as one reason for removing cases to Federal court (Miller, 1992). For over a century, Federal judges decided diversity of citizenship cases based on Federal common law. The Supreme Court, however, overturned this rule in the 1938 case *Erie Railroad v. Tompkins*. Since 1938 diversity cases have been decided on the basis of state law.[30]

Even when Federal judges apply state law, comparing Federal and state cases is problematic because of multiple sample selection problems. Cases which 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, in order to bring a diversity case to Federal court, the plaintiff must claim 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

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[29] 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.
[30] The definitive source for diversity of citizenship law is Wright (1994). Posner (1996) and Lieberman (1992) give short overviews.

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 non-partisan states, this gives us the Federal Difference $(\text{Partisan} - \text{Non Partisan})_{\text{Fed}}$. Using a similar sample of cases (cases involving out of-state businesses) we create the State difference $(\text{Partisan-Non Partisan})_{\text{State}}$. If all of the 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, i.e. $(\text{Partisan} - \text{Non Partisan})_{\text{Fed}}$.- $(\text{Partisan-Non Partisan})_{\text{State}} = 0$. 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 above, 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 1 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 which go to Federal court and a settle, win, and award equation is estimated for cases which 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. Figure 4 summarizes our estimation procedure.

As before, functions of the expected award and the expected probability of winning will be key explanatory variables in the settlement equation, thus the model is estimated in two stages.

The Forum Choice Equation

What determines whether a case is taken to Federal or state court? Initially, it is the plaintiff who decides the forum in which to sue, but if the plaintiff files in state court and diversity exists the defendant may *remove* the case to the Federal courts.[31,32] Unfortunately, plaintiff and defendants have opposing interests and we do not have data on whether a case was initially filed in Federal court or removed to Federal court. Nevertheless, diversity law and economic theory suggest some variables which could be significant predictors of forum choice.

In cases involving multiple defendants (or plaintiffs) diversity must be "complete" to exist. The plaintiff may sue N defendants, for example, and so long as *one* of the defendants resides in the same state as the plaintiff diversity does not exist.[33] Thus, the plaintiff in a product liability case may be able to avoid removal to the Federal courts by suing a local distributor in addition to the manufacturer of the product.[34] Furthermore, every defendant must join in a notice of removal; if even one defendant prefers to litigate in state court removal will not occur. We expect, therefore, that the greater the number of defendants the lower the probability of going to trial in Federal court.

We expect that both the defendant and the plaintiff will prefer a shorter time to trial. It is sometimes asserted that defendants prefer longer times to trial in order to delay costly judgements. The assertion makes little sense if judges and juries increase

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[31] To remove a case to Federal court the defendant must file a notice of removal within 30 days of receiving the plaintiff's complaint (Wright 1994, 241).

[32] The defendant's right to remove is statutory but well accepted. It is possible for a case to be remanded to state court but only if the case was *improperly* removed.

[33] For purposes of diversity, a corporation is said to reside in the state(s) in which it is incorporated *and* the state in which it has its principal place of business (if these are different states).

[34] Defendants, of course, may argue that the plaintiff has sued some defendants fraudulently. A fraudulent joinder will not defeat diversity. Pete Rose, for example, could not avoid the federal courts by suing the Cincinnati Reds in addition to Bart Giamatti, the Commissioner of Baseball (see Rose v. Giamatti, D.C. Ohio 1989, 721 F. Supp 906). Giamatti was thus able to remove the case to Federal court where he thought local bias in favor of Rose would be less prevalent.

Similar devices may be employed on the plaintiff's side. In a class action suit, for example, citizenship is determined by the named representatives of the class so careful choice of representatives may create or defeat diversity. Wright (1994) discusses many other ways to game the system.

judgements by foregone interest (the argument also ignores the higher costs of longer times to trial). Miller (1992) in fact finds that both defendants and plaintiffs cite faster court processes as positive reasons for choosing one forum over another, the only exceptions being solo practitioners who must juggle several cases at a single time. In the analysis above we used a duration model to create for each state case an expected time to trial. We run the same model on the Federal data to create an expected time to trial in the Federal courts. Using the state and Federal models we create for each case an expected time to trial if the case goes to Federal court and similarly if the case goes to state court. Subtracting the expected Federal from the expected state time gives us the Time Difference. We expect that the larger is Time Difference (i.e. the greater the time-advantage of the Federal court) the more likely it will be that a case will go to Federal court.

In addition to the number of defendants and the difference in the time to trial we include in the forum choice probit-regression the case type variables, product liability, medical malpractice, auto, and premises liability. Results are presented in Table 8. All of the explanatory variables are statistically significant at the greater than 1% level. As expected, the greater the number of defendants, the lower the probability of going to Federal court. An extra defendant lowers the probability of going to Federal court by 8% (evaluated at the mean). Also, the longer the state time to trial relative to the Federal time to trial the more likely a case is to go to Federal court. On average, the same case is dispensed with 18% faster in Federal court than in state court (the mean case in this sample takes 684 days to come to trial in Federal court and 839 days to come to trial in state court). If expected times to trial were the same in Federal and state courts the percentage of cases in our sample going to Federal court would fall from 36% to 16%. Speedier dockets are thus one of the most significant advantages to the Federal courts.

We are primarily interested in the award equations and in particular we wish to compare $(\text{Partisan-Non Partisan})_{State}$ and $(\text{Partisan-Non Partisan})_{Fed}$.[35] We define Partisan as a dummy variable equal to one if the case in question took place in a state with partisan elected judges.[36] $(\text{Partisan-Non Partisan})_{State}$ is thus equal to the

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[35] Results from the Federal and state settlement and win equations are available from the authors upon request.

[36] We use the location of the Federal court to deduce the state law which the court is using to decide the case. It is possible that a case adjudicated in a Federal court in state A is decided based upon 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

coefficient on Partisan in the state regression and $(\text{Partisan-Non Partisan})_{Fed}$ is equal to the coefficient on Partisan in the Federal regression. Results on the award regression are presented in Table 8. Most importantly, Partisan has a coefficient of .20568 (statistically significant at the 10% level) in the state regression but a non statistically-significant coefficient of .12339 in the Federal regression. Awards are thus larger in partisan elected states when state judges are deciding cases but not when non-elected Federal judges with life tenure are deciding cases. Moving an otherwise average case from a non-partisan 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 non-partisan 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 which 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 column 1 of Table 9. Partisan has a

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the injury occurred is the law to be applied. Since the vast majority of 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", statistically significant (at the 5% level) coefficient of .24708 in the state regression and a statistically insignificant coefficient of .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 non-partisan 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 non-elected Federal judges with life tenure are deciding cases.[37]

Conclusions

Judges respond to incentives just like other politicians. Understanding judicial behavior, therefore, requires an understanding of incentive structures. In 10 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 get elected. The variance of judicial award decisions may also be larger in partisan elected states (causing an increase in mean awards since awards are bounded below by zero). We

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however, is the place of the injury (and also the residence of the parties to the dispute). For more details see Posner (1998) and the Restatement (Second) of Conflict of Laws.

[37] 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.

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 non-partisan states; $230,092 of the larger award is due to a bias against out-of-state defendants and the remainder 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, 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 non-partisan states in cases decided by Federal judges with the difference in awards in partisan and non-partisan 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 suggest, 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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Table 1: Expected Awards and Win Rates by Trial Category			
	Expected Total Award	Win Rate	Trials
All	$332,285	0.556	52,551
Product Liability	$1,457,984	.427	2134
Medical Malpractice	$598,096	.328	6147
Auto	$159,734	.656	24,856
Premises Liability	$162,975	.45	7916

Table 2: Difference in Means, Medians, 75th Percentiles, Judge Only

Variable	Total Award (all awards)	Medians (pos. awards)	75th Percentile (pos awards)	Judge Cases Only (pos. awards)
Constant (Non-Business Cases)	$252,540*** (19524)	$34,000 [20,033]	$164,194 [20,033]	$204,838*** (25,914)
Partisan Out	$936,190*** (143,800)	$116,942 [320]	$800,000 [320]	$319,908* (178,696)
Partisan In	$408,450*** (60,090)	$102,505 [2256]	$526,668 [2256]	$16,801 (81,869)
Non Partisan Out	$272,780*** (84070)	$79,577 [1261]	$341,638 [1261]	$115,143 (132,549)
Non Partisan In	$138,730*** (43,003)	$88,572 [5423]	$367,708 [5423]	$15,529 (70,453)
Number of Cases	53,545	~~53,545~~ 39,393	~~53,545~~ 39,393	3712
		Differences in Differences		
Partisan Out-Non Partisan Out	936,190-272,780= 527,740*** p=0.0001	116,942-79,577= 37,365*** p=.0084nb	800,000-526,668= 458,362*** p=0.0000nb	319,908-115,143= 204,765 p=0.5

*** Significant at the .01 level

Note: In the total awards regression awards are expressed as differences from the average non-business award (constant). In the median and 75th percentile columns the awards are expressed as exact values, not as differences from non-business cases.

OLS standard errors are in parentheses in the total award column. Number of observations in each category are in square parentheses in the other columns.

nb: The p-values in the median and 75th percentile columns were calculated using the bootstrap method.

Table 3: Descriptive Statistics: Difference in Means, Trial, and Win Regressions

Difference in Mean Variables	Means (st. dev)	Trial Probit Variables	Means (st. dev.)	Win Probit Variables	Means (st. dev.)
Total Award	333,292	Trial	0.7007	Win	0.5564
Partisan Out	0.0129	Expected Years of Life Left	0.2728 (0.9840)	Expected Years of Life Left	0.2519 (0.9437)
Partisan In	0.0812	Product Liability	0.0363	Product Liability	0.0406
Non-Partisan Out	0.0392	Medical Malpractice	0.1009	Medical Malpractice	0.1170
Non-Partisan In	0.1788	Weak Joint and Several Liability	0.2394	Auto	0.4730
		Number of Defendants	0.2548 (.4232)	Premises Liability	0.1506
		Business	0.3017	Products Defense	0.0210
		Child	0.4663		
		Expected Time to Trial	6.6331 (.1815)		
		Expected Award	10.3102 (1.1415)		
		Variance of Expected Award	20.3461 (2.5949)		
		Var P	0.2330 (.0110)		

Table 4: Descriptive Statistics: State Award Regression

Variable	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
Non-Economic 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
Non-Partisan Out	0.0431
Non-Partisan In	.1853

Table 5: State Regression Results

	Equation				
Variable	Trial Probit (T)	Trial Marginal Effects (TME)	Win Probit (W)	Win Marginal Effects (WME)	Trial Award (A)
Constant	0.31574*** (.97010)		0.14470*** (-.012904)		13.128*** (1.3084)
Expected Years of Life Left	-0.00012575 (.0089267)	-0.000019889	-0.012786** (.0060506)	0.0050476**	0.45591*** (.02967)
Major Injury					1.0286*** (.091558)
Minor Injury					-0.69608*** (.085231)
Emotional Distress					-1.0132*** (0.095743)
Rape					1.5684*** (.27086)
Sexual Assault					1.2839*** (.16177)
Sexual Harassment					-0.26084 (.39584)
Bad Faith					-.18688 (.13875)
Product Liability	0.59391*** (.05719)	0.0939***	-0.27143*** (.040893)	-0.10715***	0.71113* (.38531)
Medical Malpractice	1.5733** (.10663)	.24883***	-0.57981*** (.02054)	-0.22890***	0.73897 (.71503)
Auto			0.26151*** (0.014301)	.103124**	-0.66701** (.28065)
Premises Liability			-0.26216*** (.018359)	-.10350***	-.16022 (.30666)
Weak Joint and Several Liability	0.13626*** (.028316)	0.021551***			0.006289 (0.054663)
Products Defense			-0.098835* (0.054631)	-0.039018**	
Non-Economic Cap					-0.38883*** (0.04349)
Collateral Sources					0.36266*** (.022871)
No Punitive					0.14863 (0.14293)
Punitive Cap					-0.32897*** (.022675)
Evidence Standard					0.24964*** (.02590)
Bifurcated Trial					-0.071488 (.48386)
Number of Defendants	-0.035932 (.032369)	-0.005683***			
Business	0.047787*** (.016883)	0.0075580***			
Child	-0.65011*** (.017198)	-0.10282***			

Table 5: Regression Results (Continued)

	Equation				
	Trial Probit	Trial Marginal Effects	Win Probit	Win Marginal Effects	Trial Award
Variable	(T)	(TME)	(W)	(WME)	(A)
Poverty					0.94045*** (.19452)
Expected Time to Trial	-1.1199*** (.17029)	-.17712***			
Expected Award	0.27350** (.11467)	0.04325**			
Variance of Expected Award (Risk)	-0.19021*** (.05962)	-0.03008***			
Var P	30.199*** (1.9401)	4.7763***			
Partisan Out					0.70742*** (.10477)
Partisan In					0.47967*** (.045607)
Non-Partisan Out					.35693*** (.05665)
Non-Partisan In					.35481*** (.0338)
IMR Settle					-1.2063*** (.053352)
IMR Win					-0.082936 (1.8428)
Number of Cases	75,000	75,000	52,551	52,551	29,238

* Significant at the greater than .1 level
** Significant at the greater than .05 level
*** Significant at the greater than .01 level
Note: OLS equations show corrected standard errors in parentheses (see text). Marginal effects are computed at the means.

Table 6: Robustness Tests

Variable	No Selection Effects	State Fixed Effects[b]	Business Cases Only	Prod/Med Only
Partisan Out	.679449***	.70156***	.429695***	.9075***
	(.104679)	(.10250)	(.108813)	(.2800)
Partisan In	.464236***	.33804***	.210822***	-.1203
	(.04318)	(.045359)	(.051442)	(.1283)
Non Partisan Out	.409429***	.43537***	02011229	.1928
	(.0549311)	(.053965)	(.0587857)	(.1572)
Non Partisan In[a]	.412717***	.43185***	.	.1576
	(.030204)	(.030457)		(.1015)
Number of Cases	29,238	29,238	9245	2929
Differences in Differences				
Partisan Out-Non Partisan Out	$(e^{.679-.409}-1)=.309$*** p=0.0191	$(e^{.701-.435}-1)=.304$** p=0.0189	$(e^{.429-.02}-1)=.50$*** p=.000419	$(e^{.907-.192}-1)=1.0$** p=.0160

* Significant at the greater than .1 level
** Significant at the greater than .05 level
*** Significant at the greater than .01 level
OLS corrected standard errors in parentheses.
a) Non Partisan In was suppressed in the business cases only regression to prevent perfect collinearity with the intercept.
b) OLS standard errors were used in this regression (see text).

Table 7: Descriptive Statistics: Diversity Jurisdiction Regressions

Forum Choice Variables	Means (st.dev)	State Award Variables	Means (st.dev)	Federal Award Variables	Means (st.dev)
Fed	0.372	Total Award	11.467 (2.1455)	Total Award	12.18337 (1.7898)
Product Liability	0.2139	Expected Years of Life Left	.1862 (.8074)	Expected Years of Life Left	.2769 (.9545)
Medical Malpractice	0.0142	Major Injury	0.1439	Major Injury	0.1247
Auto	0.2442	Minor Injury	0.6868	Minor Injury	0.5435
Premises Liability	0.2018	Emotional Distress	0.0458	Emotional Distress	0.0781
Number of Defendants	0.3101 (.4589)	Bad Faith	0.0458	Bad Faith	0.0290
Time to Trial Difference	.2012 (.1043)	Product Liability	0.1632	Product Liability	0.1712
		Medical Malpractice	0.0080	Medical Malpractice	0.0105
		Auto	0.3434	Auto	0.1677
		Premises Liability	0.2071	Premises Liability	0.1844
		Weak Joint and Several Liability	0.3137	Weak Joint and Several Liability	0.2098
		Non-Economic Cap	0.1830	Non-Economic Cap	0.2704
		Collateral Sources	0.4505	Collateral Sources	0.3363
		No Punitive	0.0033	No Punitive	0.0114
		Punitive Cap	0.2429	Punitive Cap	0.4012
		Evidence Standard	0.2429	Evidence Standard	0.2098
		Poverty	0.1340 (0.0552)	Poverty	0.1296 (0.0327)
		Partisan	0.2165	Partisan	0.3459

Table 8: Diversity Jurisdiction Regressions

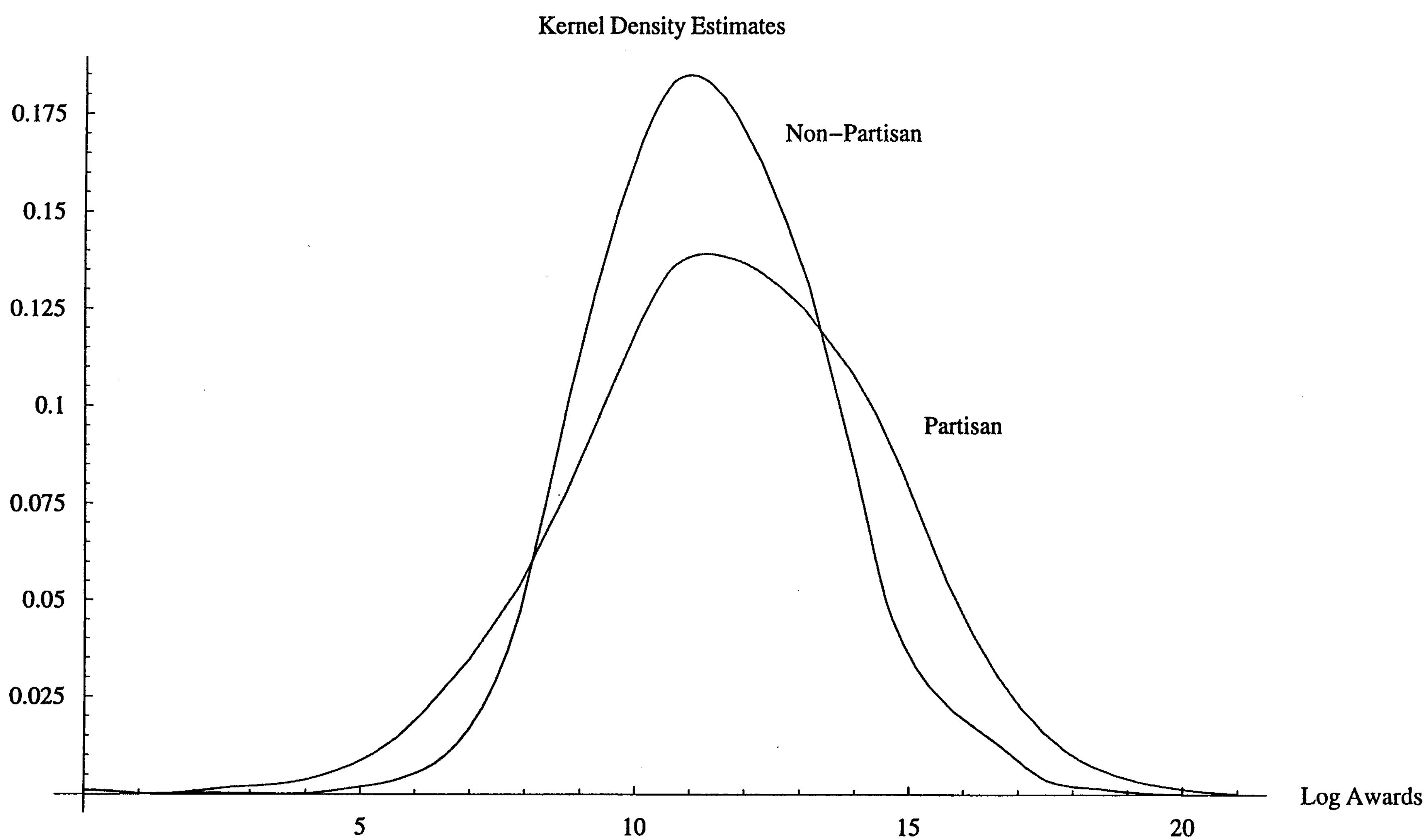
Variable	Forum Choice Probit (FC)	Forum Choice Marginal Effects (FCME)	Equation State Trial Award (SA)	Federal Trial Award (FA)
Constant	-0.15496***		23.468	9.7548*
	(.03895)		(22.065)	(5.2284)
Expected Years of Life			0.73887	0.35046**
Left (LLife)			(.59406)	(0.17548)
Major Injury			0.77818***	0.69981**
			(.294)	(0.27889)
Minor Injury			-0.27835*	0.052187
			(.27671)	(0.33046)
Emotional Distress			-.056708	-.83598
			(.47335)	(.96127)
Bad Faith			-0.51513	0.082754
			(.31419)	(0.32952)
Product Liability	-.79753***	-0.2896	7.9794	.031906
	(.061543)		(9.9903)	(1.1737)
Medical Malpractice	-1.1243***	-0.42143	11.105	.29501
	(.15369)		(14.793)	(1.83)
Auto	-1.0397***	-0.38973	-3.9189	-2.5172
	(.047899)		(7.5580)	(2.1438)
Premises Liability	-1.2467***	-0.46734	-0.59280	-1.6337**
	(.091299)		(.38107)	(0.67213)
Weak Joint and Several			0.0949	0.23572
Liability			(.29431)	(.21536)
Non-Economic Cap			-0.18340*	0.084092
			(.11036)	(.16731)
Collateral Sources			.38053***	0.085827
			(.074358)	(.13338)
No Punitive			.11385	.31302
			(2.4729)	(.56295)
Punitive Cap			-0.26556***	-.21347
			(.10113)	(..23972)
Evidence Standard			0.42125***	.14723
			(.10398)	(.18421)
Number of Defendants	-0.43278***	-0.16223	.96580***	
	(.038263)		(.18248)	
Time to Trial Difference	3.1230***	1.1707		
	(.32788)			
Poverty			1.4331*	0.91642
			(.78213)	(1.6908)
Partisan			0.20568*	.12339
			(.12411)	(.15279)
IMR FC			4.1657***	2.047
			(1.1981)	(1.977)
IMR T			-0.7701***	1.4230
			(.26566)	(1.4518)
IMR W			-21.617	-1.0549
			(33.099)	(6.0838)
Number of Cases	7690	7690	2120	1139

* Significant at the .1 level
** Significant at the .05 level
*** Significant at the .01 level
Note: OLS equations show corrected standard errors in parentheses (see text)

Table 9: Robustness Tests

	With Restricted non-Electoral Coefficients	With Circuit/Regional Dummies
Coefficient on Partisan		
State Trial Award	.24708**	0.43398***
	(.10043)	(.14127)
Federal Trial Award	.13941	.16609
	(.10416)	(.19766)

* Significant at the .1 level
** Significant at the .05 level
*** Significant at the .01 level
Note: OLS equations show corrected standard errors in parentheses (see text)


Kernel Density Estimates
Non–Partisan
Partisan
0.175
0.15
0.125
0.1
0.075
0.05
0.025
5
10
15
20
Log Awards

Estimation Procedure
First and Second Stage

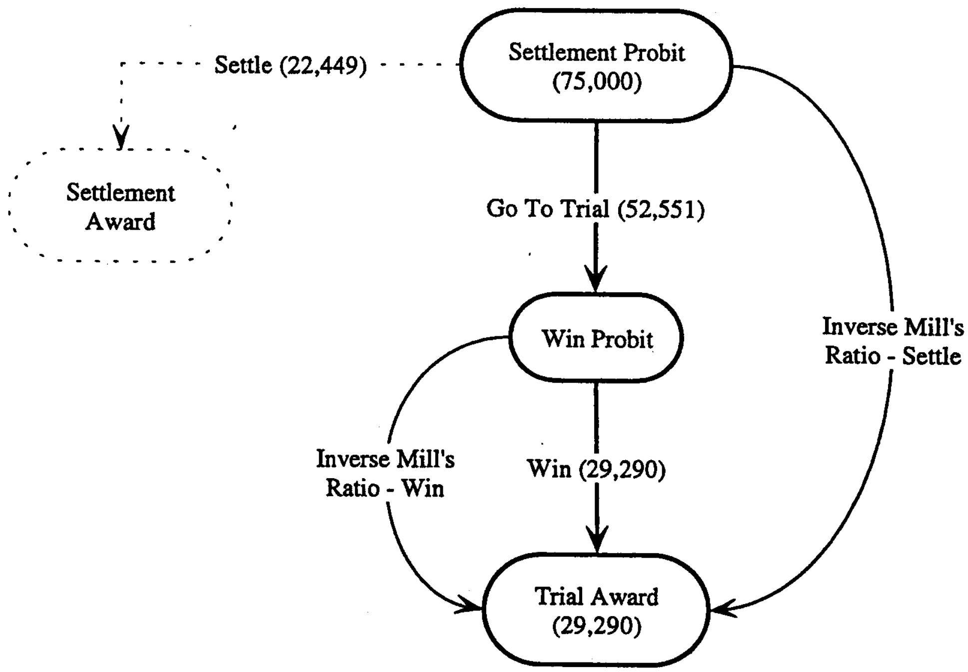
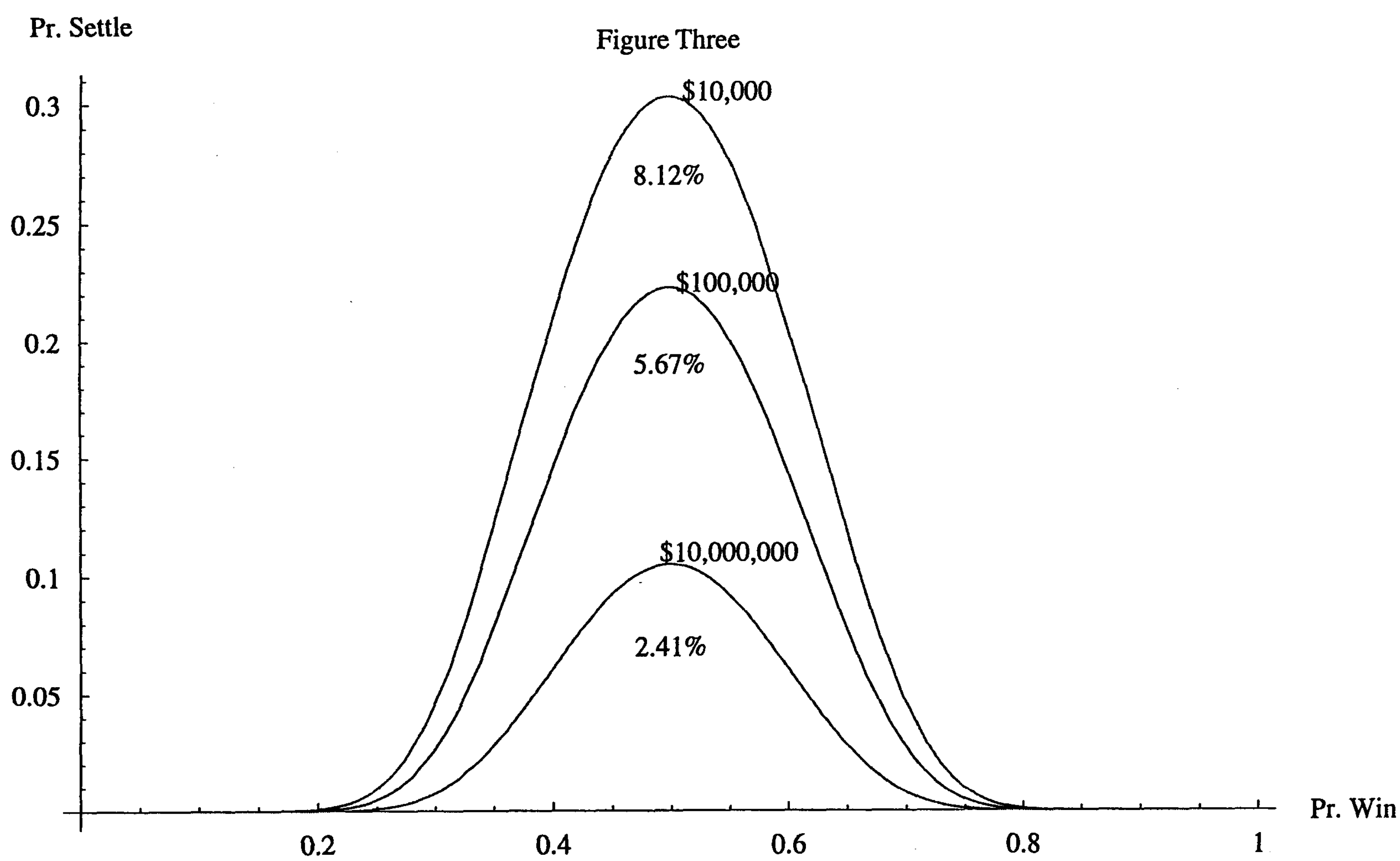


Figure Two

Figure Three



Diversity Jurisdiction
Estimation Procedure
(First and Second Stage)

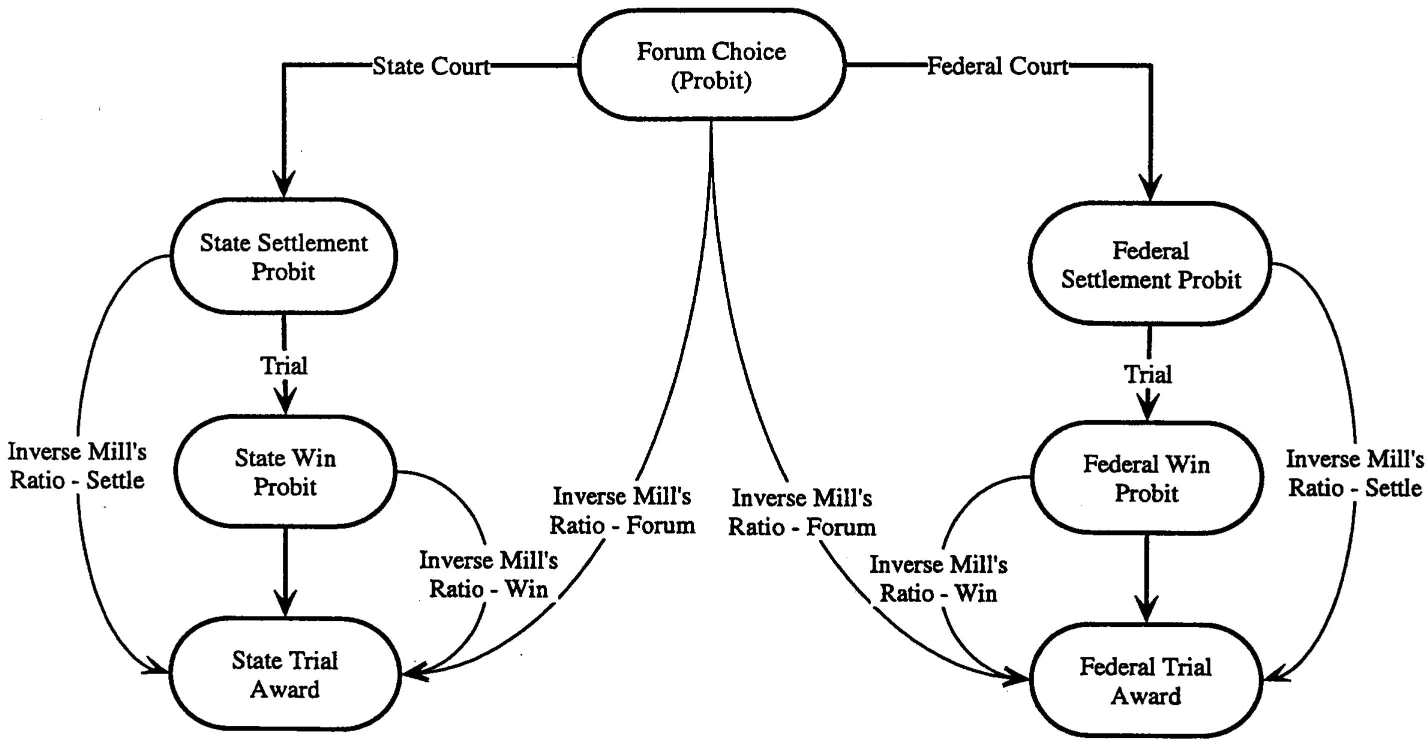


Figure Four