

JUSTICES ON THE BALLOT: CONTINUITY AND CHANGE IN STATE SUPREME COURT ELECTIONS

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NOTE TO READERS: This is the “final” version of Chapter 3 of this book. Because of the way that I’ve produced this extract, there may be a few missing references.

CHAPTER 3: SO WHAT, DO JUDICIAL ELECTIONS MATTER?

I. INTRODUCTION

An initial question to be answered about method of judicial selection generally and judicial elections more specifically is “so what?” Does the method of selection or the form of election matter in anything more than a symbolic fashion? There is an extensive body of research by political scientists starting in the 1960s (Jacob 1964) that has sought to understand what if any differences arise as a result of the way that judges are selected. Much of this research has focused on whether various selection systems produce judges with different background characteristics such as race, ethnicity, gender, religion, prior practice, type of law school attended, and local connections (Alozie 1988, 1990, 1996; Bratton and Spill 2002; Canon 1972; Dubois 1983; Dudley 1997; Emmert and Glick 1988; Esterling and Andersen 1999; Flango and Ducat 1979; Glick and Emmert 1987; Graham 1990; Hurwitz and Lanier 2003). While some of the early research considered whether the manner of selection and retention affects the decisions that judges make (Atkins and Glick 1974; Nagel 1973), starting in the early 1990s the possible impact on decisions became the central question for researchers looking at the impact of selection methods (Hall 1992, 1995; Hall and Brace 1992; Pinello 1995).

One difficulty in this body of research is that it frequently looks at the formal selection system, counting states as selecting judges through election even when most judges initially came to the bench through appointments to fill midterm vacancies. Moreover, much of the research does not consider the distinction between how judges initially obtain their positions and the mechanism through which they retain those positions. As was detailed in Chapter Two, there are many important variations, and in a large number of states there are major differences

between initial selection and retention. Drawing on ambition theory (Schlesinger 1966), one should expect that for judges who want to retain their positions—those with what Schlesinger labeled “static ambition,” attention should focus on who will decide the retention question rather than on who was responsible for the judge’s initial selection. While initial selection method may account for differences in judges’ demographics, how judges are retained is more likely to account for what judges do when they make decisions.

A second issue regarding the impact of selection and retention is reflected in claims by critics of judicial election that “judicial elections are undermining public confidence in the fairness and impartiality of the courts” (Geyh 2003; see also Phillips 2003:144) and that election spending by special interests “threatens the integrity of our courts” (Jamieson and Hennessy 2007; see also O’Connor 2007) . At least some studies of public opinion show a widespread belief that elected judges are biased toward those who contribute to their election coffers (see Gibson 2012:188n11, citing a survey done for Justice at Stake). Simple democratic theory suggests that judges who must face the electorate in order to retain their positions should be more attentive to the preferences of their constituencies than are judges who do not have to be concerned with their electoral futures (Schlesinger 1966) . All of these points accord with what many people would see as common sense, but does that common sense reflect actual reality? That is the question considered in this chapter.

In thinking about the question of whether judicial elections matter it is important to distinguish between the specific individuals who serve on a court and the processes of selection and retention. As suggested by the discussion in Chapter One of events related to the Wisconsin Supreme Court, it is easy to look at the outcome of a judicial election and say that if Candidate X had won rather than Candidate Y, the decision in Case A would have been different. However,

the same analysis applies to judges selected through appointment processes, either in terms of who is doing the appointing or in terms of whom the appointer chooses to appoint. Imagine how the history of recent Supreme Court decisions might have been different if Richard Nixon's nomination of Clement Haynesworth had been confirmed by the Senate and Harry Blackmun had spent the rest of his career as a federal appellate judge in Minnesota, or if President George H.W. Bush had chosen someone other than David Souter, or if Thurgood Marshall had stepped down while Jimmy Carter was President and had been replaced by someone other than Clarence Thomas (Leonhart 2014). The question of concern here is not who is selected but whether elections matter in ways beyond the specific individuals who are selected or retained through an electoral process.

This chapter examines three distinct ways that judicial elections generally and state supreme court elections more specifically can matter in real terms:

- The impact of elections and election campaigns on a court's legitimacy
- The impact of elections, direct or indirect, on the decisions judges make
- The impact of campaign contributions on the decisions judges make

The discussion that follows draws heavily on extant research adding several new analyses concerning several specific questions.

II. SUPREME COURT ELECTIONS AND LEGITIMACY

A. Introduction

There are three hypotheses that can be derived from the claim that elections undermine public confidence in the fairness and impartiality of the courts. Firstly, if the claim is true, one would expect that, on average, citizens' views of the courts in states without elections should be

more positive than in states using elections, particularly partisan or nonpartisan elections.

Secondly, some observers have argued that the negative campaign advertisements, particularly those on television, reduces the public's respect and support for the courts (see Gibson 2008b:60)

.¹ Thirdly, as suggested above, the need for candidates in judicial elections to raise campaign funds may lead the public to believe that such fundraising leads to judicial bias.

B. Impact of Method of Selection and Retention on Legitimacy

A major challenge for research on the impact of the use of elections to select and/or retain judges on legitimacy, support, and trust in the state courts is the sparsity of survey data about state courts, particularly data that can be compared over time and/or across states. The problem is even greater if one wants to zero in on state supreme courts because most of the extant surveys asked about either “state courts” or “local courts” generally and not about state supreme courts specifically. A further complication that this presents is that in some states how judges are selected varies by level of court, and in a few states, even geographically with regard to trial courts. A final complication has to do with how courts are labeled or the presence of multiple courts of last resort. In New York the “supreme court” is both the trial court and the intermediate court of appeals with the court of last resort carrying the name Court of Appeals. In Texas and Oklahoma there are two separate courts of last resort with the state “supreme court” dealing with noncriminal matters and a court of criminal appeals having the final say in criminal cases. It is important to keep these complications in mind as I discuss the research that examines the linkage between selection/retention methods and legitimacy.

¹ I leave for Chapter Five the consideration of whether either negative advertising systematically affects the outcome of state supreme court elections.

Wasmann et al. (1986) reanalyzed data from a national survey conducted in 1977 for the National Center for State Courts (Yankelovich 1978);² they compared the public's view of the state and local courts across formal selection systems (appointment, Missouri Plan, partisan election, and nonpartisan election), and found little variation. Subsequently, several scholars examined the impact of the formal selection method on the public's view of "courts in your community" using a 1999 survey, again done on behalf of the National Center for State Courts (National Center for State Courts 1999). One of those analyses (Benesh 2006) reported a small impact for selection method with respondents in states relying on partisan elections having a lower level of "confidence" than those in other states.³ However, a reanalysis of the same data using a different set of control variables (Kelleher and Wolak 2007) failed to find a relationship between partisan elections and confidence in the courts. Using a 2001 survey done for Justice at Stake,⁴ Cann and Yates (2008) constructed a multi-item index of diffuse, or general, support for the state courts.⁵ They found a negative relationship between diffuse support and both partisan and nonpartisan elections, but that effect was limited to those respondents who self-reported a low level of knowledge of the workings of their state court system.

Gibson (2012:49-52) briefly reported an analysis comparing citizens' views of the legitimacy of the state supreme court in states that do and do not use some form of elections to choose and/or retain members of the state supreme court. Using data from a small national survey conducted in 2007, he found that the use of popular elections of any type in choosing or

² The Yankelovich survey was the first national survey focused specifically on the public's views of the courts. There were prior studies (e.g., Curran 1977:232-34) that asked about courts in a nonspecific way; there is no way to know whether respondents in such studies were thinking of federal courts, state courts, or both.

³ One uncertainty in this analysis is how states were coded given that in some states partisan elections are used for some state courts while appointment or the Missouri Plan are used for other courts.

⁴ The questionnaire along with marginal frequencies for this survey can be found at http://www.justiceatstake.org/media/cms/JASNationalSurveyResults_6F537F99272D4.pdf (last visited June 5, 2013).

⁵ The four items in the index were "judges are trustworthy and honest," "judges are fair," "courts provide equal justice," and "court decisions are based on facts and law."

retaining members of the state supreme court had no effect on the perceived legitimacy of the court.⁶ However, drawing on studies based on survey experiments in Kentucky (Gibson 2012) and Pennsylvania (Gibson et al. 2011), Gibson (2010:107) found that “judicial elections by themselves enhance judicial legitimacy.”

The most recent national study focused on the public’s views of state courts is a 2009 survey conducted for the National Center for State Courts by Princeton Survey Research Associates International (2009).⁷ The survey included six questions related to the public’s view of the state courts:

- a question that asked respondents to indicate their level of confidence (a lot, some, not too much, no confidence) in various state institutions including “state courts”;
- a question that asked respondents whether with regards to the amount of control that the governor and members of the legislature have “over state judges and the decisions they make in court,” the governor and state legislators “should have more control than they do now, less control, or about ... the same amount of control”;
- four additional questions asked respondents to choose between pairs of statements regarding the state courts:
 1. Decisions are too often mixed up in politics *versus* courts put politics aside in making their decisions.
 2. State courts can be trusted to make decision that are right for the state as a whole *versus* often don’t give enough consideration to what’s right for the state as a whole.

⁶ Gibson measures legitimacy using a factor score that is based heavily on three items: accepts that the decisions of the state supreme court are fair and impartial, believes that the judge can be fair and impartial, considers the state supreme court legitimate (Gibson 2009:1293). While the size of Gibson’s national sample was about 350 overall, with only about 100 respondents in states without elections, the reported difference in Gibson’s measure of legitimacy is zero to two decimal places (p. 172).

⁷ David Rottman of the National Center generously made these data available to me for reanalysis.

3. The state supreme court should not be able to decide as many controversial issues as it does now *versus* it's important for the state supreme court to maintain its ability to decide such issues.
4. It's important for judges on the state supreme court to be independent and not too influenced by what others think *versus* it's better for judges to be less independent and pay more attention to what the people think.

A simple reanalysis of the responses to these six questions individually and in the form of two scales, one combining all six items and one combining the two items that specifically referenced the state supreme court, showed no statistically significant differences among respondents based on the selection/retention system used for their state's supreme court.⁸ A regression analysis that included a range of control variables also failed to produce any evidence of differences based on the type of selection system. Details of these analyses are in this chapter's Appendix A.

C. Impact of Election Processes on Legitimacy

Chapter Five discusses changing patterns of campaign contributions and expenditures and the frequency of television advertising, both positive advertising and negative advertising. That discussion shows a clear pattern of increased spending but that negative advertising is less frequent or dominant than many critics contend. What impact do these aspects of elections have on the public's view of the courts?

⁸ One caveat about the findings vis-à-vis questions asked about the "state supreme court" is that, as noted previously, in the state of New York, the "supreme court" is the higher trial court and the intermediate appellate court; the state court of last resort in New York, is called the Court of Appeals.

1. Impact of Campaign Contributions and Expenditures

Critics argue that the money aspect of judicial elections puts the judges' impartiality into question (American Bar Association 2003:viii; Caplan 2012; Kaplan 1987; Kaplan and Davidson 1998:12; Moyer 2010; Talbot et al. 1999) and that negative advertising has costs for the general legitimacy of and/or trust in the courts (National Center for State Courts 2002:7; Souders 2006:558-59). Systematic research does lend support to at least the first of these concerns. The public is cynical about judges receiving campaign contributions from those who appear before them or who have interests that are affected by court decisions (Geyh 2003:54-55; Gibson 2012:12-13; Jamieson and Hennessy 2007:901; Sample et al. 2010a:56) , and such contributions do appear to detract from the legitimacy of the courts (Gibson 2009).

2. Impact of Negative Advertising

The research regarding negative advertising provides a somewhat mixed message. Gibson (2012:12-13) found that a significant portion of the public thinks that it is acceptable for candidates for judicial office to attack their opponents.⁹ Gibson et al. (2011) found in their study of the 2007 Pennsylvania election that exposure to ads, whether positive or negative, tended to reduce support of the court, with little difference between what they labeled "traditional" ads and attack ads; they note that this finding must be understood in the context of an election that overall increased support for the court. A survey experiment study using Texas voters by McKenzie and Unger (2011) failed to find any effect of types campaign ads on support for the Texas Supreme Court, with or without controls for and interactions with the respondents' level of sophistication.

⁹ Gibson also reports that 86 percent of his respondents believed the judicial candidates should be allowed to make their policy views known.

The 2009 study for the National Center for State Courts discussed above provides a source of data for a simple test of whether the presence of attack ads in state supreme court election campaigns impacts the public's support for the court. Comparing states where attack ads had been aired in state supreme court elections by the time of the survey with states that had not seen such ads,¹⁰ both states without elections and states with elections but no attack ads, showed no statistically discernible differences between states with and without attack ads on any of the six items. The same was true for regression analyses that used several of the individual items and the two scales mentioned previously (one combining all items and one combining the two items that specifically mentioned the state supreme court). Details of these analyses are presented in this chapter's Appendix A. Thus, there is no evidence to date that negative advertising has significantly impacted the public's view of their state courts generally or their state's supreme court more specifically, although the research does not rule out the possibility of short-term, non-lasting effects in the wake of an election campaign that included substantial negative advertising.

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D. Summary and Conclusion

Gauging the impact of selection/retention systems is difficult because factors in specific states can impact support in many ways. The ideal approach would be to have good time series data for a state that changed from one selection system to another. In recent years there has been little change, with only two states, North Carolina and Arkansas, making a change since 2000,

¹⁰ Several states first saw attack ads in 2010; because the survey was done in early 2009, they were treated as not having seen such ads for purposes of this analysis.

¹¹ I located a report based on several surveys of Wisconsin residents that included questions about views of the Wisconsin Supreme Court (Jacobs et al. 2013). Regrettably, the first of these was done after the nasty 2008 election. Three surveys were done after the 2011 election discussed in Chapter One. The approval of the Court was actually higher in 2011 (about six months after the April election) than in spring of 2012 or spring 2013. There were differences in approval between those who identified themselves as Democrats and those who identified as Republicans. Unfortunately, there is not a good comparison point from prior to the 2011 election.

both shifting from partisan to nonpartisan elections. As discussed in Chapter Two, since 1946 only one state, New York in the 1970s, has completely abandoned elections for its highest court,¹² although many more states have shifted to systems employing appointment combined with retention elections or from partisan elections to nonpartisan elections. Another approach could be to look at the impact on public support for lower courts in one of the three states—Arizona, Kansas, and Missouri—where some parts of the state use elections while other parts do not (Rottman and Strickland 2006:33-39); however, such an analysis would be complicated by the fact that any news coverage or advertising concerning elections would spill over into areas where judges were not elected.

To summarize, the research on the impact of method of selection on how the public views state courts provides little support for the argument that elections have systematically negative consequences for the courts' long-term legitimacy; the one broad exception is the public's concern about the implications of the money that candidates for judicial office must raise. This one negative must be considered in light of the two single-state studies in Kentucky and Pennsylvania discussed above which found positive effects of election campaigns on legitimacy, at least as regards short-term effects. The key limitation of these two studies is that each deals with a single election cycle. One can certainly imagine a specific election that could produce at least a temporary decrease in public support for some or all of a state's courts, although a hard-fought election may produce a decrease in support among persons who preferred the loser, which could be offset by an increase by those who preferred the winner. However, the important question is not the short-term impact of any one election, but rather whether elections have

¹² Over the course of U.S. history, only one other state that adopted elections for its state supreme court has entirely abandoned popular elections: Virginia briefly had popular elections, but switched to a system of legislative selection during the Civil War. This is based on information posted by the American Judicature Society (<http://www.judicialselection.com/>, last visited June 14, 2013).

enduring impacts, either positive or negative, on how the public views the courts and the public's willingness to accept the decisions made by the courts. There is no extant evidence showing long-term effects in either direction.

III. IMPACT OF ELECTIONS ON JUSTICES' AND JUDGES' DECISIONS

A. Introduction

In Chapter One I argued that a key factor explaining change in judicial elections in Wisconsin was the mobilization of interest groups in response to several Wisconsin Supreme Court decisions related to tort law. As I will discuss in later chapters, decisions related to the death penalty served to mobilize opponents of sitting justices in California and Tennessee. Other hot-button issues have been abortion, property rights (i.e., restrictions on land use and development), and most recently same-sex marriage. If elections do matter for state supreme court decision-making, one would expect such effects to be most evident in these kinds of issues which are likely to be the vehicles for mobilizing voters.

There is a substantial body of research on the impact of electoral considerations on decisions related to the death penalty, some work related to sentencing more generally, and three studies of decisions regarding abortion rights. With some minor exceptions, the impact of judicial elections on decisions related to tort matters, same-sex marriage, or property rights has not been examined. In this section I will review the extant research on criminal case decisions and abortion, and I will present original analyses related to same-sex marriage and decisions regarding tort law; I am not able to shed any light on whether judicial elections impact decisions in land use matters. While most of the following discussion considers state supreme court justices, for decisions

related to criminal law, I will briefly reference research on the impact of elections on decisions of trial court judges.

Method of selection and/or retention can have either direct impacts or indirect impacts on judicial decision-making. By direct impacts I mean that judges make different decisions depending on how they are selected and/or retained; for example, judges who are retained by elections of one type or another may be less likely make decisions favorable to criminal defendants than are judges who do not have to face the electorate if they want to continue in office at the end of their current terms. By indirect impacts I mean that factors influencing judges' decisions differ depending on how they are selected and/or retained; for example, judges who must face the electorate may be responsive to public opinion while judges who do not have to face the electorate can ignore public opinion.

B. Criminal Cases

1. The Death Penalty in State Supreme Courts

Capital punishment has been a hot-button issue for courts since at least 1986 when Rose Bird and two of her colleagues lost retention elections for the California Supreme Court after a campaign that focused heavily on their decisions in death penalty cases (see Chapter Seven for a discussion of this election). It should not be surprising that capital punishment is the area that has received the most attention in studies of the impact of elections on judicial decision-making.

Relatively soon after the defeat of the three California justices, Hall (1992) looked at the possible electoral effects on death penalty decisions by state supreme court justices in four southern states (Kentucky, Louisiana, North Carolina, and Texas) during the 1980s.¹³ She focused specifically on the more liberal judges in cases that upheld the death penalty. She asked

¹³ One of the states was Texas, and for Texas she looked at decisions by the Court of Criminal Appeals which is the final court in criminal cases in Texas.

in what way might the electoral situation faced by these judges lead them to vote to uphold the death penalty rather than dissenting as one would predict given their broader pattern of voting in criminal cases? Her results showed that these liberal justices were more likely to vote contrary to their presumed political inclinations when they were approaching the end of their terms, if they had won their previous election by a narrow margin, if they were running in a district rather than statewide, and if they had previously run for reelection. Hall (1997) later found essentially the same pattern of relationships in an analysis of voting to uphold death penalties using all of the justices in the same four states and using all cases regardless of whether court's decision was to uphold or strike down the death sentence. Brace and Hall (1997) extended the analysis further to include death penalty decisions by eight state supreme courts (Arizona, California, Illinois, Kentucky, Louisiana, North Carolina, Ohio, and Texas) during the period from 1983 through 1988. They found that justices were more attentive to the likely preferences of constituencies if they faced more frequent elections (shorter terms) and served in more competitive political environments.

Several analyses have been conducted using data from the State Supreme Court Data Project (SSCDP) which covers cases in all states between 1995 and 1998 (see Brace and Hall 2001). The dataset includes almost 900 cases (over 5,600 individual decisions) from 34 of the 38 states that had the death penalty at that time. Brace and Boyea (2008) examined the question of whether public support for the death penalty influenced judicial decisions and whether any such influence was conditional on the method of retention. The results of their analysis showed that decisions of justices facing reelection were correlated with public opinion in the justice's state while the decisions of justices in states not using elections for retention did not show a similar relationship. In another analysis using these same data Brace and Boyea (2007) found that justices in the last

year before retirement on courts in elective states were more likely to vote to reverse in a capital case than were non-retiring justices; there was no such effect in appointive states, where the likelihood of voting to reverse was essentially the same as for retiring justices in elective states regardless of retirement plans or requirements. They also found that in states using some form of popular election as the method of retention, justices were less likely to vote to reverse in a year when the justice was standing for election than either the year prior to or the year after the year of election. Hall (2012), also using data from the State Supreme Court Data Project, focused specifically on how mandatory retirement might condition the influence of election-related factors on decisions in death penalty cases. She found that for justices not compelled to retire prior to what be their next election, the likelihood of voting to reverse in a capital case decreased if elections to the state supreme court had generally been competitive, if the justice had won his or her last election with less than 55 percent of the vote, and if the other two branches of state government were under unified Republican control (presumably an indicator of a conservative political environment). For justices who were barred from standing again for election, these effects were reduced or absent.

Canes-Wrone et al. (2014) report the most extensive and sophisticated analysis of the linkages between selection/retention of state supreme court justices and decisions in death penalty cases. Their analysis is based on 12,777 votes by justices in 2,078 death penalty appeals between 1980 and 2006. Their model included both the method of retention and public support for the death penalty conditional on method of retention, plus reselection proximity and indicators of whether the justice was facing mandatory retirement or was otherwise a lame duck; controls for various case and legal factors filled out the model. The authors report results for both before (Pre-Bird) and after (Post-Bird) the defeat of the three California justices. Post-Bird, they

find that justices in both nonpartisan and retention election systems are more likely to uphold the death penalty than are justices in partisan election states *after controlling for the justice's party, state public opinion, and electoral proximity*. Justices in both partisan election states and states where justices are subject to reappointment are responsive to public opinion while those in nonpartisan and retention states are not. Justices facing reselection within two years were more likely to uphold a death penalty but judges facing retirement or otherwise in a lame-duck status did not differ in a significant way. The analysis comparing pre- and post-Bird showed that prior to the 1986 election there was no significant difference in the decision pattern comparing justices subject to partisan and nonpartisan elections, but those subject to retention elections were *less* likely to vote to uphold a death penalty; that is, the effect of retention elections reversed in the wake of the defeat of the three California justices in retention elections.

These various studies clearly show that elections have significant influences, both direct and indirect, on the decisions of state supreme court justices in death penalty cases. The studies are not entirely consistent regarding the precise nature of those influences, but it is clear that many justices, either consciously or unconsciously, consider the electoral consequences of their decisions in these cases.

2. The Death Penalty in State Trial Courts

While this book's focus is on elections of state supreme court justices, there is also evidence of the death penalty's electoral impact on trial judges who preside in death penalty trials. Specifically, three states (Alabama, Delaware, and Florida) authorize trial judges to impose a sentence of death when a jury has *not* recommended a death sentence for a defendant; a fourth state, Indiana, allowed such overrides until July 1, 2002. Judges in Delaware do not stand for election while judges in the other three do have to face the electorate to retain their positions.

Statistics suggest that having to face the electorate inclines judges to demonstrate that they are tough on crime by imposing death sentences even when a jury has failed to recommend death.

Justice Stevens' dissent in a 1995 U.S. Supreme Court decision upholding life-to-death overrides, *Harris v. Alabama* (513 U.S. 504, 515),¹⁴ noted that in Alabama judges had overridden life sentences 47 times but death sentences only five times; in Florida judges had overridden 134 recommendations of life but only 51 recommendations of death; and in Indiana judges had overridden eight recommendations of life but only four of death. Delaware did not adopt the two-way override until 1991, but between 1991 and 1994 no Delaware judge had overridden a life recommendation while seven had overridden death recommendations (Russell 1994:11n52). Several studies have updated the figures in Stevens' dissent:

- By 2011 only one Delaware defendant had been sentenced to death after a jury recommendation of life while 12 received life sentences after the jury recommended death (Radelet 2011:798-99).¹⁵
- In Indiana between 1977 and 2002, when the state legislature abolished the life-to-death override, eight defendants were sentenced to death (one twice) against the recommendation of the jury while nine defendants received life sentences although the jury had recommended death (*id.*, p. 797).¹⁶
- In Florida between 1972 and 2011 there were 166 instances of death sentences being imposed after a jury recommended life imprisonment; all of those overrides occurred

¹⁴ In November 2013, the U.S. Supreme Court declined to revisit its earlier decision over the strong dissent of Justice Sotomayor (Liptak 2013).

¹⁵ The Delaware defendant's initial death sentence was thrown out by the Delaware Supreme Court; after the trial judge again sentenced the defendant to death, the Delaware Supreme Court once again threw it out, and ordered the trial judge to impose a life sentence.

¹⁶ None of the eight Indiana defendants who received death sentences over the jury recommendation of life was executed.

prior to 2000 (Radelet 2011:809) .¹⁷ Through 2011 there were at least 91 instances in which a judge imposed life after a jury imposed death, and 24 of those overrides occurred after 1999 (*id.*, p. 820).¹⁸ Exactly why judges in Florida stopped overriding jury recommendations of life is not clear (*id.*, pp. 810-11).

- Through mid-2011 Alabama trial judges had overridden 98 recommendations of life but only nine recommendations of death (Equal Justice Initiative 2011:7).¹⁹ One study of death overrides in Alabama through the late 1990s found evidence that judges were more likely to override life sentences in the two years immediately before they would have to face the electorate to win a new term, and that this effect developed after the 1986 defeat of the three California Supreme Court justices (Burnside 1999:1039-41).

These statistics suggest that elections influence the action of trial judges in capital cases.

While it is unclear why life-to-death overrides have ceased to occur in Florida, the overall pattern here strongly suggests that judges facing reelection are attuned to the risks of being labeled soft on crime by a potential opponent,²⁰ and to limit that risk they appear to choose to demonstrate their toughness in the most visible cases on their docket, those involving a possible death sentence.²¹ As I discuss below, these kinds of effects are not limited to capital cases: elected trial judges dealing with criminal cases are attuned to the electoral implications of their decisions.

¹⁷ Four persons have been executed in Florida after being sentenced to death despite a jury recommendation of life imprisonment.

¹⁸ The uncertainty regarding the exact number of death-to-life overrides reflects the difficulty in identifying such cases, particularly if the defendant did not appeal (Radelet 2011:812) . Death sentences are automatically appealed.

¹⁹ Eight of the defendants in these 98 cases had been executed by mid-2011 (Radelet 2011:802) .

²⁰ In the three electoral states that allow, or allowed, judicial overrides of a nondeath sentence trial judges are retained through either partisan or nonpartisan elections.

²¹ A similar effect of electoral proximity was found in a study of capital cases tried in Cook County between 1870 and 1930, The authors of the study report that a defendant found guilty was 15 percent more likely to be sentenced to death during the year that the judge was up for election; however, except in the case of a bench trial, the death-life sentence was entirely in the hands of the jury which leaves some doubt about the mechanism leading to the reported effect (Brooks and Raphael 2002:638).

3. Other Criminal Cases in State Supreme Courts

Two studies have examined the possible electoral effects on state supreme court justices across the full range of criminal case issues, and both are limited in their focus. One study (Savchak and Barghothi 2007) looked at justices who would have to stand in a retention election. The core hypotheses of that study were (1) that justices originally appointed by Democratic governors would be more likely to vote in a fashion favorable to defendants, (2) the overall political orientation (liberal vs. conservative) of the state's residents would have an influence on justices' votes, and (3) that influence of the citizens' political orientation would increase as the end of the justices' terms approached. Using the data from the State Supreme Court Data Project, the authors found that both the party of the appointing governor and the political orientation of the state's citizens worked as hypothesized even after controlling for the justices' own political orientation. They did not find a statistically discernible additional effect of the political orientation of the state's citizens as the justices' terms approached their end, although the direction of the effect was as hypothesized.²²

The second study (Cauthen and Peters 2003) was limited to Louisiana, and asked whether the district based system for election members of the Louisiana Supreme Court affected justices' decisions. Looking at 1,111 votes from 180 nonunanimous search and seizure cases decided between 1970 through 1994, the authors found that justices were more likely to render a pro-prosecution decision in cases from their home district compared to cases coming from elsewhere in the state (64 percent versus 55 percent). A logistic regression model that included a measure of district ideology and controls for the justices' own ideologies showed that justices tended to

²² A key variable missing from their analysis was whether the justice is barred from running for another term due to mandatory retirement provisions in the state.

vote consistently with the ideology of their district but only in cases from their district; in fact they tended to vote in opposition to their district ideology in cases from outside their district.

4. Sentencing in Noncapital Cases

Several studies have examined the impact of elections on sentencing in noncapital cases. Two studies, one using data from Pennsylvania (Huber and Gordon 2004) and one using data from the state of Washington (Berdejó and Yuchtman 2013), considered whether sentencing behavior varied over the electoral cycle, with the hypothesis that sentences would be tougher when a judge approached his or her reelection. Both studies found the hypothesized effects.

Another pair of studies (Gordon and Huber 2007; Lim 2008) took advantage of variation in the method used to select trial judges in Kansas. The trial courts in Kansas are organized by district, with 14 districts employing partisan elections for both initial selection and retention and 17 employing gubernatorial appointment for initial selection and retention elections for subsequent terms. Regardless of the selection system used, trial judges serve terms of four years. Not a single trial judge has been defeated in a retention election in Kansas while five to ten percent of judges running for reelection in partisan elections have lost (Lim 2008:3). Using both regression techniques and case-matching techniques, Gordon and Huber found that judges in districts using partisan elections tended to impose harsher sentences both in terms of the likelihood that the sentence involved a period of incarceration and the length of that incarceration (Gordon and Huber 2007:121-27). They also found that electoral proximity is influential in partisan election districts, but not in retention election districts; in fact for the latter the coefficients are negative, although not statistically different from zero (2007:128-30). Lim looked at the sentencing tendency of judges controlling for both election type and the partisan background of the judge; she found that the sentences of the judges facing retention elections

new closer to the presumptive guidelines regardless of the judge's partisan background. In contrast Democratic judges in partisan election districts were more likely to impose the harshest sentences while Republican judges in such districts were more likely to impose the most lenient sentences (Lim 2008: 29).

5. Summary: Judicial Elections and Judicial Behavior in Criminal Cases

The studies reviewed above make clear that the role and type of elections used in the retention, and possibly the initial selection, of judges are associated with broad patterns in the decisions made by both final appellate court and trial court judges in criminal cases. One might be concerned about the link between the election cycle and judicial decisions in criminal cases (Epstein 2013:220), which shows that defendants facing sentencing by a judge nearing election might have reason to fear a harsher sentence than if the defendant were facing a judge who had recently been elected or reelected; that is, an approaching election may bias at least some judges toward harsher sentencing. However, as Gibson (2013:227-28) points out, it is not imminently clear which is the "correct" sentence, the one given out in the absence of electoral pressures or the one given out when the judge might be under closer scrutiny by the public. In both cases the legal parameters of the sentence are specified in statutes passed by the elected representatives of the citizens. One could readily argue that judges who sentence more leniently early in their terms are failing to carry out their duty while those toward the end of their term are faithfully executing their duty.

The death penalty override cases provide a counter to Gibson's suggestions. In those cases it is the juries who make the initial decision and presumably the juries reflect local public opinion. In fact, if anything, the "death-qualified" jurors who would have made the

recommendation tend to fall on the harsher side of public opinion (see Allen et al. 1998). If such juries decide that the death penalty is not appropriate in a particular case, in what way would a judge's decision to override that determination and impose the death penalty reflect the "more correct" decision under the law?

C. Civil Cases

1. Abortion

A second hot-button issue for state supreme courts has been abortion. A pair of studies has examined the electoral influences on state supreme court decision-making.²³ In the first study Canes-Wrone and Clark (2009; see also, Caldarone et al. 2009) compared states using partisan elections with states using nonpartisan elections, and asked to what degree the impact of public opinion on abortion influences state supreme court decisions. They identified 85 abortion-related cases decided by state supreme courts in 16 states between 1980 and 2006; those cases involved a total of 597 judge-votes. They measured public opinion by pooling national surveys across states over 10-year periods. The statistical results showed that, after controlling for key facts of the case and the partisan background of the justice, justices in nonpartisan states were responsive to public opinion with a "ten-percentage point shift in public opinion in a pro-choice direction alter[ing] the likelihood of a pro-choice decision by five percentage points" (pp. 57-58). The results revealed no statistically discernible general effect for public opinion in partisan states; in fact, the coefficient suggested that, if anything, the relationship ran opposite to the direction one would expect. Moreover, regardless of the type of election system, judges were more likely to

²³ An earlier article by Brace et al. (1999:1294), covering fewer cases and using a less fully specified model, found that justices facing retention through partisan or nonpartisan elections were less likely to strike down abortion restrictions than were justices who would have to stand in a retention election.

vote in a way consistent with the tenor of public opinion on abortion in their state in the last two years of their terms.

The second study (Canes-Wrone et al. 2012) added retention election states to the mix. In the retention states there were 360 votes from 59 abortion-related cases. The results showed that justices in retention states were almost as responsive to public opinion as were justices in nonpartisan states. In this analysis the authors used a more refined measure of electoral proximity measure that separated out effects for states that leaned prochoice from those that leaned toward greatly restricting or banning abortion; the results showed no statistically discernible effects for electoral proximity, and the authors speculated that this might be due to some other refinements in the statistical modeling approach.

While it may be surprising that public opinion had no apparent affect in partisan election states, the explanation may be that that in those states voters essentially assume that candidates for the state supreme court running as Republicans oppose abortion while Democrats are likely to be pro-life. Essentially, it may be that abortion is so aligned with the political parties that public opinion can work indirectly through the partisan labels on the ballot rather than exerting a direct influence on voters. In contrast, views on capital punishment do not align as clearly with partisanship. One question that is left unanswered by Canes-Wrone et al.'s analyses arises from the absence in the analyses of states not using any form of popular elections for selection or retention state supreme court justices; specifically would an analysis including those states have shown that public opinion had no influence in nonelectoral states in line with what Brace and Boyea found in their analysis of death penalty cases?²⁴

²⁴ Devins and Mansker (2010:484-88) located 27 instances where state-level public opinion polls had asked questions about issues not related to crime or criminal justice that were then the subject of a state supreme court decision; the issues included topics such as abortion, same-sex marriage, gun control, education, eminent domain, immigration, and Indian gaming. Following the work of Marshall (1988, 2005) who has examined whether U.S.

2. Same-Sex Marriage

Same-sex marriage is the third of the prominent social issues that has come before the courts in recent years. The concern about possible decisions by state supreme courts has led 30 states to pass amendments to their state constitutions defining marriage as between a man and a woman (or allowing the state legislature to so define marriage). In some cases the amendments were written broadly to limit benefits that might flow to same-sex couples through civil unions.²⁵ Some of the amendments were passed in response to state court decisions while others were intended to block courts from ruling that limitations on same-sex marriage violated that state constitution.

Mezey (2009) provides an extensive analysis of the judicial decision-making related to same-sex marriage in state and federal courts through the first part of 2009. By mid-2009, appellate courts in twenty-four states had considered cases concerning with same-sex marriage or civil unions; in one of those states the case never reached the state supreme court for a decision on the merits. In only one additional state, Montana, did a state supreme court decide a same-sex marriage case between the controversial decision by the Iowa Supreme Court in 2009 and the *Windsor* decision,²⁶ the Montana court simply allowed challenges to provisions in Montana law that limited marriage to proceed in the lower courts. In a second state, Texas, a petition for review languished for more than two years before the Texas Supreme Court finally granted the

Supreme Court decisions align with public opinion, they looked to see whether the state supreme court decisions were in line with state public opinion. They found that justices subject to nonpartisan elections were the most likely to vote consistently with state public opinion with justices not having to face the electorate the least likely to vote in line with public opinion.

²⁵ List on Wikipedia as of July 17, 2013;

http://en.wikipedia.org/wiki/List_of_U.S._state_constitutional_amendments_banning_same-sex_unions_by_type

²⁶ As discussed in a footnote 47 in Chapter One, a Minnesota appeals court reversed a trial court decision dismissing a challenge to the Minnesota law banning same-sex marriage; however, that case never got to trial because it became moot after the Minnesota legislature legalized same-sex marriage. Given the limited nature of the decision by the appellate court, I have not included this decision in the following discussion.

petition and then heard arguments four months *after* the U.S. Supreme Court handed down its 2013 decision striking down part of the federal Defense of Marriage Act in *U.S. v. Windsor*.²⁷

Is there any relationship between the outcome of state challenges to limitations on same-sex marriage, and the method of retention used for state supreme court judges? Klarman (2013:117) observes that most of the judges who declined to strike down restrictions on same-sex marriage in the years immediately after the Massachusetts Supreme Judicial Court struck down the state law limiting marriage (*Goodridge v. Department of Public Health*, 798 N.E.2d 941, 2003) were elected. More generally, Pozen (2008:237) hypothesizes that decisions “that seek to protect traditionally disadvantaged or despised groups in new ways,” such as by striking down limits on same-sex marriage, “will be less likely to emerge from elected courts.”

The number of state appellate decisions limits the potential for statistical analysis, but one can still discern a pattern consistent with Pozen’s hypothesis and Klarman’s observation. Appendix C to this chapter lists the cases from the 25 states through 2012 including what specific issue each case dealt with and the outcome of the case. Appellate courts in ten states made decisions or issued orders favorable to same-sex unions; courts in six of those states also made unfavorable decisions. Courts in the other fifteen states that had same-sex marriage cases made only unfavorable decisions. In some cases, both favorable and unfavorable toward same-sex marriage, courts were unanimous and in others the courts were closely divided. Of the ten states where favorable decisions were made, three use retention elections, six do not use elections, and only one, Montana, uses nonpartisan elections.²⁸ Of the fifteen with only unfavorable decisions, ten use contested elections (partisan, nonpartisan, or hybrid), four use

²⁷ **NOTE TO EDITOR: I HOPE TO UPDATE THIS FOOTNOTE ONCE THE COURT RENDERS A DECISION.** The Texas case is *In the Matter of the Marriage of J.B and H.B.* which was consolidated with *State of Texas v. Angelique Naylor and Sabrina Dal* (argued November 5, 2013); as of August 1, 2014, the Texas Supreme Court had not rendered its decision.

²⁸ In Montana, if an incumbent is not challenged, the election reverts to a retention format.

retention elections, and only one does not use elections. While this shows differences related to the retention method in use,²⁹ one must be cautious in drawing such a conclusion given that four of the five states with favorable decisions that used appointment are in the liberal northeast and the fifth is Hawaii which is also politically liberal. Thus the pattern may primarily reflect the level of support for same-sex marriage rather than the method of selection; based on an analysis of state-level public opinion, Barclay and Flores (2014; see also Devins 2010:1679-83) find that state-court decisions supportive of same-sex marriage have generally come once public opinion in the state has reached the point where a majority supports eliminating restrictions.³⁰

Actions in state courts largely dried up in the wake of the defeat of three justices of the Iowa Supreme Court after that court unanimously ruled that limiting marriage to opposite-sex couples violated the state's constitution. Litigation challenging restrictions on or related to same-sex marriage sharply increased after the *Windsor* decision, although many of those cases were filed in federal court.³¹ As this is written, it is too soon to determine whether state supreme court judges facing electorates hostile to same-sex marriage will now strike down existing limitations or uphold such laws and thus leave the potential political fallout to judges in the federal courts.³²

²⁹ A simple goodness-of-fit test of the resulting 2x3 table for the null hypothesis of statistical independence produces a highly significant chi square (2 df) of 13.03 ($p < .001$).

³⁰ It may be important to take into account whether the public's view of the substance of an issue and whether they believe it is appropriate for the state supreme court to act on an issue. Devins and Mansker (2010:487, 508) report a 2009 poll in Iowa that found that only 30.4 percent of the respondents favored a court ruling that would allow same-sex marriage, while elsewhere Devins (2010:1680) refers to a 2009 poll that found that 60 percent of Iowans supported same-sex marriage or civil unions.

³¹ See <http://www.marriageequality.org/lawsuits> (last visited June 4, 2014).

³² By mid-2014, it might be that state courts had started to become more active; a news report of a federal district judge's decision striking down Florida's ban on same-sex marriage in August 2014 noted that in the preceding six weeks five *state* trial judges in Florida had struck down the law (Stutzman 2014).

3. Tort Cases

Given the prominence of tort reform and tort law as motivating factors in judicial elections (Goldberg 2007:80-83; Murphy 1994), one might expect to find substantial research examining whether there is a linkage between variables related to judicial selection and decision-making in tort cases. However, the research on this question is surprisingly thin.

In their study of judicial politics in Texas, Cheek and Champagne (see also, Carter 2006:33-34; 2004:40-43) describe how decisions moved to favor plaintiffs after justices with significant backing of the plaintiffs' bar gained a majority on the Texas Supreme Court in the early 1980s. The court swung back to favoring the defense side after business interests recaptured control of the Court in the 1990s.³³ The continued dominance of the pro-business perspective on the Texas Supreme Court is indicated by success of defendants in tort case appeals, winning 87 percent of those cases decided with opinions in the period 2004-05 (Anderson 2007:8).³⁴

An analysis of 145 tort cases decided by the Wisconsin Supreme Court between 2002 and 2012 found a sharp decline in the likelihood that the court's decision would be unanimous (Corriher 2013:8). While the analysis implies that this is a result of the contentious judicial selection process in recent years (described in some detail in Chapter One), there is no clear evidence of such a link. Arguably the increase in contentious elections is *more likely* caused by conflict on the Court regarding tort issues than vice versa.

Brace et al (2012) used the data from the State Supreme Court Data Project to examine whether the method of selection has a systematic impact on the decisions of state supreme courts

³³ The ebb and flow of the battle for control of the Texas Supreme Court is tracked in some detail over a series of articles by Walter Borges that appeared in the *Texas Lawyer* in the 1990s.

³⁴ Some of these issues were highlighted on the Frontline program, "Justice for Sale," which was first broadcast by PBS in 1999; the script can be found at <http://www.pbs.org/wgbh/pages/frontline/shows/justice/etc/script.html> (last visited July 13, 2013). A study of campaign contributions in the election for the Texas chief justice clearly showed the role of tort lawyers, both plaintiff and defense, in funding that election (Jackson and Riddlesperger 1991).

in tort cases. Unfortunately, the structure of their analysis did not provide a test of whether method of selection or other selection-related factors influence the direction of either justices' individual votes or the courts' overall decisions. In another study using the SSCDP data Shepherd (2009c:188) found that justices who had to stand for retention in a partisan election or a retention election and faced a Republican leaning electorate favored the defense side in several types of tort cases; the effect did not appear in nonpartisan election states, but did appear in states where a justice would potentially be reappointed by a Republican governor or Republican controlled legislature. She also finds that the effects are less or nil for justices in their final terms due to mandatory retirement (p. 190).

Another study (Helland and Tabarrok 2002; Tabarrok and Helland 1999) looked at the possible impact of selection system on awards in tort cases, and reported that the awards were higher in states where trial judges were elected in partisan elections; however, given that the awards were made by juries, it is not altogether clear what the causal mechanism might be linking judicial selection to jury awards. A third study (Yates et al. 2010) examined the impact of how state trial judges are retained on the level of tort litigation in a state; that study found that the state's political climate (level of citizen liberalism) conditioned the impact of selection system (partisan and nonpartisan elections versus reappointment or retention elections) with elections associated with a lower level of tort litigation in states with a conservative orientation and with a higher level of tort litigation in politically liberal states (p. 805);³⁵ however, it is unclear what the causal process at work here would be.

³⁵ An earlier analysis by Hanssen (1999) looked at the rate of civil filings in state trial courts of general jurisdiction for the period 1985-94 but found no statistically discernible difference between states that using partisan and nonpartisan elections for trial judges compared to states that using a Missouri Plan system or other systems whereby judges are appointed by the governor or chosen by the legislature (pp. 229-31). Hanssen did find that filing rates for appeals in the states' high courts was significantly higher in states that did not use partisan or nonpartisan elections for the justices of the high court (pp. 227-29) .

To assess what if any impact electoral considerations have on state supreme court decisions in tort cases, I analyzed data from State Supreme Court Data Project used by Brace et al. and by Shepherd in their analyses of tort cases noted above. Here I summarize the results of my analysis, with a more detailed discussion in this chapter's Appendix B. The dependent variable in my analysis was the individual justice's vote, coded as pro-plaintiff vs. pro-defendant. The key predictor variables were

- Type of retention system (no elections, partisan and hybrid elections, nonpartisan elections, and retention elections);
- Whether a justice in an election state was precluded from running for another term due to mandatory retirement age;
- Whether a justice in an election state who was not barred from another term due to mandatory retirement was in the last year of his or her term; and
- A measure of the liberalism of the state's citizens, conditional on type of retention system.³⁶

The regression model included control variables for type of tort, type of legal issue, absence of an intermediate appellate court that normally handled first appeals in tort cases, whether one of the parties was a governmental unit (presumably almost always as a defendant), the number of tort reform measures in effect in the state, and justice's ideology.

Because preliminary analyses indicated that patterns were different depending on whether the appeal was brought by the plaintiff or by the defendant, the analysis was done in a way that produced separate coefficients for plaintiff appeals and for defendant appeals (see Wright 1976). The sample sizes were large with 22,808 votes in plaintiff appeals and 13,290 in defendant

³⁶ I would have preferred to have a measure of state-level views of tort issues, but no such measure is available, and it's unclear exactly what questions such a measure might be based on.

appeals. While the results showed statistically significant relationships for a number of variables, the models were not particularly good predictors of the justices' decisions.

The type of retention system appears to make a difference in defendant appeals but not in plaintiff appeals. Using partisan and hybrid elections combined as the reference category, for defendant appeals plaintiffs were least successful in partisan/hybrid states and most successful in nonpartisan and retention election states, with no-election states falling between. Even though the model included a control for the political orientation of the state's citizens, I am cautious about labeling this result as demonstrating an effect of retention method given that the majority of the states using partisan or hybrid elections for retention purposes in the 1990s were located in the South; the exceptions are West Virginia, Ohio, and Michigan.

The results suggest that those justices barred from running for another term due to mandatory retirement may be less likely to side with plaintiffs in appeals brought by plaintiffs. The pattern for justices facing imminent mandatory retirement in defendant appeals was inconsistent, but probably is best interpreted as indicating no clear effect. Facing reelection within twelve months did not impact justices' votes in either plaintiff or defendant appeals.

Finally, state political liberalism had a statistically discernible effect for defendant appeals only in states using partisan or hybrid elections, with the likelihood of pro-plaintiff votes increasing with the state's political liberalism. For plaintiff appeals the effect was inconsistent, with state liberalism associated with an increased tendency to side with the plaintiff in states using nonpartisan elections, but associated with siding with the defendant in states not using elections in the retention process. Judicial ideology did generally have the effect one would expect for both plaintiff and defendant appeals, with the tendency to vote in support of the plaintiff increasing as the justice's ideology moved in a liberal direction.

Overall, while it appears that selection/retention related issues show some relationship with the direction of state supreme court decisions in tort cases, it is hard to make an argument that there are causal relationships pointing in a consistent direction. Even though tort issues have been strong motivators for key interests in judicial elections/judicial selection, the inconsistent nature of the effects summarized here precludes drawing clear conclusions about how the retention-related factors impact state supreme court decisions in tort cases.

4. Other Types of Civil Cases in the State Supreme Courts

As will be discussed in later chapters, a central conflict in state supreme court elections has pitted those aligned with consumers and injury victims against business and insurance interests. The outcomes of those struggles have shaped many of the decisions made by the state supreme courts in the states where they have occurred. Ware (1999) reports how Alabama Supreme Court justices lined up on a series of cases dealing with arbitration, a process generally viewed favorably by the business community but unfavorably by consumer representatives. The thirteen cases he examined were decided between 1995 and 1999, a period during which the court had some justices who had been funded and supported by the plaintiffs' bar and others who had been backed by business interests. In those 13 cases, business-funded justices backed a broad interpretation of arbitration agreements in 91 percent of their votes compared to only four percent for the justices backed by the plaintiffs' bar (pp. 667-68). In another eleven cases dealing with the issue of whether an arbitration agreement covered claims against a non-signatory defendant, the corresponding percentages were 67 percent and zero percent (p. 670). Ware found this basic pattern to hold up across a range of other types of issues related to arbitration that came before the Alabama Supreme Court.

Another study of arbitration focused on the method of selection rather than the orientation of individual judges as indicated by the judge's source of campaign contributions. LeRoy (2010) looked specifically at court review of employment arbitration decisions. He found that at the trial level, employees won only 31.2 percent of cases before judges who had (or would be) running in partisan elections compared to 52.7 percent in states using some other form of judicial retention. The pattern slightly reversed at the appellate level with employees winning 43.2 percent before party-affiliated judges compared to 50.0 percent before other judges (LeRoy 2010:1602). Unfortunately, the study mixes state and federal trial courts making difficult to have much confidence in drawing conclusions about the impact of selection systems.

Hanssen (1999) looked at the question of whether the method of selection/retention of state supreme court justices was associated with the number of appeals from decisions of the state's utility commission that the state supreme court decided in the period 1978 to 1982. His analysis found that courts in states using partisan and nonpartisan elections decided fewer such cases than did courts in states where judges faced either no elections or only retention elections (pp. 222-26). Unfortunately for my purposes, Hanssen did not look at whether the type of selection system was related to whether the ultimate decisions were pro-consumer or pro-utility company.

Romero et al. (2002) sought to determine if selection system had any impact on the outcome of cases dealing with racial discrimination. Using the West key number system they located 126 non-criminal cases decided over the 40 year period 1956-1996 that involved decisions on the merits (i.e., not decided on procedural questions). Their unit of analysis was the case rather than the decision of the individual justice. They found no effect at all of selection method using the four categories of partisan election, non-partisan election, Missouri Plan, and legislative or gubernatorial selection.

When might one find judges who do not have to worry about election to be attentive to some constituency? Might judges who face reappointment be more favorable to government parties than judges who face reelection? Two studies examined this question.

Shepherd looked at civil cases in which the state government is a party using the data from the State Supreme Court Data Project (SSCDP) . She found that justices who retain their seat through reappointment by the governor or legislature are more likely to vote for the government litigant than are judges on permanent appointments (life tenure or tenure until mandatory retirement age) or those having to stand for reelection. This effect is particularly strong for justices subject to legislative reappointment who are deciding cases involving the legislature as a litigant (Shepherd 2009a:1617). She also found that the effects strengthened as reselection approached.

Rice (2014) examined how judges decided economic cases involving the state government as a party. Using 201 cases from the SSCDP data he found that judges who would be retained through reappointment by the governor or the state legislature were more likely to decide in favor of the state government as economic conditions worsened than were judges who would have to face the electorate.³⁷

These two studies demonstrate that it is not simply the election process that lead judges interested in retaining their positions to look toward a relevant reselection constituency. This is what one would expect based on Schlesinger's analysis of static ambition.

³⁷ Strictly speaking, Rice contrasted those who would have to stand for election with those who would not have to stand for election, and the latter group includes justices in the states where judges serve until reaching mandatory retirement age, voluntary departure, or death without ever being subject to a retention process.

5. Summary: Judicial Elections and Judicial Behavior in Civil Cases

This analysis of the behavior of state supreme court justices in civil cases shows some likely effects, but overall it is best to describe the findings as mixed. For abortion cases there appears to be a relationship between state public opinion for states using nonpartisan and retention elections but not for states using partisan elections; the exclusion from that analysis of states not using elections from the studies of abortion cases leaves some question about what conclusions one might draw from the results. My analysis of same-sex marriage cases is suggestive, but the fact that nonelection states are heavily found in the liberal northeast (and the absence of any same-sex marriage cases from Virginia and South Carolina, the two southern states not using popular elections) leaves some doubt as to whether it is the retention mechanism that is the key causal factor. My analysis of tort cases produces results that show linkages to election-related factors but the nature of those linkages do not support a view that these factors generally shape justices' voting in a particular way. Importantly, to the extent that there are effects related to retention, Shepherd' and Rice's analyses of cases involving government litigants make it clear that this issue is not limited to judges facing popular elections.

E. Summary

The research described in this section generally supports the view that methods of judicial retention and factors related to retention can influence the decisions of judges. Not all of the effects described work in straight forward ways. The effects are clearest for criminal cases, and particularly for cases involving capital punishment. On the civil side the effects are less evident or more difficult to separate out from confounding factors. For example, while my discussion of same-sex marriage cases makes clear that courts have been most likely to be supportive of same-sex marriage in states where justices do not have to stand for election to retain their positions,

that may be due to the nature of the states not using elections rather than concerns about the retention process on the part of the justices. In the tort arena, the results summarized above (and detailed in Appendix B) raise many questions about the nature of the relationships my analysis uncovered, and it is not clear why particular variables seem to work in the ways that I found.

IV. IMPACT OF CAMPAIGN CONTRIBUTIONS ON SUPREME COURT DECISIONS

The third major question concerning the impact of elections is whether campaign contributions influence the decisions that judges make. Contributions can have direct impacts or indirect impacts, or a combination of the two. One might think of an indirect impact as the expending of funds in an election campaign to secure the election of a justice *presumed* to be favorable to the contributor's perspective while a direct impact is the buying decisions by securing the loyalty of a justice who might otherwise not be sympathetic to contributor's interest or case (Cann 2002:263).

A. Indirect Effects of Campaign Contributions on State Supreme Court Decisions

In practice campaign contributors generally direct their money to candidates the contributors believe will make decisions they prefer, what one article labeled "friendly giving" (Roscoe and Jenkins 2005:53). Regardless of whether the candidates' spending, which is made possible by the contributions from groups interested in how the candidate will vote, actually affected the election outcome, one would expect that if a significant portion of a candidate's campaign funding came from sources with particular preferences, the candidate's decision-making tendency would accord with those preferences. Such a pattern is likely to be found even in elections where the contributions had no actual impact on the outcome.

When and how campaign spending helps a candidate secure support in an election has been a significant question in the study of elections and campaigns, particularly in the context of congressional elections (see, for example, Abramowitz 1988; Cassie and Breaux 1998; Green and Krasno 1988; Jacobson 1978, 1980, 1985, 1990; Partin 2002). The thrust of the findings of this body of research is that in elections involving an incumbent, challenger spending makes a difference by increasing the challenger's vote share; whether incumbent spending matters is less clear in part because the level of incumbent spending is often in response to challenger spending (Squire 1995:900-03).³⁸ There has been less attention to open-seat elections, no doubt in part because they are much fewer in number. The most comprehensive study of those elections, by Gaddie and Bullock (2000), found that spending does matter. The share of the votes received by the Republican candidate tended to increase as the Republican candidate's expenditures increased and decreased as the Democratic candidate's expenditures increased (pp. 35-43, 175-77). Another study of open-seat elections employed a dichotomous dependent variable, Republican winner, and used the expenditure ratio as a predictor, finding that as the ratio of spending went from favoring the Republican to the Democrat, the likelihood of the Republican winning decreased (Duquette et al. 2008).³⁹

In contrast to the sizable body of research on the impact of campaign spending in congressional elections, there has been relatively little research concerning the impact of spending in state supreme court elections.⁴⁰ In a study of 260 state supreme court elections

³⁸ Examining the impact of incumbent spending presents particular methodological difficulties because of this endogeneity problem. Also, generally the literature on the impact of campaign spending predates the influx of "independent" spending.

³⁹ The ratio was modeled as a set of dummy variables, and showed that the effect was nonlinear in form.

⁴⁰ In a brief article, Arrington (1996) reported a fairly simple analysis of the impact of spending in 20 North Carolina statewide appellate elections (1988-1994), some of which were for the North Carolina Supreme Court. He found no evidence of a relationship with spending. His analysis did not distinguish between incumbent-running and open-seat elections.

between 1990 and 2004 in 18 states using partisan or nonpartisan elections,⁴¹ and Bonneau (2008) found that greater spending was associated with increased voter participation. In an examination of 166 partisan and nonpartisan general elections involving incumbents for the same period, Bonneau (2007c) found that, consistent with the research on congressional elections described above, spending by challengers increased the challenger's percentage of the vote; he found no statistically discernible effect of increased spending by incumbents.⁴² Consistent with this finding Bonneau and Cann (2011), looking at the same elections, found that funding restrictions disproportionately harmed challengers. Regarding open-seat elections, Bonneau (2006:153-54) briefly reported an analysis of the winner's vote share which includes spending by both the winner and loser; he found that more spending by the loser decreased the winner's share but there was no discernible effect for spending by the winner.⁴³

Clearly money does matter in state supreme court elections, but it is not clear how much it matters in comparison to other factors such as partisanship or the experience of the candidates. Perhaps more importantly, to adequately assess whether campaign spending indirectly impacts later decisions by the court, one would need to know not just whether more spending can increase a candidate's vote share, but how often campaign spending affects who actually wins the elections; there is no research examining that question in the context of state supreme court elections. One can point to specific elections, such as West Virginia in 2004 or Wisconsin in 2008, where the winner and/or outside groups supporting that winner used campaign

⁴¹ This study, and the others discussed in this paragraph, also included what I have labeled hybrid elections, grouping them with nonpartisan elections because the general election ballot is technically a nonpartisan ballot.

⁴² The effect for incumbent spending reported by Bonneau for incumbents is in the expected direction, but does not achieve statistical significance.

⁴³ Chris Bonneau generously provided me with an earlier draft of his article that included more detail on the results of his analysis; those results showed that the coefficient for winner expenditures, while not statistically significant, was actually *negative*, which means that, if anything, greater expenditures by the winner reduced the winner's share of the vote. Actually, what this probably indicates is that one candidate's expenditures drove the other's expenditures, and perhaps the more important expenditures came from the underdog in the election.

contributions from particular interests to mount a negative campaign to defeat the incumbent whom the contributor(s) wanted to see off the bench. However, there are probably many more examples where such campaigns were unsuccessful (e.g., Michigan in 2000—see the discussion in Chapter Six).

B. Direct Effects of Campaign Contributions on State Supreme Court Decisions

The tendency of campaign contributors to choose whom they fund based on the past behavior of candidates and/or by their expected future behavior greatly complicates determining whether campaign contributions have what I have labeled direct effects on elected officials' decisions. That is, past and expected behavior tends to determine contributions as much or more than contributions determine behavior. This problem of likely mutual causation makes it particularly difficult to sort out the actual causal influences. However, as with the impact of spending on election results, there is a substantial literature attempting to determine if and when contributions by interest groups directly influence the actions of members of Congress.⁴⁴

In a review of that literature Baumgartner and Leech (1998:131-36) described that “body of research [as] infamous for its contradictory findings” (p. 133) and concluded that campaign contributions by PACs “sometimes strongly influence congressional voting, sometimes have marginal influence, and sometime fail to exert influence” (p. 134). They also suggest that the inconsistencies probably also reflect variations in variable selection, modeling choices, and measurement choices. In a more recent meta-analysis of the impact of campaign contributions on legislative roll call voting Roscoe and Jenkins (2005), found that about one third of 357 tests

⁴⁴ In the legislative setting interest groups may use political donations for things other than directly influence a legislator's vote; they may seek “to gain or maintain access, or mobilize friendly legislators to lobby their colleagues, or alter than language of the bill, especially at the committee stage” (Roscoe and Jenkins 2005:53)

drawn from the 33 studies they examined were statistically significant.⁴⁵ Consistent with Baumgartner and Leech's suggestion that finding a relationship was partially dependent on variables included, measurement of those variables, and modeling choices, Roscoe and Jenkins reported that statistical significance varied depending on a range of such factors. For example, 62 percent of the tests were based on models that took into account the possibility of two-way causation; 30 percent of those tests were statistically significant compared to 45 percent of the tests that did not employ a simultaneous equations model.

Whether campaign contributions influence the decisions of justices is even less clear than for decisions by legislators. A number of studies have sought to link contributions and justices' decisions (Cann 2002, 2007; Cann et al. 2012; Kang and Shepherd 2011; Liptak and Roberts 2006; McCall 2001, 2003; McCall and McCall 2007; McLeod 2008; Palmer 2010; Palmer and Levendis 2008; Shepherd 2009b; Waltenburg and Lopeman 2000; Ware 1999; Williams and Dislear 2007). Most of these studies show that the decisions of at least some justices do tend to align with the interests of the groups that provide financial support for justices' election campaigns. The central issue in looking at the correlation between decisions and campaign contributions is whether contributions follow votes or votes follow contributions. Essentially, would a justice's decision in a case have been different if not for the campaign contribution? Even if one or more votes do follow contributions for a particular justice, this does not require that a justice consciously considered how he or she should vote given the source of campaign funds; it may be that the justice is more attentive to the arguments and briefs submitted by those associated in some way with those contributions. That is, much as in the legislative setting where contributions purchase access and lead to a more attentive hearing for the group making the

⁴⁵ The meta-analysis included studies examining roll call votes that occurred between 1973 and 1996 (p. 56). Studies of voting by members of the U.S. House of Representatives dominated the analysis.

contribution, in the judicial setting contributions may purchase a more careful consideration on the part of the justice which in turn leads the judge to vote differently than he or she would otherwise have.

In the context of decision-making by state supreme court justices, several of the studies cited above have attempted to untangle the causal question in order determine whether justices' voting decisions were directly affected by the sources of campaign contributions the justice received. Some of those studies focus specifically on contributions from lawyers who can be identified as representing specific interests while others have looked at contributions more broadly. The studies tackle the methodological challenge in several different ways.

Two studies employed statistical modeling that the authors argued took into account predispositions and hence allowed them to assess the impact of contributions beyond the indirect electoral impact they might have.⁴⁶ Cann (2007) looked at the impact of campaign contributions on decisions of the justices of the Georgia Supreme Court in cases decided in 2003. He focused specifically on contributions by attorneys who had appeared before the court during 2003, classifying the attorneys as liberal or conservative depending on the parties they represented before the court. He found that contributions increased the likelihood that a justice would vote in favor of the party the contributor represented. Bonneau and Cann (2009) applied the same approach to look at state supreme court decisions in Texas, Michigan, and Nevada for those courts' 2005 term, and found a direct impact of contributions in Texas and Michigan but not Nevada. I am not inclined to place a lot of weight on these findings, because of the limited

⁴⁶ The technical description of the statistical problem is that of endogeneity. That is anticipated (or past) votes can influence contributions and contributions can influence votes. The statistical model used to overcome this problem involves what is called instrumental variables, which involve finding variables that have a causal influence on contributions but not votes and variables that have a causal influence on votes but not contributions. If one is only interested in explaining votes, one need only find instruments that cause contributions but not votes.

periods covered by the studies, the limited focus on contributions from attorneys,⁴⁷ and technical issues related to the statistical estimation strategy.⁴⁸

A second strategy used in an attempt to demonstrate a causal linkage is to focus on justices who are deemed to have a strong predilection to decide cases a particular way and ask if they are more likely to go against their predilection if they have received campaign contributions from interests and/or lawyers who normally represent interests opposed to the justices' presumed predilections. McCall (2003) applied this approach using cases before the Texas Supreme Court between 1994 and 1997 that involved businesses as both the plaintiff and the defendant. All of the justices on the Texas Supreme Court at that time were decidedly conservative, and McCall's specific strategy was to look only at the small fraction of cases won by plaintiffs and ask whether in those cases justices voting for the plaintiff (typically a small business challenging a large business) had received more contributions from the plaintiff or the plaintiff's lawyer than from the defendant or the defendant's lawyer. Given the conservatism of all of the justices, McCall assumed that contributions were not driven by the justices' ideology or anticipated decisional inclination, an assumption that seems reasonable. She did find that justices were more likely to support the plaintiff's position when they had received more in contributions from the plaintiff's side of the case. This does suggest some possible direct influence, but this is a very limited study.

A third approach is to find a baseline for individual justices' decision propensity for various types of cases. This is the approach used by Palmer in his controversial study of the Louisiana Supreme Court (see Finch 2008). The first iteration of the study (Palmer and Levendis 2008)

⁴⁷ According to Bonneau and Cann (p.3) attorney contributions to state supreme court candidates in 2006 comprised only 26 percent of the contributions those candidates received.

⁴⁸ The technical issue concerns the quality of the instrumental variables used in the analysis. Unless the instruments both meet the requirement of not having a causal relationship with the other dependent variable and have a strong relationship with the dependent variable for which they are being used as an instrument, the ultimate estimate is going to be on shaky grounds (see Bartels 1991); the authors of the articles using this methodology did not provide any information on the quality of the instrumental variables, and hence it is difficult to know what to make of their results.

was based on flawed data, which was subsequently corrected and reanalyzed (Palmer 2010);⁴⁹ the results were generally consistent across the two versions, although the corrected version was still subject to sharp criticism by defenders of the Louisiana Supreme Court (see Tully and Gay 2010). To assess causation, Palmer compared the justices' decisions for the plaintiff or defendant in cases where the plaintiff contributed more to a justice's campaign, the defendant contributed more, and where neither the plaintiff nor defendant contributed.⁵⁰ One can think of the "no contribution" condition as a baseline, where a justice's decision would not be influenced by contributions by one or the other of the parties. The cases covered the period 1992 to 2006, and were limited to 177 decisions on the merits where (a) at least one justice had received a contribution from a party to the case, (b) there was at least one dissent in the case, and (c) the case was not a criminal or disciplinary matter. For most of the justices, the probability of voting for the plaintiff or defendant when neither was a contributor was virtually the same as when the plaintiff was the net contributor; however, when the defendant was the net contributor the probability of voting for the plaintiff dropped. For one justice, the likelihood of the justice voting for the defendant appeared to *decrease* if the defendant had contributed more the justice's election campaign than the plaintiff had contributed. For one justice, the probability of voting for the plaintiff when the plaintiff was the net contributor increased from the "no money" condition and decreased from the "no money condition" when the defendant was the net contributor. One justice had too few cases involving contributors for a meaningful assessment, and two showed no apparent shifts across the three conditions (p. 17). While using cases where a particular justice received no contributions does provide a baseline for comparison, limiting the

⁴⁹ Palmer sought and received independent verification of his coding. While some coding decisions might be debatable (Tully and Gay 2010:13-16), it is unlikely that correcting any remaining errors would change the thrust of his conclusions.

⁵⁰ Palmer's analysis also included logistic regression models, but those models fail to account for the endogeneity problem, and hence tell us nothing about causation.

baseline to cases where one or more of the other justices received contributions may be problematic. A second concern is the lumping of all types of cases together; one might expect that there would be potential differences based on type of case (e.g., tort vs. consumer vs. labor/employment, etc.). In the end, this study is at best suggestive that some justices might be directly influenced by campaign contributions, although if one were to generalize from Palmer's study one would have to say that it is a minority, and probably a small minority.

Yet another approach involves comparing the behavior of judges who might be directly influenced by campaign contributions and those whose electoral situation would free them from such a concern. One such situation is where a justice cannot stand for reelection due to a state's mandatory retirement requirement. While a retiring justice would be freed from any worries about future campaign contributions, the justices would probably vary in whether they feel any continuing loyalty to past contributors. Still, there is enough of a difference here that retiring justices provide a potential control group.⁵¹ Shepherd (2009b) employed this approach using data from the State Supreme Court Data Project used in several of the studies previously discussed. While she concludes that her results support a finding of difference between retiring and potentially continuing justices, at least for justices in states using partisan elections, the results are not entirely clear and the pattern is by no means a strong one (pp. 673-74).⁵²

In a second analysis also relying on data from the State Supreme Court Data Project, Shepherd (2013) used another control group to try to sort out the direct influence of campaign contributions. In her second study Shepherd focused specifically on the impact of contributions

⁵¹ Kang and Shepherd (2011:102-03) identified another potential comparison group that has some similarities to justices facing mandatory retirement: justices in the three states (Illinois, New Mexico, and Pennsylvania) where initial selection is by partisan election but retention is by retention elections. However, they do not seek to test for possible differential effects of campaign contributions using this comparison.

⁵² A major question left unanswered by what Shepherd reports is whether the differences between judges facing mandatory retirement and those not facing forced retirement differ in a statistically significant sense.

made by business interests. As one would expect, the likelihood of a pro-business vote by a justice increased as the campaign support from business increased. Shepherd looked at the level of support both in absolute terms as measured by the amount of contributions from business and relative terms as measured by the percentage of contributions from business. She also showed that the support for business was negatively related to contributions from non-business sources (p. 17). The key to her causal argument was the relative influence of contributions from business for justices identifiable as Democrats as compared to those identified as Republicans once one includes controls for the state's political ideology and other factors. She reported that contributions from business sources had a greater influence on Democratic justices than on Republican justices and she interpreted this finding as indicating a direct causal linkage. Importantly, she did not find the reverse: Republicans did not evidence a stronger effect of non-business contributions than do Democrats; if anything such contributions have a stronger influence on Democrats than Republicans. One major issue gives pause in accepting Shepherd's interpretation of the results: she reported no tests comparing whether the differences between Republicans and Democrats could be attributed to chance, and one of the key differences was very small.⁵³

C. Summary

Based on the extant studies, the only firm conclusion that one can draw from the empirical research on the link between campaign contributions and the decisions of those benefitting from the contributions is that there does tend to be a correlation between the two variables.

⁵³ At first glance one might ask why she includes no control for the justices' ideology given that justices may vary even if they share a partisan identification. The likely reason is that usual method of measuring the ideology of a state supreme court justice, what is known as the PAJID scores (see Brace et al. 2000), is so closely related to the justice's partisan background that it would not provide enough additional information to be worth including in a statistical model.

Convincing evidence on whether those contributions systematically influence the decisions that the justices make, either directly or indirectly, is lacking. Regarding indirect effects, while there is some evidence that contributions influence vote shares, there is no evidence of a systematic impact on election outcomes. Regarding direct effects, none of the efforts to date to test for such effects is convincing, and the extant evidence is inconsistent. Given the challenges in sorting out the causal puzzle vis-à-vis direct effects, it is unclear whether solid evidence is obtainable. Finally, future efforts to assess the impact of contributions on votes will be further complicated by the lack of good data on outside expenditures by interest groups.

V. SUMMARY AND CONCLUSIONS

The use of elections to select and retain judges is controversial because of the perception that the method used for selection and retention actually matters. This chapter has sought to assess whether the use of elections and factors that relate to the use of elections have measurable impacts on the behavior of judges, particularly those who sit on state courts of last resort. The analysis in this chapter suggests this broad answer to the question of whether the use of elections for choosing and retaining state supreme court does matter: **Elections do not matter as much as one might think:**

- To those who are concerned that elections—and the campaigns and money that go with elections—are harmful to the legitimacy of the court: there is little or no evidence that elections do, in fact, have the feared effect, and there is even some evidence that the use of elections with all of their warts actually increases the public’s support for the courts.
- To those who are concerned that campaign contributions are a corrupting influence on state supreme courts, directly or indirectly buying the outcomes desired by the contributors: we have little or no evidence that campaign contributions *systematically*

influence either who wins elections or lead to justices making decisions that would have been different in the absence of contributions.

- To those who are concerned that the need to run for reelection inappropriately influences the decisions that judges and justices make in specific cases or affects the willingness of judges and justices to make unpopular decisions: yes, there are such effects, but they are more limited than one might expect. Such effects are most evident in cases involving the death penalty; they are harder to detect in other kinds of cases.

These findings do not mean that some form of more sophisticated, more developed analysis could not show that some of the undesirable effects that critics of judicial elections espouse actually do exist. They do not mean that elections will never have some of the allegedly negative effects. They do not mean that further changes in how judicial elections are conducted could not have negative consequences for the public's view of the court. But, at least for now, most of the claimed negative consequences remain as hypotheses rather than as confirmed actualities.

This absence of evidence for most of the negative effects of elections that concern critics does not mean that there are no problems with those elections. Participation tends to be low in the absence of a highly visible top-of-the-ticket election, and even with some other election to bring voters to the polls, large numbers of voters do not participate; that lack of participation reflects the combination of low visibility for most of what courts do and the absence of the kinds of cues that voters rely on in elections. Judges do have to take time, in some situations a lot of time, to attend to election needs, both fundraising and campaigning. Given that even in electoral systems, most judges initially come to their positions by appointment, this is primarily an issue for sitting judges who might better devote time to their duties as judges. Many highly qualified persons who would make excellent judges choose not to seek judicial positions because they do

not want to undertake the burdens involved in the electoral process, or do not have the personality or style to be effective political candidates.

The fact that elections do not have some of the problems often attributed to them does not mean that elections are the most desirable way to staff American courts. Nor does the general absence of the problems discussed in this chapter mean that a better system that includes elections could not be devised. In the final chapter, I will turn to the issues involved in designing systems of judicial selection, and consider what systems might be better than state systems currently in use, and the realpolitik that limits possibilities for significant change.

VI. APPENDIX A: REGRESSION MODELS OF LEGITIMACY

As discussed in this chapter, the most recent national survey that focuses on the public's view of state courts was conducted in 2009 for the National Center for State Courts. This Appendix reports the details of the statistical analyses summarized above regarding whether there are significant differences in legitimacy related to whether the respondent's state employs elections to select and/or retain state supreme court justices and whether there are any such differences related to past airing of attack advertising in connection with a state's supreme court elections.

A. Dependent Variables

Four different measures of public support were employed as dependent variables, with all coded so that higher values indicated more support. Two of these were single items: The first was a question asking respondents to rate their level of confidence in state courts using the alternatives of "a lot," "some," "not too much," or "no confidence" with the responses coded 0 to 3; the mean response was 1.93 with a standard deviation of 0.81. The second was a question

asking whether the control the governor and members of the legislature had “over state judges and the decisions they make in court,” should be increased, left as is, or decreased (“should have more control than they do now, less control, or about ... the same amount of control”), with responses coded 0 to 2; the mean was 0.99 and the standard deviation 0.68. Logistic regressions were also done with the confidence question collapsed into “a lot of confidence” versus other responses.

Two scales were constructed. The first was the simple sum of the two dichotomous choice questions that specifically referenced the state supreme court:

- The state supreme court should not be able to decide as many controversial issues as it does now *versus* it’s important for the state supreme court to maintain its ability to decide such issues.
- It’s important for judges on the state supreme court to be independent and not too influenced by what others think *versus* it’s better for judges to be less independent and pay more attention to what the people think.

For these questions interviewers also recorded a voluntarily provided response of “neither” or “both equally,” and these were coded as falling between the two forced choice responses. The second scale was a single factor score based on the first principal component extracted from all six items. It is important to note that neither of these scales was particularly coherent.

Cronbach’s alpha for the two item scale was an extremely low .19; a scale employing all four forced choice items (which was not used) had an alpha of .43. For the six items, the principal components results actually yielded two components with an eigenvalue greater than 1; the first two eigenvalues were 1.70 and 1.02, and the first component only accounted for 28 percent of the variation. A simple summated version of the six items produced an alpha of .53.

B. Independent Variables

1. Key Variables

The key variables of interest here are either the nature of advertising in the elections or the method of selection/retention. For the former, the predictor was a dichotomy coded 1 if any of the supreme court elections in the state prior to 2009 involved attack advertising, 0 otherwise. This variable was based primarily on the advertising identified by CMAG (see Chapter Five for a discussion of the CMAG data), but could be coded as 1 if it was known that elections prior to 1999 (when the CMAG data start) involved attack advertising. For method of selection/retention two different versions were used; the first was a set of dummy variables using the categories, partisan elections, hybrid system (Ohio and Michigan), nonpartisan elections, retention elections, no elections (meaning no *popular* elections); the second collapsed partisan, hybrid, and nonpartisan elections with retention and no elections as the remaining categories. For both versions “no elections” was used as the reference category; also for both, states that combined retention elections with partisan elections (Illinois, New Mexico, and Pennsylvania) were coded as having partisan elections.

2. Control variables

Eight control variables were also included in the regressions:

1. An index of knowledge that equaled the number of branches of government the respondent could identify, plus 1 if the respondent knew that the state supreme court could declare an act of the state legislature unconstitutional plus another 1 if the respondent correctly knew whether “the state supreme court” was elected. Note that for this latter item, respondents from New York were treated as having given a correct answer if they responded “Yes” because the trial division of what is called

the supreme court in New York is elected even though the highest court in New York, the Court of Appeals, is not elected. This index ranged from 0 to 5 with a mean of 2.24 and a standard deviation of 1.49.

2. An index of the level of attention paid to the news based on three items asking about how closely the respondent followed national news, news about their state in general, and news about the town or neighborhood where the respondent lived. The response choices for each item were “very closely,” “somewhat closely,” “not too closely,” and “not at all.” The three items scale with an alpha of .64. The scale ranges from 0 to 9, with a mean of 6.69 and a standard deviation 1.82.
3. An index of confidence in government other than the courts which was comprised of responses to the same question used to measure confidence in the state courts, but with the object of the questions being “the office of the governor,” “the state legislature,” and “local government where you live.” The three items scale with an alpha of .64.⁵⁴ The scale ranges from 0 to 9, with a mean of 5.44 and a standard deviation of 2.02.
4. A dichotomous item indicating whether the respondent had “direct experience, contact, or involvement with a court case which brought [the respondent] into a courthouse, including being called in for jury duty”; “yes” was coded 1, and 56 percent had responded “yes.”
5. The respondent’s sex, coded 1 male and 0 female; 53 percent of respondents were male.
6. Respondent’s total household income in the previous year (2008) coded into four categories: under \$25,000 (25 percent), \$25,000 to under \$50,000 (26 percent),

⁵⁴ Including the item asking about confidence in the state courts would produce a scale with an alpha of .72.

\$50,000 to under \$75,000 (17 percent), \$75,000 or more (31 percent). This variable was used as a four point scale ranging from 1 to 4); it had a mean of 2.54 and a standard deviation of 1.17.

7. Respondent's level of education coded into seven categories, and treated as a scale ranging from 1 to 7; the mean was 4.61 with a standard deviation of 1.66.⁵⁵
8. An indicator of the respondent's race or ethnicity coded as non-Hispanic white (77 percent), non-Hispanic black (8 percent), Hispanic (9 percent), and other non-Hispanic (4 percent). This was treated as a both a four-category set of dummy variables and as a two-category dummy variable where the latter three categories were collapsed; for both versions, non-Hispanic white served as the reference category.
9. An indicator of the respondent's political party identification, coded simply as Republican (24 percent), Democrat (40 percent), or Independent (33 percent); respondents who volunteered that they had "no preference" were grouped for purposes of analysis with Independents. In the analysis party identification was treated as a set of dummy variables with Independent as the reference category.

C. Selected Results

Regression analyses were run using the five different measures of support described above:

- single item, four-point measure of confidence;
- single item, dichotomous measure of confidence (logistic regression);
- single item, three-point measure of control that should be asserted by legislators and governor over court decisions;

⁵⁵ Category 4 was "technical, trade, or vocational school after high school"; category 5 was some college or university work, including an associate degree but not a four-year degree.

- the two-item index combining the two forced choice questions about the supreme court;
- the six-item index combining the two single items with four forced choice items.

Additional variations in the analysis involved whether method of selection was treated as a three-category (no election, retention election, other type of election) or a five-category (separating out partisan, hybrid, and nonpartisan elections) variable. These various combinations produced a large number of regression models—too many to present. Tables 3.A1 and 3.A2 show selected models that are representative of the results.

1. Impact of Selection/Retention System

Table 3.A1 shows selected results for the analysis of the impact of selection/retention method: the type of election, if any, used for selection and/or retention of judges. None of the tests of the set of selection/retention variables achieve statistical significance, and only one individual coefficient achieves statistical significance. That significant coefficient is in the model for six-item support index, and it shows that support is significantly greater in the two states using the hybrid system compared to the reference category of no election; while the coefficients comparing the other three types of election states to the reference category do not achieve statistical significance, they are all positive suggesting that support may be *increased* by the use of elections. However, the fact that this pattern appears only for this one measure suggests caution in drawing any conclusions.

{ Table 3.A1 about here }

Several other variables show patterns worth noting. Experience in court appears associated with a decrease in support. Knowledge level and confidence in other political institutions are associated with an increase in support; income may be associated with an increase in support but the pattern is not consistent. All of the coefficients for the race/ethnicity variables are negative

meaning that non-Hispanic whites tend to have a higher level of support than the other four groups; however, only a fraction of these coefficients differ significantly from zero which again suggests caution in interpreting this pattern.

2. Impact of Negative (Attack) Advertising

Table 3.A2 shows the regression results for the same set of dependent variables but replacing type of selection system with whether attack ads had been aired in the state prior to 2009 (the year of the survey) in connection with supreme court elections. For the models included in the table, the showing of attack ads has no discernible impact on any of the measures of support. The results shown in the table include California and Texas among the states where attack ads had been run even though it had been 10 years or more since any such ads had been used; the analysis was also run including Texas and California with states that had not seen attack ads, but the results changed little if at all. Also, the results in Table 3.A2 are based on regressions that excluded states not using elections. Making the attack ads variable into two dummy variables, attack ads shown and no elections with elections without attack ads serving as the reference category, does not change the results.

{ Table 3.A2 about here }

VII. APPENDIX B: ELECTORAL IMPACTS ON STATE SUPREME COURT DECISIONS IN TORT CASES

A. Data, Variables, and Method

My analysis of the relationship between election-related factors and state supreme court justices' decision-making in tort cases relied on the data produced by the State Supreme Court

Data Project. As noted previously, this dataset covers state supreme court decisions from 1995-1998 by state courts of last resort; if a court decided more than 200 cases in a given year, a sample of 200 cases was drawn, otherwise all of the cases were included (Brace and Hall 2000:263). For purposes of my analysis I created a version of the dataset that used the justice's vote as the unit of analysis, omitting the votes of judges sitting temporarily on the court. I obtained information on most of the individual level characteristics from a version of the SSCDP dataset that had previously been organized by justice-vote and that was posted on the Dataverse website; I then filled in as much of the missing information that as I was able to locate as well as correcting a number of coding errors I came across.⁵⁶ I selected from the dataset cases involving tort issues; because of the way that cases were coded this included a category that combined both workplace torts and appeals related to workers' compensation.⁵⁷ Overall, tort issues were involved in 21.7 percent of justices' votes with the states varying widely in this percentage, from a high of 36.6 percent in Alabama, to a low of 9.0 percent in Indiana. Overall, 37,116 of the 171,319 votes of identifiable justices involved tort issues, and 36,098 had information for all variables included in the analysis.⁵⁸

The dependent variable in the analysis is the justice-vote coded 1 as voting in favor of the plaintiff and 0 as voting in favor of the defendant; hence, positive coefficients indicate support for the plaintiff's position and negative coefficients support for the defendant's position. As predictors I included the following variables:

⁵⁶ I created my own version of the judge-vote dataset because I discovered a number of problems in the version on Dataverse that I was able to correct by creating my own version.

⁵⁷ Given that the interests involved in workplace torts and workers' compensation are essentially the same, grouping them together for this analysis does make sense. Omitting this category of cases did not appreciably change the results.

⁵⁸ One issue in the data set is that two categories of cases that do not appear to be torts got lumped into the two general tort issue indicators (there was one indicator for private cases and one for cases involving government); these were discrimination cases and a category described in the data documentation as "other labor disputes." I omitted these cases from my analysis.

- method of retention (partisan elections, nonpartisan elections, retention elections, no elections) as a set of dummy variables using partisan elections as the reference category, and grouping the hybrid states of Michigan and Ohio with the partisan states (Nelson et al. 2013) ;
- whether the justice was barred from running for reelection due to mandatory retirement;
- whether the justice was in the final 12 months his or her current term;
- a measure of the state's liberalism (Wright et al. 1985) , scaled as a z-score with a mean of 0 and a standard deviation of 1 using the mean and standard deviation for the original variable at the state-year level;⁵⁹
- a measure of the justice's ideology (Brace et al. 2000);
- whether the appellant was the original plaintiff;
- type of tort issue (automobile accident, premises liability, medical malpractice, products liability, toxic tort, work injury, other) as a set of dummy variables using toxic tort as the reference category for the analysis of all torts and automobile accident as the reference category when the analysis was restricted to personal injury cases);
- whether one party to the case was a governmental entity;⁶⁰
- whether the state lacked an intermediate appellate court that hears most civil appeals;⁶¹

⁵⁹ The analysis was also run using the measure of citizen liberalism developed by Berry et al. (2010; 1998); the results differed little from the analysis using the Wright et al. measure. For both of these measures, I merged in the indicators from the original sources.

⁶⁰ Unfortunately, there was no way to determine if the governmental party was the plaintiff or defendant; however, I believe it is safe to assume that in the vast majority of tort cases the governmental party would have been the defendant.

⁶¹ While North Dakota has a procedure that allows for an *ad hoc* court of appeals to hear an appeal, the procedure is seldom used. It is unclear whether the Court of Appeals in Utah hears a significant number of civil appeals other than in the family law area or in the form of appeals from administrative agency decisions; I ran the analysis both including Utah as not having an IAC that hears tort cases and as having an IAC that hears tort cases; the results did not differ appreciably.

- an index of the number of tort reforms in effect in the state in the year of the decision— see Kritzer and Beckstrom (2007:993-94) for a list of the 22 reforms included in the index;
- types of legal issues presented in the appeal, including separate, nonexclusive indicators of each of the following types of legal issues: abuse of discretion/clearly erroneous decision, evidentiary issue, failure to state a claim, summary judgment, standing, mootness/ripeness (includes failure to exhaust the administrative process), governmental immunity, and jury instruction issues.

Preliminary analyses indicated strong interactions based on whether the appeal was brought by the plaintiff or by the defendant; consequently, I used a conditional model (Wright 1976) which produced separate estimates for plaintiff appeals and for defendant appeals. Preliminary analyses also indicated that the influence of state ideology interacted with type of retention system; consequently, state ideology was included as separate terms for partisan/hybrid election states, nonpartisan election states, retention election states, and non-election states. Table 3B.1 shows summary statistics for the predictor variables separately for appeals brought by plaintiffs and appeals brought by defendants.

{ Table 3B.1 about here }

The analysis of votes/decisions of members of collegial courts presents methodological issues for which there is no definitive solution. Specifically, the observations are not statistically independent, and the dependence lies on multiple dimensions. The same judge/justice votes on many cases and any given case involves votes by multiple judges/justices. For state supreme courts, there may also be interdependence on the dimension of states as well. In fact, the variables listed above are all measured at one of three levels: state (type of selection system,

presence of an intermediate appellate court, and state liberalism) , justice (ideology, age-barred, and last year of current term), and case (government party, type of tort, legal issues in the appeal). For purposes of this analysis I used generalized estimating equations (GEE), an estimating technique which allows one to specify how observations should be grouped (Zorn 2001).⁶² Because there is no definitive answer to which method of grouping is correct or best, I ran the analysis twice, once grouping by justice and once by case. As the tables below show, the choice of grouping does make a difference in the actual estimates as well as in the standard errors of those estimates. This suggests caution in interpreting the results.⁶³

B. Results

The results of the GEE estimates grouping by judge are shown in Table 3.B2 and by case in Table 3.B3. The first pair of columns of each table shows the results for plaintiff appeals and the second pair shows the results for defendant appeals. Coefficients that are statistically significant at the .05 level or better are shown in bold and those significant at the .10 level are shown in italics; the predictor label is shown in bold or italic to indicate that there is a statistically significant difference between the coefficients for plaintiff and defendant appeals. The overall test for significant differences between the two models is shown in the last line of each table (labeled “Interaction: Wald chi square”).

{ Tables 3.B2 and 3.B3 about here }

⁶² In very preliminary analyses, I also used logistic regression with robust standard errors to adjust for clustering. The results of those preliminary analyses did not differ from the GEE estimates I obtained in those preliminary stages and I chose to rely on GEE as the estimating method for the final analyses. One apparent peculiarity of GEE estimation is that the results vary depending on how a model is specified in contrast of logistic regression where several different specifications, such as a model with multiplicative interaction terms and the equivalent model with conditional effects, produced mathematically identical results. Importantly, the variations I observed with GEE estimation were inconsequential vis-à-vis the conclusions to be drawn from the analysis.

⁶³ I also attempted to run a multi-level model but the software was not able to produce a solution.

Drawing clear cut conclusions about the impact of judicial elections from the results shown in the two tables is difficult. The reasons for this difficulty are the large differences between the results for plaintiff and defendant appeals and the differences between results for estimates using grouping-by-case and results for estimates using grouping-by-justice.

For example, depending on which side appealed, the impact of type of retention system on support for the plaintiff runs in opposite directions for some methods of retention, although the effects achieve statistical significance only for defendant appeals. Using partisan and hybrid elections combined as the reference category, for defendant appeals plaintiffs were least successful in partisan/hybrid states and most successful in nonpartisan and retention election states, with no-election states falling between. While for plaintiffs' appeals none of the coefficients for retention system differed significantly from zero, the least success for plaintiffs came in retention election states when grouping was by case. Regardless of which grouping method was used, the difference in the effect between defendant and plaintiff appeals for retention elections was statistically significant.

What about the impact of state political liberalism conditional on retention method? Using the grouping-by-judge results, state political liberalism has positive effects on the justices' voting in favor of plaintiffs who appealed in states using nonpartisan elections but negative effects in states not using elections for judicial retention; neither of those effects are apparent when it is the defendant who appealed (the coefficients are not only nonsignificant but are zero to two decimal places). In defendant appeals, political liberalism increased the likelihood of justices' favoring the plaintiff in states with partisan and hybrid elections; while the corresponding coefficient was positive for plaintiff appeals it was nonsignificant. Using the grouping-by-case analysis, the only significant coefficient for political liberalism is the positive effect of political liberalism for

plaintiff appeals in nonpartisan states, although the coefficients for partisan and hybrid election states is positive and would be statistically significant at the .10 level; the coefficient for no-election states is negative and would also be statistically significant at the .10 level. None of the political liberalism coefficients for defendant appeals achieves statistical significance when grouping is by case, although the actual value for partisan and hybrid election states is slightly larger than for the grouping-by-judge results. The only clear conclusion that can be drawn from these results is that the state's political liberalism does not appear to have any kind of *consistent* effect on justices' voting when considered in relation to the method of retention.

The two other election-related variables are whether a justice in a state using elections is barred from running for another term due to mandatory retirement and whether a justice in an election state who is not so barred but is in the last twelve months of his or her current term. Last year of term shows no effect on the justices' votes regardless of which side appealed or method of grouping. Being barred by mandatory retirement reduces the likelihood of voting in support of the plaintiff regardless of which side appealed when grouping is by case. Grouping by justice, this variable does not achieve statistical significance either for plaintiff or defendant appeals; the sign of the coefficients is negative for plaintiff appeals and the magnitude is about the same as when grouping is by case, but the sign is positive for defendant appeals which is the reverse of the results when grouping is by case. This inconsistency again makes it difficult to draw firm conclusions.

Several of the control variables merit comment. The direction of judicial ideology is as one would expect, with support for plaintiffs increasing as the justice's ideology moves in the liberal direction, although the coefficient does not achieve statistical significance for plaintiff appeals when grouping is by justice. The absence of an intermediate appellate court that routinely

handles tort cases runs in opposite directions depending on who appealed (although the coefficient is not statistically different from zero for defendant appeals when grouping by case), essentially indicating that the lack of such a court is associated generally with support for the respondent. A similar pattern applies when a governmental unit is a party to a case, although the coefficients are not statistically different from zero when grouping by case; the magnitude of the coefficients is about the same as when grouping by justice. While I could not discern whether the governmental party was the plaintiff or defendant, it would seem safe to assume that in the vast majority of tort cases involving a governmental unit, the government is the defendant. The level of tort reform that has occurred in a state does not produce a consistent pattern, although when grouping is by justice, the likelihood of voting to support the plaintiff increases in defendant appeals as the number of reforms that had been adopted increased.

Some of the indicators of case-type are statistically significant, although the pattern varies depending on the method of grouping and whether the plaintiff or defendant appealed. While all of the coefficients for are positive with the single exception of products liability in plaintiff appeals, it is important to take note that the reference category is toxic torts, and thus the positive coefficients indicate a higher likelihood of favoring the plaintiff in a non-toxic tort case than in a toxic tort case. It is interesting to note the coefficients for defendant appeals are generally higher than the coefficients for plaintiff appeals, but this probably simply reflects a broad pattern of the outcome of appeals more likely to favor the respondent.

Finally, some legal issue indicators are statistically significant, although the pattern varies depending on the method of grouping and whether the plaintiff or defendant appealed. Note that the legal issue variables do not constitute a set of exclusion categories; a case can raise one or

more of the issues. Here is what the estimates suggest expressed in terms of the likelihood of a vote favoring the plaintiff:

- plaintiff appeals: possibly increases when there is an issue of governmental immunity;
- plaintiff appeals: decreases when there is an evidentiary issue or a failure to state a claim issue, and possibly decreases when the appeal involves a standing issue;
- defendant appeals: increases when there is an issue of abuse of discretion, an evidence issue, of a mootness/ripeness issue.
- defendant appeals: decreases when there is a summary judgment issue or a governmental immunity issue.

Note that there is some variation here depending on whether grouping is by case or by judge. The more consistent patterns involve plaintiff appeals involving failure to state a claim, and defendant appeals involving either evidentiary issues or mootness/ripeness.

C. Conclusion

My general conclusion from this analysis is that there is at best some weak evidence that method of retention and other variables related to elections or conditioned on elections influence the decisions of state supreme court justices in tort cases. More specifically, the influences that the analysis does show do not run consistently to favor plaintiffs or to favor defendants; some of those influences run one way when the plaintiff appeals and the opposite way when the defendant appeals. Moreover, while the analysis does show both some statistically significant individual effects and that the overall set of coefficients differs from zero, one must keep in mind the large number of observations. Simply stated, the fit of the models is not particularly strong;

GEE does not typically provide a measure of overall fit, but if I fit a model using logistic regression the pseudo- R^2 is less than .02.

Thus, while it appears that selection/retention related issues show some relationship with the direction of state supreme court decisions in tort cases, it is hard to make an argument that there are causal relationships pointing in a consistent direction. Even though tort issues have been strong motivators for key interests in judicial elections/judicial selection, the inconsistent nature of the effects summarized above precludes drawing clear conclusions about how the retention-related factors impact state supreme court decisions in tort cases.

VIII. APPENDIX C: STATE APPELLATE DECISIONS RELATED TO SAME-SEX MARRIAGE, 1973-2012

NOTE TO EDITOR: THIS PAGE AND THE FOLLOWING TWO ARE INCLUDED IN THE TABLES SPREADSHEET

State	Year	Case	Decision
AK	1998	<i>In Brause v. Bureau of Vital Statistics</i> 1998 WL 88743 (1998)	State must recognize same-sex marriages performed in other states; mooted by constitutional amendment (state had previously passed amendment limited marriage to opposite-sex couples)
AK	2001	<i>Brause v. Department of Health & Social Services</i> 21 P.3d 357 (2001)	Whether statute precluding same-sex marriage was valid was not ripe for adjudication.
AZ	2007	<i>Arizona Together v. Brewer</i> 149 P.3d 742 (2007)	Rejected attack on state constitutional amendment on grounds that it was not single issue
AZ--app	2003	<i>Standhardt v. Superior Court</i> 77 P.3d 451 (2005)	Intermediate appellate court upheld state DOMA; SSC denied petition for review w/o comment (<i>Standhardt v. MCSC</i>)
CA	2004	<i>Lockyer v. City and County of San Francisco</i> 95 P.3d 459 (2004)	City officials lacked authority to solemnize same-sex marriages, which had no legal effect
CA	2005	<i>Knight v. Superior Court</i> 26 Cal.Rptr.3d 687 (2005)	Domestic partners statute did not violate constitutional requirement of voter approval for amendment of marriage initiative; enactment of domestic partnership statute was not legislative creation of "same sex marriage" under the guise of another name
CA	2009	<i>Strauss v. Horton</i> 207 P.3d 48 (2009)	Upheld validity of state constitutional amendment banning same-sex marriage that was passed by voters in Nov. 2008
CT	2002	<i>Rosengarten v. Downes</i> 802 A.2d 170 (2002)	Court did not have jurisdiction over complaint seeking dissolution of foreign same-sex civil union
CT	2008	<i>Kerrigan v. Commissioner of Public Health</i> 957 A.2d 407 (2008)	Struck down state ban on same-sex marriage
FL	2004	<i>Kantaras v. Kantaras</i> 884 So.2d 155 (2004)	Wife's marriage to postoperative female-to-male transsexual was void <i>ab initio</i>
GA	2006	<i>Perdue v. O'Kelley</i> 280 Ga. 732 (2006)	Rejected attack on state constitutional amendment on grounds that it was not single issue
HI	1993	<i>Baehr v. Lewin</i> 74 Haw. 645 (1993)	Reversed lower court dismissal of complaint from couple seeking marriage license
HI	1997	<i>Baehr v. Muike</i> 950 P.2d 1234 (1997) Table	Affirmed lower court ruling against state, but not effective because superseded by state constitutional amendment
HI	1999	<i>Baehr v. Muike</i> 994 P.2d 566 (Table)	Reversed lower court judgment; case now moot due to constitutional amendment
IA	2009	<i>Varnum v. Brien</i> 763 N.W.2d 862 (2009)	Struck down state ban on same-sex marriage
IN-app	2005	<i>Morrison v. Sadler</i> 821 N.E.2d 15 (2005)	Intermediate appellate court upheld state DOMA; not appealed to the Indiana Supreme Court
KY	1973	<i>Jones v. Hallahan</i> 501 S.W.2d 588	Female persons were not entitled to have issued to them a license to marry each other
LA	2005	<i>Forum for Equality PAC v. McKeithen</i> 893 So.2d 715 (2005)	Rejected attack on state constitutional amendment on grounds that it was not single issue

State	Year	Case	Decision
MA	2003	<i>Goodridge v. Department of Public Health</i> 798 N.E.2d 941 (2003)	State must allow same-sex civil marriage
MA	2004	<i>In re Opinions of the Justices to the Senate</i> 802 N.E.2d 565 (2004)	Banning marriage, but permitting civil unions, for same-sex couples violates equal protection
MA	2006	<i>Cote-Whitacre v. Department of Public Health</i> 446 Mass. 350 (2006)	Upheld prohibition on same-sex marriage performed for nonresidents whose home states did not allow same-sex marriage
MA	2012	<i>Elia-Warnken v. Elia</i> 463 Mass. 29 (2012)	In-state, same-sex marriage was void ab initio when one partner had undissolved civil union from other state
MD	2007	<i>Conaway v. Deane</i> 932 A.2d 571 (2007)	Upheld state's limitation of marriage to opposite-sex couples
MD	2012	<i>Port v. Cowan</i> 44 A.3d 970 (2012)	Valid, out-of-state same-sex marriage was cognizable in the state for purposes of application of state's divorce law
MI	2007	<i>National Pride At Work, Inc. v. Governor of Michigan</i> 732 N.W.2d 139 (2007)	Public employers' recognition of domestic-partnership agreements for employment benefits violated marriage amendment
MN	1971	<i>Baker v. Nelson</i> 191 N.W.2d 185 (1971)	Minnesota law "does not authorize marriage between persons of the same sex and that such marriages are accordingly prohibited." Also finds that the the statutory prohibition "does not offend the First, Eighth, Ninth, or Fourteenth Amendments to the United States Constitution."
MT	2012	<i>Donaldson v. State</i> 292 P.3d 364 (2012)	Appeal challenging every state statute excluding committed same-sex couples from protections granted to opposite-sex, married couples, denied; Court ruled that cases challenging specific statutes could move forward
NJ	2006	<i>Lewis v. Harris</i> 908 A.2d 196 (2006)	Found that same-sex couples were entitled to civil unions that granted all benefits of marriage; did not find that this meant state must allow same-sex "marriages" but left that as an option.
NJ	2012	<i>Garden State Equality v. Dow</i> 2012 WL 540608	Plaintiffs stated claim against defendants on the basis that Civil Union Act violated the Equal Protection Clause of the Fourteenth Amendment granted reconsideration
NM	2004	<i>Victoria Dunlap v. Patricia Madrid and Hon. Louis P. McDonald</i> Sup Ct, NM, July 8, 2004, index No. 28730	Affirmed and extended restraining order preventing the issuance of marriage licenses to same-sex couples but never ruled on the legality of licenses
NY-app	1997	<i>Storrs v. Holcomb</i> 666 N.Y.S.2d 835 (1997)	Challenge to same-sex marriage ban failed when necessary party was not joined (transfer by Appellate Division as direct appeal to NYCA previously denied (88 N.Y.2d 1063 (1996))
NY	2006	<i>Samuels v. New York State Dept. Of Health</i> 811 N.Y.S.2d 136 (2006)	There was rational basis for limiting marriage to one man and one woman
NY	2006	<i>Hernandez v. Robles</i> 7 N.Y.3d 338 (2006)	upheld state's limitation of marriage to opposite-sex couples

State	Year	Case	Decision
NY	2010	<i>Dickerson v. Thompson</i> 897 N.Y.S.2d (2010)	Trial court had subject matter jurisdiction over action for dissolution of same-sex civil union
NY-app	2008	<i>Martinez v. County of Monroe</i> 50 A.D.3d 189 (2008)	Appellate court upheld state official's decision that state would recognize same-sex marriages that were performed in states where they were legal; NYCA dismissed motion for leave to appeal
NY-app	2009	<i>Lewis v. New York State Dept. of Civil Service</i> 60 A.D.3d 216 (2009)	Recognition of valid foreign same-sex marriages for purposes of state employee health benefits was permissible
OH	2003	<i>In re Marriage License for Nash</i> 2003 WL 23097095	Public policy in Ohio prohibited post-operative female-to-male transsexual from marrying female
OR	2005	<i>Li v. Oregon</i> 110 P.3d 91 (2005)	Ruled that Oregon law limited marriage to opposite-sex couples and any same-sex licenses that had been issued were invalid
RI	2007	<i>Chambers v. Ormiston</i> 935 A.2d 956 (2007)	Family Court was without jurisdiction to entertain divorce petition involving same sex couple who were married in Massachusetts
TX-app	2006	<i>Ross v. Goldstein</i> 203 S.W.3d 508 (2006)	Appellate court would not adopt "marriage-like relationship" as equitable remedy upon death of one same-sex partner
TX-app	2010	<i>In the Matter of the Marriage of J.B. and H.B.</i> 326 S.W.3d 654 (Tx. 5th Cir. Ct of Appeal 2010)	Same-sex couple married in Massachusetts now living in Texas filed for divorce in Texas. State appealed after trial judge ruled that same-sex marriage ban violated 14th Amendment; three-judge appellate court unanimously reversed trial court. Petition for review pending before Texas Supreme Court as of July 2013.
VT	1999	<i>Baker v. State</i> 744 A.2d 864 (1999)	State must allow same-sex civil unions
WA	1974	<i>Singer v. Hara</i> 247 522 P.2d (1974)	Statutory prohibition against same-sex marriages did not violate constitutional provision that 'equality of rights and responsibility under the law shall not be denied or abridged on account of sex'
WA	2006	<i>Andersen v. King County</i> 158 Wash.2d 1 (2006)	Upheld state's limitation of marriage to opposite-sex couples

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TABLE 3.A1: REGRESSION MODELS TESTING IMPACT OF SELECTION METHOD ON SUPPORT FOR STATE COURTS

	Confidence		Very Confident		Support Index		Control	
	b	std. err.	b	std. err.	b	std. err.	b	std. err.
Type of Election	F=1.05, p=.384		$\chi^2=1.07$, p=.898		F=2.34, p=.054		F=1.49, p=.203	
Partisan	-.072	.075	-.195	.302	.006	.105	-.095	.073
Hybrid	-.012	.094	.131	.365	.340	.132	.071	.091
Nonpartisan	.070	.078	-.054	.299	.117	.109	.040	.075
Retention	.018	.068	-.126	.269	.135	.096	-.045	.066
No Elections	.000		.000		.000		.000	
Knowledge Level	.065	.017	.250	.068	.124	.024	.051	.017
Attention to News	.032	.012	.000	.053	.002	.017	-.010	.012
Confidence Scale	.216	.011	.656	.061	.164	.015	-.041	.011
Experience in Court	-.185	.047	-.315	.189	-.228	.066	-.028	.045
Party Identification	F=0.60, p=.548		$\chi^2=1.90$, p=.386		F=2.82, p=.060		F=0.39, p=.679	
Republican	-.035	.059	-.275	.229	-.179	.083	-.030	.058
Democrat	.031	.054	.007	.220	-.007	.076	-.045	.052
Independent	.000		.000		.000		.000	
Gender (1=male)	.041	.045	.533	.187	.160	.064	.075	.044
Income Scale	.043	.022	-.058	.091	.151	.032	.078	.022
Education Scale	-.008	.016	.000	.068	.031	.023	.041	.016
Race/Ethnicity	F=3.81, p=.038		$\chi^2=2.40$, p=.430		F=5.50, p<.001		F=3.80, p=.010	
Black	-.198	.080	-.494	.375	-.282	.108	-.122	.076
Non-Hispanic White	-.120	.075	.117	.319	-.352	.103	-.228	.072
Other	-.136	.098	-.322	.482	-.210	.140	-.025	.097
White	.000		.000		.000		.000	
Intercept	.463	.136	-5.601	.662	-1.543	.191	.845	.131
R ² or Pseudo-R ²	.336		.197		.241		.138	
n	950		950		808		904	

p<.05 p<.01 p<.001

TABLE 3.A2: REGRESSION MODELS TESTING IMPACT OF ATTACK ADS ON SUPPORT FOR STATE COURTS

	Confidence		Very Confident		Support Index		Control	
	b	std. err.	b	std. err.	b	std. err.	b	std. err.
Attack Ads Run	.020	.051	-.154	.201	.021	.072	-.001	.049
Knowledge Level	.070	.020	.213	.076	.139	.028	.052	.019
Attention to News	.043	.014	.055	.058	.006	.019	.003	.013
Confidence Scale	.221	.012	.642	.067	.162	.017	-.041	.012
Experience in Court	-.197	.052	-.315	.209	-.292	.072	-.063	.050
Party Identification	F=1.51, p=.222		$\chi^2=4.84$, p=.089		F=4.27, p<.05		F=0.16, p=.855	
Republican	-.097	.065	-.536	.254	-.252	.092	-.020	.063
Democrat	.003	.060	-.111	.240	-.039	.085	-.032	.058
Independent	.000		.000		.000		.000	
Gender (1=male)	.015	.050	.503	.204	.091	.070	.035	.048
Income Scale	.048	.025	.017	.101	.160	.036	.084	.024
Education Scale	-.014	.018	-.009	.074	.023	.025	.035	.017
Race/Ethnicity	F=2.70, p<.05		$\chi^2=1.32$, p=.724		F=4.94, p<.01		F=4.42, p<.01	
Black	-.201	.086	-.258	.385	-.261	.117	-.128	.081
Non-Hispanic White	-.163	.080	.256	.331	-.385	.109	-.267	.076
Other	-.083	.119	-.146	.598	-.095	.169	-.092	.116
White	.000		.000		.000		.000	
Intercept	.407	.138	-5.847	.697	-1.374	.197	.777	.131
R ² or Pseudo-R ²	.340		.194		.235		.122	
n	793		793		672		756	

p<.05 p<.01 p<.001

TABLE 3.B1: SUMMARY STATISTICS

Variable	Plaintiff Appeals			Defendant Appeals		
	Percent	Mean	Std. Dev.	Percent	Mean	Std. Dev.
Plaintiff won (dependent variable)	48.7%			43.2%		
Retention System						
Partisan & hybrid elections	21.6%			29.0%		
Nonpartisan elections	23.0%			21.2%		
Retention elections	34.6%			32.4%		
No elections	20.7%			17.4%		
Barred by mandatory retirement age	5.1%			6.0%		
Last year of current term	8.7%			10.5%		
State Political Liberalism						
Partisan & hybrid election state		-0.53	0.48		-0.60	0.42
Nonpartisan election state		-0.62	0.76		-0.30	0.74
Retention election state		-0.32	0.76		-0.23	0.75
No election state		0.60	0.81		0.61	0.81
Judicial Ideology		39.22	22.72		38.94	22.07
Tort Reform Index	605.0%			649.0%		
No IAC for tort cases	24.7%			14.7%		
Government Party	24.4%			21.7%		
Type of Case						
Toxic tort	1.3%			1.6%		
Auto (traffic) accident	12.1%			10.8%		
Products liability	6.0%			7.5%		
Premises liability	11.8%			11.5%		
Medical malpractice	9.1%			8.5%		
Libel	2.9%			3.6%		
Work injury	16.4%			16.2%		
Other tort	63.4%			64.6%		
Legal Issues in Case						
Abuse of discretion	30.4%			29.6%		
Evidentiary issue	29.5%			31.7%		
Failure to state a claim	5.3%			3.4%		
Summary judgement	22.4%			15.9%		
Standing	6.2%			5.2%		
Mootness/Ripeness	3.3%			2.6%		
Governmental immunity	4.4%			5.2%		
Jury instruction	6.3%			9.0%		
N	22,808			13,290		

**TABLE 3.B2: IMPACT OF ELECTIONS ON TORT DECISIONS
(GROUPING BY JUDGE)**

	Plaintiff Appeals		Defendant Appeals	
	b	se{b}	b	se{b}
Retention System				
Partisan & hybrid elections (reference)	0.0000	---	0.0000	---
Nonpartisan elections	0.1072	0.1179	0.3863	0.1048
Retention elections	-0.0413	0.1011	0.3218	0.0991
No elections	-0.0588	0.1107	0.1679	0.1162
Barred by mandatory retirement age	-0.1876	0.1150	0.1006	0.1474
Last year of current term	-0.0195	0.0563	-0.0281	0.0826
State Political Liberalism				
Partisan & hybrid election state	0.1304	0.0905	0.2619	0.1213
Nonpartisan election state	0.3413	0.0756	-0.0009	0.0632
Retention election state	<i>0.0731</i>	<i>0.0428</i>	0.0470	0.0660
No election state	-0.1855	0.0605	-0.0119	0.0687
Judicial Ideology	0.0004	0.0012	0.0045	0.0014
Tort Reform Index	0.0055	0.0122	-0.0436	0.0133
No Intermediate Appellate Court for tort cases	-0.1971	0.0808	0.2115	0.0888
Government Party	-0.1403	0.0475	0.1368	0.0541
Type of Case				
Toxic tort (reference category)	0.0000	---	0.0000	---
Auto (traffic) accident	0.2160	0.0915	0.3307	0.1361
<i>Products liability</i>	-0.0852	0.0976	<i>0.1967</i>	<i>0.1163</i>
<i>Premises liability</i>	0.0972	0.0945	0.3660	0.1341
Medical malpractice	<i>0.1682</i>	<i>0.0979</i>	0.2219	0.1394
Libel	0.2301	0.1058	0.1403	0.1473
Work injury	<i>0.0884</i>	<i>0.0466</i>	0.3834	0.0539
Other tort	0.0602	0.0960	0.1487	0.1273
Legal Issues in Case				
Abuse of discretion/arbitrary, capricious	-0.0165	0.0310	0.1113	0.0463
Evidentiary issue	-0.0939	0.0333	0.2417	0.0399
Failure to state a claim (Rule 12(b)(6))	-0.2360	0.0589	-0.0853	0.1082
Summary judgement	-0.0069	0.0383	-0.1895	0.0567
Standing	<i>-0.1042</i>	<i>0.0610</i>	0.0493	0.0997
Mootness/Ripeness	0.0205	0.0732	0.5731	0.1096
Governmental immunity	<i>0.1455</i>	<i>0.0775</i>	-0.3049	0.1042
Jury instruction	0.0668	0.0462	-0.0502	0.0621
Constant	0.0333		-0.7356	
n	22,808		13,290	
Wald chi square (26 df)	181.33		282.73	
Interaction: Wald chi square (25 df)	274.95			

Bold indicates statistical significance at the .05 level or better; italics indicates statistical significance at the .10 level. Variable names in bold or italic indicate statistically significant differences between the coefficients for plaintiff and defendant appeals. The dependent variable is coded 1 for a vote in favor of the original plaintiff and 0 for a vote in favor of the original defendant; hence positive coefficients indicate voting that favors the plaintiff.

**TABLE 3.B3: IMPACT OF ELECTIONS ON TORT DECISIONS
(GROUPING BY CASE)**

	Plaintiff Appeals		Defendant Appeals	
	b	se{b}	b	se{b}
Retention System				
Partisan & hybrid elections (reference)	0.0000	---	0.0000	---
Nonpartisan elections	0.0756	0.1187	0.3156	0.1551
Retention elections	-0.1615	0.1035	0.2766	0.1413
No elections	-0.1215	0.1233	0.1990	0.1710
Barred by mandatory retirement age	-0.1542	0.0364	-0.1861	0.0514
Last year of current term	-0.0036	0.0258	0.0517	0.0390
State Political Liberalism				
Partisan & hybrid election state	<i>0.2175</i>	<i>0.1308</i>	0.2753	0.1755
Nonpartisan election state	0.3008	0.0816	-0.1647	0.1132
Retention election state	0.0370	0.0673	-0.0045	0.0966
No election state	<i>-0.1346</i>	<i>0.0805</i>	-0.0428	0.1105
<i>Judicial Ideology</i>	0.0023	0.0003	0.0035	0.0005
<i>Tort Reform Index</i>	0.0142	0.0117	-0.0238	0.0169
No Intermediate Appellate Court for tort cases	-0.2044	0.0713	0.1494	0.1145
<i>Government Party</i>	<i>-0.1488</i>	<i>0.0864</i>	0.1030	0.1169
Type of Case				
Toxic tort (reference category)	0.0000	---	0.0000	---
Auto (traffic) accident	0.2170	0.2153	0.3158	0.3012
Products liability	-0.0344	0.2153	0.1723	0.3019
Premises liability	0.1252	0.2186	0.3352	0.3006
Medical malpractice	0.2437	0.2230	0.2113	0.3190
Libel	0.1785	0.2695	0.0929	0.3573
Work injury	0.0566	0.0931	0.3805	0.1211
Other tort	0.0887	0.2125	0.1358	0.2991
Legal Issues in Case				
Abuse of discretion/arbitrary, capricious	-0.0199	0.0681	0.1232	0.0895
Evidentiary issue	-0.0788	0.0661	0.2860	0.0868
Failure to state a claim (Rule 12(b)(6))	<i>-0.2368</i>	<i>0.1335</i>	-0.0826	0.2271
Summary judgement	0.0056	0.0752	-0.1805	0.1190
Standing	-0.0924	0.1227	0.0190	0.1774
<i>Mootness/Ripeness</i>	0.0247	0.1753	0.5818	0.2411
<i>Governmental immunity</i>	0.1523	0.1489	-0.2612	0.1888
Jury instruction	0.0874	0.1291	-0.0260	0.1371
Constant	-0.0966		-0.7782	
n	22,808		13,290	
Wald chi square (27 df)	106.54		145.92	
Interaction: Wald chi square (25 df)	102.32			

Bold indicates statistical significance at the .05 level or better; italics indicates statistical significance at the .10 level. Variable names in bold or italic indicate statistically significant differences between the coefficients for plaintiff and defendant appeals. The dependent variable is coded 1 for a vote in favor of the original plaintiff and 0 for a vote in favor of the original defendant; hence positive coefficients indicate voting that favors the plaintiff.

