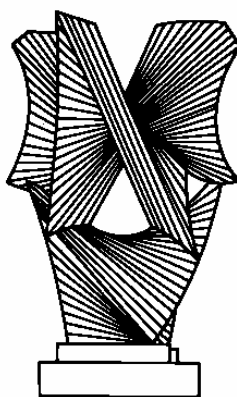


CHICAGO

JOHN M. OLIN LAW & ECONOMICS WORKING PAPER NO. 357
(2D SERIES)



Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary

Stephen J. Choi, G. Mitu Gulati, and Eric A. Posner

**THE LAW SCHOOL
THE UNIVERSITY OF CHICAGO**

August 2007

This paper can be downloaded without charge at:
The Chicago Working Paper Series Index: <http://www.law.uchicago.edu/Lawecon/index.html>
and at the Social Science Research Network Electronic Paper Collection:
http://ssrn.com/abstract_id=1008989

Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary

Stephen J. Choi
G. Mitu Gulati
Eric A. Posner¹

August 21, 2007

Abstract. Although federal judges are appointed with life tenure, most state judges are elected for short terms. Conventional wisdom holds that appointed judges are superior to elected judges because appointed judges are less vulnerable to political pressure. However, there is little empirical evidence for this view. Using a dataset of state high court opinions, we construct objective measures for three aspects of judicial performance: effort, skill and independence. The measures permit a test of the relationship between performance and the four primary methods of state high court judge selection: partisan election, non-partisan election, merit plan, and appointment. The empirical results do not show appointed judges performing at a higher level than their elected counterparts. Appointed judges write higher quality opinions than elected judges do, but elected judges write many more opinions, and the evidence suggests that the large quantity difference makes up for the small quality difference. In addition, elected judges do not appear less independent than appointed judges. The results suggest that elected judges are more focused on providing service to the voters (that is, they behave like politicians), whereas appointed judges are more focused on their long-term legacy as creators of precedent (that is, they behave like professionals).

¹ NYU Law School, Duke Law School, and the University of Chicago Law School. Thanks to Bill Landes, Tom Miles, Un Kyung Park, Richard Posner, Jonathan Wiener, and participants at a workshop at the University of Chicago Law School, for comments, and Nathan Richardson for helpful research assistance.

“If the state has a problem with judicial impartiality, it is largely one the state brought upon itself by continuing the practice of popularly electing judges”.

Justice O’Connor, concurring in *Republican Party of Minn. v. White*, 536 U.S. 765, 792 (2002).

1. Introduction

Justice O’Connor’s backhanded put-down of Minnesota’s elected judiciary reflects the conventional wisdom among lawyers and scholars that judges should be appointed by elected officials or independent commissions, and should not be elected themselves (Geyh 2003; Tarr 2003). The conventional wisdom reflects a deeply rooted conviction that voters are too unsophisticated to evaluate judges and candidates for judicial office. When judges use campaign contributions to finance simple-minded television commercials, conflict of interest is layered on public confusion. However, this conviction is hardly self-evident. In a system that uses judicial appointments, nothing forces the appointing official to select judges on the basis of their legal ability; cronyism is very common. And, as the literature on voting shows, ordinary people use various strategies for evaluating candidates whose qualifications they do not fully understand. For example, they rely on party endorsements and newspaper editorials, and the give-and-take of the campaign.² And when many people participate in a decisionmaking process, aggregation of information occurs, which can produce more accurate results than when the decision is made by only one person.

The relative merits of appointment and electoral systems are an empirical question, but what exactly should be tested? Most empirical work on this topic focuses on judicial independence, the willingness of a judge to vote against the ideological interests of the party of the elected official who appointed her or of the party to which she belongs.

² Newspaper endorsements of judicial candidates are common.

However, independence captures only a part of the judge's role. Judges are supposed to be independent but not to be arbitrary: a judge who votes against her party may still make bad decisions. And an independent judge who is lazy will not resolve many cases, or will resolve them poorly. The measures of independence that have been used in the literature imply that the best judicial system would be one in which Democratic judges voted in favor of Republican interests and Republican judges voted in favor of Democratic interests. It is as though empirical studies of central banks focused exclusively on whether central banks made decisions that contradicted the expressed desires of the government, and not on whether their decisions were correct, as a matter of monetary policy. Central bank independence is important but a central bank that always decided the opposite of what the government sought would not necessarily be a good one. The same can be said about judges.

To test the conventional wisdom that appointed judges are better than elected judges, we use a tripartite definition of judicial quality—productivity, opinion-quality, and independence. Productivity refers to the number of opinions a judge writes in a particular time period such as a year. The more opinions a judge writes, the more disputes she has resolved—and dispute resolution is the chief function of the judge. Opinion-quality refers to the opinion's reasoning. Better-reasoned opinions explain to the parties why they won or lost, but much more important, they provide guidance to future judges who face similar cases, and to people and businesses who want to avoid litigation in the first place. And independence refers to the willingness of judges to follow the law rather than the interests of political parties.

The independent variable of interest is the selection method for high court judges. In 12 states, judges are appointed by governors (or, in few instances, legislatures). At the opposite extreme, judges in 9 states run for election—and reelection—as members of political parties. In between, there are two systems that combine partisan and non-partisan elements. In 16 states, merit commissions are used: typically, an independent commission provides nominees whom the governor may appoint, while a retention election is used at the end of a judge’s term (rather than a competitive election). In 13 states, non-partisan elections are held: the public votes but judges are not permitted to advertise themselves as members of particular political parties.

A small empirical literature has investigated the relationship between selection systems and judicial characteristics in the states. As noted above, the literature has focused on judicial independence and not other attributes of quality (Cann 2007; McLeod 2007; Shepherd 2007), and suggests that appointed judges are more independent than elected judges. Our tests of independence produce more complicated results and does not favor either system in a clear way. As for overall quality, again the literature assumes that appointed judges are better – albeit, with little attempt to measure quality (Cann 2007).³ We find that elected judges are more productive. And although appointed judges write opinions that are cited more often, the difference is small and outweighed by the productivity difference. In other words, in a given time period, the product of opinion number and citations-per-opinion is higher for elected judges than for appointed judges.

³ There are a handful of empirical studies that do attempt to construct quality measures so as to evaluate judicial performance. These studies use either surveys or measures of the educational qualifications of the judges as their dependent variables (Cann 2007; Glick and Emmert 1987; Canon 1972). Surveys may reflect the biases of the respondents as we discuss in the text. As for educational and professional qualifications, those look to be more appropriate as an independent variable rather than as a measure of judging quality. (As we discuss below, there are interesting differences overlooked in the literature: elected judges went to worse law schools but have stronger local ties than appointed judges do.)

After discussing our results, we attempt an explanation for why elected judges might differ in these ways from appointed judges. We argue that elected judges tend to be politicians while appointed judges tend to be professionals. Professionals care about their reputation among a national community of like-minded professionals, while politicians care about their reputation in the local community of lay voters and politicians. Appointed judges thus labor to write opinions that will be admired, while elected judges try to satisfy as many litigants as possible by dispensing quick but adequate justice. Although our evidence does not prove that elected judges are superior to appointed judges, it casts doubt on the conventional wisdom, and broadens the scholarly debate.

2. Theory

2.1 The Determinants of Judicial Quality

Beginning with the legal realists, scholars and even judges themselves have speculated about the motives of judges, and whether judges decide cases by applying the law in a neutral fashion or in a manner that reflects personal or political views, or both (e.g., Cross 2005; Newman 1984). Because American judges have the power to strike down laws, the early controversy about judges' motives led to a debate about the proper role of judges in the constitutional system. If judges are ideologically motivated, then their power to strike down laws sits uneasily with democratic commitments; if they are not, or if their ideological motives are constrained, then judicial review has many

attractive properties, including possibly the ability to prevent majorities from exploiting minorities or otherwise supporting bad law.⁴

In recent years, this debate has reemerged in the framework of an agency model, which treats the judiciary or individual judges as agents, and the public or particular elected officials as the principals (e.g., Maskin and Tirole 2004). Agency models warn that agents, unless properly selected, monitored, and rewarded, will not act in the interests of principals. In the context of the judiciary, political institutions need to be designed to ensure that people with preferences similar to that of the public are selected to be judges, and that judges be given the right incentives to decide cases according to the law.

With respect to selection, judges should be impartial and competent, rather than partisan, ideological, eccentric, or incompetent. What selection mechanism will ensure that good, rather than bad, people are selected to be judges and that they will remain good after they become judges? Elections might ensure that people with mainstream views become judges, but the electorate may not be able to evaluate a potential judge's legal ability and other technical qualifications, let alone whether she is misleading the electorate in terms of her future fidelity to what is best for the populace. Appointments might result in competent and politically mainstream judges; however, elected officials might prefer to use judgeships as patronage positions. Unhappiness with these two extremes has led, in many states, to reliance on commissions, combinations of appointments and elections, and other complex mechanisms.

⁴ The vast literature cannot be described here. Holmes (1988) traces the history of this debate. Bickel (1986) is the source of the modern debate in constitutional law. Croley (1995) brings the debate to bear on state courts.

With respect to incentives, judges should, in principle, face sanctions if they decide cases poorly and rewards if they decide cases well. However, if rewards and sanctions are used, someone must apply them, and if that person has political power, judges might be afraid to decide cases impartially. The federal system avoids this risk by giving judges lifetime tenure on good behavior, but the danger with such a system is that it allows judges to decide cases badly or in a partisan fashion, without fear of sanction. Most state systems attempt to constrain judges by forcing them to undergo reelection or a reappointment process, but the danger is that judges will decide cases in partisan fashion so as to avoid a partisan sanction.

Some history illustrates the tensions (Hanssen 2004). Legislatures dominated the judicial selection and retention process between 1790 and 1847 on account of colonial era suspicion of the executive and the judiciary, both of which were arms of the British; however, some states retained gubernatorial appointment systems. Corruption among the legislatures and the spread of Jacksonian democracy stimulated the growth of electoral judiciaries beginning around 1850. Progressive-era distrust of electorates led to adoption of nonpartisan judicial elections at the turn of the century, and then the rise of merit plans. Merit plans generally created commissions that appointed judges, and used retention elections to discipline them. Though ideological currents clearly played a role in this history, the changes can easily be put in the agency framework, as Hanssen (2004) depicts. Reliance on partisan mechanisms reflected fears about the incentives of governors and then legislators to select and reappoint the proper sort of judge; the shift toward nonpartisan mechanisms reflected concerns about the ability of the public to

evaluate judges.⁵ Today, most scholars reject electoral systems (e.g., Fordham Symposium 2007).

The empirical literature on judicial behavior has focused primarily on federal judges, and especially on the U.S. Supreme Court (George 1998). One vein of this literature establishes that judges' voting behavior reflects partisan or ideological preferences, at least to some extent. Judges who are Republicans or who are supported, at time of appointment, by conservative media, tend to vote in an ideologically conservative way; a corresponding bias characterizes Democrats (Segal and Spaeth 2002). This work confirms that unconstrained judges do not necessarily decide cases impartially and casts doubt on the assumption that appointment systems are necessarily best. Another vein in the literature shows that structural and institutional features influence and constrain judges' incentives to vote once on the bench. For example, perhaps because of whistleblower or group polarization effects, federal judges vote less ideologically when the panel is split by party than when it is not (Cross and Tiller 1998, Sunstein et al. 2004).

A smaller empirical literature on the state courts has come to similar results. Brace and various coauthors find correlations between voting and ideology, analogous to the federal court studies (e.g., Brace et al., 2001, Brace et al. 2006). Tabarrok and Helland (1999) find that tort awards are higher in electoral states than in non-electoral states. They argue that their results reflect the stronger incentives of elected judges to redistribute wealth from out-of-state corporations to in-state voters and to please the local trial bar (see also Helland and Tabarrok 2002). Hanssen (1999) finds more litigation in non-electoral states than in electoral states. He argues that high court judges in non-

⁵ Berkowitz and Clay (2006) find that legislative preferences with respect to judicial independence may be connected to the type of legal system (civil or common law) of the nation that originally settled the areas that became states.

electoral states have more independence, and therefore are under less pressure to decide cases consistently. Greater uncertainty about the law generates more litigation. Hanssen (2000) finds that state bureaucracies are larger in non-electoral states than in electoral states, which he attributes to defensive efforts by the agencies to protect themselves from less politically constrained judges. Pinello (1995) finds that appointed judges are more likely to favor criminal defendants than elected judges are. Besley and Payne (2006) find that employment discrimination claims are more numerous in electoral states than in non-electoral states, which they argue shows that elected judges are more likely to rule in favor of employees than appointed judges are. Berkowitz and Clay (2006) find that the quality of state courts, as measured by surveys of senior attorneys at wealthy companies, is positively correlated with nonpartisan judicial retention procedures. Shepherd (2007) focuses on the political party of “retention agents”—those people who decide whether a judge will be retained or not—and finds that judges (of whatever party) are more likely to vote in favor of traditional Republican interests when retention agents are Republicans, and are more likely to vote in favor of traditional Democratic interests when retention agents are Democrats. The effect is larger when the retention process is electoral than when it involves reappointment. An early study by Landes and Posner (1980) finds that citations (including federal and out-of-state citations) of state supreme court opinions are uncorrelated with selection system.

The literature, taken as a whole, provides evidence that selection and retention institutions influence judicial outcomes—by influencing who becomes a judge, or how judges decide cases, or both. The literature also confirms that judges are influenced by political factors. The literature largely skirts our topic—whether elected judges are,

overall, better or worse than appointed judges—but offers tantalizing hints. The Tabarrok and Helland studies imply that elected judges are better agents of their constituents than are appointed judges; the social problem they identify is due to the federal structure of the country and the overlapping jurisdictions of states. Hanssen’s (1999) finding that electoral pressures force state judges to be more consistent implies that elected judges are better—more consistent opinions, all else equal, are better than inconsistent opinions. Berkowitz and Clay (2006) do use a measure of overall judicial quality based on the views of senior attorneys at wealthy companies, but, as they acknowledge (pp. 412–13), the views of these senior attorneys are hardly unbiased. Landes and Posner’s (1980) study is closest to ours, but they do not look at productivity and independence, and their study has several methodological limitations.⁶ Our results are quite different from theirs.

Following the literature, we envision the relationship between the public and the judiciary as an agency relationship. The optimal selection mechanism minimizes agency costs. Judges expend unobservable effort to decide cases. Agency costs can take two forms: laziness (resulting in bad opinions or few opinions) and ideological self-indulgence (biased opinions). The optimal selection mechanism screens out judges with a strong preference for laziness or ideological self-indulgence and/or punishes judges who are observably lazy or ideologically self-indulgent.

⁶ We use a larger sample, different variable definitions, and many more control variables. Their study is mainly about the federal courts. And their data come from a different era, before the modern concern about excessive political competition among state court justices fueled by campaign donations (Carrington 1998).

2.1 Judicial Quality and Judicial Selection Mechanisms

2.1.1 Judicial Quality

We use three measures of judicial quality.

Productivity. Judges have some discretion over how many opinions they write. Judges who write slowly will write fewer opinions. Judges who are lazy and wish to avoid writing concurrences and dissents, will also write fewer opinions. We measure a judge's productivity using the total number of opinions she writes during our sample time period (1998-2000), including majority, concurring, and dissenting opinions.

Productivity is a measure of effort, but is also a measure of quality because, all else equal, more opinions settle a greater number of legal disputes and resolve more legal issues. However, not all else is equal. A judge who writes more opinions might devote less time to each opinion, so that quality suffers. So productivity is at best only a partial measure of quality.

Citations. We assume, consistently with the rapidly expanding literature on judicial citations, that citations are a measure of quality (e.g., Baker 2007; Cross and Lindquist 2006; Lott 2005; Landes, Lessig and Solimine 1998). Better opinions are cited more frequently than worse opinions. An opinion is cited frequently because it resolves a legal question or identifies some new legal problem or represents an advance in the law or simply clarifies doctrine. We focus on out-of-state citations because this allows us to control for the possibility that in-state citations reflect local legal customs or conventions.

To check for robustness, we further subdivide citations into federal, state, and law review citations.⁷

Independence. Judges have the duty to enforce the law impartially, without regard to the legally irrelevant characteristics of the litigants or the goals of political parties. We thus assume that better judges are more independent. Some studies (e.g., Shepherd 2007) measure independence by the propensity of a judge to vote against interests associated with her party—for example, a Republican judge voting in favor of an employee and against an employer. This measure of independence focuses exclusively on the vote in the case and does not take into account the direction of the opinion’s reasoning. It would, for example, code an opinion that decides a particular case against an employee, but creates precedent through its reasoning that would assist later employee suits, as Republican. And then there is the question of whether coding a vote against an employee and for the employer, regardless of the facts of the case, gets at the Republican/Democrat distinction at all. To avoid the foregoing pitfalls, we look directly at when judges decide to write opinions against judges of the same or opposite party. We posit that a judge who writes several dissents against majority opinions authored by judges of the same party (or majority opinions against dissents of same party judges) is more independent than a judge who rarely dissents and never against a judge of the same party.

Our measure of independence is imperfect, and we address its problems and alternatives subsequently. For now, we note two problems. First, the measure can be distorted by personal animosities. Personal animosities might cause judges to refuse to join opinions as often as they otherwise would (Choi and Gulati 2004). Second, there

⁷ The problems with citations studies have been rehearsed elsewhere and we will not repeat them here (for example, on the possibility of bias, see Abramowicz and Tiller 2005; Bhattacharya and Smyth 2004; Choi and Gulati 2007).

might be judges who are excessively partisan. These excessively partisan judges, because they view their co-partisan colleagues as not partisan enough, may end up dissenting a lot against the moderate judges from their own party. Such behavior – extremely partisan behavior – may then be interpreted as independence.

2.1.2 Selection Mechanisms

State judicial selection mechanisms can be divided in several ways. The literature has not arrived at a consistent methodology, and our approach differs from those of other researchers. Nonetheless, the approaches are roughly consistent, and we test ours for robustness. We divide judicial selection mechanisms into four categories (Table 1).

<<Insert Table 1 About Here>>

Governor or Legislative Appointment. In 12 states, judges are appointed by the governor or (in South Carolina and Virginia) the legislature. Gubernatorial appointments usually require the consent of the upper house of the legislature or the participation of a special commission such as an executive council. In most of these states, judges serve a term (ranging from 6 to 14 years) and then may be reappointed in the same manner. In Massachusetts, New Hampshire, and Rhode Island, judges enjoy lifetime or near-lifetime tenure.

Merit Plan. In 16 states, judges are nominated by a nonpartisan commission, and then appointed by the governor. Judges serve a term and then are subject to a retention election, where they run alone, and voters can either approve another term or vote against them. Terms vary but on the whole are less than those in appointment states.

Nonpartisan Election. In 13 states, judges run for election. Their political affiliations are not listed on the ballot, and so voters, unless specifically informed, do not know a candidate's political party. These judges serve a term and then may run for reelection. The terms range from 6 to 10 years.

Partisan Election. In 9 states, judges run for election as a member of a political party. They serve a term in the range of 6 to 10 years for the most part and then may run for reelection.

Readers might be skeptical about whether voters care much about judicial elections and use the elections as an opportunity to reward good judges and punish bad judges. Hall (2001) finds that only 8.3 percent of state supreme court judges seeking reelection between 1980–94 were defeated. Hall, nonetheless, reports a great deal of variation across time and selection systems. In partisan elections, judges during this period were defeated 18.8 percent of the time; in 1994, 36 percent of them were defeated. And judges' electoral success appears to hinge on their ideological similarity to voters. "The fact of the matter ... is that supreme court justices face competition that is, by two of three measures, equivalent if not higher to that for the U.S House" (Hall 2001, p. 319). (For further discussion, see Dudley 1997, Aspin 1999, and Geyh 2003).

We should note that each state has a unique system; the categorizations suppress a great deal of variation. For example, the governor of Massachusetts appoints nominees of a judicial selection commission while the governor of Maine makes appointments subject to confirmation by the Senate. Massachusetts judges serve until the age of 70; Maine judges have seven year terms, at the end of which they may be reappointed by the governor, again subject to Senate confirmation. These differences have led to different

coding practices in the literature, with some authors focusing on retention (e.g., Shepherd 2007) and others on selection procedures (e.g., Besley and Payne 2006); some using only two categories, others using multiple categories, and so forth.

Our categorization is similar to that of the other authors. The main concern is that if selection is relatively nonpartisan, and retention is relatively partisan, our selection variable will be misleading. Fortunately, it appears that the degree of partisanship tends to be the same at selection and retention decisions, and indeed tenure length is negatively correlated with the partisanship of the selection process (see Table 5, below).

2.2 Hypothesis

Conventional wisdom is that appointed judges are better than elected judges. If so, appointed judges should have higher productivity, citation numbers, and independence. The existing empirical literature provides some support for the hypothesis that appointed judges are more independent. Our analysis adds an analysis of productivity and citation numbers in addition to a new measure of independence to get at the underlying question of whether appointed judges are of higher quality than elected judges.

3. Data Description

3.1 The Dataset

We examine the decisions of all the judges of the highest court of every state for the years 1998, 1999, and 2000. The District of Columbia is excluded because of its anomalous position. Two states—Texas and Oklahoma—have two highest courts, one

with jurisdiction over civil appeals and the other with jurisdiction over criminal appeals. We, in effect, treat these courts as separate state courts: so we have 52 “states.”

Each court has a certain number of seats, but we count judges, rather than seats, so if turnover occurs, a court will have more judges than seats; and, if some seats are left empty, there could be fewer judges than seats. Our dataset contains 408 judges, about 8 per court. The average judge spent 2.65 of the 3 years in our sample period on the court. And each judge wrote on average about 67 opinions per year.

We organize the data in three ways. For productivity, we run judge-year level regressions. Each observation is a judge for a particular year; there are 1082 observations—that is, the product of 408 and 2.65. For citations, we run opinion-level regressions to allow us to assess the factors that lead to citations to specific majority opinions. There are 27,596 majority opinion observations in our dataset. For independence, we run judge-level regressions on data pooled from 1998 to 2000, and hence there are 408 observations. We assume that a judge’s independence does not change over the 1998 to 2000 time period and use pooled data to obtain as large a sample as possible of opposing opinions with which to construct our independence measure. In many of our regressions, the actual number of observations is lower as a result of inadequate data for variables of interest.

3.2 Measures of Judicial Quality

3.2.1 Productivity

Productivity is measured by total number of opinions written for any given year, including dissents and concurrences (Total Opinions). Our least productive judge wrote

two opinions in one year,⁸ while our most productive judge wrote 263 opinions in one year. The mean was 67 opinions per year. Table 2 provides productivity data, arranged by type of selection system.

<<Insert Table 2 About Here>>

A pattern is evident. Judges in *more* partisan systems are more productive than judges in less partisan systems. The difference between the mean level of Total Opinions for Election Partisan compared with Appointed judges is significant at the <1% level. Judges in more partisan systems also write greater numbers of dissents than judges in less partisan systems—an issue to which we will return. The difference between the mean Dissenting Opinions for Election Partisan compared with Appointed is significant at the <1% level.

3.2.2 Opinion Quality

Our primary quality variable is the number of out-of-state citations to a particular opinion by a particular judge (Outside State Citations). We also look at narrower measures—such as law review citations and outside federal court citations.

The best measure of quality is citation by an outside court—including another state court or outside federal court. Inside state and home federal court citations are driven to large extent by precedent. Looking at only outside citations allows us to examine those citations where judges have greater discretion to pick which opinions to cite. For the most part, judges are citing these outside opinions because they are helpful, not because the opinion has precedential force. Because of this discretion, an outside

⁸ Probably because the judge left office early in the year, entered office late in the year, was sick during the year, or had administrative duties.

circuit citation represents a better indication of which opinions judges deem of higher quality. Table 3 provides citation data, categorized by selection system.

<<Insert Table 3 About Here>>

The citation pattern reported in Table 3 is strong. Appointed and other judges subject to less partisan pressures have higher per-opinion outside citation levels than both non-partisan and partisan elected judges. The differences in the mean levels of Outside State Citations for non-partisan and partisan election judges compared with appointed judges are significant at the <1% level. This pattern carries through for law review citations as well (where the differences are again significant at the <1% level).

3.2.3 Independence

Our independence measure focuses on the tendency of judges to write opinions that disagree with co-partisans when the pool of judges provides opportunities to do so.⁹ We define an “opposing opinion” as either a majority opinion when a dissent exists, or a dissent when a majority exists. We assume that a judge exhibits independence when she writes an opposing opinion against a co-partisan.

We obtain a measure of the political party for each judge in our sample. We looked to three sources of information on party membership. First, we searched NEXIS and the Internet (using Google) for any news reports on the political affiliation of the each judge. Second, we also searched for information on political contributions at the opensecrets.org website. We used the political party of the donee candidate as a proxy for the political party of judges who contributed. Third, we used the party of the governor (if

⁹ The variable is defined as follows: $indep = (demopratio - opdistratio) * (republican = 1) + (repopratio - opdistratio) * (democrat = 1)$, where *demopratio* is the fraction of majority opinions in the state written by a Democrat (and similarly for *repopratio*), and *opdistratio* is the fraction of opposing opinions written by the judge in question against a Democrat.

any) who appointed the judge as a proxy for the judge's political party. In most of the cases where we had multiple sources of information on political party, the party was consistent across these sources. When our three sources reported different parties, we gave first priority to the party identified through our NEXIS and Internet searches and second to the party identified in the opensecrets.org database.

We define *Opposite_Party* as the number of opposing opinions written, by the judge of interest, against a judge of the opposite party divided by the number of opposing opinions written against a judge of either party from 1998 to 2000. Thus, this variable measures propensity to side with co-partisans. Not all opposing opinions are driven by the ideology of the opposing judges. A judge who dissents at random would dissent 70 percent of the time against an opposite party judge if the background pool of majority opinions consisted of 70 percent opposite party authored opinions. To take into account the background pool of opinions, we define *Opposite Pool* as the total number of majority opinions authored by an opposite party judge divided by the total number of majority opinions authored by either an opposite or same party judge (not including the judge in question) from 1998 to 2000.

We define *Independence* as *Opposite_Pool* minus *Opposite_Party*. A more negative *Independence* score corresponds to a judge who writes opposing opinions against opposite party judges more frequently than the background pool of majority opinions authored by opposite party judges. Conversely, a more positive *Independence* score corresponds to an authoring judge who writes opposing opinions less frequently against opposite party judges compared with the background pool of opinions (and thus

more frequently against co-partisans). We treat a more positive Independence score as indicative of a more independent judge.¹⁰

Table 4 reports summary statistics on our Independence measure. Only judges for whom we could identify a political party were included in the analysis, and only judges from states that had a mixture of judges from different political parties were included.

<<Insert Table 4 About Here>>

Panel A of Table 4 reports the mean Independence level for judges associated with the different selection systems for high court judges. Note that unlike our productivity measures discussed above, Independence is based on pooled data from 1998–2000. We do not expect the Independence of a judge to vary by year, and pooling the data enables us to increase the amount of data used to generate the Independence measure for any particular judge. From Panel A, note that the mean Independence level was negative for judges associated with all four different selection systems. Judges tend to write opposing opinions more against opposite party judges relative to the background pool of majority opinions

Judges subject to partisan election have the highest independence. Judges subject to non-partisan election have the lowest independence. However, the differences between the mean Independence scores for non-partisan election judges and merit selection and appointed judges are not statistically significant. Only the difference between the mean

¹⁰ Consider, for example, a Republican judge sitting on a high court in a state where the other judges are split 50-50 between Republican and Democrat judges and the pool of majority opinions written by other judges corresponds to this 50-50 split. Suppose our Republican judge authors 10 dissents and 20 majority opinions where there is a dissenting opinion. And suppose that 5 of the 10 dissents are authored against a Democrat judge and 15 of the 20 majority opinions face a dissent from a Democrat judge. In this case, $Opposite_Party$ would equal $(5+15)/(10+20) = 2/3$. $Opposite_Pool$ equals $1/2$. Independence would then equal $1/2 - 2/3 = -1/6$. Because of the tendency of our Republican judge to write an opposing opinion more frequently against Democrats compared with the background pool of majority opinions, the Republican judge receives a negative Independence score.

Independence score for non-partisan election and partisan election judges is significant (at the 10 percent confidence level only), which is a bit puzzling.

The bottom line is that the data is consistent with the findings in the empirical literature that judicial opinions reflect partisan or ideological bias. However, the data does not support the received wisdom that appointed judges are more independent than electoral judges.

This partial result could reflect problems with our Independence measure. There are two main problems. First, consider the extreme case where all judges on a particular state high court are all of the same political party (say all Republican). In this case, our Independence measure will equal zero since `Opposite_Party` will equal `Opposite_Pool` (and both will equal zero since there are no Democrat-authored opinions). Our analysis in Table 4 excludes judges who come from states with no variation in political party among judges for this reason. But, by the same token, we lose data.

Second, even where all judges are not of the same political party in a state, if an imbalance exists, the range of the Independence variable will vary. Consider two Republican judges. One is in a state with 90 percent of the majority opinions written by Democrats and the other is in a state with 10 percent of the majority opinions written by Democrats. For the first, Independence can range from -0.1 to $+0.9$. For the second, Independence can range from -0.9 to $+0.1$. So the second judge could have a much more negative Independence score than the first judge simply because the range is shifted over.

To address these problems, we try a number of different approaches.

First, we create a version of the Independence variable that is less dependent on the background political makeup of a particular state court. `Independence_Indicator` is

defined as 1 if Independence is greater or equal to zero and 0 otherwise. The indicator variable addresses the range problem but also throws out information: it suggests judges subject to non-partisan elections are less independent than the other types, who are about the same. The last column of Panel A of Table 4 reports on the mean of Independence_Indicator. None of the differences in mean Independence_Indicator levels among the varying selection systems are statistically significant.

Second, we consider the possibility that the act of writing a dissenting opinion (even against opposite-partisan judges) can be a greater display of independence than writing a majority opinion (even against co-partisan dissenters). Judges who write dissents display independence in their willingness to write critically of the opinions of their colleagues. Thus, we treat dissenting activity as a possible proxy for independence. However, as Panel B of Table 4 shows, no significant differences exist in the Independence score of active dissenters compared with occasional dissenters for judges of any of the four selection systems.

Third, we consider the possibility that the different systems might have different effects on judges of varying political preferences. The bottom of Panel B shows that judges who make political contributions (Open Secrets = 1) tend to be less independent for the more partisan selection systems compared with judges who did not make any political contributions (Open Secrets = 0).

The bottom line is that appointed judges are not, overall, more independent than judges in partisan elected systems; they might even be less independent. We do find that, among judges who contribute to political campaigns, appointed judges are more

independent, but it is not clear that this signifies greater overall independence for the appointment system.

4. Multivariate Tests

We estimate a number of multivariate regression models to assess the relationship of several key independent variables, including the judicial selection mechanism, and our measures of judicial quality (used as the dependent variables in our models). We also include a number of common control variables in our models.

4.1 Selection Mechanisms

Our main interest is the relationship between the selection mechanism used by states to select and retain judges and our dependent variable measures of judicial productivity, quality, and independence. Table 1 describes the selection systems for the different states. In all the states, the selection mechanism long predates our data pool, and thus we need not worry about endogeneity—that states adopted new mechanisms in response to changes in judicial quality. Electoral systems can be traced back to the Jacksonian era. As we mentioned above, the switch to alternative systems generally occurred during the Progressive Era, and the process was more or less complete by the 1970s. More recent changes, with the exception of South Carolina, have been marginal (Hanssen 1999, Besley and Payne 2006, table 1).

To check for robustness, we also test the importance of judicial tenure data—the average tenure (meaning actual service, not de jure term length) of high court judges by state, as of the spring of 1997 (from Hanssen 1999, tble. 1) (Tenure). Note that the Tenure data does not come from our data set: we do not have information on the tenure of

the judges, many of whom are still in office. We provide summary statistics on the relationship between Tenure and the different judge selection mechanisms in Table 5.

<<Insert Table 5 About Here>>

Note from Table 5 that mean tenure rises as partisanship falls. We have two clusters—high partisanship and low tenure, and low partisanship and high tenure.

The advantage of the Tenure variable is that it allows us to compare the different selection mechanisms along a common metric: the length of time that a judge expects to remain in office. Only a few appointment states have lifetime tenure, so even the relatively apolitical mechanisms permit judges to be “punished”: elected officials or commissions unhappy with the performance of judges can refuse to reappoint them or support their reappointment. Thus, tenure indicates the vulnerability of the judge to later retention decisions more directly than do the selection mechanisms. Judges with longer tenure face less retention pressure while judges with shorter tenure face greater retention pressure. Tenure allows us to measure the real effects of the retention mechanisms while allowing us to rely less on de jure rules that might be evaded in local legal practice.

However, problems exist with the Tenure variable. If a judge does a bad job from the perspective of the decisionmaker for the retention decision, the judge may be sanctioned, in which case she ends up with a short tenure. Judges in equilibrium may respond to the threat of a frequent retention decision by catering to the preferences of the retention decisionmaker. Thus a judge with a long tenure could represent either a judge who rarely faces a retention decision or, alternatively, a judge who does face frequent retention decisions but who in equilibrium is adept at satisfying the preferences of the retention decisionmaker. The strong correlation between Tenure and state selection

systems that provide for longer (if not lifetime) tenure leads us to discount this latter possibility. Because of these potential problems with Tenure, we use Tenure only as a robustness check of our selection system results.

4.2 Control Variables

Our multivariate models of judicial productivity, quality, and independence include a common set of control variables. First, we include state level controls that attempt to capture differences among states that might account for judicial performance. Many of our state level controls relate to the state court system. We include measures for the average high court associate justice salary (Adjusted Associate Justice Salary) and the average partner salary in the state (Adjusted Partner Salary). The salary variables are adjusted for the cost of living for the metro area in which the high court is located in the state. Higher-paying states may attract higher-quality judges. Perhaps judges work harder if their salary will decline (or not rise much) if they are not retained. We include an indicator variable for whether the judges on the high court remained the same throughout our sample time period from 1998 to 2000 (Stable Court) and the size of the bench during the 1998 to 2000 period (Number of Active Judges on Bench). As a measure of resources available to high court judges, we include the average number of clerks per judge for the 1998 to 2000 period (Number of Clerks Per Judge) and an indicator variable for whether the clerks are tenured for at least one year (Long-Term Clerk). Judges may also act differently if facing a high workload, particularly if an intermediate appellate level court does not exist to help with the workload. We include the log of the number of trial cases in the state measured in 1998 ($\ln(\text{Number of Trial Cases in the State})$) and an indicator

variable for the presence of an intermediate appellate court (Intermediate Appellate Court).¹¹ The variables for the number of clerks and clerk tenure, the size of the bench, the number of trial cases, and the existence of an intermediate appellate court may all influence a judge's choice to devote time to any specific case.

Our state-level controls also include variables relating to the general characteristics of each state. Differences in overall state population ($\ln(\text{Population})$), gross state product ($\ln(\text{Gross State Product})$), and crime rates (Crime Rate) may lead to different mixes of cases and judicial responses to these cases. Because previous research suggests that state judges are influenced by judges in neighboring states (Harris 1985), and because larger neighboring states might produce different types of cases, we include a variable for the aggregate population of border states ($\ln(\text{Border Population})$). We also include a measure of the age of the state (State Age). Older states have longer judicial traditions and hence possibly a more sophisticated jurisprudence on which modern judges can draw. State Age controls for the possibility that modern judges are cited more often outside of the state just because they can draw on the older and more sophisticated jurisprudence of their particular state. Finally, we include a variable for citizen ideology based on election results in each district (from Berry et al. 1998) (Citizen Ideology Score). The background ideology of the citizens of a state may affect the preferences of high court judges.

¹¹ We also ran the regressions with a control variable for the ratio of cases resulting from mandatory appeal to the total number of cases (mandatory and discretionary), to try to control for the possibility that a court's ability to control its docket affects productivity and other dependent variables (Brace and Hall 1990). Another possible approach would have been to code for de jure rules governing appeals but these turn out to be extremely complicated and we could not determine an objective way to code them. We do not report the first regressions because of lack of confidence in the appropriateness of this control variable; in any event, it did not affect the statistical significance or (except trivially) the size of the coefficients for our main results, with a single small exception: in the independence regressions, the interaction of dissents and partisan elected judges loses its statistical significance.

Second, the multivariate models include judge level controls. We include an indicator variable for whether the judge was the chief judge of the high court (Chief Judge). A judge who is chief judge may have less time to author opinions. The chief judge may also command greater respect and receive greater numbers of citations as a result for her opinions. Alternatively, the chief may be able to assign herself the more important opinions and garner more citations that way (Langer 2003). We include the number of years between 1998 and the year in which the judge received her law degree (Post Law-School Experience) and the number of years the judge has been on the high court (Court Experience). More experienced judges may decide opinions with greater skill, leading to more citations. A variable for whether a judge retired in 2001 or earlier captures the possibility of end game problems (Retirement Close)—judges who retire soon have little to lose from deciding badly.

4.3 Productivity

Our first multivariate model focuses on judicial productivity. We estimate the following equation on judge year-level data using an ordinary least squares regression with robust standard errors:

$$\begin{aligned} \ln(\text{Total_Opinions})_i = & \alpha + \beta_{1i}\text{Election_Non-Partisan} + \beta_{2i}\text{Election_Partisan} \\ & + \beta_{3i}\text{Merit_Plan} + \sum \beta_{ji}\text{State Controls}_{ji} + \sum \beta_{ki}\text{Judge Controls}_{ki} \\ & + \text{Year Effects} + \varepsilon_i \end{aligned}$$

Our model relates the log of the total number of opinions authored by a judge in any given year to three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The three indicator variables use Appointed states as the baseline. The regression model includes year-level State Controls and Judge Controls described

above (see Appendix for definitions). The model also includes year fixed effects. Table 6 reports results.

<<Insert Table 6 About Here>>

The results in Model 1 of Table 6 contradict the hypothesis that judges subject to more partisan pressure are less productive; the opposite is the case. The coefficients on Election Non-Partisan, Election Partisan, and Merit Plan are all positive and significant at the <1% level. Partisan-elected judges are the most productive, followed by merit-plan and non-partisan judges. Appointed judges (the omitted, baseline case) are the least productive.

Our control variables largely make sense and are interesting in their own right. More highly paid judges are more productive, taking into account the opportunity cost of forgoing a practicing lawyer's salary—though the magnitude is small. Judges on a stable court are more productive, probably because collegial norms are stronger on stable courts. Chief judges are less productive, perhaps because they have administrative duties. Judges with more judicial and legal experience are more productive; but judges become less productive as they approach retirement. Judges from states with a large population are more productive because they have more cases to decide, and, because of the greater activity and diversity of the population, more law to make. The presence of an intermediate appellate court, the number of trial cases in the state, and crime variables are either statistically insignificant or small.¹²

¹² As a robustness test, we re-estimate Model 1 of Table 6 using the log of the total number of majority opinions in any given year and, alternatively, the log of the total pages written for all opinions in any given year as dependent variables. Unreported, we obtain the same qualitative results as in Model 1 in both robustness models. Judges from Election Non-Partisan, Election Partisan, and Merit Plan states are more productive than Appointed judges (whether measured by total majority opinions or total pages written). We also re-estimate Model 1 with the addition of an indicator variable set equal to 1 if the judge was in an election year and 0 otherwise (Election Year). Unreported, the re-estimated model provides the same

We replace the three indicator variables for the state judge selection system with Tenure, our alternative measure of the pressure facing judges, focusing on the retention decision. Model 2 reports that the coefficient on Tenure is negative and significant at the <1% level. Judges who face frequent retention decisions (and presumably experience a shorter expected tenure), are significantly more productive than judges who have the luxury of a longer expected tenure period. These results are consistent with the Model 1 results.

Why are judges subject to electoral pressures more productive? Perhaps productivity is used by voters as a signal of judicial competence, or by intermediaries, such as newspaper editorialists, bar associations, and parties. Judges who write few opinions, then, will be vulnerable in reelection campaigns.¹³ This would also explain why productivity declines as retirement nears (Retirement Close), as reported in both Models 1 and 2.¹⁴

If elected judges respond to electoral pressures through increased productivity, one might predict that productivity would increase with political competition. To test this hypothesis, we use a standard measure in the political science literature. This measure takes a higher value when elected officials are more frequently replaced. To Models 1

qualitative results as in Model 1; the coefficient on Election Year is negative (indicating decreased productivity) but not statistically significant.

¹³ Anecdotal evidence supports the hypothesis that elected judges feel pressure to be productive. Productivity is mentioned in newspaper endorsements and judicial evaluation materials from time to time. A clear example can be found in the reelection campaign materials of a Texas intermediate appellate court judge, which include a table with productivity statistics for a group of judges:

For the fiscal year ending August 31, 2006, Third Court of Appeals Justice Bob Pemberton ranked #1 statewide among Texas' court of appeals judges in production of original appellate opinions on the merits. These results show that Justice Pemberton is the most productive appeals judge in Texas for original opinions and the Third Court of Appeals is the most productive of Texas' 14 courts of appeals.

http://www.bobpemberton.com/2006/09/20/appeals_opinion_productivity/.

¹⁴ We also ran the regressions using total pages written by a judge for a specific year as the dependent variable: the results were nearly the same, in most cases with somewhat higher coefficients and higher levels of statistical significance.

and 2 of Table 6 we add an indicator variable, High Political Competition, for states where the level of political competition is greater than the median level. We also add interaction terms between High Political Competition and the three indicator variables for the state judge selection system (Model 1) and Tenure (Model 2). Table 7 reports our results.

<<Insert Table 7 About Here>>

Model 1 of Table 7 reports that the coefficient on High Political Competition is positive and significant at the <1% level. In contrast, the coefficients on the interaction terms between High Political Competition and Election Non-Partisan and Election Partisan are negative and significant at the <1% level. Moreover, the sums of High Political Competition + Election Non-Partisan x High Political Competition and High Political Competition + Election Partisan x High Political Competition are both negative and significant at the 5% and <1% levels respectively. While judges from Election Non-Partisan and Election Partisan states are generally more productive, the amount of increased productivity diminishes in states with a large amount of political competition (but still remains positive compared with judges from Appointed states).¹⁵ Model 2 of Table 7, in contrast, reports no significant relationship between the level of productivity and High Political Competition interacted with Tenure. These results are puzzling, and suggest that the political competition measure is either not accurate or (more likely) does not reflect the pressures on judges per se.

¹⁵ The sums of Election Non-Partisan + High Political Competition + Election Non-Partisan x High Political Competition and Election Partisan + High Political Competition + Election Partisan x High Political Competition are both positive and significant at the <1% levels.

4.4 Citations

For outside citations, we estimate the following equation for each majority opinion using ordinary least squares and robust standard errors:

$$\begin{aligned} \ln(\text{Outside State Citations}_i) = & \alpha + \beta_{1i}\text{Election_Non-Partisan} + \beta_{2i}\text{Election_Partisan} \\ & + \beta_{3i}\text{Merit_Plan} + \beta_{4i}\text{Number Dissents} + \beta_{5i}\text{West Key Pages} \\ & + \beta_{6i}\text{Opinion Length} + \sum \beta_{ji}\text{Subject Matter}_{ji} \\ & + \sum \beta_{ki}\text{State Controls}_{ki} + \sum \beta_{li}\text{Judge Controls}_{li} \\ & + \text{Year Effects} + \varepsilon_i \end{aligned}$$

The model relates the number of outside state citations (Outside State Citations) for any specific majority opinion with three indicator variables for Election Non-Partisan, Election Partisan, and Merit Plan states. The three indicator variables use Appointed states as the baseline. The model includes the number of dissents written against the majority opinion in question (Number of Dissents). A majority opinion with one or more dissents may deal with more novel issues of law and generate more citations as a result. The model includes the number of west key pages (West Key Pages) as a rough measure of the legal importance of the opinion. Similarly the model includes the length of the opinion (Opinion Length); longer opinions are more likely to contain analysis that other judges may cite compared with shorter opinions, all other things being equal. We also include subject matter fixed effects for twelve different subject matter categories, including Administrative, Attorney and Client, Capital Punishment, Church and State, Commercial, Criminal, Family, First Amendment, Labor, Property, Rights, and Torts. We use Other opinions as the baseline subject matter category.

We include both State Controls and Judge Controls, as described above, in the model. For the judge controls, instead of the Retirement Close indicator variable, we

include a series of indicator variables for whether retirement occurred prior to 2000 or in 2001, 2002, 2003, 2004, or 2005. Because the number of outside citations may drop after a judge retires, we use these indicator variables to capture more precisely when a judge retires. We also use year fixed effects. Table 8 reports the results from our multivariate outside citation model.

<<Insert Table 8 About Here>>

Our results are consistent with the hypothesis that judges subject to less partisan pressure write higher quality—more frequently cited—opinions. Model 1 reports that the coefficients on Election Non-Partisan, Election Partisan, and Merit Plan are negative and significant at the <1%, 5%, and <1% level respectively. Model 1 is consistent with the view that appointed judges write the best opinions. Also, majority opinions with dissents are cited more frequently, perhaps because the majority opinion makes new law. This reason might also explain why longer opinions are cited more often. Finally, commercial, torts, family law, and rights opinions stand out as those that are cited most frequently out of state; administrative law and capital punishment decisions are cited the least out of state. We do not have an explanation for this pattern.

Why would appointed judges write better opinions than elected judges? One possibility is that the quality of an opinion, unlike the number of opinions written, is not observable to the public, so elected judges do not have a strong incentive to write high-quality opinions. With less pressure to produce, appointed judges might prefer to advance their influence and professional reputation by writing good opinions. Another possibility

is that a system that selects for judges skilled at electioneering and politicking does not also necessarily select for judges skilled at authoring high quality legal opinions.¹⁶

To test the importance of high levels of in-state political competition, we add to Model 1 the High Political Competition variable as well as interaction terms between High Political Competition and the three indicator variables for the state judge selection system. Model 2 reports that the coefficient on Election Non-Partisan x High Political Competition is positive and significant at the <1% level. Greater political competition in-state correlates with increased quality opinions for Election Non-Partisan judges. This result is somewhat puzzling. On the other hand, the sum of Election Non-Partisan + High Political Competition + Election Non-Partisan x High Political Competition is not significantly different from zero. As well, the interaction term between High Political Competition and Election Partisan is not significantly different from zero—indicating that judges from Election Partisan states, unlike for Election Non-Partisan states, do not respond to increased political competition with higher quality opinions.

We replace the three indicator variables for the state judge selection system with Tenure, our alternative measure of the pressure facing judges, focusing on the retention decision. Model 3 reports that the coefficient on Tenure is positive but not significantly different from zero. On the other hand, the coefficient on High Political Competition is positive and significant at the 5% level. Greater levels of political competition correlate

¹⁶ As a robustness test, we re-estimate Model 1 of Table 8 using the log of one plus the number of law review citations to a majority opinion ($\ln(1+\text{Law Review Citations})$) as an alternative measure of opinion quality. Not reported, we obtain the same qualitative results as in Model 1. Judges from Election Partisan, Election Non-Partisan, and Merit Plan states produce opinions that are cited less by law reviews than judges from Appointed states. Moreover, the biggest drop off in the level of law review citations is for Election Non-Partisan opinions. We also re-estimate Model 1 with the addition of the Election Year indicator variable. Unreported, the re-estimated model provides the same qualitative results as in Model 1; the coefficient on Election Year is negative (indicating decreased opinion quality in an election year) but not statistically significant.

with higher opinion quality. Perhaps because judges in election states where there is more political competition do not compete as much based on productivity (but instead compete along other dimensions), they have more time to craft higher quality opinions.

4.5 Independence

4.5.1 Propensity to Dissent

To assess what factors correlate with a high propensity to write dissenting opinions, we estimate the following equation on judge-year level data using ordinary least squares and robust standard errors:

$$\begin{aligned} \ln(1+\text{Number Dissenting Opinions})_i = & \alpha + \beta_{1i}\text{Election_Non-Partisan} \\ & + \beta_{2i}\text{Election_Partisan} + \beta_{3i}\text{Merit_Plan} + \sum \beta_{ji}\text{State Controls}_{ji} \\ & + \sum \beta_{ki}\text{Judge Controls}_{ki} + \text{Year Effects} + \varepsilon_i \end{aligned}$$

The regression model relates the number of dissenting opinions written in a given year ($\ln(1+\text{Number Dissenting Opinions})$) with three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The indicator variables use Appointed states as the baseline. The model includes year-level versions of the state controls and judge controls as well as year fixed effects. Table 9 reports the model.

<<Insert Table 9 About Here>>

Model 1 of Table 9 indicates that elected judges, especially partisan-elected judges, dissent the most, with merit-plan judges in the middle. Appointed judges write the fewest dissenting opinions. If dissenting indicates independence, than elected judges are more independent than appointed judges are.

Model 1 of Table 9 also reports, however, that the presence of high political competition among elected officials in the state (High Political Competition) reduces the propensity to dissent for the judges in elected judge states. Note that the coefficients on the interaction terms between High Political Competition and Election Non-Partisan and Election Partisan are both negative and significant at the 10% and <1% levels respectively. The behavior of judges with respect to writing dissents in Election states is not independent of politics. Judges in states with a large amount of political competition, for example, may choose not to write as many dissents to avoid providing opponents with political targets. Such judges may instead use dissents without opinions to display their party loyalty while keeping their political profile low. By contrast, appointed judges are not affected by political competition at a statistically significant level.

Model 2 of Table 9 reports the results of our robustness test using Tenure as an alternative specification of how judges differ across the states with a focus on the retention decision. Note that the coefficient on Tenure in Model 2 is negative and significant at the <1% level. Judges who face the possibility of a short tenure (as in elected states) write more dissents. Similarly, the coefficient on High Political Competition is negative and significant at the <1% level and the coefficient on the interaction term between Tenure and High Political Competition is positive and significant at the <1% level (indicating that judges who face more frequent retention decisions are more affected by High Political Competition to reduce the number of dissenting opinions). More political competition in the state might cause judges who otherwise would compete based on writing dissenting opinions to focus on other aspects of competition.

As a robustness test, we re-estimate Models 1 and 2 of Table 9 with the addition of the Election Year indicator variable. Unreported, the re-estimated model provides the same qualitative results as in Models 1 and 2; the coefficients on Election Year are negative (indicating decreased productivity) but not statistically significant.

If dissenting indicates a willingness to express one’s honest view, then our results suggest that elected judges are more likely to do this when political competition is low, while appointed judges are less deterred from dissenting when political competition is high.¹⁷ But overall, elected judges are more willing to dissent than appointed judges are, so if dissenting signifies independence, then elected judges are more independent than appointed judges are.

4.5.2 Propensity to Write Opinions Against Co-Partisans (Independence)

Given the shortcomings of focusing solely on the number of dissenting opinions as a measure of independence, we turn to our Independence measure. We estimate the following equation on pooled data from 1998 to 2000 using an ordinary least squares regression model with robust standard errors:

$$\begin{aligned} \text{Independence}_i = & \alpha + \beta_{1i}\text{Election_Non-Partisan} + \beta_{2i}\text{Election_Partisan} \\ & + \beta_{3i}\text{Merit_Plan} + \sum \beta_{ji}\text{Subject Matter}_{ji} \\ & + \sum \beta_{ki}\text{State Controls}_{ki} + \sum \beta_{li}\text{Judge Controls}_{li} + \varepsilon_i \end{aligned}$$

The regression model relates our Independence measure (Independence) based on pooled 1998 to 2000 data for each judge with three indicator variables for Non-Partisan Election, Partisan Election, and Merit Plan states. The three variables use Appointed

¹⁷ We also find a positive correlation between dissent activity and the size of the bench (that is, the number of judges participating in cases during our period), which is consistent with Lindquist (2007), who finds the same result using federal appellate courts).

states as the baseline. The model includes pooled data version of the state controls and judge controls as described above. As a control for the subject matter composition of the pool of opinions, for each of the 12 subject matter categories (see Appendix for definitions) we compute the number of majority opinions that deal with the specific subject matter divided by the total number of majority opinions for the state in the 1998 to 2000 time period. We include this ratio for each subject matter in the model as pooled controls for subject matter in the state. Table 10 reports the results of our model.

<<Insert Table 10 About Here>>

Model 1 of Table 10 reports that none of the coefficients on the three indicator variables for the state judge selection systems are significantly different from zero. The coefficient on Number of Dissenting Opinions is, however, positive and significant at the 5% level. Those judges who write more dissents are also more independent under our Independence measure. In contrast, the coefficient on the interaction term between Number of Dissenting Opinions and Election Partisan is negative and significant at the <1% level. While judges who write more dissenting opinions generally receive a higher Independence score, this relationship does not hold true for judges in Election Partisan states.

We posit that more ideologically intense judges will tend to act less independently when faced with the possibility of an election. To test this, we add the Open Secrets measure of political contribution as a proxy for political intensity to Model 1. We also add interaction terms between Open Secrets and the three indicator variables for the state selection systems. Model 2 reports that the interaction terms between Open Secrets and Election Non-Partisan and Election Partisan are both negative (indicating less

independence) and significant at the 5% and <1% levels respectively. While more intensely political judges do not correlate significantly with a decreased independence score for Appointed and Merit Plan judges, we do see a negative relationship between political intensity and independence for the elected judges.

We lastly posit that elected judges from states with a high level of political competition will act differently than appointed judges with similarly high levels of political competition. We add the High Political Competition indicator variable to Model 2 as well as interaction terms between High Political Contribution and the three indicator variables for the state judge selection system and report the results in Model 3 of Table 10. From Model 3 note that the coefficient on the interaction term between Partisan Election and High Political Competition is positive and significant at the 5% level while the coefficients on all the other High Political Competition variables are insignificant. While Partisan Election state judges do display a difference in behavior depending on the underlying state political environment (in a way that Appointed and Merit Plan judges do not), we are not sure of why the level of Independence *increases* for Partisan Election judges in High Political Competition states. Perhaps elected judges in High Political Competition states compete other than through their opinions (and thus act less partisan in the decisions themselves).¹⁸

¹⁸ As a robustness test, we re-estimate Model 3 using Independence_Indicator as the dependent variable, set equal to 1 if Independence is greater or equal to 0 and set equal to 0 otherwise. Unreported, we obtain qualitatively similar results with some differences. The coefficient on Election Non-Partisan is now positive and significant at the 5% level, indicating the judges from Election Non-Partisan states are generally more independent under this measure than Appointed judges. On the other hand, the interaction terms between Election Non-Partisan and the Number of Dissenting Opinions and Open Secrets remain negative (and are significant at the 10% level), indicating that elected judges who write more dissents and are politically more active are less independent. Moreover, the coefficients on the High Political Competition variables are now all insignificant.

Our results are complicated and difficult to summarize but our overall sense is that elected judges are more sensitive to political competition than appointed judges are (suggesting that elected judges are less independent) but that elected judges are more likely to dissent (suggesting more independence), and that the two types of judges are otherwise roughly equally likely to write against co-partisans (suggesting equal independence). Strongly partisan judges act in a more partisan way in electoral systems than in appointment systems, but it is not clear why this matters if independence for overall judicial activity is the same. Perhaps the clearest conclusion is that the conventional wisdom that appointed judges are more independent than elected judges is a simplification and probably an exaggeration.

4.6 Productivity Versus Quality

Given the ambiguity of our independence results, what's more important: productivity or quality? As a measure of the overall influence of a judge's opinions, we calculated the aggregate number of outside citations to the opinions written by each judge in one year (Aggregate Outside Citations). The average judge in a partisan election system in one year writes opinions cited (outside of the state and home federal circuit) in the aggregate 11.3 times, whereas the average judge in an appointment system writes opinions cited in the aggregate 15.0 times (difference significant at the <1% level). The numbers for non-partisan election systems and merit selection systems are 11.3 and 14.0, respectively. At a summary statistic level, the influence of appointed judges is not significantly diminished relative to elected judges despite the lower productivity on the part of appointed judges.

To provide a multivariate test controlling for state and judge characteristics, we re-estimate our judge productivity model (Model 1 of Table 6) on judge-year level data, replacing $\ln(\text{Total Opinions})$ with $\ln(\text{Aggregate Outside Citations})$ as the dependent variable. Unreported, the coefficients on Election Partisan, Election Non-Partisan and Merit are all positive and significant at <1%, 5%, and 10% respectively, using Appointed as the base case. Judges from partisan election states in particular correlate with significantly higher levels of aggregate outside citations compared with appointed judges. In contrast with the summary statistic comparison, the multivariate test is consistent with the view that the lack of productivity on the part of appointed judges diminishes the overall influence and quality of their total judicial output of opinions.

We note parenthetically that the quality-quantity tradeoff—that judges apparently write more opinions that are of lower quality, or fewer opinions of higher quality—is itself of intrinsic interest, and seems highly plausible, yet has not been noticed in the literature.

5. Explaining the Results

We summarize our results in Table 11.

<<Insert Table 11 About Here>>

Elected judges write more opinions and while their average cite counts per opinion are lower than those for appointed judges, they garner more citations overall. From the average litigant’s perspective, elected judges provide more justice to more people—if one takes the receipt of a written explanation for the court’s decision as a

component of justice. Appointed judges write fewer opinions, but those that are written tend to be higher quality opinions—they garner more citations. Fewer litigants are receiving this high quality justice though. Judges in the different systems exhibit similar levels of independence. What explains this?

One explanation draws on differences in incentives to write opinions. The multitasking model (Holmstrom and Milgrom 1991), shows that if an agent is given two objectives (say, quality and productivity), and the activities that further only one of those objectives can be measured and monitored by the principal, then the agent will shirk on the hard-to-measure objective and invest in achieving the other.

Judges in more partisan systems may write more opinions because raw productivity is observable. Judges who write few opinions, for example, can be easily criticized and, as we have seen, some judges use productivity as a campaign issue. Quality is hard to observe. So, judges who are concerned about retention will shirk on quality, while judges with security have no such incentive. Absent political pressure, the latter set of judges might concern themselves more with their long-term reputation among judicial colleagues and lawyers.

As for independence, the propensity to vote with or against co-partisans is also hard to observe. If so, elected judges would have no more incentive to refrain from acting independently than appointed judges do. So it is, in the end, not surprising that the independence levels of elected and appointed judges are not clearly different.

A problem with the multitasking theory is our finding that elected judges write fewer opinions as political competition increases. Perhaps they need to spend more time

campaigning or forging political connections. In any event, they must think that the productivity loss does not hurt them, or not much.

But this problem also suggests that another interpretation of the data might be superior. This brings us to selection. It might be that the different systems attract different types of people to judgeships: selection matters more than incentives to behave once in office. In particular, electoral systems would seem to attract *politicians*, while appointment systems are more likely to attract *professionals*. Politicians want to satisfy the voting public, and this might mean deciding cases expeditiously and in great number. Professionals are more concerned about their reputation among other lawyers and judges, and are more interested in delivering well-crafted opinions that these others will admire.

To test this possibility, we compare judges from different selection systems. We chose characteristics that might explain the differences in judicial behavior.

<<Insert Table 12 About Here>>

The most striking differences are as follows. Compared with Appointed judges, Elected judges make more campaign contributions; are paid less; are on less stable benches; have shorter tenures; are more likely to have gone to a law school in the state in which they sit; and are more likely to have gone to a lower-rank law school. They are, in short, more politically involved, more locally connected, more temporary, and less well-educated than appointed judges. They are more like politicians and less like professionals. Elected judges also have less court experience, but the difference with Appointed judges is not statistically significant.

One might think that politicians would be more concerned about party identification, and hence would be more likely to vote together than professionals are—

but this is not the case, as our independence measures show. One simple explanation for our results is that for most cases, one's vote has no political salience. The vast majority of cases are unanimous. It is more important even for politician-judges to decide many cases correctly than to agree with their co-partisans. Political identification matters as a proxy for how one's view affects how one evaluates a case. That is why judges do tend to vote with co-partisans. But this general tendency does not produce different independence outcomes in electoral and appointment systems because independence for all but a tiny number of cases is unobservable (and therefore not a focus of judges under either type of selection system).

The selection argument is more appealing than the incentives/multitasking argument because so much of judicial behavior is hard to observe. Judges may well believe that, as long as they avoid extremes of behavior, they can count on being reelected or reappointed. Ordinary people elect hard-working locals, while governors appoint reliable professionals who will not embarrass them.

In sum, a simple explanation for our results is that electoral judgeships attract and reward politically savvy people, while appointed judgeships attract more professionally able people. However, the politically savvy people might give the public what it wants—adequate rather than great opinions, issued in greater quantity and therefore (given the time constraint) average speed.

6. Conclusion

We began this project with the assumption that the data would demonstrate that appointed judges are better than elected judges. Our results persuade us that the story is

more complicated. It may be that elected judges are, indeed, superior to appointed judges. At a minimum, the conventional wisdom needs to be reexamined.

A full comparative evaluation of the systems would require more research. In particular, there has been much concern in recent years about the rise in the cost of election campaigns for supreme court justices, and some evidence that supreme court justices are more likely to vote in favor of contributors and their interests (Liptak and Roberts 2006, Goldberg et al. 2006).¹⁹ However, by the same token campaign expenditures enhance public awareness and indicate that judges face real political competition, which, for reasons we have given, might be a good rather than bad thing. In addition, one needs to take account of the hidden costs of appointment systems, namely, that they can serve patronage purposes.

¹⁹ Goldberg et al. (2006) identify Alabama, Illinois, Michigan, and Ohio as outliers—states where campaign contributions were much greater than in other states—as of 2000, the last year of our study. We checked the independence scores of judges in these states and found Alabama and Michigan had lower mean independence scores than the mean for all states, while Illinois and Ohio had higher scores. This is an interesting topic for future research, but as of now the data do not seem reliable and accessible enough to do a rigorous test.

References

- Abramowicz, Michael, and Emerson Tiller. 2005. "Judicial Citation of Legislative History: Contextual Theory and Empirical Analysis," http://papers.ssrn.com/sol3/papers.cfm?abstract_id=725919.
- Aspin, Larry. 1999. "Trends in Judicial Retention Elections," 83 *Judicature* 81.
- Baker, Scott. 2007. "Should we Pay Federal Circuit Judges More?," unpub. m.s., available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=998395.
- Berry, William D., Evan J. Ringquist, and Richard C. Fording, Russell L. Hanson. 1998. "Measuring Citizen and Government Ideology in the American States, 1960–93," 42 *American Journal of Political Science* 327-348; updated data available from http://www.uky.edu/%7Eerford/Home_files/page0005.htm.
- Besley, Timothy, and Abigail Payne. 2005. "Implementation of Anti-Discrimination Policy: Does Judicial Discretion Matter?," *CEPR Discussion Paper* 5211, available at: <http://www.cepr.org/PUBS/NEW-DPS/dplist.asp?dpno=5211&action.x=0&action.y=0&action=ShowDP>.
- Bhattacharya, Mita, and Russell Smyth. 2001. "The Determinants of Judicial Prestige and Influence: Some Empirical Evidence From the High Court of Australia," 30 *Journal of Legal Studies* 223.
- Bibby, John F. and Thomas M. Holbrook. 1999. "Parties and Elections," in Virginia Gray, Russel Hanson, and Herbert Jacob, eds., *Politics in the American States*. Washington, D.C.: Congressional Quarterly Books.
- Bickel, Alexander M. 1986. *The Least Dangerous Branch*. New Haven, Conn.: Yale University Press.
- Brace, Paul and Melinda Gann Hall. 1990. Neo-Institutionalism and Dissent in State Supreme Courts, 52 *Journal of Politics* 54
- Brace, Paul and Melinda Gann Hall. 1995. "Studying Courts Comparatively: The View from the American States," 48 *Political Research Quarterly* 5–29.
- Brace, Paul, Laura Langer, and Melinda Gann Hall. 2000. "Measuring the Preferences of State Supreme Court Judges," 62 *Journal of Politics* 387– 413.
- Brace, Paul, Melinda Gann Hall and Laura Langer. 2001. "Placing State Supreme Courts in State Politics," 1 *State Politics and Policy Quarterly* 81–108.
- Brace, Paul, Jeff Yates, and Brent D. Boyea. 2006. "The Conditional Effects of Ideology and Institutional Structure on Judicial Voting in State Supreme Courts" *ExpressO Preprint Series Working Paper* 1831, <http://law.bepress.com/expresso/eps/1831>.
- Cann, Damon. 2007. "Beyond Accountability and Independence: Judicial Selection and State Court Performance," 90 *Judicature* 226.

- Canon, Bradley. 1972. "The Impact of Formal Selection Processes on the Characteristics of Judges – Reconsidered," 6 *Law & Society Review* 579.
- Carrington, Paul D. 1998. Judicial Independence and Democratic Accountability in Highest State Courts, 61 *Law and Contemporary Problems* 79.
- Choi, Stephen J., and G. Mitu Gulati. 2004. "Choosing the Next Supreme Court Justice: An Empirical Ranking of Judge Performance," 78 *Southern California Law Review* 23.
- Choi, Stephen J. and G. Mitu Gulati. Forthcoming. "Bias in Judicial Citations: A New Window into the Behavior of Judges," *Journal of Legal Studies*.
- Croley, Steven P. 1995. "The Majoritarian Difficulty: Elective Judiciaries and the Rule of Law," 62 *University of Chicago Law Review* 689–94.
- Cross, Frank B. 2005. "Legal Process, Legal Realism, and the Strategic Political Effects of Procedural Rules," unpub. m.s., available at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=837665.
- Cross, Frank, and Stefanie Lindquist. 2006. "Judging the Judges," unpublished draft.
- Cross, Frank and Emerson H. Tiller. 1998. "Judicial Partisanship and Obedience to Legal Doctrine: Whistle-blowing on the Federal Court of Appeals," 107 *Yale Law Journal* 2155–76.
- Dam, Kenneth W. 2006. *The Law-Growth Nexus: The Rule of Law and Economic Development*. Washington, D.C.: The Brookings Institution Press.
- Dudley, Robert L. 1997. "Turnover and Tenure on State High Courts: Does Method of Selection Make a Difference?," 19 *The Justice System Journal* 1.
- George, Tracey E. 1998. "Developing a Positive Theory of Decision Making on the U.S. Court of Appeals," 58 *Ohio State Law Journal* 1635.
- Geyh, Charles G. 2003. "Why Judicial Elections Stink," 64 *Ohio State Law Journal* 43.
- Glick, Henry and Craig Emmert. 1987. "Selection Systems and Judicial Characteristics: The Recruitment of State Supreme Court Justices," 70 *Judicature* 228.
- Goldberg, Deborah, Craig Holman, and Samantha Sanchez. 2006. *The New Politics of Judicial Elections*. Brennan Center.
- Hall, Melinda Gann. 2001. "State Supreme Courts in American Democracy: Probing the Myths of Judicial Reform," 95 *American Political Science Review* 315.
- Hanssen, F. Andrew. 1999. "The Effect of Judicial Institutions on Uncertainty and the Rate of Litigation: The Election versus Appointment of State Judges," 28 *Journal of Legal Studies* 205–32.
- Hanssen, F. Andrew. 2000. "Independent Courts and Administrative Agencies: An Empirical Analysis of the States," 16 *Journal of Law, Economics and Organization* 534–71.

- Hanssen, F. Andrew. 2002. "On the Politics of Judicial Selection: Lawyers and the State Campaigns for the Merit Plan," 110 *Public Choice* 79-97.
- Hanssen, F. Andrew. 2004. "Learning About Judicial Independence: Institutional Change in the State Courts," 33 *Journal of Legal Studies* 431.
- Harris, Peter. 1986. "Ecology and Culture in the Communication of Precedent Among State Supreme Courts, 1870-1970," 19 *Law & Society Review* 449.
- Helland, Eric and Alex Tabarrok. 2002. "The Effect of Electoral Institutions on Tort Awards," 4 *American Law and Economics Review* 341-70.
- Hirschl, Ran. 2004. *Towards Juristocracy: The Origins and Consequences of the New Constitutionalism*. Cambridge, Ma.: Harvard University Press.
- Holmes, Stephen. 1988. "Precommitment and the Paradox of Democracy," in Jon Elster and Rune Slagstad, eds., *Constitutionalism and Democracy*. Cambridge, UK: Cambridge University Press.
- Holmstrom, Bengt and Milgrom, Paul, 1991. "Multitask Principal-Agent Analyses: Incentive Contracts, Asset Ownership, and Job Design," 7 *Journal of Law, Economics and Organization* 24-52.
- La Porta, Rafael, Florencio López-de-Silanes, Cristian Pop-Eleches, and Andrei Shleifer. 2004. "Judicial Checks and Balances," 112 *Journal of Political Economy* 445-70.
- Landes, William M., and Richard A. Posner. 1980. "Legal Change, Judicial Behavior, and the Diversity Jurisdiction," 9 *Journal of Legal Studies* 367.
- Langer, Laura. 2002. *Judicial Review in State Supreme Courts: A Comparative Study*. Albany: State University of New York Press.
- Langer, Laura, Jody McMullen, Nicholas P. Ray, and Daniel D. Stratton. 2003. "Recruitment of Chief Justices on State Supreme Courts: A Choice Between Institutional and Personal Goals," 65 *Journal of Politics* 656-75.
- Lindquist, Stefanie A. 2007. "Bureaucratization and Balkanization: The Origins and Effects of Decision-Making Norms in the Federal Appellate Courts," 41 *University of Richmond Law Review* 659.
- Liptak, Adam, and Janet Roberts. 2006. "Campaign Cash Mirrors High Court's Rulings," N.Y. Times, Oct. 1, 2006, A1.
- Lott, John R. 2005. "The Judicial Confirmation Process: The Difficulty with Being Smart," 2 *Journal of Empirical Legal Studies* 407.
- Maskin, Eric, and Jean Tirole. 2004. "The Politician and the Judge: Accountability in Government," 94 *American Economic Review* 1034-54.

- McLeod, Aman L. 2007. "Judicial Performance Review: A Balance Between Judicial Accountability and Public Accountability," 34 *Fordham Urban Law Journal* 343.
- Newman, Jon O. 1984. "Between Legal Principles and Neutral Principles: The Legitimacy of Institutional Values," 72 *California Law Review* 200.
- Pinello, Daniel R. 1995. *The Impact of Judicial-Selection Method on State-Supreme-Court Policy: Innovation, Reaction, and Atrophy*. Westport, CT: Greenwood Press.
- Segal, Jeffrey A. and Harold J. Spaeth. 2002. *The Supreme Court and the Attitudinal Model Revisited*. New York: Cambridge University Press.
- Shepherd, Joanna. 2007. "Finger To the Wind: The Influence of Retention Politics on Judges' Decisions," http://papers.ssrn.com/sol3/papers.cfm?abstract_id=997491.
- Sunstein, Cass R., David Schkade, and Lisa M. Ellman. 2004. "Ideological Voting on Federal Courts of Appeals: A Preliminary Investigation," 90 *Virginia Law Review* 301–54.
- Symposium on Rethinking Judicial Selection: A Critical Appraisal of Appointive Selection for State Court Judges. 2007. 34 *Fordham Urban Law Journal* 3.
- Tabarrok, Alexander, and Eric Helland. 1999. "Court Politics: The Political Economy of Tort Awards," 42 *Journal of Law and Economics* 157–88.
- Tarr, G.A. 2003. "Selection of State Appellate Judges: Reform Proposals: Rethinking the Selection of State Supreme Court Justices," 39 *Willamette Law Review* 1445.

APPENDIX
Key Variable Definitions

| Variable | Definition |
|----------------------------|---|
| Total Opinions | Total number of majority, concurring, and dissenting opinions authored by a particular judge in one year (ranging from 1998 to 2000) |
| Outside State Citations | Total number of citations from federal courts outside the circuit that includes the state in question and from courts in other states. Citations are measured in opinions authored up until January 1, 2007 (as tracked in the LEXIS Shepard's database). |
| Opposite_Party | The total number of opposing opinions written against an opposite party judge divided by the total number of opposing opinions written against either a judge of the opposite or same party as the judge in question for the 1998 to 2000 time period. Opposing opinions include dissents written against a majority opinion and majority opinions where a dissenting opinion exists. |
| Opposite_Pool | Total number of majority opinions written by the high court judges of the opposite political party (from the perspective of the judge in question) divided by the total number of majority opinions written by judges of both the same and opposite parties from 1998 to 2000. |
| Independence | Defined as Opposite_Pool minus Opposite_Party. A more negative Independence score occurs when Opposite_Pool < Opposite Party, indicating an increased tendency to write an opposing opinion against an opposite party judge. Conversely, a more positive Independence score indicates a decreased tendency to write an opposing opinion against an opposite party judge. |
| Election Non-Partisan | Indicator variable equal to 1 if the state uses a non-partisan election to select high court justices and 0 otherwise. |
| Election Partisan | Indicator variable equal to 1 if the state uses a partisan election to select high court justices and 0 otherwise. |
| Merit Plan | Indicator variable equal to 1 if the state follows the Missouri Merit Plan or a variant (including the Tennessee Plan) to select High Court justices and 0 otherwise. |
| Tenure | The average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, tble. 1). |
| Political Competition | Folded Ranney index determined for 1995 to 1998 for the state in question (from Bibby and Holbrook 1999). The folded Ranney index ranges from 0.5-1, with higher numbers indicating greater competitiveness. |
| High Political Competition | Indicator Variable equal to 1 if the state in question has a Political Competition Score that is greater than the median for all states and 0 otherwise. |

| | |
|--------------------|--|
| Number of Dissents | Indicator Variable equal to 1 if the judge authoring an opinion is Republican and 0 otherwise. |
| West Key Pages | Number of pages in an opinion associated with the West key pages section (as provided in the West reporter version of the opinion and tabulated on Westlaw). |
| Opinion Length | Number of pages from the start of the opinion to the end of the opinion as provided in the West reporter version of the opinion and tabulated on Westlaw. For majority opinions, we measured from where the authoring judge's actual opinion starts to the end of the majority opinion. |
| Open Secrets | Indicator Variable equal to 1 if the judge authoring the opinion in question has donated to a political candidate and 0 otherwise. Political contributions are tracked by www.opensecrets.org and include Federal Election Commission records of receipts from all individuals who contribute at least \$200 from 1992 to 2006. |

APPENDIX
State-Level Variable Definitions

| Variable | Definition |
|------------------------------------|--|
| Adjusted Associate Justice Salary | For year-level data, the associate justice salary reported in the prior year for the state (so 1997 for 1998 judge-level data) divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the associate justice salary reported in 1997 divided by the cost of living adjustment for 1998. |
| Adjusted Partner Salary | For year-level data, the average partner salary reported for the year in question for the state divided by a cost of living adjustment for the year in question measured for the metro area in which the high court of the state is located. For pooled data, the average partner salary in 1998 divided by the cost of living adjustment for 1998. |
| Stable Court | Indicator variable equal to 1 if the state high court justices stayed the same from 1998 to 2000 and 0 otherwise. |
| Number of Active Judges on Bench | Number of judges who were active at any time from 1998 to 2000 for the state in question. |
| Long-Term Clerk | Indicator variable equal to 1 if state clerks are tenured for more than one year and 0 if tenure is 1 year or less. |
| Number of Clerks Per Judge | Average number of clerks per judge in the 1998 to 2000 time period. |
| Number of Trial Cases in the State | Number of trial cases in the entire state in 1998. |
| Intermediate Appellate Court | Indicator Variable equal to 1 if the opinion is in opposition to the opinion of another judge in the same case and 0 otherwise. In the case of a dissenting opinion written by the judge in question, the opinion is treated as in active opposition to the majority opinion. In the case of a majority opinion by the judge in question, active opposition exists if the majority opinion is opposed by a dissenting opinion. |
| Population | For year level data, the population of the state in millions measured in the year prior to the year in question (so the population in 1997 if the data year is 1998). For pooled data, the population of the state in millions measured for 1997. |
| Gross State Product | Gross State Product (measured as of 1998). |
| Crime Index | For year level data, overall crime rate for the state (including property and violent crime) per 100,000 population from the FBI Crime Report for the year prior to the year in question. For pooled data, the overall crime rate measured for 1997. |
| Total Population in Border States | Total population of all bordering states of the state in question (measured as of 1997). |
| State Age | Age of the state. For year-level data this is defined as the difference between the year in question and the year of admission of the state into the United States. For pooled data, this is defined as the difference between 1998 and the year of admission of the state into |

the United States.

| | |
|------------------------|---|
| Citizen Ideology Score | Measure of citizen ideology based on election results in each district, which are then used to compute a statewide average (ultimately based on interest group ratings of a given state's federal congressional delegation) (from Berry et al. 1998). |
| Misconduct | Number of misconduct removals of judges in the state in question from 1990-1997. |
| Impeached | Number of judicial impeachments in the state in question from 1990-1997. |

APPENDIX
Judge-Level Variable Definitions

| Variable | Definition |
|----------------------------|--|
| Chief Judge | For year-level data, indicator variable equal to 1 if the judge in question is the chief judge of the court in the year in question and 0 otherwise. For pooled data, indicator variable equal to 1 if the judge in question is the chief judge of the court for any year from 1998 to 2000 and 0 otherwise. |
| Court Experience | For year-level data, the difference between the year in question and the year the judge first joined the high court. For pooled data, the difference between 1998 and the year the judge first joined he high court (if the judge started on the court in 1998 or later court experience is set to 0). |
| Post-Law School Experience | The difference between 1998 and the year the judge graduated law school. |
| Retirement Close | Indicator variable equal to 1 if the judge in question retired from the bench in 2001 or earlier and 0 otherwise. |
| USNews Rank | U.S. News Ranking of the judge's law school (measured as of 2005). ²⁰ |
| In-state Law School | Indicator variable equal to 1 if the judge attended a law school located in the state in question and 0 otherwise. |

²⁰ These data were convenient; older data was either accessible or less

APPENDIX - continued
Subject Matter Categories

| Variable | Definition |
|---------------------|--|
| Administrative | Review of Agency/Government Decisionmaking (not in another subject matter category). Also includes Government Actions (e.g., State suit to comply with state statute that does not fit in other categories); private actions suing state actors for negligence, etc (unless the case involves prisoner rights which is included in the “Criminal” category of cases). |
| Attorney and Client | Attorney Misconduct; Attorney fees (unless fits in one of above categories); Disbarment; contempt of court order against attorney |
| Capital Punishment | Capital Punishment-related actions. |
| Church and State | Pledge of Allegiance; Funding for Private Religious Schools; Prayer in School; Ten Commandments |
| Commercial | Contracts; Insurance; Private arbitration; Creditor v. Debtor; Lessor-Lessee; Usury Laws; Franchise v. Franchisor; Employment Contractual Disputes; Corporate Law; Piercing the Corporate Veil; Tax; Bankruptcy; Enforcement of mechanics lien; Implied warrant of merchantability |
| Criminal | Sentencing Guidelines; Prisoners Rights; Murder; Rape; Drugs/Controlled Substances; Attorney-Client Privilege in Criminal Context; Grand Jury-related; Juvenile Criminals. Excludes Capital Punishment cases. |
| Family | Divorce; Adoption; Child Support; Probate/Inheritance |
| First Amendment | Employment issues (excluding employment contractual disputes); ERISA; National Labor Relations Board (NLRB); Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker’s Compensation claims; Retaliatory Discharge claims. |
| Labor | Employment issues (excluding (1) employment contractual disputes that are not Workers Comp or state administrative wage rate related—these go to “Commercial” and (2) excluding discrimination-type claims that fit in “Civil Rights”); ERISA; NLRB; Occupational Safety and Health Act (OSHA); Fair Labor Standards Act (FLSA); Wrongful Discharge; Labor Management Relations Act (LMRA); Family and Medical Leave Act (FMLA); Employee Benefits; Worker's Compensation claims; Retaliatory Discharge claims; State Wage Rate Claims |

| | |
|----------|---|
| Property | Takings claims; Zoning issues; Property rights; Property Licensing-Related or Permit-Related; Landlord-Tenant-Related. |
| Rights | Race Discrimination; Sex Discrimination; Affirmative Action; Civil Rights; Age Discrimination; Privacy; Handicap Discrimination; Abortion (Includes discrimination in employment context cases); Voting Rights-Voting Related |
| Torts | Federal Tort Related Act; Medical Malpractice; Products Liability; Wrongful Death; Libel; etc. |
| Other | All other cases. |

Table 1

| Appointed | Merit Selection | Non-Partisan Election | Partisan Election |
|------------------|------------------------|------------------------------|--------------------------|
| Connecticut | Alaska | Georgia | Alabama |
| Delaware | Arizona | Idaho | Arkansas |
| Hawaii | Colorado | Kentucky | Illinois |
| Massachusetts | Iowa | Louisiana | Mississippi |
| Maine | Indiana | Michigan | North Carolina |
| New Hampshire | Kansas | Minnesota | New Mexico |
| New Jersey | Maryland | Montana | Pennsylvania |
| New York | Missouri | North Dakota | Texas |
| Rhode Island | Nebraska | Nevada | West Virginia |
| Vermont | Oklahoma | Ohio | |
| South Carolina | South Dakota | Oregon | |
| Virginia | Utah | Washington | |
| | Wyoming | Wisconsin | |
| | California | | |
| | Florida | | |
| | Tennessee | | |

Table 2: Productivity

| | Number of Judge-Years | Mean Total Opinions Per Year | Mean Majority Opinions Per Judge Per Year | Mean Dissenting Opinions Per Judge Per Year |
|-----------------------|------------------------------|-------------------------------------|--|--|
| Partisan Election | 229 | 31.3 | 19.4 | 8.0 |
| Non-Partisan Election | 277 | 27.5 | 17.9 | 6.9 |
| Merit Selection | 336 | 23.6 | 18.1 | 3.7 |
| Appointed | 232 | 20.9 | 16.7 | 3.0 |

Judge-years represent yearly data for each judge in our sample. Total Opinions includes majority, dissenting, and concurring opinions.

Table 3: Mean Citations Per Majority Opinion

| | Number of Majority Opinions | Outside Federal Court | Other State Court | US Supreme Court | Outside State Citations | Law Review Citations |
|-----------------------|------------------------------------|------------------------------|--------------------------|-------------------------|--------------------------------|-----------------------------|
| Partisan Election | 4447 | 0.108 | 0.458 | 0.005 | 0.572 | 1.765 |
| Non-Partisan Election | 4952 | 0.103 | 0.603 | 0.007 | 0.712 | 1.856 |
| Merit Selection | 6079 | 0.134 | 0.631 | 0.009 | 0.774 | 1.840 |
| Appointed | 4007 | 0.211 | 0.657 | 0.006 | 0.872 | 2.273 |

Home Federal Court includes all citations from a federal district or circuit court located in the circuit that contains the state in question. Outside Federal Court includes all citations from a federal district or circuit court located in a circuit that does not contain the state in question. Other State Court includes all citations from state courts outside of the state in question. US Supreme Court includes all citations from the U.S. Supreme Court. Outside Citation is the sum of Outside Federal Court + Other State Court + US Supreme Court. All citations are from the LEXIS Shepard's database and are tracked up until January 1, 2007. Law Review Citations are for law reviews as tracked by the LEXIS Shepard's database (until January 1, 2007).

Table 4: Independence**Panel A**

| | Number of Judges | Mean Independence | Independence_Indicator (1=Independent; 0=Partisan) |
|-----------------------|-------------------------|--------------------------|---|
| Partisan Election | 83 | -0.019 | 0.453 |
| Non-Partisan Election | 89 | -0.066 | 0.345 |
| Merit Selection | 108 | -0.030 | 0.433 |
| Appointed | 72 | -0.027 | 0.435 |

Independence is defined as the $\text{Opposite_Party} - \text{Opposite_Pool}$. Opposite_Party is the number of opposing opinions written against a judge of the opposite party divided by the number of opposing opinions written against a judge of either the opposite or same party from 1998 to 2000. Opposite Pool is the total number of majority opinions authored by an opposite party judge divided by the total number of majority opinions authored by either an opposite or same party judge from 1998 to 2000. $\text{Independence_Indicator}$ is defined as 1 if Independence is greater or equal to zero and 0 otherwise. Only judges for whom we could identify a political party were included in the analysis. Only judges from states that had a mixture of judges from different political parties were included.

Panel B

| | Less than or Equal to Median Number of Authored Dissents | Greater than Median Number of Authored Dissents | <i>p-value</i> |
|-----------------------|---|--|-----------------------|
| Partisan Election | -0.021 | -0.018 | 0.9063 |
| Non-Partisan Election | -0.082 | -0.055 | 0.5911 |
| Merit Selection | -0.033 | -0.025 | 0.7801 |
| Appointed | -0.029 | -0.023 | 0.9387 |

| | Open Secrets=0 | Open Secrets=1 | <i>p-value</i> |
|-----------------------|-----------------------|-----------------------|-----------------------|
| Partisan Election | 0.057 | -0.052 | 0.0001 |
| Non-Partisan Election | -0.032 | -0.115 | 0.0893 |
| Merit Selection | -0.028 | -0.037 | 0.7817 |
| Appointed | -0.044 | 0.067 | 0.1768 |

Table 5: Tenure Data

| | Number of States | Mean | Std. Dev. | Min. | Max. |
|-----------------------|-------------------------|-------------|------------------|-------------|-------------|
| Partisan Election | 10 | 6.8 | 2.4 | 3.5 | 11.1 |
| Non-Partisan Election | 13 | 7.0 | 1.5 | 4.6 | 9.1 |
| Merit Selection | 17 | 10.0 | 4.5 | 4 | 19.1 |
| Appointed | 12 | 9.2 | 3.1 | 5 | 14.9 |
| Total | 52 | 8.5 | 3.4 | 3.5 | 19.1 |

Tenure is defined as the average tenure of high court judges for the state in question, measured as of the spring of 1997 (from Hanssen 1999, tble. 1).

Table 6: Productivity

| Dependent Variable | Model 1 | Model 2 |
|--|----------------------|----------------------|
| | In(Total Opinions) | In(Total Opinions) |
| Independent Variables | | |
| Election Partisan | 0.556** (6.290) | |
| Election Non-Partisan | 0.478** (5.700) | |
| Merit Plan | 0.426** (5.050) | |
| Tenure | | -0.045** (-5.870) |
| Chief Judge | -0.173** (-2.800) | -0.190** (-3.030) |
| Court Experience | 0.011* (2.440) | 0.014** (2.950) |
| Post-Law School Experience | 0.007* (2.250) | 0.008* (2.430) |
| Retirement Close | -0.234** (-4.230) | -0.252** (-4.560) |
| Adjusted Associate Justice Salary | 0.001** (3.100) | 0.001** (4.750) |
| Adjusted Partner Salary | 0.000 (-0.910) | 0.000 (-0.920) |
| Stable Court | 0.264** (4.120) | 0.303** (4.780) |
| Number of Active Judges on Bench | 0.000 (0.010) | 0.045 (2.530) |
| Long-Term Clerk | -0.134** (-2.610) | -0.029 (-0.580) |
| Number of Clerks Per Judge | -0.019 (-0.430) | 0.000 (0.010) |
| In(Number of Trial Cases in the State) | 0.010 (0.250) | -0.002 (-0.060) |
| Intermediate Appellate Court | 0.075 (0.660) | 0.097 (0.880) |
| In(State Population) | 0.835** (4.420) | 1.051** (5.870) |
| In(Total Population in Border States) | -0.057** | -0.046** |

| | | |
|-------------------------|----------------------|----------------------|
| | (-5.080) | (-4.200) |
| Crime Index | 0.000** (-5.050) | 0.000** (-6.050) |
| State Age | 0.005** (5.560) | 0.002** (3.170) |
| ln(Gross State Product) | -0.941** (-5.490) | -1.155** (-7.070) |
| Citizen Ideology Score | 0.002 (1.060) | -0.003 (-1.150) |
| Constant | 12.249** (6.910) | 15.025** (8.810) |
| Year Fixed Effects | Yes | Yes |
| N | 1002 | 1002 |
| R2 | 0.2004 | 0.2019 |

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

+ Coefficient significant at the 10% level or less.

* Coefficient significant at the 5% level or less.

** Coefficient significant at less than the 1% level.

Table 7: Productivity and Political Competition

| | Model 1 | Model 2 |
|---|----------------------|---------------------|
| Dependent Variable | In(Total Opinions) | In(Total Opinions) |
| Independent Variables | | |
| Election Partisan | 0.958** (7.410) | |
| Election Non-Partisan | 0.739** (6.100) | |
| Merit Plan | 0.533** (4.280) | |
| Tenure | | -0.031* (-1.990) |
| High Political Competition | 0.299** (2.750) | 0.097 (0.540) |
| Election Partisan x High Political Competition | -0.743** (-4.490) | |
| Election Non-Partisan x High Political Competition | -0.554** (-3.570) | |
| Merit Plan x High Political Competition | -0.169 (-1.310) | |
| Tenure x High Political Competition | | -0.016 (-0.870) |
| Constant | 11.970** (6.200) | 15.831** (8.340) |
| Year Fixed Effects | Yes | Yes |
| Judge Controls | Yes | Yes |
| State Controls | Yes | Yes |
| N | 979 | 979 |
| R2 | 0.2301 | 0.2055 |

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

+ Coefficient significant at the 10% level or less.

* Coefficient significant at the 5% level or less.

** Coefficient significant at less than the 1% level.

Table 8: Citations

| | Model 1 | Model 2 | Model 3 |
|---|--------------------------------|--------------------------------|--------------------------------|
| Dependent Variable | ln(Outside State Citations) | ln(Outside State Citations) | ln(Outside State Citations) |
| Independent Variables | | | |
| Election Partisan | -0.061** (-3.070) | -0.061* (-2.260) | |
| Election Non-Partisan | -0.047* (-2.580) | -0.089** (-3.690) | |
| Merit Plan | -0.081** (-4.100) | -0.109** (-4.140) | |
| Tenure | | | 0.004 (1.140) |
| High Political Competition | | -0.016 (-0.770) | 0.084* (2.300) |
| Election Partisan x High Political Comp. | | -0.008 (-0.260) | |
| Election Non-Partisan x High Political Comp. | | 0.131** (4.030) | |
| Merit Plan x High Political Comp. | | 0.051* (2.010) | |
| Tenure x High Political Comp. | | | -0.006 (-1.420) |
| Number of Dissents | 0.060** (6.840) | 0.060** (6.710) | 0.062** (7.020) |
| West Key Pages | 0.000 (0.450) | 0.000 (0.180) | 0.000 (0.380) |
| Opinion Length | 0.034** (20.320) | 0.034** (20.020) | 0.034** (20.190) |
| Administrative | -0.077** (-3.000) | -0.075** (-2.870) | -0.077** (-2.940) |
| Attorney and Client | 0.057+ (1.860) | 0.049 (1.560) | 0.051+ (1.650) |
| Capital Punishment | -0.112** (-3.400) | -0.116** (-3.530) | -0.115** (-3.500) |
| Church and State | 0.183 (0.760) | 0.175 (0.720) | 0.179 (0.720) |
| Commercial | 0.160** (6.330) | 0.159** (6.230) | 0.160** (6.250) |
| Criminal | 0.034 | 0.026 | 0.028 |

| | | | |
|--------------------|----------------------|----------------------|----------------------|
| | (1.470) | (1.100) | (1.170) |
| Family | 0.077** (2.920) | 0.070** (2.610) | 0.070** (2.600) |
| First Amendment | 0.126 (1.570) | 0.152+ (1.780) | 0.162+ (1.920) |
| Labor | -0.053* (-2.110) | -0.055* (-2.160) | -0.053* (-2.080) |
| Property | -0.005 (-0.180) | -0.006 (-0.210) | -0.002 (-0.090) |
| Rights | 0.110** (2.650) | 0.093* (2.180) | 0.106* (2.510) |
| Torts | 0.145** (5.660) | 0.147** (5.670) | 0.148** (5.670) |
| Constant | -3.031** (-8.530) | -2.578** (-6.520) | -3.067** (-8.190) |
| Year Fixed Effects | Yes | Yes | Yes |
| Court Controls | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes |
| N | 18360 | 17726 | 17726 |
| R2 | 0.1309 | 0.1342 | 0.1322 |

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

+ Coefficient significant at the 10% level or less.

* Coefficient significant at the 5% level or less.

** Coefficient significant at less than the 1% level.

Table 9: Dissents

| | Model 1 | Model 2 |
|---|---------------------------|---------------------------|
| Dependent Variable | ln(1+Dissent Opinions) | ln(1+Dissent Opinions) |
| Independent Variables | | |
| Election Partisan | 0.948** (6.190) | |
| Election Non-Partisan | 0.759** (5.520) | |
| Merit Plan | 0.005 (0.040) | |
| Tenure | | -0.113** (-5.960) |
| High Political Competition | -0.041 (-0.310) | -0.959** (-4.220) |
| Election Partisan x High Political Competition | -0.599** (-2.680) | |
| Election Non-Partisan x High Political Competition | -0.410+ (-1.950) | |
| Merit Plan x High Political Competition | 0.423* (2.520) | |
| Tenure x High Political Competition | | 0.087** (3.670) |
| Constant | 3.402 (1.430) | 4.407+ (1.770) |
| Year Fixed Effects | Yes | Yes |
| Court Controls | Yes | Yes |
| State Controls | Yes | Yes |
| N | 979 | 979 |
| R2 | 0.2500 | 0.2070 |

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

+ Coefficient significant at the 10% level or less.

* Coefficient significant at the 5% level or less.

** Coefficient significant at less than the 1% level.

Table 10: Independence

| | Model 1 | Model 2 | Model 3 |
|--|-------------------------------|----------------------|----------------------|
| Dependent Variable | Independence | Independence | Independence |
| Independent Variables | | | |
| Election Partisan | 0.052 (0.810) | 0.108 (1.630) | 0.074 (0.900) |
| Election Non-Partisan | -0.068 (-1.100) | -0.019 (-0.310) | -0.114 (-1.350) |
| Merit Plan | -0.053 (-0.780) | -0.034 (-0.490) | -0.007 (-0.080) |
| Number of Dissenting Opinions | 0.003* (2.300) | 0.003* (2.320) | 0.003* (2.350) |
| Number of Dissenting Opinions x Election Partisan | -0.006** (-3.310) | -0.005** (-2.790) | -0.005** (-2.640) |
| Number of Dissenting Opinions x Election Non-Partisan | 0.001 (0.470) | 0.002 (1.000) | 0.002 (0.950) |
| Number of Dissenting Opinions x Merit Plan | -0.003 (-0.920) | -0.002 (-0.770) | -0.003 (-0.930) |
| Opensecrets | | 0.095 (1.450) | 0.091 (1.310) |
| Opensecrets x Election Partisan | | -0.198** (-2.700) | -0.197* (-2.580) |
| Opensecrets x Election Non-Partisan | | -0.194* (-2.370) | -0.171* (-2.020) |
| Opensecrets x Merit Plan | | -0.120 (-1.580) | -0.111 (-1.240) |
| High Political Comp. | | | -0.007 (-0.090) |
| Election Partisan x High Political Comp. | | | 0.046 (0.370) |
| Election Non-Partisan x High Political Comp. | | | 0.225* (2.000) |
| Merit Plan x High Political Comp. | | | -0.016 (-0.140) |
| Constant | 2.894 ⁺ (1.890) | 1.862 (1.220) | 5.185* (2.420) |
| Subject Matter Controls | Yes | Yes | Yes |
| State Controls | Yes | Yes | Yes |
| Judge Controls | Yes | Yes | Yes |
| N | 327 | 327 | 318 |

| | | | |
|----|--------|--------|--------|
| R2 | 0.2037 | 0.2336 | 0.2584 |
|----|--------|--------|--------|

The t-statistics (in parentheses) are calculated using Huber-White robust standard errors. Variable definitions are in the Appendix.

- ⁺ Coefficient significant at the 10% level or less.
- ^{*} Coefficient significant at the 5% level or less.
- ^{**} Coefficient significant at less than the 1% level.

Table 11: Selection Mechanisms and Quality

| <i>Selection Mechanism</i> | <i>Tenure</i> | <i>Productivity</i> | <i>Independence</i> | <i>Quality</i> |
|----------------------------|---------------|---------------------|---------------------|----------------|
| Election Partisan | Lower | High | Unclear | Low |
| Election Non-Partisan | Lower | Middle | Unclear | Middle |
| Merit Plan | Higher | Middle | Unclear | Middle |
| Appointed | Higher | Low | Unclear | High |

Table 12

| | Mean Open secrets | Mean Salary Difference (Partner – Associate Justice Salary) | Mean Stable court | Mean Court Experience | Mean Post-Law School Experience |
|-----------------------|----------------------|---|----------------------|-----------------------------|--|
| Election Partisan | 0.67 ^a | 1004 ^a | 0.08 ^a | 5.77 | 29.36 ^b |
| Election Non-Partisan | 0.36 | 872 | 0.28 | 6.67 | 30.72 |
| Merit Plan | 0.22 | 972 | 0.19 | 8.47 | 32.03 |
| Appointed | 0.14 | 822 | 0.25 | 7.28 | 32.27 |

| | Mean Retirement Close | Mean Tenure | Mean In-State Law School | Mean Active Judges | Mean US News Rank |
|-----------------------|-----------------------------|-------------------|--------------------------------|-----------------------|-------------------------|
| Election Partisan | 0.42 ^c | 7.10 ^a | 0.70 ^a | 9.55 ^a | 62.94 ^a |
| Election Non-Partisan | 0.22 | 7.09 | 0.67 | 8.25 | 58.78 |
| Merit Plan | 0.23 | 10.13 | 0.70 | 7.65 | 61.40 |
| Appointed | 0.30 | 9.58 | 0.33 | 7.71 | 32.34 |

^at-test of difference in means for Election Partisan and Appointed Judge is significant at the <1% level.

^bt-test of difference in means for Election Partisan and Appointed Judge is significant at the 5% level.

^ct-test of difference in means for Election Partisan and Appointed Judge is significant at the 10% level.

Readers with comments should address them to:

Professor Eric A. Posner
 University of Chicago Law School
 1111 East 60th Street
 Chicago, IL 60637
 eric_posner@law.uchicago.edu

Chicago Working Papers in Law and Economics
(Second Series)

For a listing of papers 1–299 please go to Working Papers at
<http://www.law.uchicago.edu/Lawecon/index.html>

300. Adam B. Cox, The Temporal Dimension of Voting Rights (July 2006)
301. Adam B. Cox, Designing Redistricting Institutions (July 2006)
302. Cass R. Sunstein, Montreal vs. Kyoto: A Tale of Two Protocols (August 2006)
303. Kenneth W. Dam, Legal Institutions, Legal Origins, and Governance (August 2006)
304. Anup Malani and Eric A. Posner, The Case for For-Profit Charities (September 2006)
305. Douglas Lichtman, Irreparable Benefits (September 2006)
306. M. Todd Henderson, Paying CEOs in Bankruptcy: Executive Compensation when Agency Costs Are Low (September 2006)
307. Michael Abramowicz and M. Todd Henderson, Prediction Markets for Corporate Governance (September 2006)
308. Randal C. Picker, Who Should Regulate Entry into IPTV and Municipal Wireless? (September 2006)
309. Eric A. Posner and Adrian Vermeule, The Credible Executive (September 2006)
310. David Gilo and Ariel Porat, The Unconventional Uses of Transaction Costs (October 2006)
311. Randal C. Picker, Review of Hovenkamp, The Antitrust Enterprise: Principle and Execution (October 2006)
312. Dennis W. Carlton and Randal C. Picker, Antitrust and Regulation (October 2006)
313. Robert Cooter and Ariel Porat, Liability Externalities and Mandatory Choices: Should Doctors Pay Less? (November 2006)
314. Adam B. Cox and Eric A. Posner, The Second-Order Structure of Immigration Law (November 2006)
315. Lior J. Strahilevitz, Wealth without Markets? (November 2006)
316. Ariel Porat, Offsetting Risks (November 2006)
317. Bernard E. Harcourt and Jens Ludwig, Reefer Madness: Broken Windows Policing and Misdemeanor Marijuana Arrests in New York City, 1989–2000 (December 2006)
318. Bernard E. Harcourt, Embracing Chance: Post-Modern Meditations on Punishment (December 2006)
319. Cass R. Sunstein, Second-Order Perfectionism (December 2006)
320. William M. Landes and Richard A. Posner, The Economics of Presidential Pardons and Commutations (January 2007)
321. Cass R. Sunstein, Deliberating Groups versus Prediction Markets (or Hayek’s Challenge to Habermas) (January 2007)
322. Cass R. Sunstein, Completely Theorized Agreements in Constitutional Law (January 2007)
323. Albert H. Choi and Eric A. Posner, A Critique of the Odious Debt Doctrine (January 2007)
324. Wayne Hsiung and Cass R. Sunstein, Climate Change and Animals (January 2007)
325. Cass R. Sunstein, Cost-Benefit Analysis without Analyzing Costs or Benefits: Reasonable Accommodation, Balancing and Stigmatic Harms (January 2007)
326. Cass R. Sunstein, Willingness to Pay versus Welfare (January 2007)
327. David A. Weisbach, The Irreducible Complexity of Firm-Level Income Taxes: Theory and Doctrine in the Corporate Tax (January 2007)
328. Randal C. Picker, Of Pirates and Puffy Shirts: A Comments on “The Piracy Paradox: Innovation and Intellectual Property in Fashion Design” (January 2007)
329. Eric A. Posner, Climate Change and International Human Rights Litigation: A Critical Appraisal (January 2007)
330. Randal C. Picker, Pulling a Rabbi Out of His Hat: The Bankruptcy Magic of Dick Posner (February 2007)
331. Bernard E. Harcourt, Judge Richard Posner on Civil Liberties: Pragmatic (Libertarian) Authoritarian (February 2007)

332. Cass R. Sunstein, *If People Would Be Outraged by Their Rulings, Should Judges Care?* (February 2007)
333. Eugene Kontorovich, *What Standing Is Good For* (March 2007)
334. Eugene Kontorovich, *Inefficient Customs in International Law* (March 2007)
335. Bernard E. Harcourt, *From the Asylum to the Prison: Rethinking the Incarceration Revolution. Part II: State Level Analysis* (March 2007)
336. Cass R. Sunstein, *Due Process Traditionalism* (March 2007)
337. Adam B. Cox and Thomas J. Miles, *Judging the Voting Rights Act* (March 2007)
338. M. Todd Henderson, *Deconstructing Duff & Phelps* (March 2007)
339. Douglas G. Baird and Robert K. Rasmussen, *The Prime Directive* (April 2007)
340. Cass R. Sunstein, *Illusory Losses* (May 2007)
341. Anup Malani, *Valuing Laws as Local Amenities* (June 2007)
342. David A. Weisbach, *What Does Happiness Research Tell Us about Taxation?* (June 2007)
343. David S. Abrams and Chris Rohlf, *Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment* (June 2007)
344. Christopher R. Berry and Jacob E. Gersen, *The Fiscal Consequences of Electoral Institutions* (June 2007)
345. Matthew Adler and Eric A. Posners, *Happiness Research and Cost-Benefit Analysis* (July 2007)
346. Daniel Kahneman and Cass R. Sunstein, *Indignation: Psychology, Politics, Law* (July 2007)
347. Jacob E. Gersen and Eric A. Posner, *Timing Rules and Legal Institutions* (July 2007)
348. Eric A. Posner and Adrian Vermeule, *Constitutional Showdowns* (July 2007)
349. Lior Jacob Strahilevitz, *Privacy versus Antidiscrimination* (July 2007)
350. Bernard E. Harcourt, *A Reader's Companion to Against Prediction: A Reply to Ariela Gross, Yoram Margalioth, and Yoav Sapir on Economic Modeling, Selective Incapacitation, Governmentality, and Race* (July 2007)
351. Lior Jacob Strahilevitz, *"Don't Try This at Home": Posner as Political Economist* (July 2007)
352. Cass R. Sunstein, *The Complex Climate Change Incentives of China and the United States* (August 2007)
353. David S. Abrams and Marianne Bertrand, *Do Judges Vary in Their Treatment of Race?* (August 2007)
354. Eric A. Posner and Cass R. Sunstein, *Climate Change Justice* (August 2007)
355. David A. Weisbach, *A Welfarist Approach to Disabilities* (August 2007)
356. David S. Abrams, *More Time, Less Crime? Estimating the Deterrent Effect of Incarceration using Sentencing Enhancements* (August 2007)
357. Stephen J. Choi, G. Mitu Gulati and Eric A. Posner, *Professionals or Politicians: The Uncertain Empirical Case for an Elected Rather than Appointed Judiciary* (August 2007)